OBSERVING AND DESCRIBING TEXTUAL "REALITY": A CRITIQUE OF THE CLAIMS TO OBJECTIVE REALITY AND AUTHENTICATION IN NEW CRITICAL AND STRUCTURALIST LITERARY THEORY, SEEN AGAINST A BACKGROUND OF FEYERABEND'S IDEAS CONCERNING PARADIGMS, DOMINANCE AND IDEOLOGY.

THESIS

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ABSTRACT

This thesis sets out to examine the claims to objective reality and authentication in New Critical and Structuralist literary theories, concentrating on their claims to "objectivity" and "scientific validity."

It examines the nature of these claims in the light of the original ideas proposed by some of the major New Critics and Structuralists in the development of their respective "sciences" of literary theory.

Taking direction from the nature of reality and objectivity shown by the theorists, the thesis then attempts an assessment of the validity of some of the original perceptions and presuppositions concerning scientific objectivity and reality. It proposes that inconsistencies within the literary theories resulted from the theorists' inability to grasp the complexity and fluctuating nature of the borrowed terminology and principles that they were using. It does so by taking a closer look at the development of some of the more influential physical theories and the philosophical ideas raised by these developments.

It then uses Feyerabend's work on paradigms, dominance and ideology to attempt an assessment of the reasons for the literary theorists' perceptions and presuppositions regarding objectivity and reality. This amounts to accounting for the specific scientific models chosen as bases, and also to accounting for the desire for the "scientific approach" at all.

Its conclusions give an indication of the extent to which these original errors contributed to the theories' necessary adaptations of perspective and eventual loss of influence, and emphasises the need for the total understanding of concepts in one field by researchers in other fields, especially if those concepts are to be used by the researchers with any degree of precision.

TABLE OF CONTENTS

CHAPTER TWO - STRUCTURALISM

CHAPTER THREE - SCIENCE

CHAPTER FOUR - FEYERABEND

LIST OF FIGURES AND TABLES

PREFACE

My interest in the relationship between the Arts and the Sciences began in 1985 when I wrote an Honours paper on Computer-Generated Poetry. A few comments made by Dr Nick Visser about Relativity and Heisenberg's uncertainty principle provided the necessary spark to investigate this subject further.

By the end of that year I had read Roger Jones's Physics as Metaphor and several papers written by the scientists of the twentieth century; these confirmed that many of the notions advanced by literary theorists in the scientific authentication of their work seemed to miss subtleties, and that they had made assumptions whose validity was questionable.

The investigation, which has continued for more than five years, has been difficult, with most of the difficulty stemming from prejudices against a study that investigates such issues. In a world that has realised the merit of interdisciplinary work and "holistic" approaches, and also demands rigorous justification of hypothesis, this is surprising and disheartening, yet also serves as a justification for such a thesis.

The thesis does not aim at a "Grand Unifying Theory," but tries to show the necessity for the understanding of concepts in one field by researchers in other fields, especially if those concepts are to be used by the researchers with any degree of precision.

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To V., O. & J.,

who,

in the short time allocated,

brought me happiness.

TEXTUAL NOTE

When referring to the specific theories, I have used "New Criticism," "Structuralism" and "Formalism" as opposed to a more general "new criticism," "structuralism" and "formalism." For Russian names, I have tended to the more conventional spellings, such as Shklovsky and Tynjanov, inserting these, where necessary, into quotations (within square brackets).

In addition, when discussing the substance through which the planets were once thought to move, I have used "ether" rather than "aether."

INTRODUCTION

Nature and nature's laws lay hid in night;
God said, "Let Newton be!" and all was light.

(Pope)

INTRODUCTION

Paul K. Feyerabend has presented the proposal that a strict adherence to truth is a form of intellectual slavery to a tyrant, one that restricts our considering other possibilities. Once, he argues, the tyrant was the Church; today it is science, or rather, the public and popular perception of science, with a strange mixture of words like precision, certainty, and objectivity that have become synonyms of science.

New Critical and Structuralist literary theorists were concerned with the degree of "objectivity" and "scientific validity" in their critical approaches. This thesis will examine some of those concerns and the resulting claims in the light of their original contexts, and in relation to the work of Feyerabend.

The thesis will attempt to show the extent to which the theorists' opinions concerning the specific nature of the text and the required methods of textual investigation, as well as many of the contradictions these opinions support, were influenced by their perceptions of the nature and status of science, objectivity and reality, and will give some indication of the way in which the initial contradictions led to overall weaknesses in the theories.

The discussion will be based on arguments and proposals from many leaders in the fields. It will look both at their theoretical standpoint, and the manner in which they attempted to implement their theory. It will attempt to show the extent to which these problems were, for the most, caused by the theorists' lack of knowledge concerning the exact nature of the ever-changing principles and terminology they borrowed from science and applied to their own fields.

The validity of many of their assumptions concerning science will be questioned by undertaking a fairly detailed account of many of the scientific principles and notions on which these assumptions are based, looking at both original scientific research, and more popular impressions of this research.

It will then attempt to show how Feyerabend's work, especially his insight into paradigms, dominance and ideology, gives us a greater understanding of the origins and development of New Criticism and Structuralism. By using Feyerabend's ideas to place these theories in their historical, scientific and philosophical context, it is believed that we may receive a greater insight into both how these points of view originated and also why these theorists felt that science provided an ideal worthy of imitation as the paradigm case of objective research. We will also be able to understand how the theorists' initial contradictions eventually grew into unresolvable issues, leading to their proponents having to alter fundamental principles or lose a great deal of credibility.

CHAPTER ONE - NEW CRITICISM

From this new cavil, if thou have the heart
To try, experiment shall set thee free -
That source whence all your science has to start

(Dante *Paradise* ii, 94-96)

No human investigation can be called a true science without passing through mathematical tests; and if you say that the sciences which begin and end in the mind contain truth, this cannot be conceded and must be denied for many reasons.

(Da Vinci *Notebooks* 8)

Plato found fault that the poets of his time filled the world with wrong opinions of the gods, making light tales of that unspotted essence, and therefore would not have the youth depraved with such opinions.

(Sidney - "An Apology for Poetry" 36)

Poets are the unacknowledged legislators of the world.

(Shelley - "A Defence of Poetry" 255)

CHAPTER ONE - NEW CRITICISM

1 Introduction

There are many difficulties that confront any historian of literary criticism who hopes to deal analytically with the theories of the critics associated with the term "New Criticism." The most important of these difficulties is the wide diversity in their attitudes, not least towards science and objectivity.

Problems are compounded by the lack of clarity over the application of the term "New Criticism" itself. The difficulties range from lack of agreement about definitions and explanation of terms, to the effects of the large chronological and geographical spread of the New Critics.

In The New Criticism (1941), John Crowe Ransom lists as New Critics I.A. Richards, William Empson, T.S. Eliot, and Yvor Winters, presenting a form of criticism that is a little "unsure, inconsistent, perhaps raw" (x). Ransom sees them as primarily concerned with poetic structures, but still suffering from an emphasis on the psychological and moral issues of poetry (xi); René Wellek, on the other hand, lists Ransom, Allen Tate, Cleanth Brooks, Robert Penn Warren, R.S. Crane, Charles Morris, I.A. Richards and William Wimsatt (1970), and identifies the New Critics with the "revolt against Positivism" associated with Russian Formalism, American Neohumanism, T.S. Eliot and F.R. Leavis (42). A similarity is also noted between the emphases of New Criticism and the archetypical appraisal by Northrop Frye in Anatomy of Criticism (1957).

There is clearly some conflict of opinions concerning the nature of New Criticism. This is embodied, in Allen Tate's discussion of Ransom's New Criticism, by his comment that "I do not know what the New Criticism is,...I merely acknowledge the presences of the myth" (1970, 169). Tate ends this essay by saying

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"The New Critics look alike as Mongolians look alike to me; as Mr Ransom might look, to the Mongolians, like the late Babe Ruth" (172).

In view of the diversity revealed, I cannot perform a detailed examination of all the New Critics' views on science and objectivity. I shall indeed concentrate the study on three of the critics most often referred to in connection with New Criticism and its origins: I.A. Richards, Cleanth Brooks and W.K. Wimsatt, although I shall, from time to time, refer to several other New Critics. Richards, Brooks and Wimsatt haven been chosen because of their generally recognised stature, and also because their writings tend constantly to refer to and deal with most of the issues pertinent to this discussion. Because the opinions on the subject are often stated rather obliquely, and show varying facets of their perceptions of science, it will often be necessary to quote fairly extensively, in order for my conclusions to be demonstrated.

2 I.A. Richards

2.1 Richards's view of science

2.1.1 Autonomy and Systemisation of Science

When Richards thinks of the term "science," he is "thinking not so much of Psycho-analysis and Behaviourism as of the whole subject which includes them" (1935, 88). In other words, his vision of science is uncommonly broad and comprehensive, incorporating without question the social sciences and psychology. Indeed, Richards's picture of science is characterised more by what it excludes than by what it contains. In Richards's mind, it is a category defined especially by its difference from, and contrast in all essentials with, the category of poetry. According to Richards, one of the major factors distinguishing science from poetry revolves around the question of autonomy. He criticises A.C. Bradley for his views that the world of poetry is "a world by itself, independent, complete, autonomous" (1926, 77). Richards sees the world of poetry as a part of an experience that the reader has with the poem, which, although it must be kept free of "contamination, from the irruptions of personal particularities" (78), "has in no sense any different reality from the rest of the world" (78).

Richards sees science, on the other hand, as an autonomous reference system that advances by destroying all else (255) in the sense of marginalising all realities that do not conform with its aim, and he goes on to say that although science "<u>on occasion</u>" may use human activity, it is anti-human in character, since it attempts to reduce the importance of the human factor in its results. The

> essential point, however, is that <u>Science is autonomous</u>. The impulses developed in it are modified only by one another, with a view to the greatest possible completeness and systemisation, and for the facilitation of further references. So far as other considerations distort them they are not yet Science or have fallen out of it [my emphasis] (266).

Here, as elsewhere, autonomy and systemisation are of paramount importance to the "systematic physical sciences" (1934, 232), while poetry resists all that is systematic. Science attempts to systematise the world, an activity which Richards sees to be intrinsically dehumanising.

<u>2.1.2 Certainty of Language</u>

Also related to the idea of autonomy is language, and the connection of language with belief and emotion. According to Richards, the sign of the lack of emotional content of scientific beliefs is that they "can be stated with greater or less precision, as the case may be, but always with the same form" (279-80).

Science deals with "certainties" over which there is "no scope for dispute," in contrast with the ambiguities in poetry (1974, 236-37). In poetry, ambiguities and tensions are innately present, and the belief in the doctrine of the poem is not essential for belief in the value of the poem.

These issues are tangled ones at best, and there is no certainty that Richards was clear in his mind on the central issues. Some critics of Richards believe that he confuses these issues beyond the grounds of acceptability.[1] His general opinions and sympathies are, however, plain enough. He believes firmly that the central point of difference between the two systems lies in the language they use.

The autonomy of science is entrenched by its particular use of the language. While science uses language purely for referential purposes, poetry uses language for emotive purposes (1926, 267). The scientific use of words, a later development of language (273), which is "only capable of directing thought to a comparatively few features of the more common situations" (131) is, of course, far inferior to any usage of them made by poetry.

Furthermore:

> For Scientific language a difference in the reference is itself a failure: the end has not been attained. But for emotive language the widest differences in reference are of no importance if the further effects in attitude and emotion are of the type required. (268)

This distinction between the types of <u>language</u> runs parallel to the distinction between scientific and poetic <u>beliefs</u>, and is one of the most lastingly influential of Richards's positions. In a too blasé statement in support of this position, Wellek and Warren say, "It is fairly easy to distinguish between the language of science and the language of literature" (1980, 22). They then go on to say that

> the ideal scientific language is purely "denotative": it aims at a one-to-one correspondence between sign and referent.

> The sign is completely arbitrary, hence it can be replaced by equivalent signs. The sign is also transparent; that is, without drawing attention to itself, it directs us unequivocally to its referent. Thus scientific language tends towards such a system of signs as mathematics or logic. Its ideal is such a universal language as the characteristica unversalis which Leibniz had begun to plan as early as the late seventeenth century. (22-23)

Further supporting Richards, Wellek and Warren argue that the language of literature, on the other hand, is characterised by ambiguities, homonyms, arbitrary or irrational categories, historical accidents, memories, and associations; "In a word, it is highly `connotative.' It conveys the tone and attitude of the speaker, wants to have a direct effect on the reader, and sound symbolism is very important to it" (23). They go one stage further, adding that this ambiguity, resulting from the concentration on the sign, is necessary to art because it is necessary, through its emotive ability, to "`fictionality', `invention', or `imagination'" which are the distinguishing traits of literature (24).

This view is supported by Morris Weitz. In "Art, Language and Truth" (1953), he examines the "Emotive-Referential" dispute about language in art. He says that essentially art is "a form of emotional gesture, a kind of stamping one's feet or clapping one's hands, but ever so nicely" (591). Thus emotive language is appropriate for art, whereas science should be confined to referential language.

2.1.3 Science versus Poetry

This point of view must be seen also in the context in which, by the word science, Richards unequivocally includes subjects such as physics, mathematics, psychology; and methods and attitudes such as behaviourism, autonomy, objectivity, systemisation, empiricism, and any other practice or attitude that may be even remotely connected to these. These are the definitions, descriptions and offspring of science, and they are differentiated from poetry by their use of language.

But more than conceptual differentiations are involved here; the two categories are actually enemies. Science is not merely distinct from the "human" language of poetry, it is at war with it (1935, 88-91). Richards laments the fact that the sciences "invade every province of our thought" (89), and it is at this point that he is "thinking not so much of Psycho-analysis and Behaviourism as of the whole subject which includes them" (88). Richards makes it quite clear that any influence of science upon literature is an abomination to be avoided at all costs.

2.2 Richards's views on poetry

2.2.1 Autonomy and Systemisation of Poetry

And yet, when Richards practises criticism, he seems to overlook his position as a social critic. After all he has said about science, and in spite of denials of the point by some scientists (Dingle 1949, 14), and omissions of it by some critics (Wellek 1978, 618-9), he wishes to establish his criticism as a science. He longs for the rapid advances in physics to be parallelled in psychology, "the science of the mind" (1935, 12), so that his theories of impulse and response, using the "science" of psychology, can be furthered. Indeed, where it touches upon literary values, his entire argument about responses to literature is founded on a quasi-Pavlovian account of the individual's psychological response to poetry. Although he admits that his nerve diagram (1926, 116) is simplistic and its limitations should be recognised (117) (and few opponents have ever allowed him to forget that nerve diagram), it is nevertheless a loose attempt at applying the science of psychology further than ever before to the interpretation of poetry; though in the process, he sinks into more of a causal morass than even the most physiological and materialistic of psychologists would countenance.

In Coleridge on Imagination (1934), he says that he is attempting to extract hypotheses from Coleridge's writings, and develop them "into a co-operative technique of enquiry that may become entitled to be called a science" (xii). This must be so, for he invokes the concept of autonomy, elsewhere rejected for poetry; and autonomy is, as we have seen, a basic characteristic of the scientific approach. He goes on:

> I may ... point out that one of Coleridge's clearest and most certain principles preserves the autonomy both of the poet and the critic. "Could a rule be given from without, poetry would cease to be poetry, and sink into a mechanical art." (xii)

Confusingly, it seems now that autonomy is needed to ensure that poetry remains an art; and yet he says further that he hopes to show that "Coleridge succeeded in bringing his suggestions to a point from which, with a little care and pertinacity they can be taken on to become a new science" (42-44).

Richards shows remarkably little reluctance to harnessing the label of science to other aspects of his critical project, too. In the Preface to the First Edition of The Meaning of Meaning (co-authored with Ogden) the authors show their aim as one of looking for a "theory of Signs" (1936, viii) known as a "Science of Symbolism" (ix) for poetry. He states further that "Criticism is the science of [the meanings acquired by words] and the meanings which larger groups of words carry..." (231-32).

At this stage some readers may wish to point out that poetry is not criticism, and that the contradictions are reduced by accepting that while Richards emphasises the distinctions between poetry and science, we may still have a science of criticism. This, however, is hardly feasible if we use Richards's description of science: it would mean taking an absolute, objective tool based on techniques of isolation and close scrutiny, one which is "anti-human in character" - dealing with certainties, using an inferior language only for referential purposes - and then using this tool to examine a linguistic artifact that is dependent for it its meaning on ambiguities and subtle innate tensions, and composed of a superior language whose purposes are emotive. In addition, contradictions in his views on autonomy have already shown themselves in the above quotations, especially those on his views on Coleridge. Hence the proposed separation of art and criticism offers no solution.

Richards takes his argument further. In the introduction to Practical Criticism (1929), he says that one of the aims of the book is "to provide a new technique for those who wish to discover for themselves what they think and feel about poetry (and cognate matters) and why they should like or dislike [poems]" (3). The method that he wishes to use is that of systemisation, and he feels upset and thwarted by the "astonishing variety of responses" (12) his surveys reveal to the poems. This variety does not deter him from reducing his experimental findings about student responses to a "scientific" order. Indeed, he manages to systematise the responses very much to his own satisfaction; he is even able to draw a remarkable flow diagram of the possible responses (21).

2.2.2 Certainty of Language

On the issue of the difference between the language used by science and literature, the debate becomes even more confused. Although Richards sees scientific language as inferior, and he values the part played by the "widest differences in reference" in poetic language (1926, 268), his references to this quality of poetic language are inconsistent. At times, indeed, ambiguity in poetry, to be expected as a natural consequence of employing language firstly for its "emotive" and "non-referential" qualities, is not seen as an attribute of poetic language, but as a problem. (This view is challenged by Empson in (1966) who welcomes this quality as essential, although he is adamant that ambiguity "is not satisfying in itself" (235) but rather as a component of an integrated whole). Tolstoy, for example, is slated by Richards for viewing poetry as communication: for the problem of ambiguity makes the discussion of poetry as communication "a waste of time" (1926, 207). Ambiguity is an obstacle to communication in poetry; we need to overlook the problems caused, and assume that we are mostly reading the same thing preferring the most likely alternative. He admits, however, "These assumptions which so densely obscure the issue raise innumerable practical difficulties both for criticism and for the construction of a theory of criticism" (207). He rounds off his argument with his examination of two interpretations of a poem by Wordsworth. In this examination, he makes the dismayed point that the two readers have read two "different poems," and then adds, "Neither [interpretation] would be uncharacteristic of Wordsworth, although doubtless the first reading is the one to be accepted" (208).

Although in defence of T.S. Eliot Richards does see some merit in a little ambiguity, it is still an obstacle to the reader's forming "the poem clearly and unambiguously in his mind" (291).

In the light of this, Practical Criticism follows exactly Richards's vision of science. Firstly, its methodology is that of an empirical experiment, one of which any laboratory technician would be proud. Secondly, the concept of removing the authors' names, titles and dates from the poems offered to the students is an attempt at isolating the objects under investigation, so that these external irrelevancies (irrelevant to what Richards sees as the "meaning" of the poem) do not cloud their judgments. Thirdly, in his commentary, Richards makes it obvious that there is only one right and many wrong answers, and differences in the responses merely indicate the extent to which the students have gleaned the Truth. Clearly, Richards's vision of a pre-eminent poetic language does not lead him to sanction subjective interpretations of linguistic facts.

Later in the book, Richards has a chapter entitled "Four Kinds of Meaning." This, however, should not inspire the reader to expectations of polysemy; Richards insists that he is merely looking at four aspects - Sense, Feeling, Tone, and Intention (181-183) that make up an objective "Total Meaning" (180). The line of enquiry is directed towards the objective and the absolute. Indeed, the whole purpose of practical criticism, in Richards's view, is to guide readers away from distractions or obstructions to an unflawed appreciation of the full univocal and determinate meaning of the poem.

The road towards an absolute attitude to the poem's valuation, of course, is begun very clearly in Principles of Literary Criticism (1926). He opens the discussion by saying that the "two pillars upon which theory of criticism must rest are an account of value and account of communication" (25), and the most important is value (25-7). What is ultimately needed, then,

> is a defensible position for those who believe that the arts are of value. Only a general theory of value which will show the place and function of the arts in the whole system of values will provide such a stronghold. At the same time we need weapons with which to repel and overthrow misconceptions. (36)

For the critic to "be a sound judge of values" (114) is of crucial importance. All these statements of value, of course, presuppose that an objective, "scientific" method of measuring value is available. Since valuation should ideally be both authoritative and complete, it must appeal to objective criteria. Richards's text is intended to outline the "objective" factors that might be involved, both in the poem itself and in the mind and spirit of the reader. His attempts to "psychologise" literary response and draw a link between the mind's and the poem's search for integrity do not, however, offer any reproducible scale or means for assigning value to particular poems.

As belief is related to autonomy, so the problems caused by the debate on autonomy are compounded by the discussions on belief.

The value of the poem depends on the psychological response to the stimulus provided by the poem, and yet neither the literature nor the psyche must hold beliefs. One of his few criticisms of Lawrence, for example, castigates the novelist and poet not for the removal of beliefs in literature, but for the replacement of old ones with new (1935, 82-84). Richards makes his view of beliefs in literature clear in statements like "the necessity for independence [in literature] is increasing" (86); and "we need no beliefs, and indeed must have none, if we are to read King Lear" (67).

This radical sanitising of poetic response from social and other beliefs - including beliefs about the nature of reality - is not finally convincing. Richards seems to ignore the intimate connection between beliefs and the reader's psychic states, and the inevitable effect of this upon response. It is hard to conceive how a psyche devoid of beliefs is supposed to respond to the stimulus of a poem. By instinct, presumably.

3 Cleanth Brooks

3.1 Objectivity, Absolutes and Science

For Cleanth Brooks, the problems in the debate over objectivity, "absolutes," and science are great. As with Richards, the majority of these problems are caused by the manner in which he uses the central terms.

In The Well Wrought Urn (1947), Brooks insists on the necessity of the absolutes good and bad when discussing poetry (198-99). He realises that this goes against a popular trend which encourages us to feel that we should no longer have absolute criteria with which to measure a poem, and that applying absolutes is viewed as egotism on the part of the critic. But, he argues, if we give up criteria of good and bad, then we "have begun to give up our concept of poetry itself. Obviously, if we can make no judgments about a poem as a poem, the concept of poetry as distinct from other kinds of discourse which employs words becomes meaningless...." (199). Thus, being able to judge a poem as good or bad is not only possible in terms of absolutes, it is crucial to maintaining a clear definition of the genre.

We may not agree with the logic thus far, but there are no special difficulties with understanding it as Brooks presents it. From here, however, the argument becomes confusing; especially so as Brooks admits that we now have a situation where we

> have no confidence in absolutes of any kind; we know too much about ourselves to rest happily in subjective judgments. We try, therefore, to be more objective, more "scientific" - and in practice we usually content ourselves with relating the work in question to the cultural matrix out of which it came. [my emphases] (199)

According to this, objectivity and science are seen as one - so far this conception is similar to that of Richards. But how does trying to be more "objective, more scientific" stand opposed to "absolutes"? Surely, they are generally taken to be synonymous? Equally as puzzling is Brooks's suggestion that "relating the work" to its (changing) "cultural matrix" is a sufficient kind of second-rate objectivity: especially since this cultural context would seem to be no less easy to specify with authority or precision in any individual case, as Brooks himself notes on page 210. The only resolution to this is to assume that here the use of terms like "objective" and "scientific" refer disparagingly to theories such as Marxism which attempt a direct correlation between the production and value of the art and the cultural values of the writer, and also between reading and valuation of the art and the cultural values of the reader. As far as Brooks is concerned, the judgments he has rendered are "not in terms of some former historical period and not merely in terms of our own: the judgments are very frankly treated as if they were universal judgments"

(199). Moreover, the absolutes of good and bad, as the value of a poem is decided by Richards, are based chiefly on the degree to which the poem has been able to "avoid certain faults: sentimentality, the use of stock responses, clichés, etc" (Brooks et al 1964, 277).

To add further problems to this delicate balance of words, in Understanding Poetry (1960), Brooks and Robert Penn Warren state that the language of science "represents an extreme degree of specialisation of language in the direction of a certain kind of precision" (7), that "science aims to make statements of absolute precision [my emphasis]," and that although poetry may have a precision too, it is of a completely different kind (10). The word "absolute" as it is used here, surely cannot mean in the total sense as it used above, for this gives science the ability to make universal statements, and directly contradicts what Brooks says above; the only sense in which it can be taken then, is meaning a "degree of absolute" within a narrow view of scientific objectivity. For Brooks and Warren, the "good" and "bad" of poetry are absolutes; they should never be viewed as expressions of a specific, cultural or other, frame of reference, and they are certainly never culture-bound interpretations of the poem: Brooks criticises F.A. Pottle for taking this culture-specific point of view in The Idiom of Poetry (see esp. chapter II). Pottle adopts it as a simpler and less problematic solution to the problem of finding evaluative reference points, yet Brooks feels that it would simply lead to dangerous critical relativism. He continues "Is it really simpler? Will it not actually involve us in more complexities than would any doctrine of absolute criteria?" (1947, 209).

As we can see, Brooks's absolutist position is a difficult one to establish from his argument and from his attack on emphasis on the cultural context. At moments, he may be implying that since "science" is intrinsically culture-bound in essence (as distinct from poetry), any reference to context is unacceptably "scientific" and alien. At other moments it seems not so much the culture or historical context that is bothering him, but rather the emphasis on the empirical, almost mechanical, correspondence between what is in the poem, and what exists in the culture.

A reader might be forgiven for supposing that Brooks settles on a compromise whereby he says neither and both. It seems that a strict empirical, almost mechanical correspondence of objects is fine, as long as it concerns itself only with what is important within the world of the poem, and does not extend itself to what is outside the poem: once it does this, it has become "scientific" and therefore an unacceptable method of criticism. By doing this, he implicitly rejects the possibility that his consideration of what is important in the poem, indeed, his entire approach, is in any way determined by his cultural and historical matrix.

To the extent that we can come to conclusions at all, Brooks's ideas on absolutes, objectivity, and science, therefore, may be extracted and summarised as follows:

1. Good and Bad are absolutes in poetry, and need to be used as criteria in order to preserve the concept of poetry as we understand it.

2. Scientific objectivity is really simply subjectivity that has been bound and determined by a cultural matrix. Poetry does not work within this realm, as it works with absolutes; science, however, works within this realm.

3. The aim of science is to make precise statements - indeed its language is suited to this, but because of point 2 above, these are not absolutes, but rather subjective statements determined by the specific cultural-matrix.

4. Poetry makes statements that invoke a completely different kind of precision, for poetry works outside the scope of cultural matrices, concerning itself with absolutes.

3.2 The way of poetry

In the light of the views above, we already have an indication of how Cleanth Brooks proposes to approach the poem. In the preface to Understanding Poetry, Brooks and Robert Penn Warren lay out their objectives. The manner of looking at a poem is to

> begin with as full and innocent an immersion in the poem as possible; to continue by raising inductive questions that lead students to examine the material, their method, and their relations in the poem - that is, to make an appeal to students' "understanding" of the poetic process; then to return students as far as possible to the innocent immersion - but now with a somewhat instructed innocence to make a deeper appreciation possible. (ix)

The scenario is similar to that envisioned by Richards. The poem is an object in front of a reader. All that is important is this object. The mind, "innocent," that is, uncluttered by any ideas and beliefs, or anything else that may be infected by cultural materials, undergoes a mysterious "immersion in the poem" - then, through a process of induction and consideration of poetic techniques, the innocence may become "instructed" and then return to the poem with new eyes. We must always bear in mind that "Poetry...is incorrigibly particular and concrete - not general and abstract" (68).

In short, the student approaches the poem as a scientist would approach a beetle, microscopically analysing each piece of the structure. This approach is necessary for absolutes to be invoked; the difference is, of course, that the scientist aims at an objectivity which is clouded by his cultural matrix. The reader, however, brings nothing with him to the poem. Well, almost nothing, Brooks contradicts, for he does concede (in contrast to Richards's comment on King Lear) that an understanding of "the heroic tradition in which revenge is held to be honourable" is crucial for the understanding of Hamlet (465).

The piece then goes on to describe how the process leads the students to "experience" the poem and eventually "understand poetry as a means of imaginatively extending their own experience and, indeed, probing the possibility of the self" (ix). The value derived from the critical analysis of the poem is measured by the success it has had in returning the reader to the poem - more experienced, and able to have an even more intense interaction (16). For, ultimately, when Brooks speaks of meaning, it is the meaning of the poem for the reader, based on his interaction with it, that is important (115). The reader must bear in mind that the "total meaning of the poem, then, is to be carefully distinguished from the event, real or imagined, that occasions the poem, as well as from the material of the poem or even from particular statements in the poem" (267).

Here Brooks deals with the importance, or rather, the lack of importance, of the origins of the poem. The history behind its composition, and the author's intention, are of no consequence whatsoever. As he shows in other arguments (e.g. "Literary Criticism: Marvell's Horation Ode'" (1974), 423), to suppose that what a poet put into a poem is what he intended to put into it is useless. (More will be said on the problem of intention in the discussion on Wimsatt below).

The interaction between reader and poem and the resultant deeper understanding by the reader of himself is something that falls outside the objective viewpoints of science, and so is easily accommodated by Brooks's view of poetry; yet, again, the problems encountered are similar to those encountered by Richards. Richards expects a psyche with no previous information to respond to a set of impulses from a poem; Brooks expects a mind that has no recourse to a cultural matrix to investigate the inner constructions of a poem, such as rhythm and rhyme, have the poem revealed as an organic whole (11), and then be able to examine its own understanding of the poem.[2] At the same time, this deeply personal and subjective interaction between the reader and the poem is perfectly reducible to a few objective absolutes.

But an understanding of a previously undiscovered object surely implies that some change of mental state has occurred, and the degree of change will give some indication of the degree of understanding. If the mind approaches the poem with no ideas or beliefs, however, then after reading, the mind contains only those ideas and beliefs gleaned from the poem. These ideas cannot even be challenged or manipulated, for there was nothing originally in the mind with which to compare them. The student, now the ideal Gradgrindian, has become a little pitcher into which ideas may be poured. This idealisation surely bears little relation to the situation of readers in the real world.

Nevertheless, safe in the knowledge that when he begins looking at a poem, he brings no beliefs with him, Brooks may now make absolute statements about poems. In Understanding Poetry, when examining Shakespeare's sonnet 73, (5-10), Brooks finds himself freely able to make precise statements about what the poet does and does not do, about what is literal and what is not, about what is superficial and what is not, and exactly how the "reader's imaginative involvement" (10) will lead to these interpretations. These statements are not inferior scientific objective culture-bound statements. In fact, they are not even opinions; they are absolutes, apparently infallibly intuited. One suspects, however, that Brooks has over-estimated his ability to know how every reader in the world will interpret the poem based on his belief in the clean slate with which the reader approaches it.

In The Hidden God (1963), however, there is a telling new development. Brooks looks at the work of five 20th century writers from a Christian perspective. This may surprise us, if we have been led by Brooks to look upon such contextual stand points as critical relativism, of culture-matrix-bound objectivity. It is not clear whether the judgments offered are absolutes, or whether they are meant to be seen purely in a Christian perspective. They can only be absolutes if the whole universe is Christian. If they are from one culturally-defined perspective, they are invalid, according to Brooks's earlier statements.

One may argue that because this book is written so much later than The Well Wrought Urn (1947), perhaps Brooks has modified his views a little. This, however, is not illustrated, for the analyses in The Hidden God continue to follow the familiar principles of a belief in the absolutes of good and bad and in the possibility of access to the total meaning of an organic whole.

Furthermore, the problem posed by The Hidden God had also been found in a similar form in Modern Poetry and the Tradition (1948). Although in this work, Brooks continues to concentrate on the internal structures of poems, he battles a little with outlining the raison dêtre of the book: that of examining the works of the modern (20th century) poets. When reading the preface, we find its argument antithetical to that put forward in The Well Wrought Urn. Although Brooks still demands absolutes, the modern poets cause a problem - they "demand a radical revision of the existing conceptions of poetry" so that they may be included, with difficulty, into the "traditionally accepted pattern" (vii). In the following pages (viii-x), he goes on to note that different groups of poets need to be looked at according to different principles, and that often modern poetry presents us with major difficulties because we try to judge it by methods not suited to it. Such arguments fit uneasily with earlier statements both because something other than immutable absolutes seems to be invoked here, and because tacitly he seems to admit to the relevance of cultural influences, not only in the poem, but also in the criticism.

These questions, and those asked of The Hidden God could, of course be resolved. We could simply point to changes in Brooks's position over time, and in the face of the demands of different materials. Such variation itself, however, seems to argue against the positions which appear to be taken for granted in all these works; these aspects relating to objectivity, absolutes and science.

4 W.K. Wimsatt

4.1 The Intentional Fallacy

William K. Wimsatt is not quite as well-known among students of literature as I.A. Richards or Cleanth Brooks. This is possibly because his fame seems to rest upon the two essays that he wrote with Monroe Beardsley, "The Intentional Fallacy" (1954b) and "The Affective Fallacy" (1954a) published in his The Verbal Icon (1954), and the general assumption seems to be that his other work is simply an extension of these essays. This is rather an unfair appraisal of his work, and I hope I am not encouraging this attitude by admitting that, unfortunately, most of what concerns me in his writings is to be found almost entirely in these two essays, or else in works that deal directly with the topics of these two essays.

In the Essay "The Intentional Fallacy" (sometimes referred to as the "Genetic Fallacy"), the authors expand on what they had written in a previous piece entitled "Intention for a Dictionary," where it was argued that the intention of an author "is neither available nor desirable as a standard for judging a work of art" (3). In "The Intentional Fallacy," they make five points: (i) information about the poet's intellectual life is not a criterion by which to judge the poem; (ii) whether or not the poet's intention succeeded is not important; (iii) judging a poem is "like judging a pudding or a machine. One demands that it work [my emphasis];" (iv) the speaker, not the author, is the most important; and (v) a revision by the author simply proves that his "former concrete intention was not his intention" (4-5). The argument of the rest of the essay is based on these points.

On intention, the argument is parallel to that of Richards and Brooks, although further elucidated. Richards's view of intention and historical background, as Patrick Cruttwell points out in "Makers and Persons," is exemplified in the experiment in Practical Criticism (504) where all "external" contextual indicators were omitted from the sample of poems, together with the poets' names. (Intention is, however, paradoxically included as one of the four kinds of meaning [see page 12 of this thesis]). An example of Brooks's view is his criticism of Maurice Kelly's "interesting book on Milton, This Great Argument" in "Literary Criticism: Marvell's `Horation Ode'" (1974). In that essay Brooks chastises Kelly for making the assumption that "Milton was able to say in Paradise Lost exactly what he intended to say and that what Milton supposed he had put into his poem is actually to be found there" (423). Intentions and supposition are superfluous conceptual baggage when what matters is experiencing the poem directly. Worse, they interfere with this mystical and sublime experience.

This is echoed in The Day of the Leopards (1976), where Wimsatt attacks the view that sees a poem as the manifestation of its source or something behind it (12). In a discussion of Gray's "Elegy," in "Genesis: A Fallacy Revisited" (1968), Wimsatt notes that although the historical background of Gray is interesting, "The poem itself, if it were anonymous, would be intact" (204). "In short, though cultures have changed and will change, poems remain and explain" (1954, 39).

In line with what he wrote with Wimsatt, in his solo work "On the Creation of Art," Monroe Beardsley comments that the value of a poem is "independent of the manner of production, even of whether the work was produced by an animal or by a computer or by a volcano or by a falling slop-bucket" (301).

4.2 The Affective Fallacy

After the importance of the writer had been removed, it was inevitable that the importance of the reader should follow. John Crowe Ransom had set the pace in his search for an "Ontological Critic" in The New Criticism (1941). Ransom concludes that the critic who has come closest to being truly ontological is Charles W. Morris, although even Morris falls short because of his inability to recognise that although there are strong links between art, science, and technology, the difference between science and art is that they use different signs. Ransom (drawing loosely on Peirce's linguistic vocabulary), believes that science merely uses signs, or symbols, which "have" semantic objects, or refer to objects. Art, however, has iconic signs which "which also resemble or imitate these objects" (284-85). He further believes that, "the icons are here in the mind, they are the mental images evoked. The technical use of language by the poet is one that lifts words out of their symbolic or definitive uses into imaginative or image-provoking uses" (285-6). Thus, for science, the concern is with placing signs on definite natural pre-existing objects "out there," whereas art uses signs more closely linked to the objects, but certainly not on a one-to-one simplistic correspondence (326).

Although Ransom identifies the road to be taken by New Criticism in its attempt at isolation of the poem, he himself does not follow that road successfully. He attempts to reduce the importance of the reader, but ends only a little way away from Richards; perhaps all that he has done is drop the strict Pavlovian stimulus-response of Richards.

A more rigorous attack on the reader is provided by "The Affective Fallacy" (1954b), where Wimsatt and Beardsley argue that just as the Intentional Fallacy is a confusion between the poem and its origins, so the Affective Fallacy is a confusion between the poem and its results (21).

They believe that good criticism is based on a rejection of both of these fallacies. This isolation of the poem goes beyond that proposed by other New Critics, as the authors reject Richards's theory on beauty and responses as part of the Affective Fallacy (28).[3] They make it quite clear that they believe that their "criticism of structure and of value is an objective criticism" (82). Furthermore, although the reader's role involves "psychological" factors, these have nothing to do with his personal mental history, but

> lie in the realm of what may be called public psychology - a realm which one should distinguish from the private realm of the author's psychology....Such a criticism, again, is <u>objective and absolute</u>, as distinguished from the relative criticism of idiom and period [my emphasis]. (82)

The emphasis in the above quotation draws attention to the authors' use of "objective" and "absolute" as they are generally used; as they are used by Richards, rather than by Cleanth Brooks. In spite of this, however, the approach to the poem is similar to the approach outlined by Brooks; the poem is an object under scrutiny, the history of the poem, intention of the author, and psychology of the reader are all considered "outside and irrelevant," for the "function of the objective critic is...to aid other readers to come to an intuitive and full realisation of poems themselves and hence to know good poems and distinguish them from bad ones" (83).

The attempt at an ontological view of the poem is taken up by most of the New Critics. (We have already seen how Cleanth Brooks demands an "innocent reader.") In the same vein, Eliseo Vivas argues: "A poem is a linguistic artifact, whose function is to organise the primary data of experience that can be exhibited in and through words. With the necessary changes, this can be said of all art" (1954, 578).

An unlikely earlier ally for the advocates of the isolation of the poem is A.C. Bradley, who considers poetry "in its essence," and, although he recognises that the "imaginative experience" that readers undergo is different for every reader, he deals with this very difficult issue by simply dismissing it with: "that insurmountable fact lies in the nature of things and does not concern us now" ("Poetry for Poetry's Sake" [1926, 4]). The poem's nature

> is to be not a part, nor yet a copy, of the real world (as we commonly understand that phrase), but to be a world by itself, independent, complete, autonomous; and to possess it fully you must enter that world, conform to its laws, and ignore for the time the beliefs, aims, and particular conditions which belong to you in the other world of reality. (4-5)

What all the theorists who reject Richards's psychology have in common, is that they either do not move far enough away from him to do without psychology, or when they do, then they are forced to treat the reader's mind as a blank slate.

4.3 Wimsatt on Science

Wimsatt's attack on science is based on his philosophy developed in "The Intentional Fallacy" and "The Affective Fallacy." Here his viewpoint is the same as Brooks's, and here he uses much the same terminology, and suffers much the same fate: a descent into contradiction. Wimsatt writes in "History and Criticism" that a reliance on historical perspectives is "a parody of the scientific method" (257). In his discussion of Northrop Frye in Hateful Contraries (1965), he expands on this. The major difference between his work and that of Frye, he notes, is that Frye places a great deal of emphasis on history as an "objective conceptual system," and is not prepared to accept absolute values in poetry. This leaves Frye, with his "scientific criticism," standing before us "in the shining white garment, the rubber gloves, of the anatomist - the passionately neutral dissector" (19). It is difficult to see how these differences between his and

Frye's views alone can make Frye's approach scientific, and his own clinical investigation of the object as a machine, unscientific.

He then presents what he calls a "Tensional" theory of poetry (35-36) which goes beyond looking at the mere "grammar" (meaning the so-called "literary devices" like personification and simile) in the poem, and advances through a four-stage process of explanation, description, explication and finally, appreciation (218-241). A scientific approach, then, is that approach which bases an assessment of the poem's value on any form of intention (including any implication of a historical importance) or affect.

The impossible situation resulting from the rejection of the subjective or emotional response has already been dealt with in depth. The rejection of any form of intention in order to ensure a rejection of science results in its own inconsistencies. The inconsistencies are not so much as a result of Wimsatt's denying the relevance of the poet's intention in the judging of the poem; they are the result of his denying the direct or indirect influence of the social background of the poet, while still, in his criticism, making reference to it as if it were important. And here Cleanth Brooks is just as much at fault. For example, in Literary Criticism: a Short History (1959), co-authored by Brooks, the authors write:

> The German and English Romanticism which we have seen in our last two chapters was a late reaction, or a slowly reached culmination of reactionary trends, against the claims of scientific rationalism which had begun to show strength during the later 17th century.... (413)

They then give further examples of the influence of past and contemporary science on Coleridge and Wordsworth (413). One wonders, when called upon to judge the poem as a pudding or a machine, how it could possibly "reflect the reactionary trends against the claims of scientific rationalism"; these are, one would have thought, extrinsic matters and so, in Wimsatt and Brooks's terms, scientific and irrelevant. Here Wimsatt's argument criticism, an act which, however, in no way makes poetry a science.

Other critics generally reject the influence of the subjective psychology of the reader on the poem's reading, yet fail to explain how an ideal reader with virtually no cultural mind is to understand a poem. They go further by denying any correlation between the historical background (and intention) of the writer and the art produced, on the grounds that these issues, or any considerable acknowledgement of their role, are scientific in character, not poetic. Yet, once having identified what they mean by science, the critics build most of these self-same scientific parameters into their practice, and set out to use those "scientific" methods to analyse the poem.

Furthermore, in their attack on the historical matrices, they never for once consider that any element of their own approach (e.g. rejection of stock phrases and clichés), may be culture bound, and a product of their historical circumstances. We recall periods when quotations and traditional poeticisms would be considered an ornament in a poem, for example. A common attribute (in Critical Practice (1980), Belsey calls it a weakness [19]) in their criticism is the focus on the text, uncluttered by theoretical perspectives, although they ignore the fact that even this is a principle in a theoretical perspective.

Furthermore, we have seen that to attempt a solution by drawing a rigorous distinction between an absolutist, objective scientific criticism and a subjective, non-scientific poetry, as supporters of Richards might be tempted to do, leads to further problems. We have seen that several of the critics reject the notion that New Criticism is a science at all. The fact that René Wellek can write: "None of the New Critics would have thought that their methods of close reading were `scientific' nor would they have identified criticism with close reading" (1978, 619) also indicates the degree to which the uncertainty of the term "New Critic" introduces complexities when discussing the critics.

It is evident that because of inconsistencies and sometimes rather casual use of terms like "science," "objective," and "absolutes," the direct relationship between science, poetry, the theory of criticism, and the practice of criticism is very unclear in New Criticism. The divergence frequently occurring between critical principle and critical practice further complicates matters. In theory, the focus of the attack is often science; although they use as their banner the alternative focus on the text, when they actually perform an evaluation of a text, science and the "scientific" still remain the standard for accuracy and universalising of the results.

In this chapter my chief concern was an examination of the view of science and objectivity espoused by the theories of New Criticism. As will be seen in Chapter Three, the theorists of New Criticism were invariably working with presuppositions about the nature of science which were anachronistic and outdated by the most advanced standards of their own day, and yet had been absorbed into the cultural unconsciousness over the previous century. Only by a certain attention to period and culture and linguistic context in this regard is one able to see the degree to which the New Critics' use of a particular (often unexplained) scientific vocabulary was itself a product of the time and place in which they lived, its own special predispositions and blindnesses. Since it is hard to comprehend the confusion that reigns in their own references to science, whether as anathema or as methodological standard, without paying some attention to the inherited cultural traditions about science and interpretations of science's role which led to this double vision, some attention will be given to the broader cultural and linguistic connection in the next two chapters.

CHAPTER TWO - STRUCTURALISM

> Models are neither true nor false; their value is judged by the contribution they make to our understanding of the systems they represent.
>
> (McMillan and Gonzalez, 7)

> While you and i have lips and voices which
> are for kissing and to sing with
> who cares if some oneeyed son of a bitch
> invents an instrument to measure Spring with?
>
> (e.e. cummings)

> You, I think, for all your cleverness, have failed to grasp the truth; you have not observed how great a part geometric equality plays in heaven and earth, and because you neglect the study of geometry you preach the doctrine of unfair shares.
>
> (Plato *Gorgias* 508)

CHAPTER TWO - STRUCTURALISM

1 Introduction

The establishment of Structuralist literary theory[1] resulted from the convergence of several scholarly traditions, stretching over at least two thousand years.

Two major areas examined by Structuralism, the social and the linguistic implications of art, had been considered by Plato and Aristotle respectively, in ways that anticipate aspects of Structuralist thought. As Todorov points out, the origins of semiotics and related issues began as early as Aristotle (Poetics), Plato (Cratylus) and Sextus Empiricus (Outlines of Pyrrhonism and Against the Logicians) (1982b, 15-59).

Terence Hawkes (11-15) notes that the first "scientific" work of a structuralist nature was Giambattista Vico's The New Science. Vico argues that man creates the myth of the social institution or activity based on the structures he perceives within his own mind, and which are implicit in his language. These become entrenched and viewed as "natural," often serving as an unperceived basis for his philosophies and religions. He then makes himself subject to the myths of his own creation and lives according to them. Vico says:

> We shall show clearly and distinctly how the founders of gentile humanity by means of their natural theology (or metaphysics) imagined the gods; how by means of their logic they invented languages; by morals, created heroes; by economics, founded families, and by politics, cities; by their physics, established the beginnings of things as all divine; by the particular physics of man, in a certain sense created themselves... (367)[2]

In this century, the detailed ground work for Structuralist literary theory was laid by the works of Ferdinand de Saussure (Course in General Linguistics [1959]) and Claude Lévi-Strauss (Structural anthropology [1963]). From these linguistic and anthropological developments, Structuralism evolved by a process of assimilation and repudiation. Influenced by related theories, especially Formalism, it searched constantly for a model that would benefit from the unifying principles learnt from Saussure and Lévi-Strauss, and also be able to mature into a Structuralist literary theory that could properly call itself a Structurally-based science of literature. In spite of its diversified origins, the issue of its being a science, or at least growing into one, was seldom disputed; in fact, most of its critics (such as some of the New Critics already mentioned) criticised Structuralism specifically because it was a science. It is upon this firm belief that Structuralist literary theory bases its authenticity, so much so that when its relationship to science was questioned, so was its validity as a literary theory.

2 Ferdinand de Saussure

Ferdinand de Saussure's major contribution is a work that he did not actually write. Course in General Linguistics[3] (1959) was created from lecture notes taken by his students. In spite of this, the work very clearly lays out Saussure's ideas, and is generally accepted as Saussure's writing.

The work begins with the sentence "The science that has been developed around the facts of language passed through three stages before finding its true and unique object[*1] [my emphasis]" (1). The rest of the chapter is a history of the development of the study of language into the science of linguistics. Roy Harris, however, notes that "the reader is never told exactly what the requirements for a `science' in Saussure's sense actually are" (3). Moreover, he adds that Saussure also never tells what he means by "true and unique object" (4).

A key to the latter mystery is supplied by Saussure when he writes "The concrete object of linguistic science is the social product deposited in the brain of each individual, i.e. language"*[2] (1959, 23). In this quotation, as elsewhere (6-7, 9), Saussure draws attention to the fact that language is to be looked at as a social product. Thus it is both the result of, and (through the act of speaking) the means to social interaction; hence, unavoidably, the symbiotic relationship between language and society is to be studied. In order for this to occur, however, it is necessary to have a grasp of the elements and constituents of the language. More important, it is necessary to understand the relationship between these constituents. This also gives the reader an insight into what Saussure means by the word "science," for the methodology of understanding the "structure" of language is the essence of linguistics as a science.

Although many of the terms are changed and are viewed in more detail by later Structuralists, Saussure's work remains the basis for this study, and it is necessary to grasp the fundamentals in order to understand the aims of Structuralist literary theory. Even more important in terms of this study, Saussure's actual practice must be taken as an example of his method, since it is his methodological procedure which becomes the model for scientific inquiry as Structuralism was later to construe it.

Figure 1 is based on Saussure's various arguments and diagrams in Course in General Linguistics (1959). It does not show all of the components of the system, but it is detailed enough for this study.[4]

The attention that Saussure gives to the linguistic sign cannot go unnoticed, and is certainly understandable. The relationship between a word and the object designated by it had already received some attention by scientists such as Galileo and Descartes (Drake; Reiss; Romanowski), and a great deal of work had already been done on the classification of signs, probably the most exhaustive being the study by C.S. Peirce between 1867 and 1910

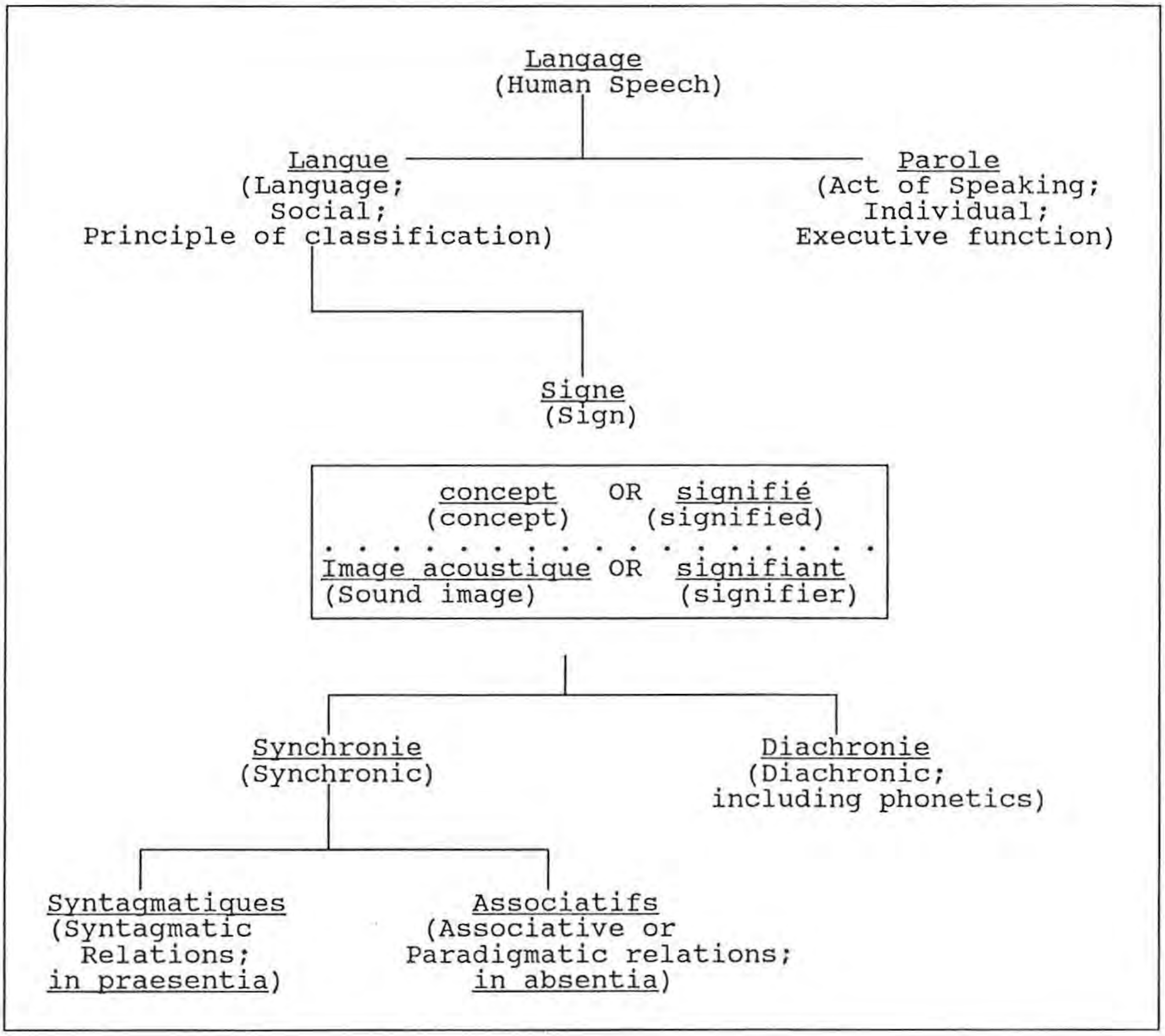


Fig.1: Simplified chart showing relationship of linguistic terms used by Ferdinand de Saussure.[4]

(1931, 291, 545-559; 1932, 230-2, 308; 1933, 551; 1958, 333-379). Rather than simply classifying the signs, Saussure examines them from a different perspective. In the first place, he sees the language sign as part of a general developing science of signs in society. He calls this science semiology (from the Greek semeîon "sign"), and says it would attempt an understanding of the laws that govern all signs (15-17, 68). In addition, not only does the study of the components of the linguistic sign offer a clue to a methodology for linguistics in general, but the sign is also the component upon which the importance of all other linguistic components depends.

The science of linguistics is not interested so much in the individual elements of the system, but rather in the interrelational structures of the elements. The element that facilitates this structure in linguistics is the linguistic sign: the connection of the more fundamental components (signifier and signified, for example) in the sign subtends the connection of the isolated linguistic signs into the largest linguistic unit, the sentence (13).

The sentence is the ultimate linguistic communication, but it can only be effective as an expression if every sign in that sentence adequately reflects all the other aspects in the specific social institution it represents or in the social scheme at large. Put another way, in the terms of Vico, the social function of communication can only be effective if the individual components of that communication adequately reflect the myths that underlie the function.

A noteworthy point which unfolds in this thesis, and is a hallmark of most other Structuralist approaches, is the absence of evaluation. Structuralism neither condemns nor praises. It simply exposes. The nature of the components is immaterial; Structuralism is, in its own eyes, a systematic science, and simply seeks to identify the relationship between all the components, in order to understand the structure of the system with regard to its society.

3 Claude Lévi-Strauss

Although Saussure recognised that there was a similarity between the study of linguistics and "human sciences" such as sociology and psychology, he insists on linguistics being set apart from "anthropology, which studies man solely from the viewpoint of his species, for language is a social fact"*8 (6). This is somewhat ironic, considering that the major area to be influenced by Saussure's work, and in turn to contribute immensely to Structuralism, was the anthropological work of Claude Lévi-Strauss.

This irony in no way invalidates either Saussure's notions nor Lévi-Strauss's ideas in Structural anthropology (1963). What interests Lévi-Strauss is not so much the connection between the subject of linguistics and the subject of anthropology, but rather the scientific structural approach to linguistics as an indication of the proper approach that one should take in anthropology.

Lévi-Strauss makes use of the concept of structures, taking note of the work by Vico and Saussure, and also the early work of Roman Jakobson (Donato 1967, 553-4). He does not, however, attempt simply to substitute his terms for Saussure's. In fact, as we shall see later, he feels that his insistence against this is crucial to the science of anthropology. In addition, Lévi-Strauss takes note of Trubetzkoy's belief that the study of linguistics should concentrate on the unconscious and should focus on attempting to analyse the relations between terms in a linguistic system in an attempt to understand the laws by which the system is governed (33; Trubetzkoy 1969, 1-4).

Although Lévi-Strauss is obviously primarily concerned with an anthropological study, his comments on the science of linguistics and upon structural approaches to intellectual activities in general, as well as upon signs and social myths, have added to Saussure's work to form a basis for Structural literary theory.

3.1 The Science of Linguistics

As with Vico and Saussure, Lévi-Strauss is convinced that linguistics is a science. He says that it is the social science in which most progress has been made. Moreover, it is "probably the only one which can truly claim to be a science and which has achieved both the formulation of an empirical method and an understanding of the nature of the data submitted to its analysis [my emphasis]"*9 (31).

From this quotation, we can make two important deductions. Firstly, although a subject may qualify as a "social science", that does not necessarily make it a true science - or a "taxonomic" science similar to the zoological sciences (Culler 1975, 26-27). Secondly, what would make it a true science (as happens with linguistics) is its empirical method (again argued by Lévi-Strauss in 1970, 11) and the degree to which it can "understand" (i.e. process and order) the relevant information. It must aim towards "objectified thought" (11). This approach will allow linguistics to play "the same renovating role with respect to the social sciences that nuclear physics, for example, has played for the physical sciences" (1963, 33).

Probably the major difference between a social science and a true science, Lévi-Strauss believes, is the fact that in a true science the observer does not in any way affect the subject under observation, whereas in social sciences, the observer has a direct effect on the object he is observing. For this, Lévi-Strauss quotes Norbert Wiener, who points out that the astrophysicist cannot influence his object, and that the atomic physicist is only interested in general and average trends. Thus the "effect of the bias of the observer plays no role" (55-56).

Although Lévi-Strauss has quoted Wiener correctly, he has ignored the qualifications that Wiener has made, such as when he admits that in atomic physics "it is true that anything we do will have an influence on many individual particles which is great <u>from the point of view of the particle.</u>" Wiener also admits that there are "some exceptions" to the generalisation concerning "average mass effects" (Wiener 189-190). These omissions are themselves clues to the presuppositions about science adopted by Lévi-Strauss, and to their absolute character for this thinker, and will be dealt with in more detail in the next chapter.

Lévi-Strauss goes on to say that the laws that govern terms of kinship at a "`micro-sociological' level" must be sought for beneath the appearance in the same way as "the linguist discovers his at the infra-phonemic level or the physicist at the infra-molecular or atomic level" (35).

For the moment it is important to note that when Lévi-Strauss says that linguistics is set apart from other social sciences because linguistics more closely resembles a true science, his image of a true science is a rigorously objective and exact atomic physics. He evidently sees no essential disparity between the kinds of science in which linguistics and atomic physics are engaged. The image he adopts, however, is not necessarily the vision of their own activity which advanced physicists always endorse. The implications and limitations of this attitude will be looked at in more detail in the next chapter.

Lévi-Strauss realises and acknowledges that his science works with limited (i.e. not universal) information. He sees this as no handicap, however, for a linguist, too, needs less than total information to understand the grammar of a language. Thus, although he is aiming for the ideal of an empirical and objectified science, he also makes it quite clear that he understands that there is no final truth: the "scientific mind does not so much provide the right answers as ask the right questions" (1970, 7).

He also says that he does not wish to "drive the sociology of the family toward a sterile empiricism, devoid of inspiration" (1963, 51). This admission, which seems to allow for an irreducible imaginative component in scientific inquiry, does seem to introduce a contradiction; unless one is prepared to compromise by saying that the scientific approach is rigorous, exact and systematic, but its absolute relationship to reality may always be questioned. After all, only if a gap exists between the scientific description and the true state of affairs beyond it is there room for "inspiration" and variety. If, however, this relationship between description and facts is indeed always open to question, then emphasis upon a scientific approach in the first place seems less justified, or, at any rate, less logically consistent. This issue, too, will be looked at in detail in the next chapter.

I mentioned above Lévi-Strauss's rejection of the notion of a one-to-one correspondence between linguistics and anthropology. Although he makes statements claiming that prohibitions such as incest, in a social structure, "constitute a misuse of language" (i.e. an incompatibility of the components of the kinship system as regards the overall structure of the kinship system) (1969, 495), he also says that we cannot simply transpose arguments in linguistics into anthropology, for this will not work as it fails to meet the requirements of a "truly scientific analysis [which] must be real, simplifying and explanatory" (35ff).[*10]

The term "real" in the above list must be read with some sophistication, as we have already begun to see. More important is his emphasis on the explanatory effect that may be produced by laying bare the simple underlying relationships that consistently appear between elements.

To ignore the relationship between the terms, he says, would be to miss the underlying structure: this was the error of both traditional linguistics and traditional anthropology (46).[*11] We must, however, bear in mind that a "kinship system does not consist in the objective ties of descent or consanguinity between individuals. It exists only in the human consciousness; it is an arbitrary system of representations, not the spontaneous development of a real situation" (50).*12

This statement raises an important issue. If reality is based on or at least identified by the structure, and the structure is something created within the minds of human beings, then (in agreement with the postulation of Vico) the arguments suggest that humans are creating their own reality. If this is the conclusion we are to draw - and there seems to be little to prevent us from doing so - then clearly "science" and "scientific method" are being used in an unusual and novel context. In few of the passages in which he valorises a scientific approach, however, does Lévi-Strauss make absolutely explicit that this is a new valuation of the terms employed.

Lévi-Strauss should be given the credit for seeking "to transcend the contrast between the tangible and the intelligible by operating from the outset at the sign level" (1970, 14). In other words, his vision of science extends considerably beyond the materialism of nineteenth-century physics, and is not to be identified with it (see Chapter Three). And yet there are still ambiguities in his employment of the scientific label. David Lodge touches upon this topic when he tries to define the Post-Structuralist stance towards Lévi-Strauss (1988, 107-108). He sees Derrida, in particular, as criticising his Structuralist predecessor for actually realising, on the one hand, that "there is no such ground" as might make his anthropological researches properly scientific in character, while yet, on the other, failing to draw the radical conclusions that inevitably followed. According to Lodge, Derrida

> sees Lévi-Strauss as making this disconcerting discovery in the course of his researches, and then retreating from a full recognition of its implications. Lévi-Strauss renounces the hope of a totalizing scientific explanation of cultural

> phenomena, but on equivocal grounds - sometimes because it is impossible (new data will always require modification of the systematic model) and sometimes because it is useless (discourse is a field not of finite meanings but of infinite play). (1988, 107)

Certainly, Derrida presents Lévi-Strauss as having failed to break entirely free of the empiricist conceptual baggage.

> I have said that empiricism is the matrix of all faults menacing a discourse which continues, as with Lévi-Strauss in particular, to consider itself scientific. If we wanted to pose the problem of empiricism and _bricolage_ in depth, we would probably end up very quickly with a number of absolutely contradictory propositions concerning the status of discourse in structural ethnology. On the one hand, Structuralism justifiably claims to be the critique of empiricism. But at the same time there is not a single book or study by Lévi-Strauss which is not proposed as an empirical essay which can always be completed or invalidated by new information. (Derrida 1978, 288; Lodge 117-118)

3.2 Signs and social structures

We have seen that Lévi-Strauss continually emphasises that he does not wish to perform a simple substitution of the terms in the linguistic model for their approximations in his science of anthropology. We have also seen, however, that he has not managed to examine the anthropological structures without recourse to the _actual_ concepts of linguistics. One finds that he constantly refers not only to the structural relevance of Saussure's linguistic system, but also to the actual terms such as synchrony and diachrony and the arbitrariness of the sign (208-09), and in fact to the entire concept of language as a social institution (even referring to elements of the social structure as "signs" [67-80; 1969, 493-96]).

Nonetheless, this dependence of anthropology on linguistics has not precluded influence in the other direction. Structural linguistics was seen as the source model for a scientific anthropology, and yet anthropology had begun to contribute to the field of linguistics, not merely in supplying insights into the structures, but supplying content, especially in areas such as associative linguistics. As studies in each field increase in depth, it becomes ever-increasingly obvious that the linguistic and cultural structures are interdependent. To this extent, then, Lévi-Strauss's insights were fruitful, and Saussure's vision of a comprehensive semiotics was borne out by the workable nature of his method and the illuminating results obtained.

Structuralism began in the area of linguistics, moved to anthropology, and was able to accommodate concepts from both as it grew. In spite of the growth, it did, as we have seen, contain within itself a contradiction, or at least a tension: between an appeal to a rigorous, objective, scientific, Structural model comparable to pre-quantum atomic physics on the one hand, and on the other hand the knowledge that the structures (and thus, the reality) being examined were products of the human observer. One might expect that, into whichever field Structuralism developed next, it would carry this unresolved contradiction: arguably this is exactly what occurred.

In spite of this inherent tension, we can recognise that Structuralism had united the Aristotelian emphasis on classification, and the Platonic belief that forms and structures were more significant than appearances, into a conception of scientific method that was not simply empirical. It sought to investigate a human reality that it assumed contained and would reveal structures, by applying to it the ready-made structures derived from linguistics. On the premise that all institutions could be understood as signifying systems, it applied to them the grid offered by linguistics in order to reveal their systematic nature. "Science" and "system," "scientific" and "systematic" were thus seen as interrelated concepts. The ontological status of the systems revealed, however, were not deeply investigated. Their reality and importance were, in fact, assumed without much questioning.

Though other human institutions might be arbitrary, Saussure's linguistics seemed not to be. His analyses were accepted on faith, as it were, for their clarity and apparent logic. Hence, scientific authority was given by the degree to which any branch of semiotic study could approximate to the Platonic form offered by linguistics. Science no longer meant induction and experiment, but the application of a pre-ordained method to new and diverse facts. Since linguistic structures were paradigms for scientific activity, no-one sought to ask how the scientific character of Saussure's own system might in turn be established and assured, or debated on what grounds its claims to "scientific" certainty might be theoretically secured. It seemed self-evidently the simplest case, the basic set of "atomic" building blocks, on which other studies could be reared, as chemistry depended on physics.

4. Formalism

4.1 The Formalist Model

The crucial step of a development from Structural linguistics to a "scientific" Structural literary theory was provided by the Russian Formalists. Although there was a wide diversity of theorists grouped as Russian Formalists, and the theory borrowed perspectives from many other fields, most notably Saussurian linguistics (Peter Steiner 59-61), the Russian Formalists were united in seeking an objective and "scientific" understanding of literature, or rather, in Roman Jakobson's terms, "literariness" (literaturnost). Here they anticipated and provided a lead to the scientific and systematic ambitions of later Structuralists.

Although much of the Formalists' work was done before the 1920's, because of the degree of exclusion from Western

Structuralism, it was only after 1920 that the notions of a scientific Formalist linguistic and literary theory began to have a direct effect on Western Structuralism. In 1917, Victor Shklovsky praised Leo Jabunsky for writing "one of the first examples of scientific criticism [in which] he indicates inductively the contrast...between the laws of poetic language and the laws of practical language" (1965, 11). Shklovsky's article, itself, is concerned with "laws of expenditure and economy in poetic language" (11). In his essay "Potebnya" he says "The creation of a scientific poetics must begin inductively with a hypothesis built on an accumulation of evidence" (qtd. in Eichenbaum 114).

Roman Jakobson, who spans both stages of the movement in the USSR and Czechoslovakia, and whose work goes beyond Formalism as such, was instrumental in showing the way. As a linguist, his linguistically-oriented considerations of literature grounded literary theory firmly upon the newly developing science of linguistics.

Although a little simplistic, the following logic can be seen to be operating, and may serve as a guide in the study of the development from linguistics to a literary theory:

1. Structural linguistics is the science of language.
2. Literature consists of language.
3. Literature, too, is thus open to linguistic-style analysis, or to methods adapted from linguistics for the purpose.
4. Therefore Structural literary theory, modelled on linguistics, is the science of literature.

The first point, that of Structural linguistics being a science, has already been examined in the previous few pages, and needs no further elaboration here.

The second point draws upon the fact that literature consists of language. Detailed elaboration of the manner in which the literary theory is to be grounded in linguistics is typified in some of the writing of Roman Jakobson, who takes the linguistic nature of literature to be its most fundamental and vital feature. Equally, he feels, no linguist worth his salt can afford to ignore literature as one form of linguistic expression. In his essay, "Closing Statement" (1960a), he makes it quite clear that "the linguist whose field is any kind of language may and must include poetry in his study" (377).

Here we should note that although Jakobson is dealing with poetry and poetics, he uses the term poetic in a broader sense of any verbal art and literature. He argues for the prominence of poetics in literary studies, for poetics is concerned with the "differentia specifica of verbal art in relation to other arts and in relation to other kinds of verbal behavior" (350). He goes on to say: "Poetics deals with the problem of verbal structure, just as the analysis of painting is concerned with pictorial structure. Since linguistics is the global science of verbal structure, poetics may be regarded as an integral part of linguistics" (350).

It is the manner of analysis, the third point of the scheme above, that gives a clearer indication of the relationship between linguistics and poetics.

From his linguistic model of communication showing the factors of language Jakobson developed and superimposed his model showing the functions of language (see figure 2a and 2b). He concentrates on the poetic function which is "not the sole function of verbal art but only its dominant, determining function, whereas in all other verbal activities it acts as a subsidiary, accessory constituent" (356). The poetic function

CONTEXT

ADDRESSER MESSAGE ADDRESSEE

....

CONTACT

CODE

Fig.2a: Jakobson's scheme of the **factors** of language. (1960a, 353)

"Each of these six factors determines a different function of language. Although we distinguish six basic aspects of language, we could, however, hardly find verbal messages that would fulfill only one function." (353)

REFERENTIAL

EMOTIVE POETIC CONATIVE

PHATIC

METALINGUAL

Fig.2b: Jakobson's scheme of the **functions** of language. (1960a, 357)

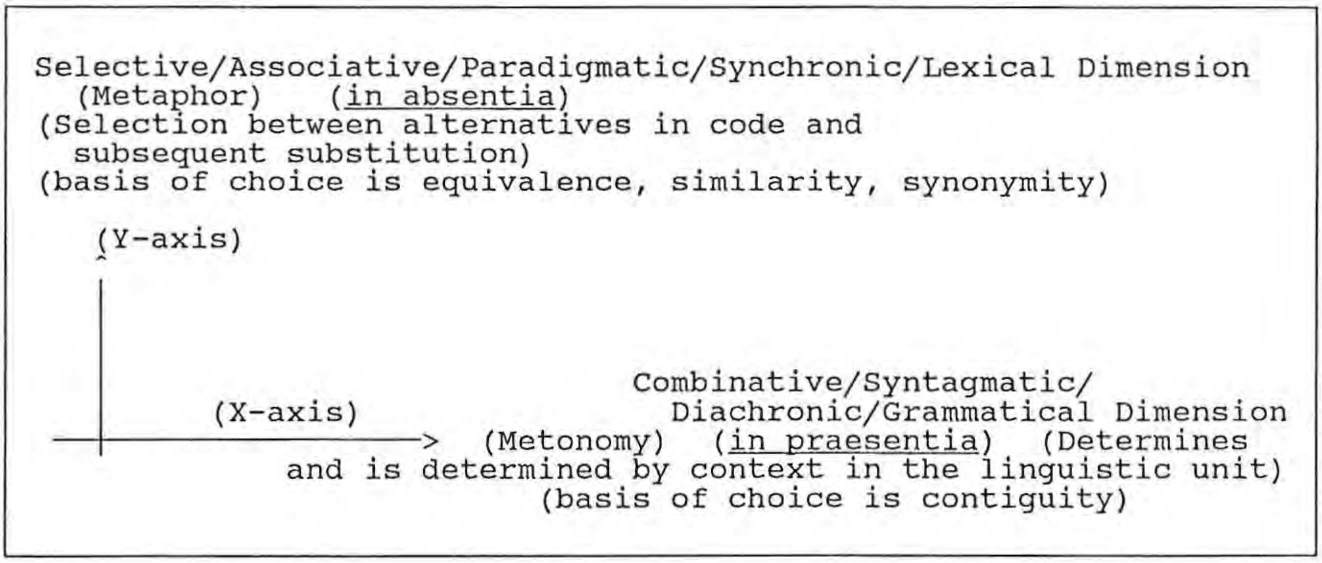
"We may complement our scheme of the fundamental factors by a corresponding scheme of functions" (357).

is that which directs attention to the qualities of the communication or "message", as a self-conscious linguistic construct.

Jakobson goes on to reinforce the connection between Structural linguistics and poetics by viewing the poetic function in terms of synchrony and diachrony (352), as used by de Saussure and further developed by Lévi-Strauss (fig.3). He concludes:

> To sum up, the analysis of verse is entirely within the competence of poetics, and the latter may be defined as that part of linguistics which treats the poetic function in its relationship to the other functions of language. Poetics in the wider sense of the word deals with the poetic function not only in poetry, where this function is superimposed upon the other functions of language, but also outside poetry, when some other function is superimposed upon the poetic function. (359)

The important issue to note is that, while his literary theory is embedded in linguistics, and makes use of linguistic terms and expressions, Jakobson does not simply transpose Saussure's structures into his own model, but adapts them. In doing this, he is parallelling the approach taken by Lévi-Strauss, with the concept of a bricolage or improvisation by careful adaptation. In this way he is able to develop the linguistic ideas without destroying the concepts of a structure and autonomy and without losing the use of the already established semiological terms that are crucial to the systematic scientificity of linguistics, for "[p]oetry is simply language in its esthetic function" (qtd. in Erlich 94). He says further: "Poetics deals with problems of verbal structure, just as the analysis of painting is concerned with pictorial structure. Since linguistics is the global science of verbal structure, poetics may be regarded as an integral part of linguistics" (Jakobson 1960a, 350). He believes, then, that



<u>Fig.3 Chart showing relation of Jakobson's "modes of arrangement")</u> constructed from Jakobson (1960a, 358-9; 1968), Jakobson and Halle (53-82), Hawkes (78-9) and Visser (1982b, 54-58). It also indicates the similarity to terms used by Saussure, (see Fig.1), although Jakobson does find problems with Saussure's insistence on the prominence of the linearity of language (Jakobson and Halle 60-61).

"<u>The poetic function projects the principle of equivalence from the axis of selection into the axis of combination</u>." (1960a, 358) poetics is understanding the linguistic characteristics and verbal structure of verbal art, and he argues against the separation of linguistics on the one hand from poetics seen more traditionally as a type of subjective and evaluative "criticism" on the other (350-52). This belief is continued through almost all of his writings (e.g. 1968).

I have taken some pains to emphasise this second point in the logic outlined above, for later in the chapter when I review Jakobson's discussion of the scientific nature of linguistics, we should remember that what he says also applies to literary study.

Our most important considerations, however, are raised by the fourth step in the logical sequence on page 43. Here we are concerned with the claim that a literary theory based on Structural linguistics is a science of literature. This is a claim we find emphasised by most of the theorists who accompany or follow Jakobson. In his essay "The Theory of the `Formal Method'" (1965) in which he takes a retrospective view of the Formalist movement, Eichenbaum begins with a sentence that could stand almost as its credo: "The so-called `formal-method' grew out of a struggle for a science of literature that would be both independent and factual; it is not the outgrowth of a particular methodology" (102). Throughout his essay, Eichenbaum constantly refers to these ideas, stating not only that this is a "scientific approach" (102), but also giving us a clear indication of the nature of the science to which he is referring. He says:

> We posit specific principles and adhere to them insofar as the material justifies them. If the material demands their refinement or change, we change or refine them. In this sense we are quite free from our own theories - as science must be free to the extent that theory and conviction are distinct. There is no ready-made science; science lives not by settling on truth, but by over-coming error. (103)

As Eichenbaum saw it, the Formalist idea was the beginning of a "special science of literature" which would become a "specific ordering of facts" focussing "on an empirical study of the material" (103). It was developed as an answer to the Symbolist scholarship with its "subjectivity and tendentiousness" and which "lacked both a scientific temperament and a scientific point of view" (105). He continues:

> We had to oppose the subjective aesthetic principles espoused by the Symbolists with an objective consideration of the facts. Hence our Formalist movement was characterised by a new passion for scientific positivism - a rejection of philosophical assumptions, of psychological and aesthetic interpretations etc. Art, considered apart from philosophical aesthetics and ideological theories, dictated its own position on things. We had to turn to facts and, abandoning general systems and problems, to begin "in the middle," with the facts which art forced upon us. Art demanded that we approach it closely; science, that we deal with the specific [my emphasis]. (106)

In support of this, he quotes Jakobson who attacks the literary historians who, "instead of a science of literature, ... created a conglomeration of homespun disciplines" by using "everything - anthropology, psychology, politics [and] philosophy" (107). Jakobson goes on to claim, "If literary history wants to become a science, it must recognise the artistic device as its only concern" (qtd. in Erlich 77), and that to "incriminate the poet with ideas and feelings...is just as absurd as the behavior of the medieval public which beat up the actor who played Judas" (77).

In the same vein, the absolute autonomy of literature - in fact, of all art - is taken up by Shklovsky who says "Art was always free of life and its color never reflected the color of the flag which waved over the fortress of the city" (77); and "A work of literature is the sum-total of all the stylistic devices employed in it" (90). This drive to create an autonomous science was necessary "if literary criticism were to advance beyond drawing room chitchat and make serious claims to scholarship" (Bennet 48).

In all these statements and in the concerns raised, we are reminded of the New Critics, and especially Richards - a distinction, however, is on the point of evaluation. Crucial to the Formalists is the notion that evaluation is not the goal of analysis. Jakobson says:

> Unfortunately the terminological confusion of "literary studies" with "criticism" tempts the student of literature to replace the description of the intrinsic value of a literary work by a subjective, censorious verdict. The label "literary critic" applied to an investigator of literature is as erroneous as "grammatical (or lexical) critic" would be applied to a linguist. (Jakobson 1960a, 351-352)

The aim of the poetics is to get away from any evaluation based on personal tastes, to "an objective scholarly analysis of verbal art" (1960a 350-52). This idea is a reiteration of the point made by Saussure, and, as we shall see later, it becomes a crucial anchor for Structuralist literary theory.

To sum up, then, the indications are that the Formalists had developed a "scientific" model of analysis. The model was based on the science of linguistics, dealt only with facts in a positivistic, empirical and objective manner, rejected subjective evaluation and criticism, and was independent of all other subjects, ideologies, and history. It is when the scientific model is rejected, however, that the real illumination of the model occurs.

4.2 The break with the scientific model

The Formalists eventually broke away from their scientific model.

Generally two reasons might be given for this, and these interpretations help to give a deeper insight into what was being considered as "scientific."

Firstly, one might like to assume that the theorists were not that insistent on the scientific model in the first place. This hypothesis is supported by critics such as Lemon and Reis, who doubt that the degree of scientificity and independence expressed by Eichenbaum above was emphasised at the time (101). Lemon and Reis's view is supported by Victor Erlich, who contends that the comments related to "Judas" and "Art's colour" quoted above are

> extravagant assertions which the young Formalist champions would have considerable trouble in sustaining. One need not, however, take Jakobson or [Shklovsky] at their word. Even though their actual views at the time were "radical" enough, it is obvious that they did not quite mean what they said and were at least vaguely aware of having overstated their case. (77)

As we have seen, however, Eichenbaum is not the only theorist who discusses a "scientific" model, and this degree of insistence on the scientific model by so many of the other theorists is simply too impressive to ignore.

The second explanation for the rejection of the scientific model of literary theory, is repression by the State. We must remember that Formalism existed in the newly created Communist State of the USSR. Formalism and Communism came to blows over the responsibility of the artist and the critic to society and history. To posit the values of objectivity and positivism alone was not enough to placate the proponents of a specifically dialectical materialism. Hence, we can understand that any autonomous scientific model that subordinated the importance of history, culture and ideology would be unacceptable (see, for example, Trotsky 196ff.). The argument runs, then, that the Russian Formalists were forced into including these notions into their scientific literary theory, thereby destroying the very claims of their theory to scientific objectivity and independence.

Erlich, again, however plays down this influence (77, 90,

128) as does Fredric Jameson (1972, 93) and Raman Selden (1985, 8), who contend that the incorporation of the importance of history into the Formalist theory was not entirely forced.

Erlich, however, does admit that the final rejection of the scientific method by some of the major proponents of Formalism, as expressed in articles like Shklovsky's "A Monument to a Scientific Error," seems "to have the earmarks of an externally induced capitulation" (Erlich 136). Although Visser notes that the attempted accommodations "brought the Formalists to confront problems they had previously dismissed or overlooked" (1982a, 16), the suppression was too much for the Formalists to withstand. With some critics like Gorbacëv calling for the "enemy" Formalists to be sent "to forced labour under good surveillance" (qtd. in Erlich 138), and with theorists being hounded out in Moscow (Shklovsky had been threatened with arrest for his political "sins" in 1922 [136]) and later in Czechoslovakia, the turn from a scientific model was not entirely uninfluenced by external developments.

A third point of view on these developments is possible. We could argue that while the origin of the changes may have been external, the actual changes do not indicate the dropping of the scientific model. Rather, we could argue that as Jakobson changed direction, he did not move away from a scientific model, but that his notion of what "science" was had changed, and in order to secure a really scientific model, the Formalists' ideas on what constituted a scientific literary theory would have to change with it.

We need to look at this proposal a little more closely. Some critics, especially Erlich (134-136, 285) and Raman Selden (1985, 8, 21) are fond of referring to the essay by Jakobson and Jurij Tynjanov (1928) in which they reject the autonomous and scientific standpoint of Formalism, and attempt to incorporate aspects of history and ideology into their work. The Formalist

Breitburg attacked this approach taken by Jakobson, saying, "A shift away from monism, even idealistic monism, to pluralism means moving one step away from a truly scientific approach to literature" (qtd. in Erlich 128). Some argue along the lines that in this essay Jakobson and Tynjanov were attempting to retain their scientific approach, while trying to appease those who oppose the very principles upon which this form of scientific approach is based. Erlich further maintains that very few people were fooled by this attempt.

If we look closer at the article, however, we notice something other than an attempt at appeasement emerging. This essay is so crucial both to the critics and to the argument that I am proposing, that I feel justified in quoting the whole of the opening paragraphs.

> 1. The immediate problems facing Russian literary theory and linguistic science demand a precise theoretical platform. They require a firm dissociation from the increasing mechanistic tendency to paste together mechanically the new methodology and old obsolete methods; they necessitate a determined refusal of the contraband offer of naive psychologism and other methodological hand-me-downs in the guise of new terminology.
>
> Furthermore, academic eclecticism and pedantic "formalism" - which replaces analysis by terminology and the classification of phenomena - and the repeated attempts to shift literary and linguistic studies from a systematic science to episodic and anecdotal genres should be rejected.
>
> 2. The history of literature (art), being simultaneous with other historical series, is characterized, as is each of these series, by a complex network of structural laws. Without an elucidation of these laws, it is impossible to establish in a scientific manner the correlation between the literary series and other historical series [my emphasis]. (Jakobson and Tynjanov 47)

Generally, the first sentence of the last paragraph is seen as the recognition of the importance of history in the literary theory, and the break from the insistence on scientific autonomy as the basis for a literary theory (Erlich 134-35). A brief discussion of the passage based primarily on the emphasised lines, however, raises points that are largely ignored by

Erlich.

In the first paragraph, it is quite obvious that what Jakobson is rejecting is a mechanistic, reductionist, purely empirical view of science - a weakness also to be rejected by Lévi-Strauss (1963, 51). He disbelieves that such a view can exist without a "precise theoretical platform." It is <u>this</u> conception of science that influences Jakobson's arguments.

In the second paragraph, he shows that literary and linguistic studies need to be maintained as a "systematic science," and shows also that he does not wish to let formalism degenerate into an eclecticism - again emphasising the need for the theoretical platform. He thus rejects the sublimation of literary theory into other spheres of study.

In the third paragraph, he emphasises the scientific nature of the studies associated with literature: the mention of history should be looked at in conjunction with the concepts of synchrony and diachrony discussed in the remaining paragraphs (Jakobson and Tynjanov 47-49).

If we look at this sentence a little closer, moreover, we can see that the writers do not so much reject a scientific model in favour of a historical one; rather they note that history is open to a Structural analysis in much the same way that linguistics and literature are. In addition, although the writers do acknowledge the importance of the historical laws, the writers are saying that a comparison between historical series and the history of literature can only be made at a Structural level. They are <u>not</u> saying, however, that the portrayal of history in literature, or history's effect on literature, must be allowed to affect an evaluation or a description of the literariness, in the manner in which the historicists or Marxists would advise.

The discussion of history, therefore, must be seen in the context of and in conjunction with the concepts of synchrony and diachrony in the model of poetics - as the writers discuss in the remaining paragraphs of the essay. There is hardly anything new in this formulation about the use of these terms - these, are, in fact, simple incorporations from Saussure's initial Structuralist model with which Jakobson had been working all along.

Jakobson and Tynjanov's argument, then, is not that the scientific model must be rejected in favour of a historical dialectical model, but rather a recognition that history is also affected by Structural laws, that any comparison between history and literature can only be effected through awareness of these laws, and, most importantly, that the narrow mechanical view of science must be removed from the model.

This rejection of the mechanical view of science is not surprising: although while in Russia, Jakobson had been closed off from a great deal of Western science, he was not entirely ignorant of what was happening, and years before had taken note of developments in science. As we shall see in the next chapter, since the turn of the century this issue was being debated by many scientists, and as early as 1919 Jakobson realised that the philosophical implications of scientific theories such as Relativity could not be ignored, as they directly altered perceptions of truth and common sense (1919, 31-32; 1921, 35-36; 1931, 285-7).[5] As Erlich also reports, later attacks against Formalism on the issue of science became so emotionally charged that they lost perspective. He says, "Soviet nuclear physics was found to be infected by the Formalist bacilli under the guise of widespread adherence to the theories of Niels Bohr" (148).

It is especially in Jakobson's later writings, moreover, that it becomes clear that he is aware of the importance of the influence of a non-mechanistic and non-empirical science. (Although they belong to a period after 1930, these essays are indicative of the trend that arguably had been established before 1930).

In his later work, in which he still insists on the autonomy of the science of linguistics, he recognises that inter-disciplinary work with other sciences is fruitful (1967, 655). He agrees with Edward Sapir that the modern linguist must "share in some or all of the mutual interests which tie up linguistics with anthropology and culture history, with sociology, with psychology, with philosophy, and, more remotely, with physics and physiology" (655). Linguistics can even learn from the "diverse aspects of mathematics - set theory, Boolean algebra, topology, statistics, calculus of probability, theory of games and information theory" (661), and he demonstrates at length its relationship to biology (672-678). He agrees with the mathematician Hadamard that linguistics is a

> bridge between mathematics and humanities. Mathematicians and linguists can and must cooperate for the benefit of both domains, without violating the autonomy of either of them. It would be erroneous to confine the contacts between linguistics and mathematics to purely quantitative, statistical questions. Invariance in variations is a crucial problem both for topology and linguistics, and there arise many common operational concepts. The need for semiotics, a general science of signs, is equally realized in the theory of languages and in mathematical logic. (1958a, 87)

He makes several parallells between the science of linguistics and physics, chiefly on the level of the atomistic nature of the theories of the structure of matter in relation to linguistics in the narrowest sense (1949a, 425; 1949b, 106; 1960b, 395, 402, 412). He argues also for a strong relationship between the structure of the elements of linguistics, and the structure of the elements of quantum mechanics (1953, 227; 1958b).

> The development of the science of language and particularly the transition from a primarily genetic standpoint to a predominantly descriptive approach strikingly corresponds

> to the contemporary shifts in other sciences, particularly to the difference between classical and quantum mechanics. This parallelism seems to me highly stimulating for the discussion of linguistic typology. I quote a paper on Quantum Mechanics and Determinism delivered by the eminent specialist, L. Tisza, at the American Academy of Arts and Sciences: ["]quantum mechanics [and let us add: modern Structural linguistics] is morphologically deterministic, whereas the temporal process, the transitions between stationary states, are governed by statistical probability laws. Both Structural linguistics and quantum mechanics gain in morphic determinism what they lose in temporal determinism.["] [text, but not quotation marks, within square brackets appears in Jakobson's original](1958b, 527)

While he says that "acoustics is the only branch of physics that shares a common subject matter with the science of language" (1967, 689), he points out that they do have common problems, such as the "inseparability of objective content and observing subject" (689), that they may apply similar methods, such as Niels Bohr's principle of complementarity (660, 689-90;), and that both areas are exploring issues of symmetry, anti-symmetry [sic], and "`temporal' or `morphic' determinism and of reversible fluctuations or irreversible changes" (690). These are not to be dismissed as occasional comments; Jakobson went so far as to hold a joint seminar on physics and linguistics with Niels Bohr at M.I.T. The seminar centred on the relationship between linguistics and "the so-called `exact sciences' and, particularly, to physics" and recognised the need for an understanding of linguistics by scientists (690).

These and related issues are dealt with by Jakobson in several other articles (1958b 527; 1962b, 652; 1961, 575-79; 1962a, 600; 1959, 262-266).

Although most of these arguments relate directly to linguistics, as noted and demonstrated above repeatedly, Jakobson continually emphasised the inseparability of the study of linguistics and the study of poetics.

What is apparent, then, is that the Formalist model was originally scientific, having as its basis the science of linguistics, and its philosophy of autonomy, empiricism, objectivity and mechanical description. Later, while Jakobson continued to insist on the scientific nature of linguistics, his view of what was scientific had altered over the years.

At this stage it is enough to note that Jakobson's new view of science was of a non-mechanical, and yet still systematic, procedure. The main agent prompting Jakobson to formulate these ideas can be seen as a direct exposure to theoretical physics, including Relativity, and more importantly Niels Bohr's developments in quantum physics. Whether Jakobson was justified in his belief about the lack of mechanics to be found in Niels Bohr's views of quantum physics, and whether these views were held by many scientists, is something that will have to held over for the next chapter.

4.3 Tzvetan Todorov

Of course, Roman Jakobson was not the first, and certainly not the only theorist to anticipate a Structuralist literary theory. Claude Lévi-Strauss had made attempts at a Structuralist approach to the stories of Oedipus (1963, 212-216), and also in later works (1970), although these were chiefly attempts to find the social structures behind the myths through the social archetypes. Other valuable work was done by Vladimir Propp in The Morphology of the Folk Tale, A.J. Greimas (1966), Valéry (1930), and later by Gérard Genette (1980; 1982).

Tzvetan Todorov bases his literary analysis on the belief in a universal text from which other texts are extracted (1968a;b; 1969b).

His argument is a formulation of works by many writers such as A.A. Potebnya, Max Müller, G.L. Pemyakov, Diderot, Shklovsky, André Jolles, Gezzi and Etienne Souriau on proverbs (or "morals") of stories, metaphors, myths, stereotypes, dramatic characteristics and character roles (1973b, 162-166). Most of what Todorov writes supports and develops ideas expressed by Jakobson.

Firstly, he argues for the autonomy of a science of literature, in a way that resembles the views of the earlier Jakobson more closely than some of the later ideas mentioned above. He stands opposed to the eclectic view that recognises the validity of the study of literature from vantage points of the social sciences such as linguistics, psychoanalysis, sociology, and history of ideas. He feels that the statement that the unity of these studies is the unique object, literature, is "contrary to the elementary principles of scientific research" (1981, 8). As a parallel, he says that no-one tries to form physics, chemistry and geometry into a "science of bodies", for each has a totally different goal in mind. "It is hardly necessary to repeat that <u>the method creates the object, that the object of a science is not given in nature but represents the result of an elaboration</u> [my emphasis]" (8). The argument is very close to Vico's; just how indicative it is of the sciences to which Todorov refers, however, will be looked at in the next chapter.

In addition to opposing the eclectics, Todorov believes that in order to remain a science, poetics must steer clear from identifying too closely with any <u>one</u> of these social sciences. This is simply naive "projection" (1969a, 235), and such "an activity is related to science insofar as its object is no longer the particular phenomenon but the (psychological, sociological, etc.) law that the phenomenon illustrates" (1981, 6). These methods then, would merely result in interpretations that will tell us more about the science than the actual literature, for we will be viewing the aspect of literature purely through the eyes of that science and in relation to other aspect of that science (1966a, 24-25; 1973b, 154-55).

He does not deny that poetics should note the dependence of literature on language, but emphasises that this is not simply ordinary language - it is not that "literature depends...on language (the genus) but on artistic language (specific difference); or again, literature is an art (genus) of words (specific difference)" (1973a, 1169). He further acknowledges that it is imperative that we recognise the importance of the relationship between language as a study of signs on the one hand, and literature, as a theory of language on the other (1969c, 190). But Poetics must also recognise that linguistics stops at the sentence (1973a, 1170). From this point on the science of literature, poetics, must provide illumination of the underlying structures.

Secondly, in becoming a science of literature, Poetics should not concern itself with evaluation. Todorov fights against the close reading or explication de texte approach advocated by the French New Critics, which is simple "commentary" (1969a, 235), and regards the establishment and examination of the structural laws of the text as the correct scientific approach (1981, 3-6). A science, he argues, is "a coherent body of concepts and methods aiming at knowledge of underlying laws" (1973b, 154). It is not a "Structuralist criticism", for this is a contradiction in terms, for criticism seeks to interpret, while Structuralism "is a scientific method implying an interest in impersonal laws and forms, of which existing objects are only the realizations" (1973c, 73).

Finally, in search of a quality similar to Jakobson's "literariness," poetics must move beyond the individual text and the genre into a realm in which it studies "general laws which govern the functioning of literature, its forms and varieties (it thus presupposes the existence of such laws)" (1982a, 2) in the same way that the "object of Aristotle's Poetics is not a certain poem by Homer or a certain tragedy by Aeschylus, but tragedy or epic" (1969a, 235-236). In addition, "Poetics will have to study not the already existing forms but, starting from them, a study of possible forms: what literature can be rather than what it is" (1966b, 33).

His final argument for a general theory of literature is justified on the grounds "to deny the legitimacy of a general theory of literature has never been the equivalent to the absence of such a theory, but only to the prejudice which leads to not making such a theory explicit, not inquiring as to the status of the concepts employed" (1969a, 237).

For Todorov, then, a scientific poetics of literature is a study that is autonomous and never eclectic; and that, while recognising the importance of linguistics and using its terms and notions, also recognises its limitations. In addition, this study takes the idea of searching for laws a stage further into prediction: in the study and understanding of the underlying structures and laws of a phenomenon, a scientific poetics gives the Structuralist the power to predict and account for phenomena encountered by the science: in this case, all literature.

5 Roland Barthes

It may seem strange that I turn to Roland Barthes as an advocate of Structuralist notions, when he is perhaps better known for essays on the rejection of these Structuralist, especially scientific, notions. I will examine some of his ideas not in spite of this anomaly, but because of it, for, parallelling Jakobson, Barthes's initial postulations and the processes involved in the change of his ideas are illustrative of the concept of science held by Structuralism. In order to appreciate this, however, we need to begin by looking at Barthes's views of social myths.

5.1 Myth

By myth, Barthes means a type of speech, a system of communication, a mode of signification, a form. It is not an object, a concept or an idea. "Myth is not defined by the object of its message, but by the way in which it utters this message..." (1972b, 109). Important to bear in mind at this stage, as Susan Sontag points out in her preface to Barthes's Writing Degree Zero (1968b), is that

> "myth" doesn't mean that a concept (or argument or narrative) is false. Myths are not descriptions but rather models for description (or thinking) - according to the formula of Lévi-Strauss logical techniques for resolving basic antinomies in thought and social existence. And the converse is also true: all explanatory models for fundamental states of affairs, whether sophisticated or primitive, are myths. (xx)

Different sectors of a society have different myths, or different views of life. When, however, a particular group becomes dominant, the myths of that group become the viewpoint most commonly used. As the speech of one group, it becomes "depoliticized speech," that is, it allows the political ideology of the dominant class (1975, 32) to become normalised, factual, and in this way, gives the "historical intention [of the myth] a natural justification" (1972b, 142-143).

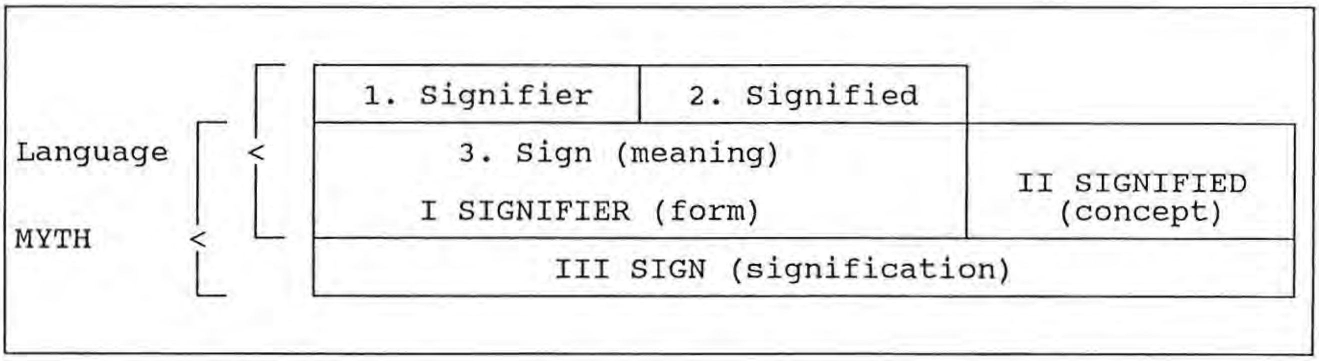
In the development and expression of man, the channel that is crucial for his cultural and ideological development is language. As Vico had argued, "Man does not exist prior to language, either as a species or as an individual" (156-157). As the myth is entrenched, it is supported by the language that is used, and, in fact, in turn, feeds the language. The language, of course, is the mode of expression, both in normal speech and most importantly, in the literature as an art form: thus the myth is perpetuated through the artistic expression of the society, as a universal truth. The language of the myth then creates what man expects from reality, and how he interacts with that reality. The myth as expressed in the language, defines and creates the individual's role in society by socialisation, and, in the terms of Vico, can be seen to create the man.

According to Barthes, in our western society, the political group that became the dominant force was the bourgeoisie (1972b, 137-145). Thus, the myths of the bourgeoisie have become so entrenched in all aspects of our social structure, that they have become our "reality."

5.2 Mythology

Directly influenced by Vico, Saussure, Lévi-Strauss and Jakobson (1971a, 159-161), Valéry, and also by the "mystification" process of Bertolt Brecht (Barthes 1979, 39-40), he believes that the myths of a society are so closely connected to the language (and thus the literature) used by that society, that they themselves take on the characteristics of signs, and work themselves into becoming languages or metalanguages. The sign, which is the link between the language and the myth as a second-order semiological system, enables us to understand the culture (1972b, 114). (See fig.4).

The study of myths is known as mythology, which is "a part of semiology inasmuch as it is a formal science, and of ideology inasmuch as it is a historical science: it studies ideas-in-form" (112). Although Barthes is primarily concerned with literature, the science of mythology, as a part of the general science of semiology (111), recognises that all social activities can be seen as expressions of this myth, and can be analysed in terms of this myth. Barthes demonstrates this in numerous texts (1967a; 1972b; 1979; 1985) where he examines a wide range of social activities. In his large work on Fashion



<u>Fig.4: Barthes's view of the relationship between Language as a first order semiological system, and Myth as a second-order semiological system, and incorporating the concepts of meaning, form, concept and signification.</u> (1972b, 115-127)

In this figure, we can see how the link between the language and the myth as a second-order semiological system, the sign, enables us to understand the culture (1972b, 114).

The sign of the language (holding the `meaning'), becomes the SIGNIFIER (holding the`form') for the MYTH, and thus the language has become a language-object for the MYTH, and the MYTH becomes a metalanguage. The semiotician is no longer concerned with the composition of the sign in the language, but only with the SIGN in the MYTH. The word signification "is here all the better justified since myth has a double function: it points out and it notifies, it makes us understand something and it imposes it on us" (117). The meaning of the myth is entrenched in history; the myth does not <u>hide</u> anything, its function is <u>distort</u> (121). It is largely for this reason that, unlike the connection between the constituents in the language sign, which is arbitrary, in myth, the signification is a motivated ideograph (126-7). Moreover, literature as a language can only connote reality, never really denote it, and "<u>logos</u> thus appears irredeemable severed from <u>praxis</u>" (1963a, 266-267).

There are three ways in which to read the sign: (1) by focussing on an empty signifier, believing in a literal signification, thus becoming a producer of myths; (2) by focussing on a full signifier and perceiving its dual nature of form and meaning, and the manner in which the distortion of one affects the other, thus deciphering the myth, understanding the distortion, and in this way, demystifying the sign; (3) Finally, by focussing on the signifier as a whole made up of meaning and form, it becomes the reality, thus consuming the myths. The history then, which is reflected in the sign, becomes a natural, unquestionable reality (1972b, 127-131).

As a result, any value judgments on an object that are assumed to be in any way universal, words like "good" or "bad," are merely indications of the extent to which the object reflects and supports the myth.

(1985), he says that even the functions of Fashion "are analogous to those found in all literature and can be summarized by saying that, through the language which henceforth takes charge of it, Fashion becomes narrative" (1985, 277). This demonstration of semiology as a general science of signs fulfils his much earlier aims (1967a), and the even earlier predictions by Saussure (1959, 15-17, 68), by Jakobson (1960a, 351) and Lévi-Strauss.

In the preface to the 1970 French edition of Mythologies (1972b), Barthes writes:

> I had just read Saussure and as a result acquired the conviction that by treating "collective representations" as sign-systems, one might hope to go further than the pious show of unmasking them and account in detail for the mystification which transforms petit-bourgeois culture into a universal nature. (9)

5.3 The Structuralist Critic

The Structuralist's or mythologist's task when faced with literature is to delve beyond the surfaces of texts, and through understanding and analysis of the system of signs which make up the language, to attempt to grasp the bourgeois myths upon which the literature is based. He needs to make a scientific analysis of the structures within the text, and in this way, he will be able to "demystify" the "reality," and expose it for what it is: at worst, a system of oppression of ideas other than those acceptable to the ideology; at best, simply a convenient system of conventions, representing, in Fredric Jameson's terms, the "political unconscious" (1981, 17-102).

Structuralism is not a "canonical method" (Barthes 1970b, 79), but an activity, the goal of which is to "reconstruct an `object' in such a way as to manifest thereby the rules of functioning (the `function') of this object"*13 (1963c, 214).

The aim of the Structural analysis, through "dissection"

(1970b, 80) and "articulation" (1963c, 215-217) is to find the structures and meanings that have been created or fabricated, how they have been created, and how new creations of meanings are possible, and thus allow "Structural man", "Homo significans", to gain a deeper understanding not only of the world of the text, but the world that led to the text (214-19).

5.4 Evaluation as Myth

True to the Structuralist approach, Barthes realises that the idea of an objective evaluation is nonsensical. The form of his argument can be seen as closely related to Vico's idea of the myth creating the object.

As we have seen, any art is embedded in the culture from which it originated, and, in turn, it encourages and develops that culture, and, as such, any evaluation of art, using terms like "good" or "bad," is merely an indication of the extent to which this art reflects the concerns of this culture (1972b, 114-131; 1971d, 100-101). More specifically, in Western societies, because the dominant myths are those that serve the purposes of the bourgeois ideology, "good" literature is simply literature that reflects the expectations of this bourgeois ideology.

Barthes acknowledges that Structuralism has to be able to change as history changes, for the structures of history supply the bases for the structures in the literature (1963c, 220). It is with this in mind that Barthes writes the "scandalous" Sur Racine (Sturrock; Barthes 1963b), a psycho-analytical cum anthropological look at Racine's drama (9-12). There is no scope for interpretation, only for dissemination; the various meanings do not constitute ambiguity, but rather a stereographic plurality (1971c, 159).

A recognition of the status of evaluation is crucial to the understanding of Barthes's point of view. It is obvious that Barthes is arguing from a Marxist-oriented perspective. Yet the important work, that is, enabling the identification of the myth as a sociologically defined parameter through which to view and evaluate art and then "create" a new reality of "Art," is a synthesis of the issues discussed by Vico, Saussure, Lévi-Strauss, Jakobson, and even to some extent, Todorov. As importantly, it allows us a better vantage point from which to recognise the significance of later changes, such as those indicated by his essay "The Death of the Author" (1968a), and his rejection of the scientific model of analysis.

As indicated above, Barthes's identification of the sign systems in many other spheres of social interaction is a fulfilment of a prediction made by Saussure. More than this, however, it also solves one of the problems that was mentioned earlier - that of the axiomatic status of the structure itself. In the conclusion of the discussion on Lévi-Strauss, it was noted that the although the structural system had been useful in explaining kinship relationships, there was very little justification for the existence of the particular system, other than that "it worked." Barthes, however, had shown that behind the structure lay the social myths, and, in fact, the structure was based on the configurations of the social myths.

With the implication here being, once again, that the observer is creating the very reality that he supposed to be examining, we can see the potential for a conflict between old and new views of the scientific model.

5.5 Crisis and change

It is widely recognised that during the mid to late sixties, Roland Barthes underwent a major turn-around in opinions (Selden 1985, 74-75; Culler 1975, 38; Washington 81-85;

Lavers 26-31, 196). In rejecting much of what he had earlier proposed, Barthes was, in fact, one of the earliest to note and give impetus to the development of Post Structuralism and even Deconstruction from within Structuralism.

5.5.1 The Death of the Author

One of the greatest changes in Barthes's point of view has been seen as the opinion expressed in his essay "The Death of the Author" (1968a), and elaborated on in S/Z (1970a), which concentrates on the role of the Reader as a consumer of texts.

For many critics, the fear that is raised by "The Death of the Author" is that it seems to confirm Structuralism as a scientific, inhuman or human-denying theory. The opponents of Structuralism who believe this, however, should never forget that at the basis of all Structuralist theories is the sign, which can only exist as a process in a human mind that combines the signifier with the signified. Man is, and must remain, Homo significans.

More culpable than these critics are those who speak patronisingly of Barthes for thinking that he has discovered something new with his account of the author's death, when in fact the author was removed from the process of criticism by the New Critics under the influence of W.K. Wimsatt and others (see the discussion on the Intentional Fallacy in Chapter One). The image that one gets of Barthes in such patronising accounts is that he is a bright man who has not done his homework (see especially Selden 1985, 75; Washington 38-85).

True enough, Barthes does argue against the concentration by criticism on the author's intention. But this needs to be looked at in relation to his other statements and the subtleties that are often ignored.

Firstly, it emphasises that it is a shift away from the author to the reader; the fault here is, admittedly, that

Barthes does not recognise, or at least acknowledge, the extent to which several writers (from Plato to Shelley), _have_ dealt with this.

Secondly, the argument concerning New Criticism misses the point: as we saw in the previous chapter, the removal of the author from New Critical analysis was carried out so that the _evaluation_, that is, determining the _value_ of the text as an independent "organic whole," would not be influenced (hampered) by "outside," "irrelevant" data such as the _identity_ of the author. Structuralism, however, has never aimed at _evaluation_ at all, (or at most, relegated it to a secondary level [Culler 1975, 117-119]). The removal of the author is performed through a different conceptual perspective. When Barthes removes the author, he does so in order to remove the concept of an original work _stemming only from the mind of the Author_. The Author, as an omnipotent creator, is dead. The role of the author is now simply a meeting point of the myth and the language, particularly as it is determined by the history in which the author finds himself. The author is only to be looked at as an influencing structure in the creation of the text. Even for Barthes, this is not a totally new concept, as he had dealt with the issue some time before, in several other essays (1955; 1960a; 1960b).

Later, Barthes refines his position further, to see the text as a nexus of linguistics codes, a privileged site for the intertextual and intracultural productions of meaning, enabled by the possibilities inherent in art, language, and culture, but limited by them too.

This removal of the author's subjectivity from the position of primary interest in the understanding of the text does not, therefore, present the work of art as an objective entity, a "well-wrought urn" to be studied in grand isolation on its own independent merits, as American New Criticism might have wished.

The New Critics removed the author for the sake of leaving the aesthetic all the more final and complete in all its resplendent autonomy. For Barthes, on the other hand, this illusion of independence is dispelled: the literary text is no more than a temporary and uncertain conjunction of common linguistic codes or "lexias" (1970a) and is thus utterly dependent for its meaning on what is outside itself.

Barthes's "objectivity", then, does not imply the independent existence of utterly substantial atoms, as in the classical picture. Objectivity, for Barthes, applies only to the system, not to the artistic text per se. The literary entity finds its true nature by situating itself within the pre-existent system of signs and meanings, and has its sense only by virtue of the reader and that relationship.

5.5.2 Disillusionment with the scientific model

More important, and more directly pertinent to this thesis, is Barthes's rejection of the scientific model. At first, Barthes had been content to note that Science and Art were moving closer to each other: "when the scientist and the artist endeavour to construct or to reconstruct their object, their activity is the same; and once these operations are terminated, they refer to the same historical intelligibility..."(1963a, 277). Although the science of Structuralism included "itself within its object,...it is this infinite `reflexiveness' which constitutes...art itself: science and art both acknowledge an original relativity of object and enquiry" (277-278). The result, however, was a crisis caused by the realisation that the subject and object of discovery in Structuralism were too closely influencing one another, and therefore the procedure could no longer be termed "scientific."

The issues involved in the crisis are best seen in his article "Science versus Literature" (1967b). Firstly, he makes it quite clear that when he speaks of Structuralism he means "a certain mode of analysis of cultural artifacts, insofar as this mode originates in the methods of linguistics" (897). This, of course, is the guiding philosophy of most of his earlier work.

The problem arises with the second realisation, and that is that both Structuralism and literature have "emerged from language" and they have much more than an affinity; "the two are homogeneous" (897). As a result, Structuralism needs to choose whether it should

> maintain a scientific distance between itself and its object, or whether, on the other hand it agrees to compromise and abandon the analysis of which it is the bearer in that infinitude of language that today passes through literature; in short, whether it elects to be science or a writing. [my emphasis]" (898)

So Barthes feels that maintaining a distance between the subject under investigation and the method of investigation is crucial for a science, and if Structuralism wishes to examine language, it cannot do so and still call itself a science. He elaborates in later works, saying that the criticism, if viewed as a metalanguage, will also become part of the text as a social phenomenon (1971c, 164), and also that semiotics itself has become a new mythology, and that it struggles now to separate the "signifier from the signified, the ideological from the phraseological" (1971b, 166-167).

His argument against a science of Structuralism goes further than merely subject-object dichotomy: Structuralism also does not have the objectivity that is demanded of a science:

> Objectivity and rigour, those attributes of the scientist, which are still used as a stick to beat us with, are essentially the preparatory qualities, necessary at the time of starting out on the work, and as such there is no cause to suspect or abandon them. But they are not qualities that can be transferred to the discourse itself, except by a sort of sleight-of-hand, a purely metonymical procedure which confuses precaution with its end product in its discourse. (1967b, 898)

The crisis is thus caused by a clash of fundamental hypotheses that had shown up as irritating contradictions in Lévi-Strauss's works and now demand immediate resolution: in order to be acceptable, the criticism must be objectively scientific; in order to be scientific, however, the approach must be able to draw a clear distinction between the object of research and the methodology involved. As soon as the observer has a direct influence on his object of investigation, the investigation is no longer scientific. The Structuralist critic, however, has, by creating a discourse, created the object, and is also using language to study language. Thus Structuralism can no longer be termed scientific. Barthes's "euphoric dream of scientificity" (Culler 1975), is shattered.

He sees the resolution to this problem in Structuralism's moving away from the scientific notion of distinguishable object and subject, and instead having to learn to question its own language and to rejoin the literature, "no longer as an `object' of analysis but as the activity of writing" (1967b, 898).

It resulted in his later work appearing to many as "not merely un- but antiscientific" (Lavers 28). In "Structural Analysis of Narratives" (1966), Barthes criticises those critics who wish to follow the example of the experimental (physical) sciences, and use an inductive approach. At this stage, however, it is obvious that Barthes is still basing his analysis on the science of linguistics, the developments of Propp, Bremond, Greimas, and Todorov, and the notion that while linguistics ends at the sentence, the processes of langue investigation are to be found in the investigation of narrative (83-117).

At the same time, he rejects the argument by Structuralists like Todorov of set laws of literature that are to be discovered and then used for predictions about texts. In S/Z (1970a) he begins by outlining a subtle yet important difference between his beliefs and those of other Structuralists on this point. Here he says that he is wary of the practice of seeing a totality in the minute, and of the methods of those who extract structures from individual texts and then attempt to find a grand text for all other texts (9).*14

It is crucial to grasp the importance for the development of literary theory of the rejection of the scientific model. As this objective scientificity was the very basis of the objectivity and authenticity of Structuralism, Structuralism threatens to "deconstruct" itself - in fact, as Lavers notes in her discussion of S/Z, objectivity and subjectivity were of necessity dismissed by Barthes (Lavers 202; Barthes 1970a, 16-17).

5.6 New Myths for Old

What is strange, however, is not that such a view developed; but that Barthes seemed caught off-guard by this development in his own intellectual life. As early as 1953 (1968b) and 1957 (1972b), he had rejected the notion of the "natural dichotomy between the objectivity of the scientist and the subjectivity of the writer" (1972b, 12), and had further recognised the relationship between history, reality and myth, and also that each has the power not only to influence but to create the others.

In order to grasp this, we need to take into account that in his writings, Barthes uses the term "bourgeois" as one which falls between the narrow class definition and the broad, crude rallying cry that dismisses anything disagreeable as "bourgeois." He uses the word in the sense of self-satisfied and seemingly self-evident and self-justified common doctrinal views of reality.

There is also the understanding that if the social activity or text under investigation is a product of its socio-historical setting, then surely the writing of the Structuralist or mythologist commenting on that text is also a product of his own socio-historical setting?

The implications are that while mythology begins as an attempt to show that the perceived reality is the result of a particular doctrine or myth, the process of mythology itself becomes so successful, that it, itself, becomes entrenched as a doctrine or myth, becomes, in a sense, "bourgeois," and must be analysed.

In "The Structuralist Activity" (1963c), he writes that*15

> all thought about the historically intelligible is <u>also participation in that intelligibility [and] Structural man is scarcely concerned to last</u>; he knows that Structuralism, too, is a certain form of the world, which will change with the world; and just as he experiences his validity (but not his truth) in his power to speak the old languages of the world in a new way, so he knows that it suffice that a new language rise out of history, <u>a new language which will speak him in his turn, for his task to be done</u> [my emphasis]. (219-220)

He had earlier realised that the Structuralist must be aware that he is not seeking the truth or even "hidden meanings", for that is a fallacy, and he must also realise that his discoveries are influenced by his own moment in history, his own ideology, and that his criticism is the language or metalanguage of that ideology. He says that the "the capital sin in criticism is not ideology, but the silence by which it is masked"*16 (1963d, 257). Thus, although one is speaking <u>in</u> a myth <u>about</u> a myth, as myth is simply a viewpoint and not something that is "false", all that is necessary is for the mythologist to realise that he also has a viewpoint - an argument that was never disputed by Barthes.

By 1971, he realised that his own work had taken on the characteristics of a myth: he says that his ideas had been adopted, and his form of life and thought were merely being denounced as petit-bourgeois - the "mythological doxa" had been created, and all the elements of it had become "the" discourse (1971b, 166-67).

That Barthes was unprepared for the fulfilment of his prophesy is indeed surprising.

6 Conclusion

Barthes had realised that Structuralism had moved into a new realm of discovery. Following the trend from the ideas isolated in Vico's work, it had attempted a scientific examination of the manner in which myth assumed an unquestioned reality, and it used this to develop further a scientific approach to analysing literature. At its height, it had developed into a theory that could analyse the structure of literature, looking at language as a linguistic construction, and as a social institution. It also equipped the critic to delve into the myth behind the literature in order to understand the reality of the text that had been created in the literature.

But the text had always stood between the myth and the Structuralist who was conducting a scientific analysis. It was only in the later stages that writers like Barthes realised that as Structuralism developed into a theory, the Structuralists were developing tools that themselves were forming into a "self-evident" myth, and thus transforming themselves into what would be seen as reality.

In order to further the exploration, the examination would have to become self-reflexive, and to deal with the epistemology of the theory itself (we can see here the opening of doors for the Post-Structuralist studies). It is at this point that the two mutually exclusive tenets of Structuralism emerged:

1. In order to be a science, Structuralism needed to maintain a "scientific distance" between the observer and the object.
2. Structuralism was creating its own object, and indeed, the method of its investigation, language, was also the object under investigation.

Structuralism seemed to have lost its very basis of authentication. The responses to this ranged from works like Jonathan Culler's Structuralist Poetics (1975), which were brave attempts to revitalise Structuralism by a clearing out of "poisons", such as the problem of science. Others, like Peter Washington's Fraud (1989), were simply naive, clichéd and sloganised rebellions against any theory, and threatened to do more damage to the scholarly opposition to literary theories that to the literary theories themselves.

The solution to the problem, I believe, was not the rejection of the methodology nor of the notion of its being scientific. Rather, as we have seen was the tendency of some of the writers themselves, the solution was to question the validity of the early Structuralists' interpretation of the word science. In the above pages, we have seen that writers like Lévi-Strauss recognised the inherent weaknesses and contradictions of the mechanistic and positivistic scientific model, and that Jakobson, especially, shifted his emphasis not so much from a scientific model *per se*, but rather from this very mechanistic and positivistic model.

At the end of Chapter One I stated that in order to further examine the ideas of New Criticism, we would have to investigate the problems raised by looking closer at science. The above conclusion indicates that the same process must be performed in order to resolve the contradictions displayed in Structuralism. The time has come to do just that.

CHAPTER THREE - SCIENCE

I do not know what I may appear to the world; but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.

(Newton)

"Beauty is truth, truth, beauty, - that is all
Ye know on earth, and all ye need to know."

(Keats "Ode on a Grecian Urn," ll.49-50)

More recently, two new flavours were added to the [quark] model, denoted by t and b for "top" and "bottom" (or, more poetically, for "true" and "beautiful"), which brings the total number of quarks to eighteen - six flavours and three colours.

(Capra 1983, 348)

Let us worry about beauty first, and truth will take care of itself.

(Zee 1986, 3)

CHAPTER THREE - SCIENCE

1 Introduction

In the previous chapters I looked at the claims to authentication of New Critical and Structuralist literary theories, and noted that amid the conflicts, there is a recurrent issue: the relationship of the theories to science. The problems that arose and were noted during the discussion, moreover, stemmed from the literary theorists' ideas of science, especially their notions of autonomy, objectivity, subject-object relationship, and language. Furthermore, the discussion often indicated that a clarification of many of the complications would be found by analysing the theorists' views on science. These issues are to be handled now, and will be undertaken in parallel with an examination of prominent conceptions of the science of this century, by way of comparison and correction, where necessary. Before this examination, however, I will need to make my position a little clearer.

This chapter will not attempt to give a coherent history or philosophy of science of the early twentieth century; should such a history or philosophy be possible, this would not be the place for it. The scientific concepts raised will be only those that have relevance to the problems seen in the previous chapters - obviously, however, these concepts will be looked at in relation to their historical and philosophical context. Although I shall use some insight provided by philosophers of science, readers of philosophy may possibly find that some of the ideas offered seem crudely formulated - this is because most of the work, of necessity, will not come from philosophers, but from the practising scientists themselves.

I have decided to concentrate on physics, primarily sub-atomic physics. This field has been chosen for three reasons, the first two of which will be demonstrated in the course of the discussion.

Firstly, at the time that New Criticism and Structuralism were being developed, the most influential revolutionary discoveries in scientific knowledge occurred in physics, and physics was seen as the paradigm of science. The revolution brought about by Relativity and Quantum theory can easily be compared in the magnitude of its implications and effects to the discovery of the Copernican and Newtonian laws.

Secondly, the issues that were raised in the previous chapters - autonomy, objectivity, subject-object relationship, and the importance of language - are issues that were central to the sub-atomic developments in the first half of the century, and continue to be important today.

Thirdly, and perhaps most importantly, in the previous chapters we have seen that it is physics, more specifically atomic physics, to which the New Critics and the Structuralists have repeatedly turned when discussing a science that holds parallels to the work they are producing (Richards 1955; Lévi-Strauss 1963, 33-36; Jakobson 1949a;b; 1958b; 1960b; 1967; Todorov 1981). This will be looked at in more detail later in this chapter.

I realise that because this is a study of literary theory, it is quite likely that the reader will have a limited knowledge of physics. While some of the scientific concepts will be explained so that their implications will be obvious for the argument, further elaboration of terms marked by an asterisk (*) will be given in the glossary (pp. 151-176).

2. Science in terms of the literary theorists.

By the end of the nineteenth century, chiefly as result of Descartes, Copernicus and Newton, physics had constructed a seemingly tidy explanation of everything from pebbles to the solar system. There is a case for arguing that the psychological background of such an explanation is originally to be found in Western theology and the belief in a universe perfectly ordered and structured by God (Whitehead 14-24). The new explanation, however, had broken from even Kepler's assumption of the role of God in the creation of the universe, and considered "nature not only independently of God but even independently of man...aimed at its `objective' description or explanation" (Heisenberg 1958b, 8-9). The word "objective" is the key: the object was independent of the observer, who acted independently. This independent observer, by understanding the laws of nature, expressed in the scientific "doctrines" of the day, could find the rational order in the universe, and could express and communicate them in "timeless" mathematical formulae that ensured the "precision of expression which is necessary for exact verification" (T.H. Huxley 1893b, 60-66), aided by "iron" laws such as gravitation (1893a, 161). It was based on unquestionable causal relationships and could be used to make predictions; any discrepancy between the prediction and result could be explained very easily without invalidating the theory.

This model of physics, known as "Newtonian physics" or "classical physics," "appears absolute," and the "pronouncements of classical physics are precise and determining" (Heisenberg 1935, 41).

It was a model typified by our familiar high school physics equations of motion, such as $v = v_o + g\triangle t$, the verification of which could be achieved by experiments which were simply "observation[s] under artificial conditions" (T.H. Huxley 1893b, 60) "which ensure that the process will occur in its pure state" (Marx 90), and which verify that "Nature is ascertainable by our faculties to an extent which is practically unlimited" (Huxley 1893a, 163).

The success of these laws in physics led to their influencing the other sciences. Einstein writes: "all physicists of the [nineteenth] century saw in classical mechanics a firm and final foundation for all physics, yes, indeed, for all natural science...[my emphasis]" (1951a, 21; Einstein and Infeld 65; Capra 1982, 31; Jones 18).

This is the model that was considered to be "science" by the literary theorists, and when they referred to atomic physics, the implication was that the laws of atomic physics were largely extensions and refinements of these mechanical laws. There is certainly nothing in their work to suggest that the laws of atomic physics were inconsistent with the other mechanical laws.

It would, of course, be preposterous to assert that all literary theorists who made a claim for the "scientific" nature of their discipline constantly had atomic physics specifically in mind; we have seen how linguistics itself occupied in many instances the place of a paradigm for scientific activity. It is clear from the many illustrations we have already given, however, that atomic physics as such was a frequent point of reference beyond, and in some instances, parallel to, the reductionist analyses of Structural linguistics. We do see certain reservations being expressed here and there about the placing of literary theory in the general company of physics and linguistics as a scientific pursuit, but on the whole these were in the nature of modifications rather than rejections of the notion.

For instance, there are Brooks and Wimsatt who argued that the objectivity of science was related to a cultural matrix (although it was precisely this mechanical link that made it "scientific"), and Jakobson, who had found fault with the mechanical model by 1928. Although Barthes's science of Structuralism deals with the relationship between the activity and the society, it is a direct causal relationship, and one which is rejected as unscientific specifically when the clear-cut distinction between subject and object becomes blurred.

We also saw that the fundamentals of the science of literary theory lay in the language: although there was some disagreement as to the merits of precision and ambiguity, both theories agree that scientific language is precise, denotative, and that the association between signifier and signified is clearly defined and determined; in normal language, however, there is ambiguity and connotation, and a "scientific" criticism can overcome this to some extent by using "scientific" and technical terms.

For this thesis, the only really central distinction between New Criticism and Structuralism is in the area of evaluation; the New Critics arguing that objectivity allowed an absolute evaluation; the Structuralists arguing that objectivity denied an absolute value, for value was culture-bound.

But in their appeal to science in the traditional sense, and to physics in particular, it becomes more than uncertain whether all the theorists had a very clear and accurate idea of science.

One glaring aspect needs to be addressed. Many of their descriptions of physics relate to the physics of the nineteenth century or its aftermath; the literary theorists were working in the twentieth century. The validity of their arguments rests on the assumption that although the discoveries of science since the nineteenth century might have changed our knowledge of science, the principles of science had not changed.

3 Changes.

This is perhaps the first weakness of the theorists' ideas. What happened had not seemed so much a scientific change, as a scientific revolution. Three questions are raised by this: What is a "scientific revolution"? If such a revolution occurred, why did the literary theorists not notice it? And finally, what actually happened at the turn of the century to constitute the specific scientific revolution that developed contemporary to their own work?

3.1 What is a Scientific Revolution?

Many philosophers have discussed the types of changes that occur in science, and one of the most controversial is Thomas S. Kuhn (Popper 1981, 80-106; Feyerabend 1981a, 160; Horgan; Lakatos and Musgrave; Meiland; Siegel). In spite of the controversy surrounding Kuhn, a study of his works and ideas can give a clearer insight into the philosophical, theoretical and epistemological background of the scientific activity.

In his most popular work The Structure of Scientific Revolutions (1970c), Kuhn claims that scientific researchers work within specific paradigms, which are "universally accepted recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners" (viii). The paradigm determines what facts are relevant and important, and is in turn, justified by these facts. In the same way, it also establishes what problems are relevant and important - those falling outside the paradigm are unimportant. As this paradigm comes to define (and restrict) the activity of science, and becomes known as "Normal Science," those who conduct an investigation outside the limits of the paradigm are not being "scientific" (23-37, 60-61). Discoveries or facts that conflict with the paradigm may be discovered, but, as the paradigm is accepted as the yardstick, these facts are generally rejected.

If, however, more relevant but inconsistent facts are found, a crisis develops, and a change of paradigm may occur; a major claim of the new paradigm is that it can resolve more problems than the old (153). This paradigm change is known as a scientific revolution (6, 62, 77ff). The revolution is generally accompanied by "fundamental philosophical analyses of the contemporary research tradition" (88), and the language (102, 149-150) and even the world-view of the scientists will be affected (111-135).[1]

Generally the old paradigm will have been so deeply entrenched the level to which prejudices could sink.[2] The fear of science bred an attitude succinctly expressed by Gladstone in 1885: "Let the scientific men stick to their science, and leave the philosophy and religion to poets, philosophers, and theologians" (qtd. in Young 1980a, 96). There was the predictable result: "What remained common was popularization and uncertain generalization" (96).

In short, an entire philosophical split had developed, and this split was born of and, in turn, nurtured, ignorance. Although some literature took note of science (Beer 1983; 1986; Chapple 1986; Cosslett; Davie; Henkin; O'Hanlon, Korg; Paradis and Postlewait; Welsh; Young 1980a), it was often very crudely expressed, and humans could seem to be developing (or regressing), in C.P. Snow's words, into "Two Cultures."

C.P. Snow illustrates his point in his Two Cultures (1965), when he discusses the attitudes of non-scientists who dismiss scientists as ignorant for not having read major literature. He says that he has been provoked by this, and has

> asked the company how many of them could describe the Second Law of Thermodynamics.* The response was cold: it was also negative. Yet I was asking something which is about the scientific equivalent of: Have you read a work of Shakespeare's? (14-15)[3]

One of the greatest unintentional vindications of Snow's work is in the form of an attack by F.R. Leavis in Two Cultures? The significance of C.P. Snow (1962). This work is, it seems, little more than a tirade of insults that steers clear of the unalterable fact: because they have been immersed in their own fields, educated people do not have access to their total culture.

As Snow comments on the Second Law of Thermodynamics, "the majority of the cleverest people in the western world have about as much insight into it as their neolithic ancestors would have had" (15). We could therefore expect that the revolution would not have been especially noticeable by the literary theorists, as it was occurring, for the most, in another "culture." An obvious the level to which prejudices could sink.[2] The fear of science bred an attitude succinctly expressed by Gladstone in 1885: "Let the scientific men stick to their science, and leave the philosophy and religion to poets, philosophers, and theologians" (qtd. in Young 1980a, 96). There was the predictable result: "What remained common was popularization and uncertain generalization" (96).

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As Snow comments on the Second Law of Thermodynamics, "the majority of the cleverest people in the western world have about as much insight into it as their neolithic ancestors would have had" (15). We could therefore expect that the revolution would not have been especially noticeable by the literary theorists, as it was occurring, for the most, in another "culture." An obvious result of this would be the possibility that the literary theorists who debate the desirability of a "scientific literary theory" are sometimes themselves unwitting and inappropriate victims of widespread cultural inertia of the kind Snow describes.

3.3 What makes this change a revolution?

In addition to the philosophers and poets who rejected the scientific model of the nineteenth century, and unnoticed by many non-scientists, a rejection was occurring within advanced scientific circles. It was not a rejection of science, but a rejection of the mechanical model or paradigm and its epistemological principles. Maxwell and Faraday had already doubted the model's strength and its "common sense" (Preyer 56), and Kelvin's rejection of Maxwell's electromagnetic theory on the grounds that it could not be reduced to this model (Barber 598) was no longer convincing. Einstein argued that the proponents of the mechanical model continued in their attempts

> to base Maxwell's theory of electro-magnetism...upon mechanics as well. Even Maxwell and H.[Heinrich] Hertz, who in retrospect appear as those who demolished the faith in mechanics as the final basis of all physical thinking, in their conscious thinking adhered throughout to mechanics as the secured basis of physics. (1951a, 21)

Many of the scientific models that claimed to be against a mechanical view chose methods that still aimed at observing, classifying and characterising material phenomena, and then extracting laws from which to derive a general mechanical picture of the universe (Mandelbaum 23-25, 43; Turner 1980, 50-52; 57-59; Young 1980a, 87).

By the end of the century, however, the epistemology of science had become a major issue for the practising physicists, and the debate surrounding it had reached explosive level. The debate involved the already acknowledged leaders in science - Helm, Stallo, Mach, Otswald and Planck - and new young thinkers, like the "heretic," Einstein. Discussion centred on mechanics, thermodynamics, atomic theory, and the ether* (Holton 235-242). A large emphasis of the arguments was, ironically, not so much on what we might expect from physical researchers, but rather concerned the ability of the observer to know what he thought he knew, and to know what constituted a "proof" of this knowing. On the horizon, there loomed anomalies "foreign" to science; anomalies like subject-object relationships, subjectivity, interpretations not easily verified, philosophical implications, the relationship of the research to society, and the ability of the language of mathematics to explain the universe.

Something had to happen: according to Kuhn, either a new discovery would finally unite the few but fundamental contradictions that existed, or it would expose them as indications of a need to shift perspective.

The crux of the paradigm change postulated by Kuhn is to be found in the most unimportant part of any nineteenth century experiment: the observer. In the nineteenth century experiment, while the observer may be responsible for arranging the experiment, by definition of a scientific experiment, he should not directly influence the outcome of the experiment.

4. The Role of the Observer.

4.1 Relativity

4.1.1. The observer determines the frame of reference from which to observe the object.

In 1905, Einstein published what was to become known as the Special Theory of Relativity* (1905c; 1923a).

That this moment, coupled with Einstein's other work discussed below, indicated the start of a scientific revolution in the Kuhnian sense has been noted by several of the practising scientists of the day. Max Born says he "saw the revolution [and] lived on the barricades while the shooting went on" (qtd. in Agassi 1973, 620); it was a moment that could "be considered as being at once the culmination of classical ideas and the starting point of the new ones" (Born 1956, 38). Louis de Broglie says that it "overturned the most traditional notions of physics" at a time when "physicists had known for two decades that the old theories were beset with difficulties whose origins they were unable to comprehend" (1951, 109-111). Max Planck commented that should it prove "to be correct...[Einstein] will be considered the Copernicus of the twentieth century" (qtd. in Pagels 27), while Witkowski exclaimed "A new Copernicus has been born!" (qtd. in Bernstein 87). Einstein, acknowledging that the foundations had been laid earlier, writes: "The results of the work of Faraday, Maxwell, and Hertz led to the development of modern physics, to the creation of new concepts, forming a new picture of reality" (Einstein and Infeld 125), and with Maxwell's field theory, "a new reality was created, a new concept for which there was no place in the mechanical description" (151).

Relativity postulates that measurements of time and length are not absolute, but are relative to the observer's frame of reference or <u>coordinate system</u> (<u>CS</u>) ("Koordinatensystem"), (Einstein 1905c, 891-907; 1923a, 37-51; (Einstein and Infeld 167) or to a "certain definitional system" (Reichenbach 1951, 293-5). The Special Theory provides the physicist with a tool for calculations of aberrations in space and time which occur at uniform velocities[4] close to the velocity of light, and finally leads to the equation of $E=mc^{2*}$.

The theory relies on two fundamental ideas: firstly, all the laws of physics are valid for all <u>CS</u>s, and secondly, the velocity of light in a vacuum ($3x10^8 ms^{-1}$ or 300 000 kilometres per second) is constant, and "independent of the state of motion of the emitting body" (Einstein 1905c, 891-892; 1923a, 37-38).

Although not many objects move at velocities comparable to the velocity of light, the sub-atomic particles, the building blocks of the universe, do, and it is important that we should be able to say something definite about them.

For ten observers, then, there are ten sets of measurements. Who is correct? This question cannot be asked, for any observation can only make sense if it is related directly to the observer's CS. Relativity, says Christensen, "makes the observed inseparable from the observer; the former's very nature is dependent upon the latter" (39).

The implications of Relativity are profound, and extend beyond physics. Firstly, the constancy of the velocity of light in a vacuum defies logic and common sense. If a train moves at 40 kilometres per hour, and a passenger throws a stone out of the window in the direction in which the train is travelling, the stone is travelling at a velocity that combines that of the train and the velocity of the stone relative to the passenger. The light emitted from a space craft, however, travels at $3x10^8ms^{-1}$, whether that space craft is stationary or travels backwards or forwards at any speed.

Worse than simply defying common sense, was the problem of empirically confirming this principle, and applying Relativity to other sciences - a point which led to the rejection of Relativity by many eminent physicists, including Ernst Mach and Walter Kaufmann (Magie; More; Holton 246-7).[5]

The importance of Relativity to general philosophy and its implications for truth and ethics is well-documented (Bachelard 1951; Capek; Eddington 1968; Ferré; Margenau 1951; 1978, 333-350; Northrop 1951; Reichenbach 1951; Swenson 238-255; Ushenko; Wenzl), and has been discussed to some depth by Einstein himself (1936; 1940a; 1940b). Researchers in other fields applied the notion of established relativisation to Art (Craven), Psychology (Squires) and History (Gasset). While Reichenbach is hesitant over the applicability of Relativity to fields like ethics, he acknowledges the philosophical implications of the theory (289). For a start, our conception of absolute space and time alters: time is that which is measured by clocks; space is that which is measured by measuring rods; the very definition of simultaneity is open to question (Einstein 1905c 891-897; 1923a, 37-43), and has been debated since (Beauregard; Grünbaum 1962; 1963; Sellars). This concern is not particularly new: issues relating to it have been touched upon by several philosophers, from Plato to Kant (Kant 1934, 47-61). What is new is that the erstwhile mathematical precision of measurement of time and space has evolved into being relative to a specific CS only, and measurements are mere definitions, and have meaning only with respect to a specific CS (Einstein 1905c, 895-897; 1923a, 41-43). We cannot even begin to look for a physical, absolute and ultimate truth. Einstein's comment on the subject is: "To the question: Do you consider true what Reichenbach has asserted above [on the relationship between Relativity and philosophy], I can answer only with Pilate's famous question: `What is truth?'" (1951b, 676).

Does this make all measurements and interpretations meaningless? No. To say Relativity shows no more than "everything is relative" is an over- simplification or "nonsense" (Russell 1969, 19). Relativity provides formulae by which an observer may account for differences between his mathematical description of an object and the mathematical description made from other CSs. It therefore further provides him with the tool to predict mathematical descriptions from any other CS.

4.1.2 The implications of Relativity on the "scientific" literary theories

While it might be dangerous at this stage to draw a direct methodological parallel between the scientific observer examining an object and a literary theorist examining a text, a "scientific" literary theory can be expected to accommodate important developments in science. What can be inferred from Relativity is that the work by Richards and other New Critics, who were partly inspired by scientific models of objectivity, and who insisted on an absolute interpretation, must be seen as inappropriate to the new mood of physics. Although when he wrote, the perception of science as an unqualified, absolute and factual statement of reality could be presented only with reservation, Richards's tone remains uncompromisingly Newtonian in its insistences and absolutes. Although often still unresponsive to scientific developments, Structuralism's position on these matters was somewhat different. As we saw in Chapter Two, Jakobson's and later, Barthes's, rejection of a scientific model was, in fact, a rejection of the mechanistic model in favour of ideas akin to the newer physics. In addition, while the early Structuralists and Formalists certainly insisted on the objectivity of the text, they and their successors were in agreement in rejecting any possibility of absolute evaluation. As emphasised by the Structuralists, in any society, numerous culturally defined languages or modes of discourse are used, and these have an influencing effect on the development of the groups of society. Any text, then, will be interpreted along various lines, and thus an absolute objective evaluation of it is impossible.

It goes further than this. In order to see just how well Structuralism follows these ideas, we should look at Relativity from a different angle, drawing analogies between the language of literary analysis of texts and mathematical methods of description.

1. The scientific description of physical reality in a given CS is performed by using the language of mathematics, where a formula is a sentence in that language.

2. An observer in a second CS provides a description of the incident, in the same language, but his sentence is different.

3. Relativity provides the linking sentence between the two

observers' sentences (and any others) so that all sentences are comprehensible to all observers.

The observations made from the various CSs, and their communications, may be seen as discourse in this analogy; hence Relativity provides a communication link between discourses. Having no set, definite, objective and absolute CS for all observers is unimportant, provided there is a link between the discourses, so that each observer can understand what is being observed by another observer, and the basis for such an observation. In addition to denying an absolute evaluation, then, any "scientific" literary theory that conforms to relativistic models ought to be able to provide the link between discourses.

New Criticism, fraught as it is with different interpretations amongst even its staunchest proponents does not even consider the provision of such a link. Structuralism, however, does. For Structuralism, the link is provided by linguistics.

We have seen how the notions, elements and relations of the linguistic model, including terms like signifier, signified, synchrony and diachrony, were transferred from linguistics into anthropology in order to understand the material provided by the subject. These principles were then also transferred directly into literary theory by Jakobson (who had, incidentally, argued for linguistics as a "bridge between mathematics and humanities" [1958a, 87]), and refined by others.

In addition, apart from work already discussed, Barthes refers specifically to this role of linguistics (1973a; 1973b). Although he argues that neither linguistics nor socio-linguistics has achieved this goal completely since "the linguist is...obliged to reduce the separation of the social languages of the lexicon - even of fashion" (1973a, 118), advances have been made. He continues:

> Hence, the most interesting situation, i.e. the very opacity of the social relation, seems to escape traditional scientific analysis. The basic reason...is of an epistemological order: confronting discourse, <u>linguistics has remained, one might say, at a Newtonian stage: it has not yet experienced its</u>

> <u>Einsteinian revolution; it has not theorized the linguist's place in the field of observation</u> [my emphasis]. (1973a, 118)

He defines this role as "relativization," explaining that part of accepting this role demands a refusal of "the <u>adiaphoria</u> of traditional science" (118-119). Although these essays were written in 1973, at a stage where Barthes had supposedly rejected the scientific model, his rejection is once more an echo of Jakobson: a rejection of an absolutist mechanical approach.

We need to examine one more feature of a "scientific" literary theory that would be demanded if Relativity were taken into account by the model. In the description of the object, the physicist's attention is no longer on the object only, but also on the language he uses in the description; the language becomes part of the description. A "scientific" literary theorist, therefore, should also recognise this:

> 4. The object under observation is no longer merely the original object, but also the very language changes that are occurring in the description. Put another way, the language being used in the description has become part of the object under investigation.

This statement is reminiscent of Barthes's discovery that the language he was using to discover the myths in the text was itself becoming increasingly important to the investigation (1967b, 897-98; 1971b, 166-67; 1971c, 164). Ironically, he saw this as "unscientific," as he tried to maintain his "scientific distance" between himself and the object.

Hence Structuralism's claim to be a "scientific literary theory" has at least this in its favour: its recognition of the importance of the language of the description, and a rejection of the mechanical view of science with an absolute interpretation of an observation.

Relativity, however crucial, was not the only revolutionary development in early twentieth century physics, and in pursuing my analogy, I shall examine these other major changes in order to see how they might effect the claims of New Criticism and Structuralism to be scientific literary theories, at least on the model offered by nuclear physics.

4.2 Waves and Particles: The observer creates the object.

4.2.1 Light

We have seen that Relativity's greatest contribution to physics was that it provided the mathematics for an observer in one CS to communicate with an observer in another. In addition, its greatest contribution to philosophy was that it shook the notion of an absolute measurement.

In both of these, however, the observer's CS affects his observation, but the observer does not affect the object of observation. This state of affairs, however, was about to change.

In the same edition of the journal in which Special Relativity appeared, Einstein published an explanation for the phenomenon known as the photoelectric effect* (1905b; 1906). Einstein based his ideas on Planck's radiation work - work which implied a discontinuous universe, an idea that most physicists, including Planck himself, "the most reluctant revolutionary of all time" (Pearce Williams 51), found difficult to accept (Barber 596; Galison; Guilleman 1968, 48-51; March 190-1; Polanyi 67-68).

Until this time, light had been perceived as consisting of waves. According to Einstein's interpretation, however, a particle, the photon*, was the basis of all radiation, and his argument could easily be experimentally confirmed (Compton 1929; Einstein and Infeld 257-270).

If Relativity had not been enough to set a revolution in motion, Einstein's photoelectric explanation would have completed the task. The implications were as astounding as those of Relativity: Einstein notes that "It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built" (Einstein 1951a, 45; Kuhn 1970c, 83).

Although it was to win Einstein the Nobel in Prize in 1921, its radical nature is indicated by the reluctance of many physicists to accept it; as late as 1913, it was still regarded by Max Planck and Walther Nernst as a "slip"[6] (Popper 1981, 97). As Paul Dirac says, it was "a very striking and general example of the breakdown of classical mechanics - not merely an inaccuracy in its laws of motion, but an inadequacy of its concepts to supply us with a description of atomic events" (1947, 2-3).

Worse than this, however, a paradox which might seem to invoke an Orwellian "doublethink" developed - the particle nature of light did not deny, but rather confirmed the wave nature of light. As Niels Bohr says:

> The acuteness of the dilemma is stressed by the fact that the interference effects offer our only means of defining the concepts of frequency and wave length entering into the very expression for the energy and momentum of the photon [i.e., as a particle; my emphases]. (1951, 202-3)

This is because the theoretical verification of the particle nature lies in the formula E = hv, which assumes that light is made of waves.

In the discussion of Relativity, we saw that the question "Who is correct?" is virtually meaningless. In the discussion of light, the question "Is light a particle or is light a wave?" is equally as meaningless, for light is sometimes waves, sometimes particles. The philosophical distinction between Relativity and wave-particle duality is that while Relativity noticed that the observer's CS affects his observation, wave-particle duality notes that the nature of light is determined by the configuration of the observer's apparatus; the "experiments do alter things" (Eddington 1939, 112): the observer has altered the object under investigation.

4.2.2 Matter

In the early 1920's there was perhaps some comfort to be found in the knowledge that this wave-particle duality dealt only with the rather intangible substance called light, and not with real objects. There was still the possibility that the wave-particle duality could be a result of the nature of the photon, and have no bearing on other matter. Electrons, at least, were real objects.

Work done on the structure of the atom by Rutherford (1911; 1914; C.G.Darwin 1914) and Bohr (1913a;b;c;d) had given a clearer idea of an atom - it could be viewed as a microcosm of our solar system, with a nucleus in the centre, and electrons orbiting.

In his doctoral thesis and in other papers (1924; 1925), Louis de Broglie* postulated that not only does the wave-particle duality exist with photons, "real" particles like electrons also display wave characteristics. This was verified by Compton's earlier experiments (1923) and later by Davisson and Germer (1927), and the wave of an electron was just as easily experimentally demonstrable as the wave of light (Heisenberg 1935, 46). In his 1929 Nobel Speech, de Broglie sums up the findings: "We can no longer imagine the electron as being just a minute corpuscle of electricity: we must associate a wave with it. And this wave is not just a fiction: its length can be measured and its interferences calculated in advance" (1939, 179).

The revolutionary nature of de Broglie's thesis was indicated by the reaction of his thesis examiners; scared to stake their reputations on a thesis that might be "crackpot," they turned to Einstein, who replied: "It may look crazy, but it really is sound!" (qtd. in March 211-212).[7] Of his own impressions, de Broglie says:

> The evolution of my own conceptions in the critical period 1923-8 on the interpretation of wave mechanics, proves also at what point he who puts forward the fundamental ideas of a new doctrine often fails to realize at the outset all the consequences; guided by his personal intuitions, constrained by the force of mathematical analogies, he is carried away, almost in spite of himself, into a path of whose final destination he himself is ignorant. Having habits of mind

> formed in great part by the teaching he has received and by the ideas which prevail around him, he often hesitates to break with customs and seeks to reconcile with them those new ideas whose necessity he perceives. Nevertheless, little by little, he finds himself forced to arrive at interpretations which he had not in the least foreseen at the beginning, and often ends by being all the more convinced of them the longer he has tried in vain to avoid them. (de Broglie 1960, 144)

The description given here and the reactions to Planck's and Einstein's work is important, for it shows, supporting Kuhn, that a revolutionary discovery or hypothesis is not merely one that introduces new data, but one that shakes the very foundations of the field of research - even the very psychology of the researchers (Bachelard 1963; 1984). These are foundations which even the researcher is reluctant to disturb. The revolutionary nature of the hypothesis is perhaps best indicated by the resistance to it, and the resultant complications it introduces for those who wish to remain in the old paradigm. As Heisenberg notes: "Once one has experienced the desperation with which clever and conciliatory men of science react to the demand for change in the thought pattern, one can only be amazed that such revolutions in science occur at all" (qtd. in Zukav 211).

The complexity of physics at this stage is summed up by Wolfgang Pauli, who wrote to Kronig in 1925: "At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics" (Kronig 22; Kuhn 1970c, 84).

5. The cause of the problem.

The cause of the problem was simple: language did not match reality. There was confusion between the model of the object as described by language, and the object itself - a point foreseen by scientists such as Maxwell and Lord Kelvin (Welsh 138-140).

Not only in the description of the constituents of the atom, but in the actual processes and overall structure, the physicists had problems with common language (Zukav 220, 270-284). Viewing the atom as a microcosm of our solar system, or the photon waves as behaving like sound waves were analogies that generated as many problems as they solved; inevitable as the use of such analogies were, they were not identical with the reality they purported to describe.

The confusion had been caused by the metaphoric nature of language that emerged when one said "that is a particle", or "that is a wave." The statement "light consists of particles" does not mean the same as "light is not made of waves," and the question "Is light a particle or is light a wave?" is as meaningless as "Is Hamlet a prince or is Hamlet a son?" As noted above, depending upon the experimental apparatus, the photon (or electron) sometimes displays particle-like characteristics, and sometimes wave-like characteristics. An attempt at a strict determination "misses the point" (Born 1956, 48): all that can be said is "light is like light" (Brown 140).

In a letter to Henry Stapp, Heisenberg notes that "at these points we always get easily at the limitation of language, of concepts like `existing,' `being,' `idea,' etc" (Stapp 1972, 1113, 1114). Born reiterates this sentiment with, "we are not justified in concluding that the `thing' under examination can actually be described as a particle in the usual sense of the term" (1962, 99). He continues:

> The ultimate origin of the difficulty lies in the fact (or philosophical principle) that <u>we are compelled to use words of common language</u> when we wish to describe a phenomenon, not by logical or mathematical analysis, but <u>by a picture appealing to our imagination. Common language has grown by everyday experience and can never surpass these limits</u>. Classical physics has restricted itself to the use of concepts of this kind; by analysing visible motions it has developed two ways of representing them by elementary processes: moving particles and waves. <u>There is no other way of giving a pictorial description of motions - we have to apply it even in the region of atomic processes, where classical physics breaks down</u> [my emphases]. (Born 1962, 99)

In the words of Schrödinger, "Matter has ceased to be the simple palpable coarse thing in space that you can follow as it moves along, every bit of it, and ascertain the precise laws governing its motion" (1961, 13).

Arriving at a point where they had to consider what to call a particle (Einstein and Infeld 280), the physicists could use Heisenberg's "matter waves" (1958a, 48-9) or Eddington's "wavicles" (1929, 201), or use quotation marks. Even more appropriately, they might later have adapted Jacques Derrida's practice, recognising that the "sign ~~is~~ that ill-named ~~thing~~" (1974, 19ff; 1973, 129ff), and so write "This ~~is~~ a ~~particle~~." (This is not as extreme a suggestion as it may seem - possibly some of the earliest "Post-Structuralist" work was done by Einstein and Infeld in their discussion of ether, when they discussed the "`e__r' problem" [176, 177]).

As my comment above indicates, problems in physics with the nature and limitations of ordinary language are parallelled by similar difficulties in a number of cultural fields, not least of them literary theory, derived as Structuralist and Post-Structuralist variants are from the linguistic insights of Ferdinand de Saussure. Indeed, Derrida's Post-Structuralist or "Deconstructionist" critique of philosophical language and emphasis on its limitations in attempting to describe ultimate reality, can be seen as occurring after a similar moment of crisis to that which engulfed the nuclear physicists in the 1920's.

Saussure himself had emphasised the arbitrary nature of the sign, and warned against confusing signifier and signified. In contrast, as we saw in Chapter One, the arguments by those such as Richards (1926, 267-8; 1934, 279-280; 1974, 236-37), Wellek and Warren (1980, 22-23) and Ransom (284-326), were built upon a vision of the direct denotative nature of scientific language. This was recognised as false by the scientists: as Hanbury Brown says, "One of our most common intellectual sins is to confuse a concept with the reality which it represents, and to use it outside its proper domain of validity; in religious language that is the sin of idolatry" (140).

In 1922, Heisenberg had asked Bohr, "If the inner structure of the atom is as closed to descriptive accounts as you say, if we really lack a language for dealing with it, how can we hope to understand atoms?" Bohr replied "I think we may yet be able to do so. But in the process we may have to learn what the word `understanding' really means" (qtd. in Zukav 219).

6. Solutions

6.1 First Moves.

Bohr and others continued to try to find a "correspondence between classical mechanics and quantum theory (Bohr 1926, 265; 1925; Uhlenbeck and Goudsmit 1925; 1926). In 1926 Erwin Schrödinger published a series of papers in which he argued that an electron does not follow a planetary orbit around a nucleus, but is, in fact, a wave motion in perfect phase (1926a;b;c;d;e; 1953). Schrödinger insisted on the reality of the wave, and his model was mathematically sound. Although it was important, and the concept is still used today, other physicists doubted his work, claiming that its usefulness was limited on the grounds that it did not always conform to observations (Bohr 1928, 585-590; Born 1962, 96-104; Dirac 1927a;b; 1963; 1966, 1-4; Einstein 1940a, 490; Heisenberg 1985, 127). As far as Dirac was concerned, Schrödinger had learnt that it was sometimes "more important to have beauty in one's equations than to have them fit experiment" (Dirac 1963, 47).

An option that pointed towards a solution of a different kind was offered by Pascual Jordan, Max Born and Werner Heisenberg (Bohr 1928; Born 1926; 1956 6-16; 1962 96-100; Born, Heisenberg and

Jordan 1926; Sir C.G. Darwin 1928; Jordan 1926a) who claimed that the wave was a probability wave. Heisenberg continues:

> Born had made the first step by calculating from Schrödinger's theory the probability for collision processes; he had introduced the notion that the square of the wave function was not a charge density as Schrödinger had believed, that it meant the probability to find the electron at a given place. (1985, 127)

Born himself writes of the subject:

> the whole course of events is determined by the laws of probability; to a state in space there corresponds a definite probability, which is given by the de Broglie wave associated with the state. A mechanical process is therefore accompanied by a wave process, the guiding wave, described by Schrödinger's equation, the significance of which is that it gives the probability of a definite course of the mechanical process. (Born 1962, 97)

De Broglie himself, finding fault with Schrödinger's work and his own pilot-wave thesis, yet reluctant to give it up, eventually sided with Heisenberg's camp (de Broglie 1960, 150-164).

This line of investigation, then, tended towards a method of dealing directly with reality without using normal language.

6.2 A different language.

6.2.1 The nature of the language

Based on their notions that the closest one could come to describing a wave in normal language would be to say that the wave was not a real wave, but only a probability wave, a group of physicists led by Heisenberg and others attempted to solve the problem of language by a purely mathematical solution. In their description, mathematics was seen as a "refinement of general language, supplementing it with appropriate tools to represent relations for which ordinary verbal communication is imprecise or too cumbersome" (Bohr 1963, 60).

In the light of this, and with very little understanding of the laws of matrix (Heisenberg 1985, 127), Heisenberg and H.A. Kramers developed the theory of matrix mechanics, which saw the atom as a phenomenon created purely out of mathematical and statistical data.

The statistical language did at least offer a way out of the problem of naming the sub-atomic phenomena, so that "wavicles" and the like could be dispensed with. When speaking of any phenomenon, one only really spoke in statistical terms. Born sums up the attitude: "Let us therefore not ask exactly what a particle is, but be satisfied to know that it is in a definite, though fairly large, region of space. The contradiction between the wave and corpuscular theories then disappears" (1956, 31-32).

6.2.2 The accuracy of the language

Statistics, however, cannot address particular facts about individuals; and the closest it comes to certainties is setting limits or paradigms of probabilities for a group. In the light of this, "the theory can only predict probabilities, and these are determined by the waves (they are the squares of the amplitudes)" (Born 1956 159). Oppenheimer gives an indication of this, when he says:

> If we ask, for instance, whether the position of the electron remains the same, we must say "no"; if we ask whether the electron's position changes with time, we must say "no"; if we ask whether the electron is at rest, we must say "no"'; if we ask whether it is in motion, we must say "no." (qtd. in Capra 1983, 166)

In addition, the idea of statistical probability had to be refined in order to determine the degree of probability of finding the particle in a specific position in the moving wave. In order to give this with any degree of mathematical certainty, Heisenberg introduced what was to become known as the "Uncertainty Principle*" ("prinzipiell unbestimmt") (1927, 172-198; 1985, 127-128). The uncertainty principle introduced the notion that when one spoke about a movement of a particle, one could not determine the position and the momentum absolutely - the accuracy of one of these would have to suffer at the expense of the other (Heisenberg 1958a, 46-48; Born 1956, 47). The degree of inaccuracy was then safely and mathematically determined.

The belief was that this method would by-pass the philosophical problem, for although one might measure one aspect, there is no intention of denying the other's existence; we simply do not know what its value is. Unfortunately, in a manner reminiscent of the problem of wave-particle duality, this is exactly the philosophical problem. As Paul Dirac writes:

> [I]t becomes important to remember that science is concerned only with the observable things and that we can observe an object only by letting it interact with some outside influence. An act of observation is thus necessarily accompanied by some disturbance of the object observed [my emphasis]. (1947, 3)

Does this mean that if we measured the momentum, the particle would have no definable position? Common sense tells us no, but observation in science often tends to conflict with the prejudices of "common sense": common sense also told the ancients that the sun revolved around the earth, and that heavier objects fall quicker than light objects - it also denies that the constancy of the velocity of light. By the uncertainty principle, however, we are left with two possibilities, none of which are satisfactory: direct observation of the event can only give approximations; the particle has unknown properties which materialise only as the result of the observation. Bohr noted "a sentence like `we cannot know both the momentum and the position of an atomic object' raises at once questions as to the physical reality of two such attributes of the object [my emphasis]" (1951, 211).

6.3.3 The inaccuracy of the language.

There was the recognition that while a mathematical description may be accurate, it cannot perform what normal language can - carry an image from one mind to another. The very denotative factor of an "ideal" mathematical language would deny it the ability to compare an unknown with a known in order to make the unknown accessible as an image. While analogies in normal language are weaknesses, this connotative element allows us to compare the unknown (e.g. the atom) to the known (e.g. the solar system) in order to make the unknown accessible as an (albeit inaccurate) image.

The reason for the hesitation over accepting Heisenberg's "matrix mechanics" came from the reluctance of the physicists to abandon the advantages for understanding of the mental image. Schrödinger's "wave mechanics" equation, in spite of the problems mentioned above, at least gave a picture. Heisenberg's, on the other hand, leaves us with the suspicion that reality is made up of statistics and probabilities. Because reality had always been discussed in terms of normal language (even in classical physics where mathematical symbols were simply reflections of real events and objects), a rejection of the metaphorical language meant a rejection of the discussion of reality.

Thus the paradox: if one wishes for an absolute precision of language, one resorts to mathematics, and foregoes a clear picture for the imagination; if one wishes for a clear picture for the imagination, one resorts to metaphoric or normal language, and then loses precision. As Einstein wrote "As far as the laws of mathematics refer to reality, they are not certain; and as far they are certain, they do not refer to reality" (qtd. in Capra 1983, 49).

The attempt at clarification of the object, then, does not result in a clearer mathematical description, but rather in a rejection of the "picture" of the object and a concentration on the language, mathematics, that is being used. In linguistic terms, it means an insistence on the importance of the signifier over the signified. In effect, what happens is an attempt at making the signifier the signified so that the mathematics becomes the reality. In the same way that Relativity emphasised the shift from the object to the language, so in quantum physics we have an equivalent epistemological shift in the description of the atom's structure.

In literary theory, the relationship between reality and language has been a point of debate since Aristotle. It had expressed itself as a problem in the linguistic theories of Saussure and the later semioticians. One doubts it can be coincidence that in the early twentieth century, the paradox of the inability of language to grasp reality and the necessity of language for communication of reality became major themes in literature like Conrad's Lord Jim, Under Western Eyes, and Victory, and Joyce's Finnegans Wake and Ulysses.[8]

6.3.4 The inaccuracy of "reality."

Not only was Heisenberg's version of reality unappealingly statistical, like Relativity, it also led to questions concerning the notions of causality per se. The position of the particle - indeed the outcome of the experiment itself - seemed to be dependent on probability and chance with the only definite action coming from the observer, who, by the experimental arrangement, could almost be accused of creating the picture to fit his own theoretical beliefs (Born 1956, 14-16, 33-48; 1971, 228-229; de Broglie 1960, 242; Dirac 1947, 4).

The nature of scientific truth, in Reichenbach's words, was not an absolute truth at all, for "scientific statements can only attain continuous degrees of probability whose unattainable upper and lower limits are truth and falsity" (qtd. in Popper 1980, 29-30). Uncertainty, chance, and lack of absolute objectivity in physics were not due to errors caused by inadequate experimental methodology or equipment; it was a fundamental law of physics in much the same way that $V = v_o + g\triangle t$ was fundamental to mechanics.

Scientists did not miss the vast philosophical implications of the uncertainty principle to physics and to fields outside their own (Born 1962, 98-102). Niels Bohr, especially, often stressed "the analogy in epistemological respects between the limitation imposed on the causal description in atomic physics and situations met with in other fields of knowledge" (1951, 233-234; 1937a;b; 1948).

7 The importance of the theory.

Each solution to each problem had resulted in new problems, the grand solution to which seemed to lie in only one direction: a recognition of the importance in the theoretical, epistemological CS as a determining factor in the physicists' physical CS. Sir Arthur Eddington had noted that the "conclusion is that the whole of those laws of nature which have been woven into a unified scheme - mechanics, gravitation, electrodynamics and optics - have their origin, not in any special mechanism of nature, but in the workings of the mind" (Eddington 1921, 198).

Since the insights are so similar in form, a comparison with the Structuralists is in order here. Indeed, with hindsight, we may liken this degree of understanding by the physicists to that stage set by Vico, Saussure and Lévi-Strauss, whose work empowered the radical departures of the later Structuralists and Post-Structuralists on epistemological matters.

Furthermore, in a way that parallells the eventual realisation of the later Structuralists (see the Jakobson-Tynjanov essay [52-55 of this thesis] and Barthes's writings [69-76 of this thesis]), Niels Bohr argued in that the physicists had learnt that "no content can be grasped without a formal frame and that any form, however useful it has hitherto proved, may be found to be too narrow to comprehend new experience" (1951, 240).

The "myth" that became the dominant paradigm in particle physics was called the Copenhagen Interpretation.

7.1 The Copenhagen Interpretation

The Copenhagen Interpretation was the view held by Bohr, Heisenberg and Dirac. It was in alignment with (although independent of) the view of the philosopher William James (Stapp 1972, 1103-1111), that what is determined to be true, is simply that which works in accordance with our experiences (James 1909, 136-220) and that the automatic assumptions that we have about the discontinuity between "the subject and the object" have to be questioned (102-120).

The Copenhagen Interpretation acknowledged that the experimental apparatus and observer were part of the experiment by virtue of the apparatus being used, that although classical descriptions were inadequate, only classical descriptions could be used at the macro-level, and as regards the behaviour of the particles, only probabilities could ever be determined (Bohr 1928; 1929; 1937a;b;c; 1951, 209-229; 1963; Heisenberg 1958a, 55-56).

The implications of Bohr's work were profound. He argued that although there is an outside world, our descriptions are not of <u>it</u>, but rather of <u>our experiences of it</u>. His view was very close to Todorov's view that the "method creates the object...the object of a science is not given in nature but represents the result of an elaboration" (1981, 8). The aim of science, Bohr felt, through "beauty and self-consistency" (1934, 92) ("<u>Schönheit und inneren Zusammenhang</u>") (1929, 483) "is both to extend the range of our experience and to reduce it to order" (1934, 1). He says further that in the description of nature, "the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience" (18). He argues:

> The epistemological problem under discussion may be characterized briefly as follows: For describing our mental activity, we require, on one hand, an objectively given content to be placed in opposition to a perceiving subject, while on the other hand, as is already implied in such an assertion, no sharp separation between object and subject can be maintained, since the perceiving subject also belongs to our mental constant [my emphasis]. (96)

Bohr argues that it "is wrong to think that the task of physics is to find out how Nature is. Physics concerns what we can say about Nature" (qtd. in Pagels 78). Sir James Jeans writes: "The true object of scientific study can never be the realities of nature, but only our own observations on nature" (1958, 175-76). This view is echoed by Hanbury Brown when he says that this "new view of reality shows us that all our description of objects are not of what they are `like in themselves' as was envisaged in the Mechanical Philosophy, but are descriptions of how they behave in response to the observations that we choose to make" (140). Although Heisenberg insists that physics is an exact science, we can see that his definition of an exact science is different from that still espoused by many of the literary theorists:

> When we speak of the picture of nature in the exact sciences of our age, we do not mean a picture of nature so much as a picture of our relationship with nature....Science, we find, is now focussed on the network of relationships between man and nature, and which we as human beings have simultaneously made the object of our thoughts and actions. Science no longer confronts nature as an objective observer, but sees itself as an actor in this interplay between man and nature. The scientific method of analysing, explaining and classifying has become conscious of its limitations, which arise out of the fact that by its intervention science alters and re-fashions the object of investigation. In other words, method and object can no longer be separated. The scientific world-view has ceased to be a scientific view in the true sense of the word. (Heisenberg 1958b, 28-29)

This passage makes especially clear how some arguments for a "scientific" literary theory are working on inadequate presuppositions. The very nature of the word "science" had changed through the effect of quantum physics. As Feyerabend has noted, the emphasis of quantum physics on philosophy made it closer to Aristotelian empiricism than to any work by Galileo and Newton (Feyerabend 1965b, 154-168). Those who sought an objective scientific language to communicate about a distinct and definable object to the mind of a separate observer were being "unscientific" as far as particle physics was concerned. Any literary theorists who hankered on scientific grounds after the same end were, by the same light, mistaking their ideals.

7.2 Opposition to the Copenhagen Interpretation

The fight among the physicists, however, was far from over. The Copenhagen Interpretation was unacceptable to many of the most prominent scientists - Einstein, Planck, von Laue and Schrödinger - who aimed for absolute objective description of physical reality.[9] Some time before, Max Planck had expressed his fears at the results that notions similar to those now being expressed in the Copenhagen Interpretation could have for physics:

> Are not indeed all so-called "Laws of Nature" essentially but more or less effective rules by means of which we summarize the temporal course of our sensation as accurately and as conveniently as possible? If that were so, then not only common-sense but exact Science would have been fundamentally at fault from the beginning. (Planck 1914, 69)

His argument may appear convincing, but only because it appeals to common-sense - and we have seen enough evidence by now to mistrust common-sense as a primary recourse. It is especially disheartening that someone like Max Planck, who had experienced the full force of prejudice on the grounds of the "Laws of Nature" and "common-sense" should use this argument.

Einstein never could "abandon, actually and forever, the idea of direct representation of physical reality in space and time; [nor]...accept the view that events in nature are analogous to a game of chance" (1940a, 492), and wrote to Max Born that he "would be very unhappy to renounce completely causality" (Born 1971, 23). Although he had very much earlier noted the statistical nature of quantum theory, the degree of chance ("Zufall") was seen as a weakness (Schwäche) of the theory (1917). In a similar vein, and expressing sentiments similar to those expressed by Pauli the following year, he wrote to Niels Bohr in 1924:

> I cannot bear the thought than an electron exposed to a ray should by its own free decision - aus freinem entschluss - choose the moment and the direction in which it wants to jump away. If so, I'd rather be a cobbler or even an employee in a gambling house than a physicist. (qtd. in Brown 1986, 81)

In 1926 he wrote to Born: "Quantum mechanics is certainly imposing. But an inner voice tells me that it is not the real thing. The theory says a lot, but it does not really bring us any closer to the secret of the `old one'. I, at any rate, am convinced that He is not playing dice" (Born 1971, 91). We have already seen above how Planck relied on common sense. Einstein, on the other hand, relies on intuition or "scientific instinct" (Einstein 1936, 377).[10]

In order to prove his view, Einstein designed a thought experiment* (Gedanken-experiment) in conjunction with Podolsky and Rosen, later known as the EPR* experiment, and published in the form of an answer to the question: "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" (1935). The paper hankered after an "objective reality, which is independent of any theory" (777) and came to the conclusion that the uncertainty was due merely to the fact that quantum theory did not give a complete description of reality (780). In October 1935, in an article with the same title as the EPR experiment, Bohr argued that the experiment, supported by General Relativity, actually corroborates the Copenhagen Interpretation (1935, 701-702;

Bell 1964). Nevertheless, Einstein remained sceptical to his death: in 1954 he said to Heisenberg: "Well, I agree that any experiment the results of which can be calculated by quantum mechanics will come out as you say, but still such a scheme cannot be a final description of Nature" (Heisenberg 1985, 128).

On the distance between the physical reality and the observer, the group was, perhaps, a little less sure. Einstein had often argued that there is an unbridgeable gap between observer and reality, and in spite of EPR, he also often acknowledged that a theory made up totally of observable phenomena was unlikely; often, the theory actually determined what could be discerned (Einstein 1936; 1940a; 1951b, 683-684; Heisenberg 1985, 127-128; Holton 254-267). "Physical concepts," he stated often, were "man-made," "free" or "arbitrary creations of the human mind," and were not determined only by observations of the external world (Born 1971, 192; Einstein 1936, 350; 1940a, 487; Einstein and Infeld, 31, 294), and "single experiences must be correlated with the theoretic structures in such a way that the resulting coordination is unique and convincing" (1940a, 487). Feyerabend mentions that Einstein warns against sticking too closely to one epistemological system (Feyerabend 1968, 199), but at the same time, Einstein also argues that

> The reciprocal relationship of epistemology and science is of a noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is - <u>insofar as it is thinkable at all</u> - primitive and muddled. (1951b, 683-684)

Bohr's statement above that "<u>[t]he scientific world-view has ceased to be a scientific view in the true sense of the word</u>," however, has also been unacceptable to many modern philosophers. Karl Popper rejects "the whole subjectivity probability theory [which] invades the realms of physics" (1980, 233-234), and is "<u>the great quantum muddle</u>" (1967, 18-20). In a similar light, Herbert Dingle says that "It is a common complaint among the educated, <u>but</u>

not professionally scientific, public that the physics of this century has become absurd [my emphasis]" (1952, 256). Having seen that many of the major physicists have expressed dismay at some aspects of the seeming absurdity of physics at some time, we are tempted to ask ourselves whether Einstein, Pauli, Heisenberg, Bohr, Born and Dirac are to be considered "not professionally scientific"?

7.3 The philosophy of the Copenhagen Interpretation

In the beginning of the chapter, I noted that a reader of philosophy or psychology would perhaps note the naiveté and crudity of the philosophic formulations that have been offered in this chapter. Einstein himself recognises an element of truth in the notion that "the man of science is a poor philosopher" (1936, 349), and asks "[w]hy then should it not be the right thing for the physicist to let the philosopher do the philosophizing?" The answer is that "at a time when the very foundations of physics have become problematic as they are now...[the scientist] must try to make clear in his own mind just how far the concepts which he uses are justified and are necessities" (349). The scientist needs to recognise that "[t]he results of scientific research very often force a change in the philosophical view of problems which extend far beyond the restricted domain of science itself" (Einstein and Infeld 51). As Max Born notes, "Every scientific period is in interaction with the philosophical systems of its time, providing them with facts of observation and receiving from them methods of thinking" (1956, 38).[11]

The physicists who supported the Copenhagen Interpretation argued that the others - and even they themselves - were trapped by an ancient way of thinking about their world. On the part of their opponents, there was a reluctance to drop the planetary model of the atom, and a difficulty with applying words like particle, wave, mass and matter (Born 1956, 20-26; Einstein and Infeld 33-34). Heisenberg criticised them for being too entrenched in old modes of thinking. He argued that the problem is not merely that the other camp is prejudiced; the very language they use links the notion of science with absolutes of measurement, position, energy, and temperature, and it is not surprising that they were unable to accept his view, for the "statement that such an objective description is not possible in the world of atoms...was indeed very revolutionary" (1985, 392). Niels Bohr supports this contention, writing

> it has been difficult to reach mutual understanding not only between philosophers and physicists but even between physicists of different schools, [and] the difficulties have their root not seldom in the preference for a certain use of language suggesting itself from the different lines of approach. (1951, 240)

Heisenberg also looks at the problem of conceptualisation from a philosophical point of view. In a letter to Henry Stapp, he asks of ideas, "do these ideas `exist' outside of the human mind or only in the human mind? In other words: have these ideas existed at the time when no human mind existed in the world?" (Stapp 1972, 1113). It is obvious that while this question needs to be considered for quantum physics, it is unlikely that any person could answer it without recourse to a specific philosophical, ideological, cultural or religious standpoints.

The extent to which language is embedded in myth and thus creates reality was a major recognition for the Structuralists - ironically, as we saw above, this emphasis on the medium of observation which denied the Structuralist the ability to "maintain a scientific distance between itself and its object" (Barthes 1967b, 898), led Barthes to believe that he was being "unscientific." The recognition of the same problem by the physicists led to one important difference - once they realised the extent to which their physical CS was embedded in the epistemological CS, instead of rejecting their ideas as "unscientific," they simply redefined the word science in order to include these new ideas. Precisely what had led up to this since the turn of the century had escaped most of the literary theorists, except, perhaps for the later Jakobson. The other literary theorists, to a greater or lesser extent, could not free themselves from the old idea of science.

What had never escaped the physicists, even those who opposed the Copenhagen Interpretation, was that the epistemology of their own subject was crucial to the understand of the subatomic events, and also influenced the understanding in other fields (Bohr 1937a;b). Einstein made the point quite clear when he wrote:

> The whole of science is nothing more than a refinement of every day thinking. It is for this reason that the critical thinking of the physicist cannot possibly be restricted to the examination of the concepts of his own specific field. He cannot proceed without considering critically a much more difficult problem, the problem of analyzing critically the nature of every day thinking. (1936, 349)

Max Born argues along the same lines:

> The justification for considering [physics] as a philosophical doctrine is not so much its immense object...as the fact that the study of this object is confronted at every step by logical and epistemological difficulties; and although the material of the physical sciences is only a restricted section of knowledge, neglecting the phenomena of life and consciousness, the solution of these logical and epistemological problems is an urgent need of reason. (1956, 37-38)

As Henry Stapp writes, the Copenhagen Interpretation was based on the assumption that the "theoretical structure did not extend down and anchor itself on fundamental microscopic space-time realities. Instead it turned back and anchored itself in the concrete sense realities that form the basis of social life" (1972, 1098).

Roszak argues that the days of the scientist believing that he works in splendid isolation, independent of his society,[12] are over:

> Scientists who long for academic isolation and the license this grants them to pursue their careers without once looking beyond the narrowest professional assessment of their work, begin to live parasitically off the surface of their heritage. Worse still, they themselves cease to be whole persons; they become disembodied amnesiac intelligences, disclaiming responsibility to any other aspect of their nature than the intellect and its fascinations. (Roszak 217)

8 Structuralism in the 1960s and later.

The contemporary search for ultimate matter indicates that the debate and the problems concerning the awareness of language and epistemology, are still to be fully resolved. Although the search for an ultimate matter is hardly new, what is new is the extent to which the researchers have realised that their epistemology has directed their research.

The start of the search for ultimate matter is perhaps with Thales of Miletus and Anaximenes (Schrödinger 1961, 55) and culminating in Greek times in Democritus's atomistic model. Democritus believed in matter as discontinuous, consisting of minute indivisible particles called atoms separated by a universal vacuum, as opposed to Aristotle's belief in a continuous universe (Huxley 1893b 67).[13]

Until 1928, the physicists "knew" that they had discovered the elementary particles. Because of Dirac's work in 1928, however, the concept of an elementary particle was eroded, as all of these particles seemed compound systems of other particles (Heisenberg 1985, 128-29).

Up to 1964, physicists had battled to find methods of classifying the "arbitrary jumble of elementary particles" (Gell-Mann and Rosenbaum 72) that had by then been discovered. In 1964, Murray Gell-Mann introduced the notion of quarks* which were the building blocks of the "elementary" particles called hadrons*. The quark model was difficult to accept; although it was a useful "mnemonic and aid when calculating with unitary symmetry groups" (Close v), the model was simply "a formal mathematical model" which was a "simpler and more elegant scheme" than had been proposed before (Gell-Mann 1964, 214) - whether or not quarks actually existed was a moot point.

It is important to bear in mind that a quark has never been found outside of the particle of which it is a constituent; thus, when physicists speak of finding new quarks, they have actually "created" the particle in the following way:

> 1. the quark model offers a perfect mathematical explanation for the existing particles. Thus, we assume that it is correct.
>
> 2. a new composite particle has been discovered, and this new particle cannot be explained in terms of the existing quarks.
>
> 3. thus, a new quark has to exist - its properties will be determined by the properties of the newly-discovered composite particle. This is perfectly illustrated by the "finding" of the "beauty" quark through the J-particle.

Thus, when one says that five quarks have been found, what one means is that if one uses the theory of quarks as a basis of matter, one needs five quarks in order to explain all known hadrons. It is not so much that a hadron consists of quarks; it is more that they behave <u>as if</u> they consisted of quarks. This sounds familiar: it is not so much that light consists of particles, it is more that light behaves <u>as if</u> it consists of particles. When physicists claim "[n]o particle has been found that does not fit into the quark picture" (Trefil 151), it does seem a somewhat hollow victory.[13]

Although there seems to be a tidy arrangement of quarks into three, possibly four or five families (Cline 1988, 42-49; Schramm and Steigman 1988, 44-50), there are several questions to be answered, and it remarkable that many of them are similar to the questions that had been asked of the other "elementary" particles by Murray Gell-Mann in 1957: how many quarks are yet undiscovered? Are they really elementary particles? If so, how would we know?

The most important statement in favour of the quark model is that it works.

In this regard, researchers who assume the validity of the quark model do recognise that many of the results may be obtained from other models (Sakita). In order to establish the significance of any system, however, it is important to understand the philosophy of the system. It is this type of realisation that sought a basis for the earlier work of the physicists, and resulted in the Copenhagen Interpretation philosophy. It is also this that Vico had established, and what Barthes supplies to the Structuralist model - the recognition that the ideas and constructions do not exist prior to man, they are his creations, based upon his social organisation.

Some theoretical physicists have used Heisenberg's and Bohr's philosophy and matrix (Bohr 1928; Heisenberg 1926; 1943a;b; Jordan 1926b;c) to construct theories other than the quark model: the most noted are perhaps S-matrix* (Capra 1979; 1983; Chew et al 1957; 1958; Heisenberg 1985; Jeans 1959; Stapp 1971) and Bootstrap* (Capra 1979; Chew 1968; 1970; Chew et al 1964; Stapp 1971; Zukav), which for a time have commanded a following, although these are now felt to be outdated by mainstream physics which supports the quark model (Polkinghorne 82-91; Trefil 217-18).

On what basis does the quark model rest? The question, according to the supporters of the Copenhagen Interpretation, is the same as that asked of light and the electron, and has already been answered: on the very language and assumptions of classical physics which, ironically, we have constantly rejected as being unacceptable in quantum physics. (David Bohm goes so far as to find a link between the search and the internal grammatical structure of our language [Bohm, 27-62]). They see the insistence on these classical assumptions and visualisations as simply reflecting the persistence of the outlook of those who fought the basis of the Copenhagen Interpretation.

As Heisenberg notes, the dominant question always seems to remain "Of what does this object consist and what is the geometrical or dynamical configuration of the smaller particles in the bigger object?" (1985, 129). We see that here that the assumption is that there are smaller particles, and the job is to find them - this, as far as Einstein is concerned, is a natural determination of classical mechanics (1936, 360). Heisenberg sees the search for quarks as a prejudice influenced by the atomistic philosophy of Democritus (129), and is unsurprised that such a search did lead to some ultimate theoretical particle, in this case, quarks. The point, he insists, is to change the question, and instead of looking for fundamental particles, to look for "fundamental symmetries [that] define the underlying law which determines the spectrum of the elementary" (129-130; 1975, 57-58).

9. Conclusion

We have been looking at the literary theorists' image of science as it has stood in the twentieth century, and have explained it in relation to the ideas expressed on the subject by physicists. As physics stands at the moment, and as it has stood since Planck's constant, it proclaims the fundamental weakness of the belief that mechanics is the same as science. While mechanics does have its place, especially in the macro world, the discoveries of Relativity and sub-atomic physics have implications affecting any literary theorist who looks for a "scientific" literary theory.

The point demonstrated in this chapter is that since the beginning of the century, the idea of "science" has undergone a revolution that affected all spheres of physical research, from the epistemological to the experimental.

From a comparison between what was said of a scientific model of literary theory in the preceding chapters, and what we have seen in this chapter, we can draw two conclusions:

Firstly, the New Critics' notion of science was based purely on the Newtonian notions, and were thus not dealing with a scientific model at all, but rather a mechanistic one.

Secondly, as was eventually the case with Structuralist literary theory, science began to look closely at its role as creator:

> We have found a strange foot-print on the shores of the unknown. We have devised profound theories, one after another, to account for its origin. At last, we have succeeded in reconstructing the creature that made the foot-print. And Lo! it is our own. (Eddington 1921, 200-201)

With these words, Eddington ends his Space Time and Gravitation, and these words still describe the physics of the twentieth century. But the situation he describes is also that of Post-Structuralist literary theory which has had to come to terms with a very similar realisation, contrasting it with its immediate predecessors. Where Structuralism had hoped to attain to the respectability and objectivity of a "scientific" description of the literary work and its conditions, Post-Structuralism recognises that this aim is impeded by the conditions of language itself. The seeds of doubt, it was discovered, lay buried in Structuralism's own first principles among the implications of Saussure's linguistic system.

It is ironic that when these revisions were deemed necessary by the theorists, they occurred for internal and philosophic reasons, not because they had appreciated the effect of the scientific revolution. When they rectified their oversight, as Barthes did, and recognised the determining part played by their own language, they believed that they were being "unscientific."

CHAPTER FOUR - FEYERABEND

An artist's inspiration, or a scientist's theory, reveal the unpredictable power of human imagination.

(Leon Brillouin 1964, iv)

[F]or poets as for all other psychics the approval of Science has become superfluous and the age of apology is over.

(Paul Waldo-Schwartz 1976, 93)

It is time to cut [scientists] down in size, and to give them a more modest position in society.

(Paul Feyerabend 1975, 304)

CHAPTER FOUR - FEYERABEND

1. Introduction

In Chapter One and Chapter Two, I identified the principal basis of the claims to objectivity and authentication in New Critical and Structural literary theory as the supposed scientific pretensions of the theories in question. Two issues, however, were further raised in the process. The first was the question of the accuracy of the theorists' view of science, and this was evaluated in Chapter Three by an examination of the nature of physics in the twentieth century. The second issue was the question of why the scientific model had been sought at all.

Paul Feyerabend notes that these two issues "arise in the course of any critique of scientific reason" (1981i, 203). He puts these two issues in the form of two questions: "What is science?" and "What's so great about science?" (203; 1978, 73).

As we saw above, "What is science?" formed the basis of Chapter Three, and I examined this issue by concentrating on the work of practising scientists rather than philosophers, although reference to the philosophers was made. Chapter Four, in contrast, will deal with the issue raised by the second question: "What's so great about science?", and conversely, I shall concentrate on the philosophers rather than the physicists, although reference to the physicists will be made.

I have already looked briefly at Kuhn, and will do so further here, as he serves to introduce the chief points of the issue at hand. For the main, although I shall occasionally refer to other philosophers, I shall concentrate on Feyerabend, whose work supplies an intriguing and useful theoretical framework for dealing with this issue.

2. Paradigm-discipline shift (PDS)

2.1 Paradigm-discipline shift in science: the Newtonian model

In Kuhn's model of scientific revolutions, one premise is that in order for the paradigms and the changes to be successful, the acceptance of the new paradigm must be based purely on its consistency, and it may never be regarded as the infallible, sacred truth (1970c, 150ff.; Watkins 26).

As seen in Chapter Three, the success of the Newtonian model was so great that it became identified, even in the minds of many literary theorists, with science itself. Science was Newtonian physics to such perceptions. Even when the Newtonian model was no longer unquestioned, it continued to have importance within science: Einstein writes "In spite of the fact that, today, we know positively that classical mechanics fails as a foundation dominating all physics, it still occupies the centre of all our thinking in physics" (1936, 359).

The process whereby a dominant paradigm becomes synonymous with the discipline, I shall call, for the moment, a paradigm-discipline shift (PDS). I have taken the word "discipline" from Kuhn's own refinement of the term "paradigm" as a "disciplinary matrix" that indicates a particular epistemological matrix within the discipline (1970b, 271; 1974, 462-463). I should, however, emphasise that Kuhn does not see his model working in all disciplines, for he believes that these changes can occur within science only (1970c, 160-164) - a point sometimes unfairly ignored by his critics (Feyerabend 1981b, 24).

In order to justify my belief that Kuhn's description might be used in a general manner, I shall firstly examine the two chief assumptions that he uses to exclude other disciplines from this description - (i) that science is autonomous and (ii) that scientists eventually totally accept the new paradigm. In Chapter

Three, I mentioned that many philosophers continue to debate with Kuhn on many issues concerning the applicability of his model to science. I shall largely ignore this debate to deal only with the two issues mentioned in order to demonstrate that the model need not be confined to science alone. I shall then examine the possibilities of applying Kuhn's model to literary theory. From the possibilities and problems experienced in this examination, it may further be possible to determine reasons for the literary theorists' choice of science as a measure by which to obtain authentication of their literary theory.

2.1.1 The autonomy of science

The major argument supporting Kuhn's distinction between science and other fields such as "art, political theory or philosophy" (1970c, 160), is grounded on his belief in the autonomy of science, which necessitates the insulation of scientists from the politics and other concerns of their society (160ff; 1974, 460-462). This principle exists to ensure the minimum of bias and ideological prejudice on the part of the scientists pursuing their disciplines.

There are several reasons for doubting whether Kuhn's insistence on autonomy is absolute even within the context of his own work. Firstly, this distinction must be seen in the light of a general ambivalence that Kuhn has on the relationship between Art and Science. Although he finds the polarity "disquieting and...unwelcome" in spite of some benefits (1969, 403-405), he recognises that the separation is not as obvious as one might believe, and he concurs with E.M. Hafner that "[t]he more carefully we try to distinguish the artist from the scientist, the more difficult the task becomes" (403; Hafner 390).

Secondly, in Chapters Two and Three, especially in the discussion of epistemology and language, we saw how such a belief in the autonomy of science may rest on illusion. Much evidence was marshalled (see pp. 108-114) to support such a contention.

One understands at an emotional level why Kuhn wished to insist on the uniqueness of the scientific pursuit, and its separateness from the contemporary social concerns of other kinds. During this century science has had repeatedly to cope with ideological intrusions upon its right to free and disinterested inquiry; governments who have attempted to interfere with the pursuit of knowledge they found politically unacceptable, have also had scant respect for any privileged distinction between science and general culture, including literature. Both are a fair target for censorship or ideological perversion. The Nazi opposition to Relativity because Einstein was Jewish (Bernstein 169-173) was as forceful as its attack on undesirable literature, and in the USSR, the attack of Zhdanovshchina on literature (Zhdanov) was matched by its attack on the "bourgeois atomic physicists" of the Copenhagen School with their "devilish tricks" (Zhdanov qtd. in Graham 386; Erlich 148; Trotsky 198). In the United States, organisations that have restricted the study of Evolution and other scientific notions in schools, have also restricted the use of works like The Wizard of Oz, Cinderella and Romeo and Juliet (Lear; Stein). In such circumstances, Kuhn might well want to assert the privileged "difference" of science from other sorts of cultural ventures.

The Einsteinian revolution cannot alone be credited with the recognition of the fact that science was not an absolutely autonomous venture, for this had already occurred to some extent. A famous example of a scientist bowing to social circumstances in an act of self-censorship is Darwin's omission of an explicit discussion of Man in his Origin of Species, "apart from one cryptic sentence" (Burrows 16). The Einsteinian revolution led to an actual incorporation of many erstwhile "foreign" cultural concepts into the fundamentals of scientific discovery (See pp. 85-114 of this thesis). Inspiration was sought in many extra-scientific fields. But the real root of the matter is that Kuhn's own work, by implicitly and explicitly demonstrating the importance of communal, historical (and hence social and cultural) factors in the progress of scientific enquiry, itself bridges the dimensions of culture and science. If scientific advance is not a matter simply of absolute objective truth, but of paradigms and their changes, then of necessity it includes a cultural element.

2.1.2. The role of the paradigm

The other important distinguishing feature between the sciences and the non-sciences, according to Kuhn, is the paradigm. He acknowledges that while other disciplines may undergo revolutions (1970b, 243; 1970c, 160-173), in these disciplines there is not the same dramatic paradigm change from one state with a total domination of one paradigm to another of a similarly exclusive kind. In science, after a period of time, he argues, the new paradigm is accepted by the scientific community as a whole.

In the previous chapter, I demonstrated at length how some of the major issues involved in a paradigm change - especially the reluctance of the older practitioners to accept the new paradigm, and the difficulty of communication between the two sets of scientists - could be seen to have occurred in twentieth century physics. The historical overview suggests that although forceful changes do occur, there is still room to doubt that they include the totality of scientists at any time.

Firstly, while Kuhn's idea of progress might not be towards an ultimate, knowable truth (Chalmers 108) as many of his critics have said (Hacking 1983, 65-74; Toulmin 1970; Watkins 34-37), some linear movement is implied by the manner in which Kuhn refers to "scientific progress" (1970a, 20-1; 1970b, 238), "advance" (1970b, 249-250), evolution from primitive beginnings (1970c, 171-173); and recognises "partial communication" between paradigms (1970b, 232, 266ff.). Admittedly though, this movement is not of the same extent as that advocated by others such as the earlier G.A. Sarton

(Sklair) and Stephen Toulmin (1961, 99-115) who emphasise a linear evolution in the scientists' objective (albeit theory-guided) explanation of Nature.

If the Copenhagen interpretation of quantum theory can be taken as the fullest expression in philosophic terms of the quantum paradigm, it is quite clear that its dominance in the world of physics and physical thought is not absolute. The wave-particle duality, for example, has fallen into the background to a degree, since the quark model tends more convincingly to be visualised in particle terms. Furthermore, many modern philosophers, such as Popper and Lakatos, have rejected the Copenhagen Interpretation (Lakatos 1970, 145). The Copenhagen Interpretation, then, is, and always was, just that: an interpretation. Although it is still dominant, it does not have absolute dominance (See note on Copenhagen Interpretation in Glossary).

Kuhn's demands for the autonomy of science and the unanimous acceptance of the new paradigm by the discipline are the two major reasons advanced for the inapplicability of Kuhn's model to subjects like linguistics (Derwing 9-25; Percival; Searle) and Fine Art (Ackerman 1969; E.M. Hafner 1969).[1] Whether or not a successful application to these disciplines is feasible, is a debate beyond the scope of this thesis. We can see, however, that these two reasons advanced for limiting a paradigm-discipline shift (PDS) to science are unfounded, or at least debatable, and the applicability of a PDS in a general sense to other disciplines is worth exploring.

2.2 Paradigm-discipline shift in literary theory: New Criticism

In addition to showing that only a modified version of Kuhn's model is applicable to science, we also need to investigate the possibility of applying this modified version to literary theory.

Some of the paradigms of literary theory are shown clearly in a diagram constructed by Raman Selden, showing the relative positions of literary theoretical paradigms based on Jakobson's schemes of factors and functions of language (fig.5) (Selden 1985, 4). Merely that there are various paradigms is not the point, however, for Kuhn acknowledges that paradigms do exist in other disciplines. In order for Kuhn's model to be applicable to literary theory in the same way in which it is applicable to physics, it must fulfil two important functions.

Firstly, one paradigm must be dominant to the extent that it undergoes a PDS, and this would be indicated by the practice and language of that paradigm becoming the practice and language of the discipline. In fact, it must be so deeply entrenched that the practitioners within this paradigm will see themselves in a chain of development of the discipline, and not consciously as representatives of a paradigm that is dominating other paradigms: for these practitioners, there are no paradigms and they do not practice their craft according to a paradigm, but according to the obvious, axiomatic principles of the discipline; those who claim to practice it in any other way are false practitioners.

Secondly, the paradigm must have the power to crush research in other possible paradigms for a long period of time, and make much of this other research seem negligible.

Undoubtedly, if there is any paradigm or approach to literary theory that may be said to have undergone the comparable PDS, it is one that is not explicitly shown in Selden's model: New Criticism. As Rabinowitz points out, until at least 1967 "New Criticism would have been regarded as the critical equivalent of a diplomatic language in the world of literary theory" (24-25).

The issue to be examined is the possibility of a parallel between the PDS of the Newtonian paradigm in science, and a PDS of New Criticism in literary theory. For this parallel to be valid, New Criticism must fulfil the two roles mentioned above, and also have other similar characteristics relative to its own discipline.

MARXIST

ROMANTIC FORMALISTIC READER-ORIENTED

STRUCTURALIST

Fig.5: Selden's scheme of literary theories (1985, 4), based on Jakobson's schemes of the factors and functions of language (see ch.3, fig.2).

"Feminist criticism cannot be given a place in our diagram because it is not an `approach' in the sense that applies to other kinds of theory. Feminist criticism attempts a global re-interpretation of all theories from a distinctly revolutionary standpoint" (4).

2.2.1 Objectivity

We saw in Chapter One that New Criticism's most distinguishing feature was its accent on objectivity, and its supposition of a clear distinction between subject and object, with an object easily separated from other "irrelevant" information. It was, in fact, according to Richards, this very feature that New Criticism shared with Newtonian physics that allowed it to be called "scientific" in the first place.

This stress upon objectivity had important consequences for the field of literary pedagogics. Although the emphasis of New Criticism shifted with later work by Leavis and Frye, and other paradigms such as Marxist, Structuralist, Feminist, or Reader-oriented work rose in prominence, New Criticism has an endearing common quality with Newtonian physics: its methods are very simple to teach.

As Rabinowitz notes, the various journals of New Criticism "proclaimed the pedagogical advantages of the `scientistic method' [sic]" (26) of New Criticism. In fact, as Rabinowitz further notes, the emphasis on the pedagogic value of the approach is further illustrated when Leavis attempts to introduce concepts like moral and spiritual values. Since these are far more problematic to teach, the analytic method of practical criticism continues to overshadow the new material. Hartman goes so far as to say that the utilitarian value of New Criticism for the purposes of teaching is so strong that it is leading to an "impotence" in criticism (501).

Many other paradigms, especially those like Marxism and Feminism, demand a reassessment of concepts within the value systems and philosophy of our culture. Consequentially, these extraneous aspects first need to be grasped before literature may be examined from their perspectives. New Criticism, however, does not depend on the knowledge of any other paradigms, as it has built-in presuppositions that are part of our culture, and, as a result, are largely unquestioned (Hawkes 154-156).

In Chapter One, we saw that one of the major weaknesses of the arguments put forward by New Critics was this inability to recognise that their theories were culture-bound expressions; in fact, as Paul de Man argues, writers such as Cleanth Brooks, Robert Penn Warren, René Wellek, Austin Warren, Reuben Brower, M.H. Abrams, R.P. Blackmur, William Wimsatt and Monroe Beardsley would not

> have considered themselves theoreticians in the post-1960 sense of the term, nor did their work provoke as strong reactions, positive or negative, as that of later theoreticians. There were polemics, no doubt, and differences in approach that cover a wide spectrum of divergencies [sic], yet the fundamental curriculum of literary studies as well as the talent and training expected for them were not being seriously challenged. New Critical approaches experienced no difficulty fitting into the academic establishments without their practitioners having to betray their literary sensibilities in any way; several of its representatives pursued successful parallel careers as poets or novelists next to their academic functions. (de Man 1982, 6)

These theorists are characteristic of theorists working within a paradigm that has undergone a PDS and become dominant, as they were oblivious to the extent to which their views were affected by their ideology, and we saw also in Chapter One how they reproached literary theorists who attempted to take cognizance of this. Paul de Man goes on to say that

> Those who reproach literary theory for being oblivious to social and historical (that is to say ideological) reality are merely stating their fear at having their own ideological mystifications exposed by the tool they are trying to discredit. They are, in short, very poor readers of Marx's German Ideology (11).

Above, I noted that for many years Newtonian physics was science, and science was Newtonian physics; Terence Hawkes notes that "`New' Criticism was criticism itself" (152).

Before we write these New Critics off as naive, we should note that this trend has continued today in spite of lessons learned, and many present writers still believe that they are operating outside an ideology or literary theory.[2]

2.2.2 Dominance of language and practice

Although its style has changed, and although it has lost the prominence it once had, the extent to which New Criticism has been a dominant theory can be seen also by the manner in which its language dominates literary theory in a similar fashion to Newtonian physics's domination of the language of science.

In Chapter Three, we saw the dominance of the language of Newtonian Physics, and that words like mass, matter, velocity and momentum were not seen as technical words with meanings belonging to and designated specifically by a particular theoretical standpoint, but were seen as absolute, axiomatic scientific terms: it was for this reason that the change of their meaning demanded by the revolution was difficult; in some cases impossible.

In literary theory, we have much the same phenomenon. Terms like image, theme, analysis, close reading, ambiguity, and irony, are not seen as technical terms with special meanings belonging to and designated specifically by a particular theoretical standpoint; they are seen as intrinsic, axiomatic necessities to the study of literature. On the other hand, to those who hold such opinions, terms like ideology, signifier, logocentric and patriarchal are all forms of alien jargon belonging to literary theories.

The PDS in literary theory whereby the dominant paradigm, New Criticism, has become confused or synonymous with the study of literature is further indicated by the fact that it guides the teaching of literature at high schools and undergraduate courses at Universities. This PDS is as powerful and as significant to all literary theory, as the PDS of Newtonian physics was - and indeed still is - to science in its broadest manifestations.

<u>2.2.3 Rejection of the work of other paradigms</u>

The parallels between Newtonian physics and New Criticism are instructive, but hardly definitive. That both systems achieved a parallel domination of their respective fields and that both emphasised objectivity, which contributed to their pedagogical advantage over other paradigms, are matters of history. It would be wrong to push the analogy too far (although these links have been investigated by Selden 1984, 37-40). What is of central interest here is the similarity in their function as paradigms in their respective disciplines, a similarity which, if close enough, would support the applicability Kuhn's theoretical model to literary theory. In this regard, there is one other notable criterion which applies to both cases: the dominant paradigm effectively excludes much research in other spheres. That this was once true of New Criticism is widely recognised.

Although, as I have shown, the New Critics maintained a strong hold on literary theory, Raman Selden's model is not a model of heretical views: each paradigm within the model is a recognised field in its own right. Although I have argued that a degree of plurality in scientific thought does exist, the evidence is that scientific research is not as plural as outlined in Selden's scheme above. It is therefore impossible to apply even the modified version of Kuhn's model to literary theory with exactitude.

This does not mean that this discussion of Kuhn's model has been fruitless, for we need to go one stage further, and consider why, in spite of a dominant trend in literary theory, a plethora of other perfectly recognised viewpoints has arisen, and why this is not parallelled by science.

In order to answer this question, we need to turn to Feyerabend, "the passionate liberal" (Newton-Smith 125).

3. Ideology-reality shift (IRS)

3.1 Feyerabend and Kuhn

It is true that Feyerabend often objects, as others do (Watkins; Popper 1970; Toulmin 1970), to fundamental points in Kuhn's hypothesis, such as Kuhn's puzzle-solving attitude towards science, and his description of the paradigm and "normal" science.

Furthermore, Feyerabend openly attacks, and sometimes even insults Kuhn (1981c, 160) with his "friendly vehemence" (1970, 198);[3] thus some temerity is involved in suggesting that their thought is linked. Nevertheless, he is a great deal closer to Kuhn than he would like us (and perhaps himself) to believe. This is not entirely surprising, as the issues dealt with by Kuhn are central to the arguments of Feyerabend. This is perhaps best illustrated by the fact that much of what Feyerabend writes is in direct response to Kuhn's writings and discussions, or deals at length with Kuhn's notions, often similarly argued, and sometimes openly defending him (1962; 1965a; 1967, 392; 1968; 1970; 1975, 35ff.; 1978, 66-117; 1981a;c;d;h).

Feyerabend does see some merit in the use of Kuhn's "paradigm," and uses this concept (1981d;j, 129-30) and that of a revolution (1981i, 208) himself. Because he is more comfortable with Imre Lakatos, however, Feyerabend sometimes prefers Lakatos's term, "research programmes," instead of "paradigms." On the other hand, Feyerabend recognises that the terms are virtually synonymous, and that Kuhn and Lakatos's work share much common ground (Feyerabend 1981i; 219-220). Lakatos himself supports this when he explains that "research programmes" are characterised "by a certain <u>continuity</u> which connects their members" and which maintains their "methodological rules" (Lakatos 1970, 132). Furthermore, Lakatos recognises that this <u>continuity</u> is "reminiscent of Kuhnian "normal science'" (132), and he gives as an example of a research programme Newton's gravitational theory (133).

Further, in line with Kuhn, Feyerabend acknowledges the degree of incompatibility or incommensurability between paradigms or research programmes, by acknowledging that scientific terms often have entirely different meanings in different paradigms (1965a).

3.2 Feyerabend's Model

As far as this thesis is concerned, however, Feyerabend's most important work is his view of science in relation to other spheres of study, for this explains why Kuhn's model (although giving us an insight into the relationship among various literary theories) is not directly applicable to literary theory, and also why the literary theorists wanted a scientific model in the first place.

It is here that we come directly to grips with Feyerabend's question "What's so great about science?"[4] which he sees as woefully unexamined by both Lakatos and Kuhn.

Feyerabend's thesis is explained in the opening pages of his "How to defend Society from Science" (1981c). This essay outlines a model of historical change, discussed in this and many of his other works (1975; 1978; 1981i), which I have broken down into three stages.

Stage one

In the first stage of the model, there are many disciplines or "forms of thought" (1975, 295) which Feyerabend calls myths or ideologies, and there is an equality of importance (but not necessarily of goals, or methods) amongst the ideologies. Science is but one of a list of ideologies including voodooism (1975, 49-51), metaphysics, (180), Christianity (1981c), and magic (1981i, 205). "Truth" within an ideology is simply an expression of the relationship between a statement and the accepted rules or norms of that ideology. This is comparable to James Leach's denial of the notion of a value neutrality in science, where he argues that "the goal of such varied disciplines as history, law, and science is not merely truth for its own sake but truth modified by other goals and criteria" (94).

Feyerabend feels that ideologies cannot be judged good or bad, but that they all should have a comparable equality (1978, 27-31, 101-105), that all ideologies, whether business, science, religion or even prostitution (1975, 217, 308) have the right to set standards for entry into their own fields, but they do not have the right to impose these standards as standards on other ideologies. Science for example, should be seen as merely

> one of the many forms of thought that have been developed by man, and not necessarily the best. It is conspicuous, noisy, and impudent, but it is inherently superior only for those who have already decided in favour of a certain ideology, or who have accepted without ever having examined its advantages and its limits. (1975, 295)

Stage two

In the second stage of the model, however, there is a change. One ideology becomes dominant, and becomes an expression for "a comprehensive system of thought" or truth and reality (1981c), in very much the same way in which Roland Barthes saw bourgeois ideology becoming the expression of an accepted "reality" (Barthes 1972b, 114-145). This ideology, which is now seen as the

> semblance of absolute truth, is nothing but the result of an absolute conforminism....The myth is, therefore, of no objective relevance; it continues to exist solely as the result of the effort of the community of believers and of their leaders, be these now priests or Nobel prize winners.....it destroys the most precious gift of the young - their tremendous power of imagination, and speaks of education. (Feyerabend 1975, 45)

It becomes the standard, the norm, and forms part of the general education (26). Just as the truth of an ideology was meaningful only in terms of the ideology, so absolute Truth is thus merely "the dogmatic defence of that [dominant] ideology" (1981c, 158).

In the second stage, the dominant ideology becomes synonymous or confused with reality, and we may apply a similar term to the PDS used above in Kuhn's model, and describe this phenomenon as an ideology-reality shift (IRS).

Stage three

A third stage in the model occurs. One of the other ideologies may expose the fact that what seems to be the truth of Reality is really only the truth of the dominant ideology that has undergone the IRS, and thereby may "liberate" mankind from the dominance of this ideology. Feyerabend says

> [a]ny ideology that breaks the hold [which] a comprehensive system of thought has on the minds of men contributes to the liberation of man. Any ideology that makes man question inherited beliefs is an aid to his enlightenment. A truth that reigns without checks and balances is a tyrant who must be overthrown and any falsehood that can aid us in the overthrow of this tyrant is to be welcomed [my emphasis]. (1981c, 156-7)

What may happen in this stage is one of two possibilities: the first is the ideal: the demise of the "tyrant," and the return to stage one where all ideologies are equal. This is the real liberation of men's minds, Feyerabend argues. He says:

> I want to defend society and its inhabitants from all ideologies, science included. All ideologies must be seen in perspective. One must not take them too seriously. One must read them like fairytales, [sic] which have lots of interesting things to say but which also contain wicked lies, or like ethical prescriptions which may be useful rules of thumb but which are deadly when followed to the letter. (1981c, 157)

The second possibility, however, is that mankind will put so much trust into the new "liberating" ideology, that instead of its remaining the liberator, the new ideology will supplant the old, and become new tyrant, the new dominant ideology, undergoing its own IRS.

We can end this section by making a brief note. From the discussion, one can see that Kuhn's "discipline" has as many affinities with and similarities of function as Feyerabend's "ideology" (although Kuhn would be hesitant to call voodooism a discipline). Certainly, for the purposes of this discussion at least, we are performing no violence by conflating relevant aspects of the two interpretations, in a situation where our primary concern is the diachronic relation among literary theories.

3.3 Science and Reality

Feyerabend gives a brief disclosure of the historical developments leading to the IRS in our culture.

He argues that in the seventeenth century, Christianity was the ideology that dominated our culture's thought (Stage two). Science then challenged the dominating and authoritarian hold of Christianity, and became the liberating ideology, thus breaking the hold on reality that the Christian ideology had had (Stage three). Instead of continuing this liberation (to Stage one), however, science then replaced Christianity, and the ideology of science became the standard of Truth and reality (Stage two).

For further support of this notion, Feyerabend looks at the role of science in education (1967, 387-391; 1975, 217-220; 1978, 73-76; 1981c, 157), and says:

> A scientific culture leads to a scientific education both in the sense that one is educated and even trained to think scientifically, and in the quite different sense that the methods of training themselves are built up in accordance with the most recent scientific fashion. These methods of training, which are used already at an early age and which may be reinforced later on by participation in a particular science, will of course influence the character, the whole being of the individual trained. (1967, 390)

He explains further:

> Scientific "facts" are taught at a very early age and in the very same manner in which religious "facts" were taught only a century ago. There is no attempt to awaken the critical abilities of the pupil so that he may be able to see things in perspective. At the universities the situation is even worse, for indoctrination here is carried out in a much more systematic manner. Criticism is not entirely absent. Society, for example, and its institutions, are criticised most severely and often most unfairly and this already at the elementary school level. But science is excepted from criticism. In society at large, the judgment of the scientists is received with the same reverence as the judgment of the bishops and cardinals was accepted not too long ago. The move towards "demythologization" for example, is largely motivated by the wish to avoid any clash between Christianity and scientific ideas. If such a clash occurs, then science is certainly right and Christianity wrong. Pursue this investigation further, and you will see that science has now become as oppressive as the ideologies it had once to fight. (1981c, 157)

This point is very similar to a point made by Barthes (and the fears of I.A. Richards [1935, 88-89]). Barthes argues that science is not defined by content, method, ethic or mode of communication, "but simply by its status, that is its determination by society; the subject-matter of science is everything that society deems worthy of being handed on. In short, science is what is taught" (1967b, 897).

Feyerabend argues further that to point to the benefits of science is illusory:

> Nor am I entirely convinced that living for a long time in fear of cholesterol, radiation, nervous breakdown, aging [and] communism is so much better than living for a somewhat shorter time in the fear of a benevolent god and a few malicious demons and heretics; or that the comforts of psychoanalysis outweigh the comforts of religious faith. (1967, 390)

The indoctrination of thinking in a scientific manner is carried out in the same way in which others are trained in terms of religion (1967, 391-401), a method that resists all heretics and also deals with its heretics in a similar fashion to the manner in which religion did: religious heretics were killed, and scientific heretics "are still made to suffer from the <u>most severe</u> sanctions this relatively tolerant civilization has to offer" (1981c, 157).

Mavericks of science like Louis Jacot are still treated in this fashion (Jacot 91-96); another more prominent name springs to mind: Immanuel Velikovsky. Velikovsky's most known work is his Worlds in Collision (1972b) first published in 1950, and obliquely supported by other texts such as Ages in Chaos (1952) and Earth in Upheaval (1956), which gives an account of the history of earth and solar system that is "heretical" (Asimov 1977, 7-15). Many of Velikovsky's ideas are similar to those of Louis Jacot (Stecchini 1966b), and, predictably, the reaction to his work has been of a similar nature to the reaction to Jacot's, although far more extreme, to the extent that de Finnetti referred to the scientists as forming a "despotic and irresponsible Mafia" (qtd. in Stove 8). Some of the reactions to Velikovsky's work include: dismissals of his notions as "absurd", "crank", "bunk", "ad hoc", "lies", "charlatan[ism]", "fraud[ulent]", "incompetent", "Black Arts", "nonsense and rubbish" and "quack[ery]"; scientists refusing to test and refute or corroborate his work; denunciations by several critics who boasted never to have read the book; journals refusing to publish his work, or even his replies to criticisms published in those journals; newspapers and critics thinking his work was anything from a communist plot to a preparation for nuclear war; universities and publishers criticised and threatened for teaching and publishing his findings (Macmillan was eventually forced into signing the rights of Worlds in Collision over to Doubleday); finally, supporters of his work having their employment suddenly terminated; (Abell; de Grazia 1966b; Goldsmith; Juergens 1966b; Kallen; Rose; Storer; Stove; Talbott; Treash).

What Velikovsky had done in Ages in Chaos, as Einstein expressed it, was to compel "the roaring astronomical lion [Shapley] to pull in a little his royal tail" (letter qtd. in Kallen 39). The damning evidence against Velikovsky is, of course, this: "[I]f Dr. Velikovsky is right, the rest of us are crazy" (Harlow Shapley, qtd. in Rose 31).[5]

The vehemence in the rejection of Velikovsky and Jacot is the result of an attitude within science that goes beyond Kuhn's model, and is explained by Feyerabend's - it further indicates why Kuhn's model cannot be applied to literary theory. This attitude in science has little to do with the respect that other paradigms have for the dominant paradigm, for this is merely a function of the status of the ideology in which the paradigms are placed; the attitude is explained by the respect that other ideologies have for science as a whole. While it is true that works like Velikovsky's ask questions like "Who determines scientific truth? Who are its high priests? How do they establish their canons? [and] What effects do they have on the freedom of inquiry, and on public interest?" (de Grazia 1966a, 2), this is hardly unique, for these same questions are asked openly in other ideologies. That, of course, is the point. In other ideologies, these questions are asked only of that <u>ideology</u>. In science, these questions are asked of <u>reality</u>.

<u>3.4 The Hierarchy of Ideologies</u>

In addition to Feyerabend, many commentators have argued that in order to reinforce science's dominant position relative to other ideologies, scientists encourage the degree of specialisation noted by Snow by sustaining the impression that their subject is closed off to all others (Barber 600-601; Burrows 47-48; Barzun 20-21, 171; de Grazia 1966b; Einstein 1940b, 20; Feyerabend 1967; 1975; Mullen). This often disastrously impedes its own work, for while other people may become scientifically illiterate, scientists may become illiterate in other areas of science, in divisions within their own specialities (Barzun 209; Einstein 1940b, 15; Ziman), and even illiterate <u>per se</u> (Atkinson; Barzun 116-120). Nevertheless, Feyerabend argues:

> [S]cience still reigns supreme... because its practitioners are unable to understand, and unwilling to condone, different ideologies, because they have the power to enforce their wishes and because they use this power just as their ancestors used their power to force Christianity on the people they encountered during their conquests. (1975, 299)

This enables science not only to be separate, but also to increase its dominance over other ideologies by assuming its superiority. The extent of the superiority felt by some scientists is indicated by G.H. Hardy (1967) whose comments hardly need criticising. He argues that "Greek mathematics is `permanent', more permanent [sic] even than Greek literature. Archimedes will be remembered when Aeschylus [and, presumably, Plato, Aristotle, Sophocles and Euripides are]...forgotten, because languages die and mathematical ideas do not" (81), and "there are probably more people interested in mathematics than in music" (86).

Science, however, following the example of its predecessor, does not dominate all ideologies equally. Although the Christian ideology had been the dominant ideology possessing the "truth" of the culture, it was not a dominant ideology ruling over a number of equal inferiors, like a king over peasants. Rather, the other ideologies sought their own degrees of power, and in order to gain acceptance, respect and authentication, they adopted a Christian world-view, supporting, associating with and identifying with this reality, thereby serving the dual purpose of finding their "rightful" places in a strict hierarchy, and vindicating the "rightful" place of the dominant ideology. Even as the Copernican revolution got under way, the main instigators and developers, Copernicus, Galileo and Newton and even Blaise Pascal, were anxious to demonstrate that their views were easily acceptable to the Church (Bronowski 122-153; Capra 1983, 26-27; Cohen 1987; Feyerabend 1978, 44-53; Mandrou 228-246; Colin Russel), for "[s]cientists themselves felt that science should justify God and His world" (Barber 599).

3.4.1 Improving hierarchical status

Inter-disciplinary work does occur, but it is not inter-disciplinary in the true sense. To gain credibility and authenticity, according to Feyerabend's model, other disciplines or ideologies seek the "science" behind their thought, and clamber towards it, claiming their "scientific" status, believing (rightly so) that this will enhance their status in the eyes of the public (and increase their funding) (Barzun 205, 212) - especially when they see $140 million spent on the Stanford linear accelerator, "the `world's largest useless machine' as its director has called it" (Feyerabend 1967, 413)). One imagines, with some justification, that these vast structures could well be thought of as the "cathedrals" of our own age by future archeologists; this estimate would have its justices.

Researchers in other fields ignore (or are unaware of) the fact that there are numerous schools of scientific practice (apart from those seen in Chapter Two) and seldom question the validity (or even existence) of a single "scientific method" (Barber 601-2; Bohr 1951, 240; Edidin 31; Feyerabend 1965b; 1978, 73, 98-99; 1981i, 208-9; 1987; de Grazia 1966b; T.H. Huxley 1893b) and further ignore the glaring logical, irrational inconsistencies that have existed in science and are covered by <u>ad hoc</u> hypotheses and assumptions, and are often <u>necessary</u> to science (Feyerabend 1965b, 168-179; 1968, 20; 1975, 35-6, 55-143, 201-206; 1978, 8-16, 40-53, 109-117; Toulmin 1967, 76-78; Popper 1957a, 29-34). They attempt to incorporate "scientific" methods such as empiricism (Feyerabend 1968) and its jargon (Barzun 20-21; Horgan 15; Heyl 142-144), in the hope of capturing the scientific coherence and purity that is supposed to exist in all branches of science (Polanyi 1967, 73-74; Habermas 1972, 315; Feyerabend, 1975); and, by following a type of naive inductivism (Chalmers 1-37), hope to "improve the status of their field" (Kuhn 1970b, 245; Canguilhem 20; Feyerabend 1970, 198-99, 301-2; 1978, 73-76; Foucault 1967; 1980, 78-92; Habermas 1972, 302-3; 1987, 244-5) by turning their subjects into "social sciences" or "human sciences." They ignore warnings from practising scientists on the dangers of believing that science has presented us with the one, final and acceptable truth (See pp. 85-110; Einstein and Infeld, 292; Planck 1950, 33-34; Popper 145, II, 246; 1980, 53). Similar conclusions have been drawn by Foucault.[6]

The abuse of the word "science" in this way is as old and as strong as its position in this hierarchy, and one is hard-pressed to find a field of human thought that does not aspire to be "scientific." As early as 1867 Marx notes the abuse of the word science by economics (91, 337-8, 564), sociology (161 ft. 26), and politics (409);[7] Physical Education changes its name to "Human Movement Science"; Education becomes "Education Science" and Politics, "Political Science"; we also have "Library Science, Administrative Science, Speech Science, Forest Science, Dairy Science, Meat and Animal Science and even Mortuary Science" (Chalmers xvi); reports are supported if believed scientific, and dismissed on the grounds that they are "unscientific"[8]; advertising makes use of "scientific prefaces" and "a touch of scientism" (Barthes 1979, 47; Chalmers xv).

3.4.2 On the base of the hierarchy

On the other hand, as Feyerabend further argues, subjects like Astrology and plant communication that are "unscientific" - in spite of the information they may deliver - are relegated to the base of the hierarchy with other myths, unsubstantiated beliefs, nonsense (Feyerabend 1978, 91-96), and other "spooky things like telepathy" (Stenger 51), and are refused support from institutions such as the National Science Foundation (Feyerabend 1981i, 205-6).

Opponents of the dominant scientific view are often dismissed with cheap rhetoric like "Every schoolboy knows that the earth..." (Gardner 1967, 16). This argument does little to bolster science, for if one added to this a list of everything that every schoolboy (and scientist) "knew" a hundred years ago, the list could become embarrassing.

Of course, all is not lost, for some myths and beliefs may be confirmed by science. That is to say, science may devise explanations for phenomena that were known and yet previously suppressed by it because they fell outside the scope of science. When this happens to aspects of subjects like Astrology (Feyerabend 1978, 91-96) or ancient cosmologies, medicine, biologies and practices like acupuncture and faith healing (102-105, 137), these are seen as a step up the hierarchy for these views. We say "they weren't so stupid after all" instead of asking "what else has science rejected as a lie, and what else will we, who have submitted "to the meanest kind of (intellectual and institutional) slavery" (Feyerabend 1975, 300), continue to reject until science changes its mind, and deigns to "dignify... it by discussing it" (Sagan and Page xiii)?

In our discussion of Darwin in Chapter Two, I showed how his views had clashed with the Church. Rarely discussed is that, in the nineteenth century, Darwin's theory was declared "inconsistent with physics" and therefore unacceptable (Toulmin 1984, 53). Today, it is inconsistent with various religious, political, and other "alarming" trends developed in works like Barry Fell's <u>America B.C</u>. and Jeffrey Goodman's <u>Psychic Archaeology</u>, but because Darwin is <u>now</u> acceptable to physics, these alternative views are simply "propaganda" (Godfrey 21-32). What is more, the change in Darwin's status resulted not so much from a refinement in his theory, but from a realisation, as Charles Lyell had foreseen and warned, that its rejection "held good only on the assumption that nineteenth-century physics already knew all the actual sources of the earth's heat" (Toulmin 1984, 54).

<u>3.4.3 Science as the Truth</u>

The tendency outlined in Feyerabend's model is that people see science as the ultimate truth: perhaps best illustrated today by the way in which the courts treat science: a manner not very much unlike many once treated the Delphic oracle. In the American legal system, in adherence to the precedent of Frye v. United States, the courts

> will go a long way in admitting expert testimony deduced from a well-recognised scientific principle or discovery, [on the condition that] the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs [my emphasis]. (qtd. in Giannelli 1205)

Some cases may advance a broader demand, and ask that the scientific procedures be acceptable to the "scientific community as a whole" (Giannelli 1209),[9] although much of the information has often been found to be inconsistent with later discoveries (Giannelli; Lander 1989; Neufeld and Coleman 1990; Rosenhan 1973).

Nobody asks (in spite of the demonstrated reluctance by many scientists to equate "science" with "truth") in Feyerabend's view "What's so great about science?" "The excellence of science is assumed, it is not argued for" (1978, 73). In Feyerabend's terminology, we need to take cognizance of the fact that "scientifically proven" means "in accordance with a science", not necessarily, he would argue, with truth.

Feyerabend's model proposes, then, that we have redefined "the approach to truth as the result of what scientists do" (Kuhn 1970a, 20), as David Hawkins implies (555). In Foucault's words, "`Truth' is centred on the form of scientific discourse and the institutions that produce it" (1980, 131). We have forgotten that the only reason for its status is as a result of a society that venerates an ideology, and yet, as C.P. Snow pointed out, knows nothing about it.

We have forgotten, Feyerabend argues, that the sciences

> are our own creation, including all the severe standards they seem to impose upon us. It is good to be constantly reminded

> of this fact. It is good to be constantly reminded of the fact that science as we know it today is not inescapable and that we may construct a world in which it plays no role whatsoever.... (1970, 228)

4. Conclusion

Feyerabend has not worked in a philosophical vacuum. His outrage against the "Scientific Culture" is a direct credit to C.P. Snow, and the ideas and concepts he uses are extensions, revisions and rejections of portions of Kuhn's hypothesis.

The three most important points that are relevant to this chapter have been dealt with extensively by other philosophers contemporary with Feyerabend: the history of science (almost all of the philosophers), the power of science in our society (Foucault; Hempel), and the validity of the degree of objectivity or exactitude that we attribute to science (Bachelard (1963, 83-134; 1984, 85-134); Hempel; Jeans (1958; 1959); Lakatos; Popper; Putnam (1981); Shapere (1964; 1981); Toulmin). While several may mention all three aspects, all but Feyerabend concentrate on one aspect, and make passing reference to the others.

While Kuhn's model questions the prejudice and bigotry which supports one scientific paradigm in favour of others, Feyerabend's goes further. Using ideas from both Snow and Kuhn, Feyerabend's model questions the prejudice and bigotry maintained by scientists and non-scientists towards the scientific ideology above others, and the tendency that researchers in these other ideologies have of following what they perceive to be scientific values and patterns of research. In dealing with the issue "What's so great about science?", Feyerabend looks closely at all three aspects mentioned in the paragraph above, arguing that science is an ideology whose dominating position, role and function relative to other ideologies have been defined by its culture, and yet which denies its relationship to culture in order to emphasise its autonomy and domination, whose history of epistemology denies the very principles it espouses, and which then seeks to impose these principles upon a society of "willing slaves" (Feyerabend 1975, 300).

In the previous paragraph, I used the word "arguing" rather than "proving" or even "showing" for good reason. Feyerabend's model is a useful tool for the examination of ideologies in our culture. It is only that. It cannot be taken as the ultimate statement on the subject. We cannot, and dare not, claim the superiority of Feyerabend's views over related arguments proposed by the other philosophers mentioned above, for the very persistence of Feyerabend's model as a model worth studying relies on its being one useful theory amongst many, one which demands an examination of others and the hierarchy in which they find themselves, but above all, one which demands a constant examination of itself.

In much the same way that Barthes's analysis contained, and nurtured the seeds of its own destruction, so Feyerabend's model, too, contains the seeds of its own destruction - these seeds might grow only by our ignoring the spirit of the model, and elevating it to the tyrant of a newer tyranny. Feyerabend does not fight the intellectual and social tyranny of science alone; his target is intellectual and social tyranny per se.

CONCLUSION

Gravity, Mass, Energy, Time, behind a pall,
God said, "Let Einstein be," and light was all.

(Anon.)

CONCLUSION

In the previous chapter, attention was drawn to the parallel between the models of Kuhn and Feyerabend. Kuhn's model recognises that there is no one absolutely correct paradigm, and a paradigm's dominance within an ideology is not ground enough to assume that it is superior to other paradigms. Feyerabend's model follows this pattern, and extends beyond science to reality, saying that there is no one absolutely correct ideology, and an ideology's dominance within reality is not ground enough to assume that it is superior to other ideologies. Viewing these two models in parallel, one could understand how the Newtonian paradigm came to dominate other paradigms within the ideology of science, but also, because of the dominance of science over other ideologies, came to represent the truth of reality.

This view certainly gives insight into why there was adherence to (outdated) tenets of science by non-scientists, why there was a reluctance to drop the Newtonian model even on the part of the scientists themselves, and why "scientific" literary theories failed to take cognizance of changes in science. Kuhn's model, the replacement of the paradigms, explains part of this reason, but not all. What emerges from a consideration of both theories is that there was a reluctance to drop the Newtonian Paradigm on the grounds that this Newtonian model or paradigm was common-sense reality.

It seems that people "were browbeaten into confusion by the glory of Newtonian physics" (Lakatos 1970, 92) in much the same way that Feyerabend believes that "the early Church fathers... introduced conservative doctrines in the guise of family prayers (which transformed the commonsense of the time) and which thereby gradually transformed commonsense itself" (1981i, 219). Since the eighteenth century, in fact, science had been seen as little more than "organised common-sense" (Whitehead 143), and Feyerabend's argument is a rather detailed vindication of Coleridge's contention that common-sense is simply the science of yesterday, now become a prejudice.

With this in mind, we are able to declare an end to the argument that has been mounting since the first conception of the possibility of a scientific literary theory with the help of what has been learned from our claims to objective reality and authentication in New Critical and Structuralist Literary theory. Feyerabend's model exposes the common tendency to believe that for any discipline or ideology to be judged in any way worthy of a following, it must be "scientific." Literary theory has not been immune from this necessity.

It would be naive, however, to accuse the New Critics and Structuralists of simply pandering to the desires of the society in which they live in order to gain credibility. Although social forces may have played a role, as we saw in the discussions in Chapters One and Two, the turn towards a science of literature was not so much simply a naive craving for a source of alternate authority as a response to an epistemological need. We saw that the dissatisfaction with the previous notions of the study of literature had reached a climax in the early years of the twentieth century, and this resulted in a questioning of the assumptions behind these notions. The subject that had most successfully questioned an outdated theory of thought was science; the model that had most successfully done so was the Newtonian model. Being, for the most part, somewhat unaware of the dramatic changes occurring in science, and ignoring the erstwhile philosophical problem entering the "pure" sciences, it was only natural that the thought of the theorists should tend towards the view of "the literary creation as an object" (Miner 11) to be examined in isolation.

Even Richards, who had warned against the dehumanising effect of the "systematic physical sciences" (1934, 232) with its inferior language (1926, 268-273), was unable to resist the supposed benefits and sanctions conferred upon literary theory by science.

So entrenched an idea seemed the necessity for the scientific model, that when the limitations of appeals to this court became evident to a few theorists, these theorists were unable to break with a scientific model, and simply changed their ideas of science. When the authority of the Newtonian model was being threatened by the Copenhagen Interpretation, Roman Jakobson realised that in order for his ideas to be scientific, he would have change his philosophy; attempting to show a relationship between Formalism and Newtonian physics was of little significance. Authentication demanded science. Science had changed, and in order to be scientific, Jakobson had to change with it.

It is for much the same reason that Roland Barthes had supported the earlier scientific model - his final rejection of "science" signalled the collapse of Structuralism. One cannot but wonder what would have happened to Structuralism if Barthes had realised that his grounds for the rejection of the scientific model (chiefly the impossibility of the distinction between method and object) were already scientifically acceptable notions.

Science offered an objectivity and authentication which, for the New Critics, was able to operate unencumbered by issues of cultural importance, and for the Structuralists, was able to accommodate these issues and incorporate them into the theory. In each case, literary theory was able to claim a superior foothold upon the hierarchy of disciplines by its appeal to science.

I believe that I have made it sufficiently clear that while sometimes criticising ideas and assumptions of many of the theorists who desire to observe and describe some form of "textual reality", in no way do I belittle their contribution to literary theory. What the study has shown, however, is that while we acknowledge the great strides made by these theorists, our acclamation must be tempered at several points:

a) New Criticism, in the words of Rabinowitz, has been loved "unwisely and for too long" (40). Its original successes in the study of literature have blinded many to major inconsistencies in the use of terms by the theorists (a result probably of their repudiation of any defined theoretical perspective, without realising that this in itself is a theoretical perspective). In addition, New Criticism's direct but changeable and unsteady relationship with science gives its description as a scientific literary theory a dubious status.

b) Vico's proposal that we create our own myths has became a fundamental part of Structural literary theory as is shown by all the later Structuralists, and yet at the same time disables any attempt to base the authenticity of theoretical propositions on science alone - unless we are prepared to qualify the Truth of science. In an attempt at humour, Peter Washington writes that once one has begun thinking of Lévi-Strauss's work as myth, "structuralism itself appears not as a science but as a myth, and that is perhaps the best way to think of it" (68). Washington misses the point noticed by Vico, and finally confirmed by Barthes: although a study could be a myth without being a science, it cannot be a science without being part of a myth, for science itself functions as myth. Science, indeed, as we have begun to see, functions very largely in these pages as a "mythology" in Barthes's sense: as a body of imagery and atmosphere that almost indefinably confers status on those who align themselves with it, or invoke it for their own credo.

c) An issue central to the debate was the influence of language. The work done by the both linguists and physicists indicates that the exact demarcation between social organisation, culture, epistemology, language, methods of description, and object being described are blurred: they seem to exist on a continuum rather than as separate entities. Almost all who were at some point aware of the influence of culture (apart, perhaps, from some of the New Critics) have become conscious that the distinction between describing an object and creating the object was not as clear as one would have liked - certainly not as clear as the mechanics of Newtonian physics would have us believe. The knowledge of this leads us to understand (contrary to many theorists and some scientists) that both art and science are variously and inevitably influenced by the "color of the flag which waved over the fortress of the city."

d) It is necessary at last to conclude that the word "scientific" has been used in so many contexts, by scientists and non-scientists, and applied to so many fields in a broad and comprehensive manner, that its meaning is uncertain.[1] As we saw in Chapter One, much confusion that arose did so because of the inconsistent use of the terminology associated with science. As far as literary theory is concerned, it would perhaps be more advisable to abandon the word "scientific" or else use it in a narrow and predefined manner, with a full awareness of the ambiguities and limitations of what is being claimed.

e) Indeed, if Feyerabend's model is to be taken as a warning to literary theorists, we must see it as a warning not simply or prohibitively against the use of science, but rather as a collective measure: as timely counsel towards self-knowledge. This uncomfortable knowledge we must bear with us is that no ideology or paradigm we as theorists or critics look to for support is capable of endowing our words with the immutable, exclusive, and eternal properties of unchanging truth.

GLOSSARY OF SCIENTIFIC TERMS USED IN THIS THESIS

GLOSSARY OF SCIENTIFIC TERMS USED IN THE THESIS

This glossary does not serve as a general dictionary of scientific terms; it is designed only to be used by readers with limited or no understanding of the scientific concepts discussed in the thesis, and to be used in conjunction with information gathered from the body of the thesis.

The terms explained, therefore, and the depth and manner of explanation, are determined only by the necessity of the knowledge for an understanding of this thesis: thus there are terms crucial to other spheres of physics not mentioned here, and sometimes seemingly trivial detailed work is done on topics. Scientific notation* has been used for the figures.

Occasionally terms not used in the thesis are also explained, as even the explanations may need more elaboration. For this, I shall make no apology - perhaps if C.P. Snow's words had been heeded by education institutions, much of this could be removed.

Further explanatory notes may be found in works like Bransden and Joachain, Einstein and Infeld, Isaacs, Kane and Sternheim, and Pitt.

aether See ether.

Analyticity One of the principles of S-Matrix* theory. It states that the "energy and momentum are transferred over macroscopic spatial distances only by particles,... that this transfer occurs in such a way that a particle can be created in one reaction and destroyed in another only if the latter reaction occurs after the former, ... and that the transfers of energy cannot occur with velocities exceeding the speed of light [$3x10^8 ms^{-1}$]" (Capra 1979, 16). See also Poincaré Invariance,* Unitarity,* and Crossing.*

antiparticle See Tables 1-6 below.

atom The smallest part of a chemical reaction. It was originally postulated by the ancient Greeks, and the actual model was refined by works by Dalton, Rutherford (1911; 1914), Bohr (1913a;b;c;d) and Sommerfeld. Generally the solar system picture of the atom is of a nucleus holding nucleons* surrounded by electrons* in set orbits around it.

atomic clock Sometimes used to refer to Caesium clocks*. An example is the one based on the absorption of radiation of a frequency of 23 870 hertz by ammonia (Pitt, 70). (See also Note added to Special Relativity and table 7 below).

atomic particles See Tables 1-6 below, and see under individual particle names.

baryon 3-quark Hadron* with a half-integer spin*. See Figures 6.1 and 6.2 and Tables 1 and 3 for examples.

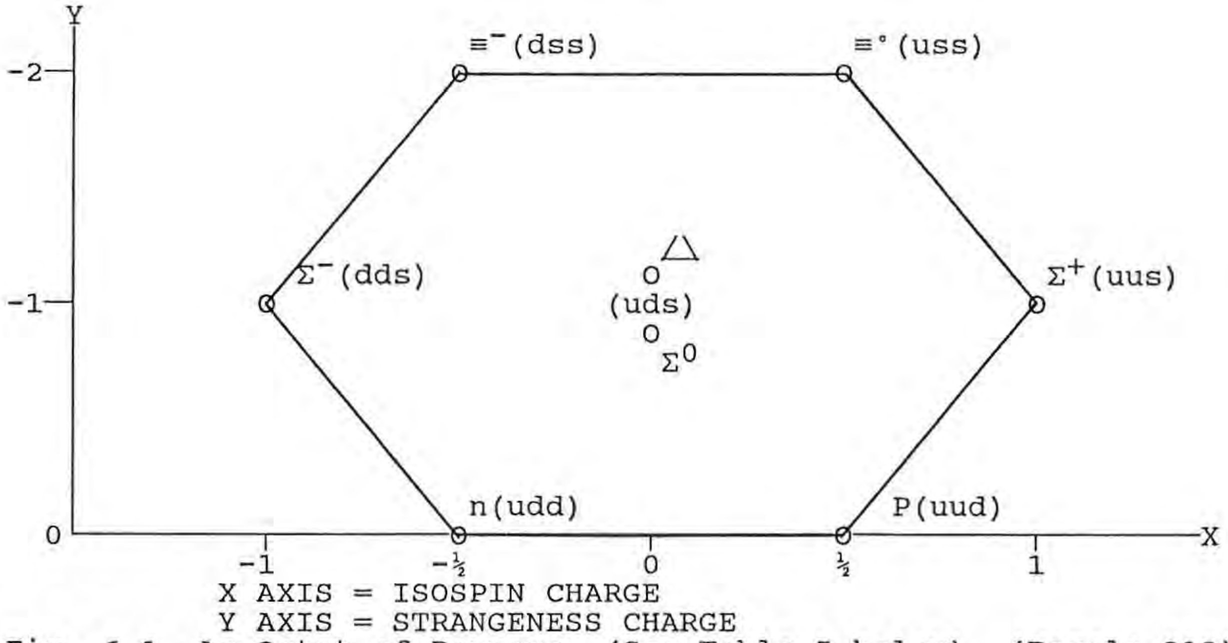


Fig. 6.1 An Octet of Baryons. (See Table 5 below) (Pagels 212)

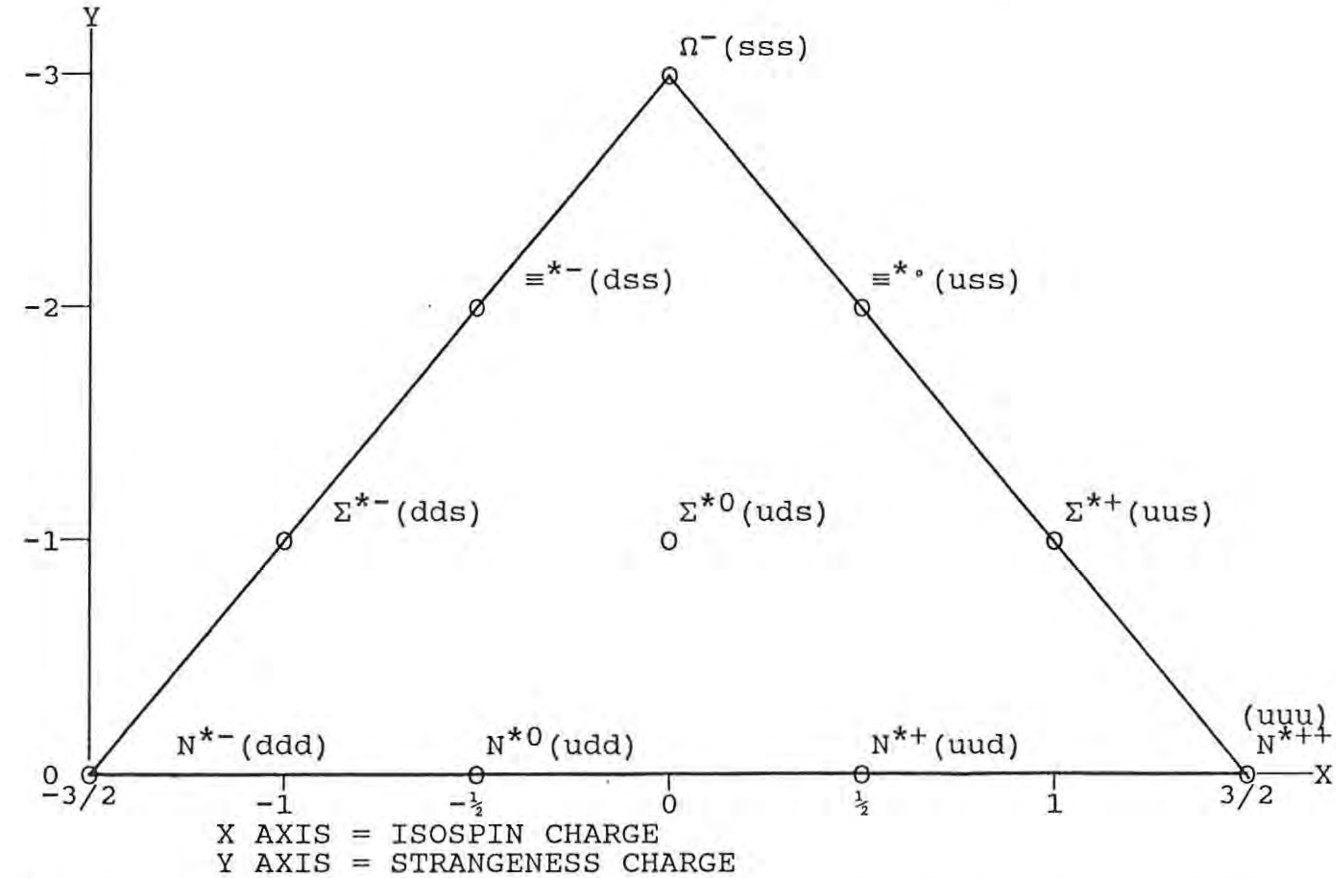


Fig. 6.2 A Decuplet of Baryons. (See Table 5 below) (Pagels 212)

Bohr-Heisenberg microscope A product of a thought experiment*, that gives an explanation of the uncertainty principle, devised by Bohr and Heisenberg at Copenhagen (Bernstein 175). Heisenberg's explanation is as follows:

> One could argue that it should at least in principle be possible to observe the electron in orbit. One should simply look at the atom through a microscope of a very high resolving power, then one would see the electron moving in its orbit. Such a resolving power could to be sure not be obtained by a microscope using ordinary light, since the inaccuracy of the measurement of the position can never be smaller than the wave length of the light. But a microscope using y-rays with a wave length smaller than the size of the atom would do. Such a microscope has not yet been constructed but that should not prevent us from discussing the ideal experiment....
>
> The position of the electron will be known with an accuracy given by the wave length of the y-ray. The electron may have been practically at rest before the observation. But in the act of observation at least one quantum of the y-ray must have passed the microscope and must have been deflected by the electron. Therefore, the electron has been pushed by the light quantum, it has changed its momentum and its velocity, and one can show that the uncertainty of this change is just big enough to guarantee the validity of the uncertainty relations....
>
> At the same time one can see that there is no way of observing the orbit of the electron around the nucleus. The second step shows a wave pocket [sic] moving not around the nucleus but away from the atom, because the first light quantum will have knocked the electron out of the atom. The momentum of light quantum of the y-ray is much bigger than the original momentum of the electron if the wave length of the y-ray is much smaller than the size of the atom. Therefore the first light quantum is sufficient to knock the electron out of the atom and one can never observe more than one point in the orbit of the electron; therefore, there is no orbit in the ordinary sense.
>
> The next observation - the third step - will show the electron on its path from the atom. Quite generally there is no way of describing what happens between two consecutive observations. It is of course tempting to say that the electron must have been somewhere between the two observations and that therefore the electron must have ascribed some kind of path or orbit even if it may be impossible to know which path. This would be a reasonable argument in classical physics. But quantum theory it would be a misuse of the language which, as we will see later, cannot be justified. We can leave it open for the moment, whether this warning is a statement about the way in which we should talk about atomic events or a statement about he events themselves, whether is refers to epistemology or to ontology. In any case we have to be very cautious about the

wording of any statement concerning the behaviour of atomic particles. (Heisenberg 1958a, 46-48; see also Born 1956, 47; Gamow 1958; Jones 20)

We should, however, remember that many objections to the use of Galileo's telescope were based on the perception that it worked for earthly objects, but not for celestial objects (Feyerabend 1975, 121-123).

Bootstrap theory An alternative model to the quark* model. The basis for it is that it is logically consistent. Recognising the importance of the three principles of S-Matrix* (Poincaré Invariance,* Analyticity* and Unitarity*) and yet also that S-matrix* could not be formed into one mathematically consistent theory which satisfied all three, the Bootstrap proposal was made by Geoffrey Chew and others (Chew 1968; 1970; Chew et al 1964).

The following theoretical perspective of the S-matrix and bootstrap models are summed up by Henry Stapp:

1. Physical entities, such as elementary particles, correspond to probabilities.
2. The actual things in quantum theory are responses.
3. A response is an event (or occurrence, or process) rather than an object.
4. The only events known to exist are mental events.
5. A mental event links prior mental events in a particular way, and creates corresponding new possibilities for subsequent mental events.
6. Mental events are associated with living forms. However, as natural phenomena, they should be remembers of a general class that includes also similar events not associated with living forms.
7. A collision of elementary particles is similar to a mental event: It links prior collisions in a particular way, and creates corresponding new possibilities for subsequent collisions. (1971, 1319)

The Bootstrap model is essentially a development of the S-Matrix model. It accepts only the basic principles of the S-Matrix, but goes further: it accepts no fundamental entities whatsoever: no fundamental laws, equations or principles. All other established laws and theories must merely be seen as creations of the mind, of approximations - that is the closest that physics can get to a real picture of reality (Capra 1983, 317).

Bootstrap, therefore, endorses the probable tendency of matter to exist, as proposed by the S-Matrix model, but goes on to say that even if we could explain everything, we would merely be explaining what we thought we were explaining, and would never get to a reality.

We can see that Bootstrap is directly related to, and even becomes a part of an entire philosophical view of the world. As Capra points out, "If the bootstrap hypothesis is correct, its philosophical implications would be very profound.... [it implies that] the observed patterns of matter are nothing but reflections of patterns of mind" (Capra 1979, 19).

In a private discussion with Capra, Chew once remarked: "Our current struggle with [certain aspects of advanced physics] may thus be only a foretaste of a completely new form of human intellectual endeavour, one that will not only lie outside physics but will not even be describable as `scientific'" (qtd. in Zukav 1984, 331)

In spite of its pragmatic success, the real fault in S-matrix and bootstrap is the same as that found in Heisenberg's matrix: the picture in the mind. As a result of classical mechanics, the idea of particles is still dominant. "It is probably the retention of this fundamentally incorrect metaphysical assumption that is the origin of the conceptual difficulties that arise in naive attempts to [go] beyond a pragmatic understanding of quantum theory" (Stapp 1971, 1320).

Bootstrap shares one other important feature with the Copenhagen Interpretation. In the words of its originator, it is "unscientific" (1968, 762).

Just as many saw classical mechanics as providing a system on which to base many intellectual activities, some see Bootstrap and S-matrix as models for social structures. (Marilyn French, 541 ff.; Capra 1982; 1983).

c Constant of the velocity of light in a vacuum. $c = 2,997\ 924\ 58 \times 10^{8} ms^{-1}$ (for calculation $c = 3 \times 10^{8} ms^{-1}$).

Caesium clock A clock using caesium for the accurate measurement of time. See atomic clock*, note added to Relativity*, and table 7 below.

Clocks See atomic clock,* Caesium clock,* and Relativity.*

complementarity An integral part of the Copenhagen Interpretation*. Introduced by Bohr (1928) as "complimentarity" or "correspondence"

He recognised that a paradox existed in the Copenhagen Interpretation. The laws of physics at subatomic level did not correspond to those of classical physics, and yet "<u>however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms</u>" (1951, 209). More than this, difference in experimental perceptions (such as whether light should be viewed as a particle or a wave) were to be determined in advance by the observer (211-229). This implied "the <u>impossibility of any sharp separation between the behaviour of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear</u>" (210), for the "complementary phenomena appear under mutually exclusive experimental arrangements" (218), and it is always necessary to recognise that the "<u>whole experimental arrangement</u> had to be taken into account" (222). This phenomenon remained an epistemological arguing point for Bohr, as he points out in several other works (1929; 1937a;b;

1963). As far as Bohr was concerned, an analysis of anything more detailed than a study of probabilities was "in principle excluded" (1951, 235). See also Born 1962, 98-102.

He saw that quantum mechanics had the same "beauty and self-consistency" (1934, 92) ("Schönheit und inneren Zusammenhang" (1929, 483) as classical mechanics, and argues:

> The epistemological problem under discussion may be characterized briefly as follows: For describing our mental activity, we require, on one hand, an objectively given content to be placed in opposition to a perceiving subject, while on the other hand, as is already implied in such an assertion, no sharp separation between object and subject can be maintained, since the perceiving subject also belongs to our mental constant. (1934, 96)

It is this concept in the Copenhagen Interpretation, that raised the greatest objections in the USSR, but from the ideologues such as Zdhanov, rather than physicists (Jammer; Müller-Markus) until the 1960's.

Copenhagen Interpretation (See also Complementarity*). The Interpretation of quantum physics led by Heisenberg and Bohr, opposing Einstein. Of the term, Rosenfeld writes in a letter that it is a term "which we in Copenhagen do not like at all. Indeed, this expression was invented, and is used by people wishing to suggest that there may be other interpretations of the Schrödinger equation, namely their own muddled one" (Stapp 1972, 1115). The major issues of relevance to this thesis regard the importance of the observer in the experiment, the degree of chance and probability, and the completeness of the quantum description of nature.

A detailed description of the earlier debate around the Copenhagen Interpretation can be found in Stapp (1972), Bohr (1951), Heisenberg (1958a, 44-58) and the other references mentioned in the body of the thesis.

The Copenhagen Interpretation, especially Bohr's contribution, received great ideological criticism in the Soviet Union (Jammer; Müller-Markus). Since the 1960's there has been growing and diversified philosophic and scientific opposition to the Copenhagen Interpretation, which is seen merely as one interpretation. Recent information on this and other issues such as causality can be found in Ballentine; Bell 1987; Bub; Bunge (1963; 1967a; 1967b); Christensen; d'Espagnat (1976, 250-259; 1987); Fine; Gödel; Jammer; Legget; Penrose; Popper (1967; 1980) and Vigier et al.

constants See Table 7, and see individual constants under their individual names.

Crossing A property resulting from the three principles of S-Matrix* theory. "The fact that the probability amplitude [in a reaction] is an analytic function of the particles' momenta (including their energies) means that it may be analytically continued to regions where some of these momenta become negative." Thus, in an interaction, a probability amplitude of an ingoing particle may also be interpreted as the amplitude of an outgoing antiparticle. (Capra 1979, 16) See also Analyticity,* Poincaré Invariance* and Unitary.*

de Broglie waves The de Broglie wave length of a particle is equal to h/p where h is Plank's constant, and p is the particle's momentum. de Broglie argued that "any moving body may be accompanied by a wave and...it is impossible to disjoin the notion of body and prorogation of wave" (1924, 450). As Bachelard later expressed it, "a wave is like a hand of cards and a particle like a bet on the outcome" (1963, 97; 1984, 98) An example of a de Broglie wave is a follows: an electron with a velocity of 1/5 the velocity of light has a wavelength of $1,2x10^{-11}$m, and the earth on its orbit around the sun has a wavelength of $3,7x10^{-63}$m.

Calculations:

electron: rest mass = $m_e = 9,109\ 558 \times 10^{-31}kg$

velocity = $1/5c = 6x10^{7}ms^{-1}$

Planck's constant = $h = 6,626\ 196 \times 10^{-34}Js$

$$\text{momentum} = p = m_e v = (9,109\ 558 \times 10^{-31}kg)\ (6x10^{7}ms^{-1}) = 5,4657x10^{-23}kgms^{-1}$$

$$\text{wave length} = h/p = (6,626\ 196 \times 10^{-34}Js)\ /\ (5,4657x10^{-23}kgms^{-1}) = 1,212m^{-11}m$$

earth: rest mass = $m = 6x10^{24}kg$

velocity = $2,9886x10^{4}ms^{-1}$

$$\text{momentum} = p = mv = (6x10^{24}kg)\ (2,9886x10^{4}ms^{-1}) = 1,7931x10^{29}kgms^{-1}$$

$$\text{wave length} = h/p = (6,626\ 196 \times 10^{-34}Js)/(1,7931x10^{29}kgms^{-1}) = 3,7x10^{-63}m$$

$E=mc^2$ See Einstein's Law.*

Einstein's Law (for relativity, see Relativity*) Expressed in the formula $E=mc^2$, where E is Energy, m is mass, and c is the velocity of light in a vacuum ($3x10^{8}ms^{-1}$). It is more than simply a statement that energy and mass are transferable - a measurement of mass is the same as a measurement of energy in the way that a measurement of a road in kilometres is the same as a measurement in miles, metres, or yards. The same applies to potential energy - a compressed spring has more energy and more mass than the same spring relaxed (Einstein 1905c; 1923a; March 156; Pitt 119).

electron An elementary particle in the atom*. In the solar system picture of the atom, the electron orbits the nucleus in set orbits. For relation of electron to other particles, see tables 1 and 2. For charge and mass constants, see table 7 below.

elementary or fundamental particle A particle which cannot be further subdivided. See further information in tables 1-6 below, and under individual particle names.

EPR A thought experiment* designed by Einstein, B. Podolsky and N. Rosen (1935) and answered by Niels Bohr (1935), questioning the completeness of quantum theory, especially in the light of the Copenhagen Interpretation.* This thought experiment still provides interesting physical and philosophical considerations today (Bell 1964; Aspect et al; Davies and Brown).

ether Sometimes spelt "aether." This has nothing to do with the ether commonly associated with medicine. It was a hypothetical substance once thought to be responsible for allowing the transmission of electromagnetic waves. Its existence was effectively disproved by the results of the Michelson-Morley* experiment and Relativity*, although it was not abandoned easily. In 1911 William F. Magie described the abandoning of the ether in favour of Relativity as "a great and serious retrograde step in the development of speculative physics" (118). Louis Jacot still insists on its existence (Einstein and Infeld, 106-122, 172-176; Jacot, 38-40, 103-4; Lorentz; Michelson; Pitt, 140)

fundamental particle See elementary particle.*

h Planck's* constant. $h = 6{,}626\ 196 \times 10^{-34}$Js

hadron sub-atomic particles consisting of quarks,* and are further subdivided into baryons* and mesons* (Pitt, 175). See Figures 6.1 and 6.2 and Tables 1, 3, and 4 for examples.

Heisenberg's microscope See Bohr-Heisenberg microscope.*

Heisenberg's uncertainty/indeterminacy relation/principle First published in Heisenberg's 1927.

A radiation process will involve photons with energy and momentum $E = h\nu$ and $P = h\sigma$, where h is Planck's constant*, and ν is the number of vibrations per unit time, and σ is the number of waves per unit length (Bohr 1951, 203). Bohr explains the principle as follows (For a second, less mathematical explanation, see Bohr-Heisenberg microscope*):

> On one hand, the co-ordinates of a particle can be measured with any degree of accuracy by using, for example, an optical instrument, provided radiation of sufficiently short wave is used for illumination. According to the quantum theory,

however, the scattering of radiation from the object is always connected with a finite change in momentum, which is the larger the smaller the wave-length of the radiation used. The momentum of a particle, on the other hand, can be measured with any desired degree of accuracy by measuring, for example, the Doppler effect of the scattered radiation, provided the wave length of the radiation is so large that the effect of recoil can be neglected, but then the determination of the space co-ordinates become correspondingly less accurate. (Bohr 1928, 582)

The degree of accuracy is determined by the equation $\triangle q \cdot \triangle p \approx h$, where q is position, and p is momentum, and $\triangle q$ and $\triangle p$ are the "suitably defined latitudes in the determining of these variables." (Bohr 1951, 208-209)

indeterminacy relation/principle see Heisenberg.*

Invariance, Poincaré See Poincaré Invariance*

J-particle (or psi particle) The discovery of which led to the fourth family of quarks*. See tables 1 and 4 below.

lepton Elementary particle with half-integer spin.* See table 1 and 2 for examples.

Lorentz-Fitzgerald contractions See Relativity.*

Maxwell's Demon A thought experiment* introduced by James Clerk Maxwell in 1872 (Maxwell 1899, 338-339) and further elaborated upon by William Tompson (1874). It was part of a debate concerning the statistical nature of (and resulting in a (mistaken) contradiction of) the second Law of Thermodynamics* (Daub 1980, 222-235).

Michelson-Morley experiment An experiment designed to investigate the "relative motion of the Earth and the Luminiferous ether" (Michelson 120), which effectively disproved the existence of the ether. See ether* and Relativity.*

meson 2-quark Hadron* with a integral spin*. See tables 1 and 4 for examples.

nucleon A constituent of the atomic nucleus, i.e. a proton or neutron*.

nucleus In the solar system picture of the atom,* the nucleus is the centre around which the electrons* orbit.

particles, sub-atomic Some of the major particles are listed under their names. For many of the others, see tables 1-6 below.

partons A collective name for gluons* and quarks.* These are the constituents of the proton and the neutron. (See Tables 1, 5 and 6 below).

photoelectric effect First noticed by Hertz (1887), it is the effect of the discharge of electrons* from metal as a result of illumination by light. A brief history of its importance is as follows:

The two-century debate between Newton's corpuscular (particle) theory of light and the wave theory of light based on lectures and experiments by Huygens, Thomas Young in 1807, Fresnel in 1818, Maxwell in 1860, Hertz in 1887, and Michelson in 1893 established beyond doubt that light consisted of waves. Experiments proving the wave-nature of light are well documented, and, as Max Born notes, "we have seen that, in every phenomenon of interference, we can perceive the light waves as clearly and evidently as water waves or sound waves" (1956, 31; Einstein and Infeld, 105-119; Zukav, 79-87).

In 1900, Max Planck introduced the concept of a unit of radiation, known as a quantum (1900a;b;c; 1901a;b; 1914, 67-70; 1932, 212-213). He argued that radiation was not continuous, but was discontinuous, and could only be absorbed in whole numbers of quanta, and that the energy of a quantum is related inversely proportionally to the wavelength, and proportionally to the frequency of the radiation. The energy contained in a quantum is given by the equation $E = hv$, where v is frequency of the light, and h is a constant known as Plank's constant.* (Planck only actually fully accepted his own notions of quanta and discontinuity in 1912, after the impressive work by Einstein [Galison 81]).

Based on this work and experiments by Philip Lenard (1900), other physicists produced a strange phenomenon. The amount of energy required to free an electron from a metal plate was not dependent at all upon the <u>intensity</u> of the light, but rather upon the <u>frequency</u> of the light. This effect was known as the photoelectric effect.

In his explanation of the photoelectric effect, Einstein established that as the intensity simply established the number of quanta striking the electrons, and the frequency indicated the energy of the individual quanta (photons*), it was the energy of the individual quanta of light, called photons, that was responsible for the emission of the electrons (1905b; 1906). In order to accept this, we have to accept that light consists of <u>particles</u> - this phenomenon has been demonstrated equally as well as the existence of light as waves (Compton 1929; Einstein and Infeld, 257-270).

The **photoelectric equation** gives the maximum kinetic energy of the electron, and is given by $E = hf - \Phi$, where E is the kinetic energy, and Φ is the work function. (In the paper, Einstein uses β for Planck's constant, v for the frequency, and P for the work function [1906, 146]).

photon A quantum of electromagnetic radiation, having the energy of $E = hv$, where v is its velocity (c)*, and h is Planck's constant.*

Planck's constant and formula In the belief that the emission of radiation can only in discrete amount called quanta, the energy of the quanta is proportional to the frequency of the radiation; this is expressed in the equation E = hv, where E is the energy, v is frequency of the radiation, and h is the constant of proportionality, 6,626 196 x 10^{-34}Js. (Compton 1929, 507; Planck 1900a;b;c; 1901a, 561; 1901b). (Note: In the 1900b paper, the variable "h" is denoted by "b" (120-122; 1959, 174-175), and in 1901b, the equation is given as h = 6,55.10^{-27}erg.sec (563). Allowing for the fact that 1 erg = 1 x 10^{-7}Joules, a slightly different notation, and later refinements, the figure is now expressed as above.)

Poincaré Invariance One of the principles of S-Matrix* theory. It states that the "reaction probabilities (and thus the S-matrix elements) must be independent of displacements of the experimental apparatus in space and time, and independent state of motion of the observer." (Capra 1979, 15). See Also Unitarity*, Analyticity* and Crossing.*

positron (or positive electron) An elementary particle* which has an electron mass and positive charge equal and opposite to that of the electron,* and the mass equal to that of the electron. See tables 1, 2 and 7 below.

proton A positively charged elementary particle*. See tables 1, 3, 5 and 7 below.

psi particle See J-particle.*

quark model (see table 5 for list of quarks)

In order to have some idea of the nature of quarks, it is necessary to understand their history. In 1957 Murray Gell-Mann and E.P. Rosenbaum published an article entitled "Elementary Particles." In this article they exhibited what seemed to many to be a useful classification system of what appeared to be the "arbitrary jumble of elementary particles" (72). The authors, however, were prudent enough to conclude their article with a series of questions; one of which was "Are all the particles we have mentioned really elementary, or are some of them just compounds of other particles?" (88). By 1962, there was further doubt that these particles were elementary, and Gell-Mann and Y. Ne'eman had independently developed a sophisticated method of classification that became known as the "Eightfold Way" (Ne'eman 1961; Gell-Mann 1962, 1079-1080; Chew et al 1964, 89-93). The classification system was a vast improvement on other works, and had been used to predict the existence of other particles (Close, Marten and Sutton, 99). Its fault, however, was that it still left the feeling that nature was made up of mathematical abstractions, or worse, that at a base level, particles were merely made up of themselves in a confusing muddle.

The term "quark" was introduced by Murray Gell-Mann (1964, 214-215). In a footnote, he explains that the term is taken from James Joyce's Finnegan's [sic] Wake, [from the line "Three quarks for Muster Mark!"] Gell-Mann 215; Joyce 383).

Later, experimental data (Bjorken and Paschos 1969; Feynman 1969) indicated that protons* and neutrons* were made of "fundamental pointlike constituents" known as partons,* which were "free, structureless particle[s]" (Bjorken and Paschos, 1975-1976).

Heisenberg feels that the quark model asks the wrong questions: questions like "how many quarks does a proton consist of?" being answered by "three" safely ignore the fact that it could also consists of five quarks and two antiquarks, and that one quark could actually consist of two quarks and one antiquark (Heisenberg 1985, 129).

Quarks are thought to be the fundamental constituents of all hadrons*. Whether they are real or not is still debatable, but even those who believe they are not, find that the quark model is a useful device for classification of hadrons.

Although quarks have been given names like "charm," "truth" and "beauty" which are called "flavours," and also come in various "colours," these terms are merely useful labels used to indicate physical and mathematical properties.

So far three families have been detected, with a possible fourth. Each family is associated with a lepton* family in the following way.

FAMILY	1	2	3	4
LEPTONS	v_e e^-	v_μ μ^-	v_τ τ^-	v_L L
QUARKS	u d	c s	t b	t' b

The truth quark (t) and the fourth family have not yet been detected. (Cline, 44)

There are four reasons that it is unlikely that a single quark will ever be seen:

1. An infinite amount of energy would be needed to break up the hadrons in order to separate the quarks - this energy is simply not available.

2. Even if this energy were available it would not separate the quarks, but, according to the formula of $E = mc^2$, it would supply new matter. For example, if we tried to separate a hadron consisting of an up and down quark, the energy supplied would form two new quarks which would then combine with the original hadron, split, and form two hadrons: the quarks would still be locked inside the hadrons.

3. There is the notion that if a quark were to be found - or a particle that exhibited quark properties - it would by definition not be fundamental: "A truly elementary particle -

completely devoid of internal structure - could not be subject to any forces that would allow us to detect its existence. The mere knowledge of a particle's existence, that is to say, implies that the particle possesses internal structure!" (Chew qtd. in Capra 1983, 303).

4. There is the knowledge that the new particle, may simply be "the first-seen member of a new branch of the hadron family" (Chew 1970, 23-24. See also Nambu, 1976).

Relativity

1.1. special First published in Einstein's 1905c. The Special Theory is a culmination of work done by Michelson (1881) and Morley* in 1887, (Lorentz 1923) and Lorentz and Fitzgerald in 1892, although "it appears that the Michelson-Morley played no direct role in [Einstein's] construction of the special theory of relativity" (Feyerabend 1987, 706), and he was unaware of the work done by the latter two.

Apart from what has been given in the thesis, more specifically, Relativity recognises that at velocities close to the velocity of light, the length of a moving object viewed by an observer who is <u>stationary relative to that object</u> will be perceived as shorter than the length observed by an observer <u>moving with the object</u>. Furthermore, should the moving observer perform an action, the length of time taken to perform the action will be perceived by the stationary observer as longer than the time perceived by the moving observer.

<u>Calculations</u>:

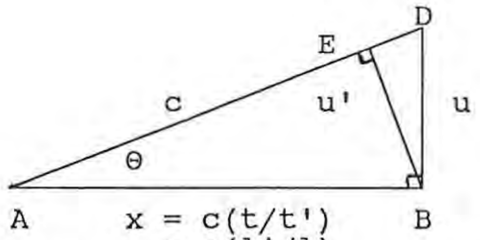
Time: Two observers observe an action. Observer A is in motion at a velocity u relative to the action, and Observer B is stationary. t' is the time taken for action according to observer A, and t is the time taken for the action according to Observer B. The relationship can be shown in the equations where:

$$t' = t / \sqrt{(1 - u^2/c^2)} \quad \text{or} \quad t = t' \times \sqrt{(1 - u^2/c^2)}$$

Length: Two observers observe an rigid bar. Observer A is in motion at a velocity u relative to the bar, and Observer B is stationary. l' is the length of the bar according to observer A, and l is the length of the bar according to Observer B. The relationship can be shown in the equations where:

$$l' = l \times \sqrt{(1 - u^2/c^2)} \quad \text{or} \quad l = l' / \sqrt{(1 - u^2/c^2)}$$

The equations can also be represented as two-dimensional Cartesian diagrams, if the velocity of any motion (u) is viewed as the y-component of the velocity of light. U is the velocity of the observer in motion, according to his time to cover <u>BD</u> as measured in the ground observer's <u>CS</u>. U' is the velocity of the obsever in motion, according to the ground obsever's time that it takes to cover <u>BD</u> as measured in the ground observer's <u>CS</u>. The following right-handed triangle is derived:



$$x = c(t/t') = c(l'/l)$$
$$x = (c)\{\cos(\text{cosec } u/c)\}$$

and thus:

$t' = t/(\cos\Theta)$ and $t = t'(\cos\Theta)$
$l = l'/(\cos\Theta)$ and $l' = l(\cos\Theta)$
u' (<u>BE</u>) $= x(\sin\Theta)$

An example illustrates the effect of relativity: Observer A observes observer B smoking a cigarette and moving at a velocity of $2{,}7\times10^8 ms^{-1}$ (0,9 the speed of light). According to observer B, the cigarette is originally 6cm long, and takes 8 minutes to smoke. According to observer A, however, the cigarette is originally less than 2,7cm long, and yet takes nearly 18½ minutes to smoke. We should also bear in mind that for ten thousand different observers, there are ten thousand different sets of measurements. (See also Dorling).

(Because of the constancy of the velocity of light, if the smoking observer B had turned on a torch, both he and observer A would have seen the light from the torch travelling at $3\times10^8 ms^{-1}$).

In the original works, the object used was a rod. For this examples, I have used a cigarette to make the situation a little more familiar. The calculations involved are thus:

(1) time difference:
t' = time observed by observer A = ?
t = time observed by observer B = 8 minutes (480 seconds)
u = velocity of B relative to A = 0,9c
c = velocity of light, taken at $3 \times 10^8 ms^{-1}$

According to observer A, the time taken to smoke the cigarette is:

$$t' = t / (\sqrt{1 - u^2/c^2})$$
$$= 480s / [\sqrt{1 - \{(0{,}9c)^2/c^2\}}]$$
$$= 480s / [\sqrt{1 - \{7{,}29\times10^{16}/9\times10^{16}\}}]$$
$$= 480s / \sqrt{0{,}19}$$
$$= 480s / 0{,}436$$

= 1101,196s

= 18 mins and 21 seconds

(2) length difference:
l' = length observed by observer A = ?
l = length observed by observer B = 6 cm (0,06m)
u = velocity of B relative to A = 0,9c
c = velocity of light, taken at 3 x $10^8 ms^{-1}$
According to observer A, the length of the cigarette is:

$$l' = l \times (\sqrt{1 - u^2/c^2})$$

$$l' = 0{,}06m \times [(\sqrt{1 - \{(0{,}9c)^2/c^2\}}]$$

$$l' = 0{,}06m [(\sqrt{1 - \{7{,}29 \times 10^{16}/9 \times 10^{16}\}}]$$

$$l' = 0{,}06m \times 0{,}436$$

$$l' = 0{,}02615m$$

$$l' = 2{,}62 \text{ cm}$$

Using the triangle method:

if x = (c){cos(cosec u/c)}
then x = 1,3076x10^8ms-1
and if sine Θ = (u/c) = 0,9
then Θ = 64,158°
and cos Θ = 0,4359

thus t' = t/(cos Θ)
becomes t' = 480s / 0,4359
t' = 1101,192 s
t' = 18 mins and 21 secs

and l' = l(cos Θ)
becomes l' = 0,06m x 0,4359
l' = 0,02615m
l' = 2,62cm

Note: In 1971, experiments using Caesium* clocks placed in jet aircraft and flown around the world gave added proof of the time changes predicted by special relativity. (Hafele 1971, Hafner, 1971, Hafele and Keating 1972a; 1972b)

2. general First published in Einstein's 1916 (1916; 1923b). While special relativity gave formulation for frames of reference in non-accelerating (or uniform) motion, general relativity supplies information on accelerating frames of reference. As far as clocks are concerned, "the general theory predicts that a clock in a stronger gravitational field will run slowly compared with a similar clock in a weaker field" (Hafele 1971, 267). (See also Einstein and Infeld; Ryder).

S Matrix See S-matrix.*

Schrödinger's Cat A thought experiment* which attempts to show the weaknesses of the Copenhagen Interpretation's* insistence on observables as the only quantities (see, for instance, uncertainty principle).

In this experiment a cat is placed in a sealed container, and the system is connected to a radioactive source which has a 50/50 chance of releasing a particle within one minute. If a particle is released, then a poison gas pellet will be released into the box; if not, the pellet will not. Schrödinger says that according to the Copenhagen Interpretation, after one minute, until we have seen the cat, the cat cannot be said to be dead or alive, but exists in an indefinite state. Only after looking at the cat do we change the probability function of the states: the one disappears, and the other materialises (Pagels, 136-140).

scientific notation A method of denoting very large or very small numbers, using the form $2,45 \times 10^{-4}$ where the superscripted figure gives an indication of the placing of the decimal to the left (if negative) or right (if positive) of its present position. For example, $2,45 \times 10^{-4}$ is the same as 0,000245 and $2,45 \times 10^{4}$ is the same as 24500. Specific factors have also been given names; below is an example of these:

Factor	Prefix	Symbol	Example
10^{-18}	atto	a	
10^{-15}	femto	f	
10^{-12}	pico	p	
10^{-9}	nano	n	1 nanosecond = 1ns
10^{-6}	micro	μ	= 10^{-9}seconds
10^{-3}	milli	m	1 millimetre = 1mm
10^{-2}	centi	c	= 10^{-3}metres
10^{-1}	deci	d	1 centimetre = 1cm
10	deka	da	= 10^{-2}metres
10^{2}	hecto	h	
10^{3}	kilo	k	1 kilogram = 1kg
10^{6}	mega	M	= 10^{3}grams
10^{9}	giga	G	
10^{12}	tera	T	

(Pitt 422; Kane and Sternheim, inside front cover pages)

S-matrix (or Scattering Matrix) An alternative to the quark* model. S-Matrix relies on the principles of the Poincaré invariance,* Unitarity* and Analyticity,* and each hadron can play three roles: a composite of other hadrons*, a constituent of other hadrons, as part of an exchange between constituents, thus contributing to the binding forces of the hadrons (Capra 1979, 17). See also Bootstrap* and Crossing.*

S-Matrix theory is essentially a mathematical statistical description of nature, the ground work of which had been laid by Heisenberg and Jordan in the 1920's. (Heisenberg 1926; Jordan 1926b;c) and later more accurately defined by Dirac (1928a;b) Wheeler (1937a;b), Heisenberg (1943a;b) and others (Chew et al 1957; 1958; Mandelstam 1958; Iagolnitzer 1978). Since then, as more particles have been discovered, it has had to be revised in order to accommodate them, but has held it statistical nature.

The philosophical perspective of S-Matrix is essentially that defined by Bohr concerning observer-object relationship, and probabilities (Stapp, 1971, 1303-1306; Jeans 1959, 286-307;). It opposes the view based on Schrödinger's work that motions between states can be observed, for, it was felt that Schrödinger's ideas did not account for Relativity (Heisenberg 1985, 127; Bohr 1928, 585-590; Stapp 1971, 1316-1318).

S-matrix holds the position that "Every particle consists of all other particles" (Heisenberg 1985, 129). S-matrix concerns itself with processes involved in hadron reactions, arguing that a particle "is not an independently existing unanalyzable entity" but rather, "a set of relationships" (Stapp, 1310) determined by a network of probabilities based on previous processes. As Capra explains, "At the subatomic level, matter does not exist with certainty at definite places, but rather shows `tendencies to exist', and atomic events do not occur with certainty at definite times and in definite ways, but rather show `tendencies to occur'" (1983, 77-8). He argues further

> The interconnections in such a network cannot be determined with certainty but are associated with probabilities. Each reaction occurs with some probability which depends on the available energy and on the characteristics of the reaction, and these probabilities are given by the various elements of the S matrix. (1979, 14)

The following 2 diagrams show a variety of processes that can occur: the only definites are given by the beginning and the end products.

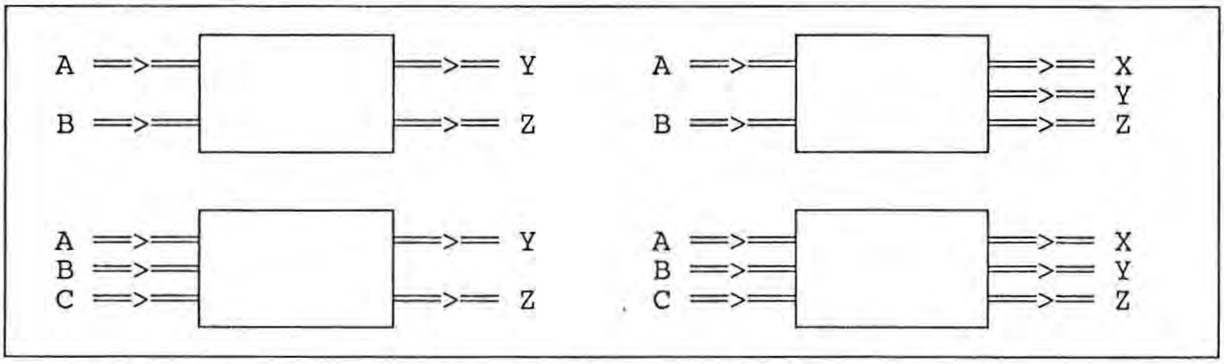


Fig.7.1: Diagrams showing various S-Matrix scattering processes. (Capra 1979, 13)

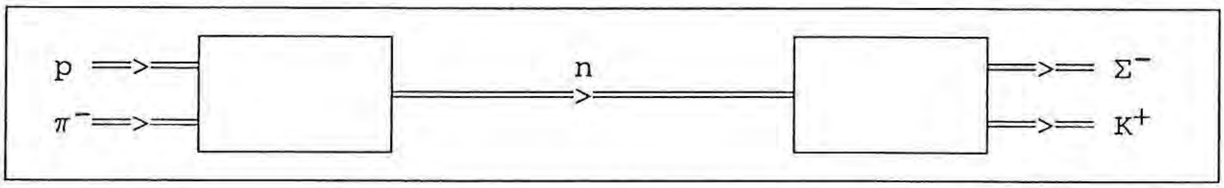


Fig.7.2: Diagram showing two successive reactions interconnected through a neutron. (Capra 1979, 13-14)

The neutron may "be seen as a bound state of the proton [(p)] and the pion [(π^-)] from which it arises, and also a bound state of the sigma [(Σ^-)] and kaon [(K^+)] into which it decays" (14; 1983, 291).

We need to remember two important things:
Firstly, hadrons often form merely one stage in a series of reactions, as indicated below:

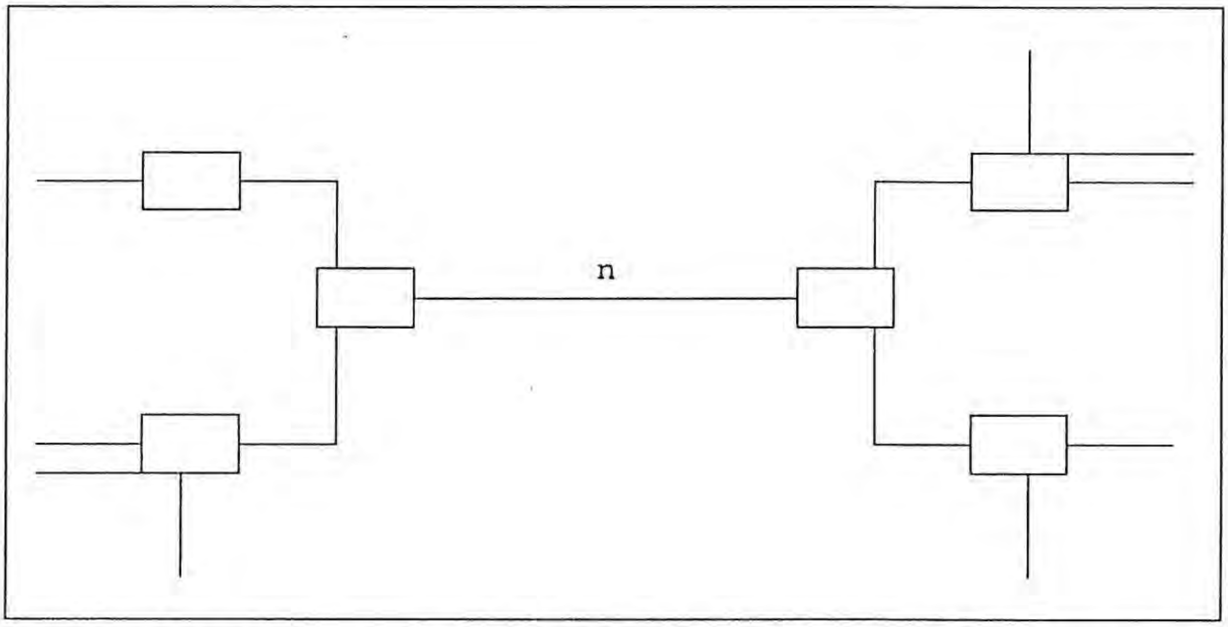


Fig.7.3 Diagram showing network of interconnected hadron reactions (1979, 14).

"It should be remembered, however, that S-matrix diagrams are not space-time diagrams but symbolic representation of particle reactions. The switching from one channel to the other takes place in abstract mathematical space" (1983, 302 fnt.).

Secondly, because of the constant variations, it makes little sense to speak of the "constituents" of hadrons. "A hadron...does not consist of a definite arrangement of constituent part but shows tendencies to undergo various reactions, and these tendencies define the hadron's `constituents'" (Capra 14). This results in an equivalency of hadrons described by Chew as "nuclear democracy," in which "each particle helps to generate other particles which, in turn, generate it" (qtd. in Capra 17), and the concept of a quark is seen more as a characteristic than a particle (19-20).

This contradicts common-sense. We have see, however, that common-sense is not a valid measuring stick. As Stapp says:

> Ordinary words are tied to our common-sense ideas about the world. Thus in situations where these common-sense ideas are in question, one must be careful about what the words are supposed to mean.....the point of the argument is to show that common-sense ideas about the world are definitely inadequate, and that, moreover, they fail already at the macroscopic level. (1971, 1314-1315).

"Quantum theory is simply a theory that makes predictions about things that are described as results of measurements," says Stapp (1971, 1318). In reply to the question, what are the "actual things?" he says that "the actual things are preparations, measurements, and their results....Thus the actual things are formulated in terms of a complicated interconnection between descriptions, technicians, measuring devices and their actions upon each other." (1318). The entire mechanistic view of the world is rejected. All we can measure are experiences, and "experiences must be viewed as parts of webs [of experiences], whose parts are not defined except through their connections to the whole." (1319)

See references in main text, and also Streater, 1964.

spin The intrinsic angular momentum of a particle (Pitt 359).

tachyons Recognising that the velocity of light may not be crossed, there is the hypothesis that there are particle which already travel faster than light. These particles are referred to as tachyons, as opposed to tardyons*, which travel slower that the velocity of light (Trefil, 215-6).

tardyons See tachyons.*

thermodynamics The study of the laws in the relationship between heat and other forms of energy. First law of thermodynamics: This recognises that heat is a form of energy, and that in closed system, the amount of energy is constant ($\delta Q = dU + \delta W$, where δQ is heat absorbed, dU is the increase in internal energy, and δW is work done.) Second Law of thermodynamics: Systems tend to evolve from a state of being highly ordered to states of less order; thus the degree of entropy (disorder) tends towards a maximum value (Pitt, 379, Kane and Sternheim, 227). (See also Maxwell's Demon* above).

thought experiments Hypothetical conceptual experiments designed to investigate ideas in physics - these have usually not been tried (sometimes are unable to be tried) in laboratories. Although they serve a useful purpose, the fact that they are hypothetical, and sometimes metaphorical reduces their applicability. (We should note, however, that many of Galileo's experiments were, in fact, thought experiments (Chalmers 78-79)). The most important examples of thought experiments include Maxwell's Demon,* Schrödinger's Cat,* EPR,* and the Bohr-Heisenberg microscope* (Heisenberg 1927).

uncertainty/indeterminacy principle (Heisenberg's) See Heisenberg.*

Unitarity One of the principles of S-Matrix* theory. It states that the "outcome of a particular reaction can only be predicted in terms of probabilities and, furthermore, that superimpositions of probability amplitudes associated with different experimental results correspond themselves to possible experimental results." (Capra 1979, 15) See also Analyticity,* Crossing* and Poincaré Invariance.*

wave-particle duality See photoelectric effect* and de Broglie.*

TABLES OF SUB-ATOMIC PARTICLES

These tables show only the major particles; some of these have not been mentioned in the thesis, but have been included in the table in order to show relative values and family placings. We should remember that "lifetime" of unstable particles, means "average lifetime;" because of the statistical nature of subatomic physics, we cannot make statements about individual particles (Capra 1983, 188).

TABLE 1: RELATIONSHIP OF FAMILIES OF SUB-ATOMIC PARTICLES

LEPTONS
See Table 2 for examples.
Interact either by the electromagnetic interaction or the weak interaction, and have no apparent internal structure. They have a half-integer spin, and are called fermions.

HADRONS
See Tables 3 & 4 for examples.
Interact by means of the strong force. Hadrons consist of quarks, and are further subdivided into BARYONS and MESONS

BARYONS
See Table 3 for examples.
Heavier, consisting of 3 quarks.
Decay into protons.
Like leptons, baryons have a half-integer spin, and are called fermions.

MESONS
See Table 4 for examples
Lighter, consisting of 2 quarks.
Decay into leptons and photons.
Like photons, mesons have an integral spin, and are called bosons.

QUARKS
See Table 5 for examples
Are bound to form Baryons (3 quarks) and Mesons (2 quarks) by means of GLUONS.
Collectively, quarks and gluons are known as partons, the constituents of protons and neutrons

(Tables 2-6 taken from Close, Martin and Sutton).

TABLE 2.: LEPTONS

NAME	SYMBOL	MASS	LIFETIME	CHARGE	SPIN
ELECTRON	e^-	0,511 MeV	Stable	-1	½
POSITRON	e^+	0,511 MeV	Stable	+1	½
MUON and ANTIMUON	μ^- μ^+	105,6 MeV	$2x10^{-6}s$	-1 +1	½
TAU and ANTITAU	τ^- τ^+	1,784 MeV	$3x10^{-13}s$	-1 +1	½
ELECTRON NEUTRINO and ANTI-NEUTRINO	v_e $\bar{v}_e$	0(?) <50 eV	Stable(?)	0	½
MUON NEUTRINO and ANTI-NEUTRINO	v_μ $\bar{v}_\mu$	0(?) <0,5 MeV	Stable(?)	0	½
TAU NEUTRINO and ANTI-NEUTRINO	v_τ $\bar{v}_\tau$	0(?) <70 MeV	Stable(?)	0	½

TABLE 3.: HADRONS: A. BARYONS (3 quark construction)						
NAME	SYMBOL	MASS	LIFETIME	CHARGE	SPIN	QUARK* CONTENT
PROTON	p	938,3 MeV	Stable(?) $>10^{32}$years	+1	½	uud
ANTI-PROTON	$\overline{p}$	938,3 MeV	Stable (?) $>10^{32}$years	-1	½	$\overline{uud}$
NEUTRON	n	939,6 MeV	In nuclei:stable; Free: 15 minutes	0	½	ddu
ANTI-NEUTRON	$\overline{n}$	939,6 MeV	In nuclei:stable; Free: 15 minutes	0	½	$\overline{ddu}$
LAMBDA	Λ	1,115 GeV	$2{,}6x10^{-10}$s	0	½	uds
ANTI-LAMBDA	$\overline{\Lambda}$	1,115 GeV	$2{,}6x10^{-10}$s	0	½	$\overline{uds}$
SIGMA (sigma-plus)	Σ^+	1,189 GeV	$0{,}8x10^{-10}$s	+1	½	uus
SIGMA (sigma-minus)	Σ^-	1,197 GeV	$1{,}5x10^{-10}$s	-1	½	dds
SIGMA (sigma-zero)	Σ^0	1,192 GeV	$6x10^{-20}$s	0	½	uds
XI (xi-minus)	Ξ^-	1,321 GeV	$1{,}6x10^{-10}$s	-1	½	dss
XI (xi-zero)	Ξ^0	1,315 GeV	$3x10^{-10}$s	0	½	uss
OMEGA MINUS	Ω^-	1,672 GeV	$0{,}8x10^{-10}$s	-1	½	sss
CHARMED LAMBDA	Λ_C	2,28 GeV	$2x10^{-13}$s	1	½	udc

*See table 5 for symbols of quarks.

TABLE 4.: HADRONS: B. MESONS (2 quark construction)						
NAME	SYMBOL	MASS	LIFETIME	CHARGE	SPIN	QUARK*1 CONTENT
PION (pi-zero)	π^0	135 MeV	$0,8x10^{-16}s$	0	0	$u\bar{u}$ or $d\bar{d}$
(pi-plus) PION (pi-minus)	π^+ π^-	140 MeV	$2,6x10^{-8}s$	+1 -1	0	$u\bar{d}$ $d\bar{u}$
KAON (K-zero)	K^0	498 MeV	Short: $10^{-10}s$*2 Long: $5x10^{-8}s$*2	0	0	$d\bar{s}$
(K-plus) KAON (K-minus)	K^+ K^-	494 MeV	$1,2x10^{-8}s$	+1 -1	0	$u\bar{s}$ $s\bar{u}$
J/PSI	J/w	3,1 GeV	$10^{-20}s$	0	1	$c\bar{c}$
(D-zero) D (D-plus)	D^0 D^+	1,87 GeV	$10^{-12}s$ $4x10^{-13}s$	0 +1	0	$c\bar{u}$ $c\bar{d}$
UPSILON	Y	9,46 GeV	$10^{-20}s$	0	1	$b\bar{b}$

*1 See table 5 for quark symbols.

*2 The K^0 and the $\bar{K}^0$ form a quantum system whose superposition yields two physical properties, which reveal matter-anti-matter symmetry (CP violation). These are the short lived K^0_s and the long lived K^0_l

TABLE 5.: QUARKS					
NAME (Flavour)	SYMBOL	MASS	LIFETIME	CHARGE	SPIN
UP and ANTI-UP	u $\bar{u}$	~5MeV	Stable*	+2/3 -2/3	½
DOWN and ANTI-DOWN	d $\bar{d}$	~10MeV	Variable*	-1/3 +1/3	½
STRANGE and ANTI-STRANGE	s $\bar{s}$	~100 MeV	Variable*	-1/3 +1/3	½
CHARM and ANTI-CHARM	c $\bar{c}$	~1,5 GeV	Variable*	+2/3 -2/3	½
BOTTOM (or BEAUTY) and ANTI-BOTTOM	b $\bar{b}$	~4,7 GeV	Variable*	-1/3 +1/3	½
TOP (or TRUTH) and ANTITOP	t $\bar{t}$	~>30 GeV	Variable*	+2/3 -2/3	½

* As quarks occur only in pairs (making mesons) or triplets (making baryons), their lifetimes are variable, depending on the nature of the individual meson or baryon. The up quark, being the lightest, is as stable as the proton that contains it. Each quark also consists of three colours (red, green and blue), while each anti-quark has threee anti-colours (cyan, magenta and yellow). Collectively, quarks and gluons (see Tables 1 and 6) are known as partons.

TABLE 6.: GAUGE BOSONS					
NAME	SYMBOL	MASS	LIFETIME	CHARGE	SPIN
PHOTON	y	0	Stable	0	1
W (W-plus) (W-minus)	W^+ W^-	83GeV	10^{-25}s	+1 -1	1
Z	Z	93GeV	10^{-25}s	0	1
GLUON	g	0	Stable	0	1

TABLE 7: LIST OF CONSTANTS

velocity of light (in vacuum)	c	$2{,}997\ 924\ 58 \times 10^{8} ms^{-1}$
charge of electron (-) or proton (+)	e	$\pm 1{,}602\ 191\ 7 \times 10^{-19} C$
rest mass of electron	m_e	$9{,}109\ 558 \times 10^{-31} kg$
rest mass of proton	m_p	$1{,}672\ 614 \times 10^{-27} kg$
rest mass of neutron	m_n	$1{,}674\ 92 \times 10^{-27} kg$
electronic radius	r_e	$2{,}817\ 939 \times 10^{-15} m$
Planck's constant	h	$6{,}626\ 196 \times 10^{-34} Js$

1 second (s) is defined as 9 192 631 770 accumulated periods of the frequency of the atomic transitions of an "ideal" caesium* (^{133}Cs) beam frequency standard.

END NOTES

END NOTES

CHAPTER ONE - NEW CRITICISM

1 In A history of Modern Criticism, Vol. 5 (1986), René Wellek describes the confusion that is developed in Richards's description of belief (in Practical Criticism 272-280) where he attempts to distinguish between emotional (poetic) and intellectual (scientific) beliefs. Wellek, however, makes the issue far more complicated than it is made by Richards. Speaking about the manner in which Richards deals with John Donne's sonnet "At the round earth's imagined corners," Wellek writes on page 227 (page references in this quotation are Wellek's references to Richards's Practical Criticism):

> The "poem requires actual belief in its doctrine for its full and perfect imaginative realization" (272-73). A little further on we are required to give "emotional belief" to Shakespeare's "The Phoenix and the Turtle" (270), but then a few pages later Richards modifies this again and says sensibly that "the idea is neither believed, nor doubted nor questioned: it is just present" (275) and that "the question of belief or disbelief, in the intellectual sense, never rises when we are reading well [my emphasis]." (277)

The problem is caused by the contradictions of the emphasised pieces. The resolution is rather simple: Richards does not write this. At this stage, Richards is dealing with the difficult topic given by his chapter heading "Doctrine in Poetry," and is examining the ability of a reader's appreciating the poem, and the extent to which this appreciation is affected by the reader's belief in (adherence to) the doctrine that is being espoused in the poem. He then says:

> But as the assumptions [of the doctrine in the poem] grow more plausible, and as the consequences for our view of the world grow important, the matter seems less simple. Until, in the end, with Donne's Sonnet (Poem III), for example, it becomes very difficult not to think [my emphasis] that actual belief in the doctrine..." (272-3)

The important words here being "it becomes very difficult not to think." These words have been left out of Wellek's quote, and make Richards look like a confused and bungling idiot. Richards in fact posits this as a possible problem, not as a statement of fact, which he then attempts to resolve in his distinction between intellectual and emotional belief. Although his debate is sometimes difficult to follow, it is not nearly as complicated as the monster that Wellek has created.

There are further other disturbing inaccuracies in the text, (e.g. on pp. 222, 225 and 227) which can only be resolved by direct consultation with Richards's work.

2 Holloway points out that this is dangerous, as it encourages the reader constantly to seek for deeper inter-relatedness of constructions, leading to a "cult of complexity." Ironically, he sees this as the result of the criticism being "distorted by a norm appropriated from science" (483) (See also 486-9).

3 Here they are supported by T.S. Eliot who believes that the question of "is this a good poem?" cannot be answered by any "theoretic ingenuity" (1948 16-17). Although exactly how he stands on this is not too clear, as he later speaks about the importance of history in the appreciation of poetry (136ff). Although in "The Frontiers of Criticism" (1956), he claims that background knowledge is often interesting, but "is not relevant to our understanding of the poetry as poetry" (536).

CHAPTER TWO - STRUCTURALISM

1 Although Structuralism covers a vast field, unless obviously used in other contexts, the terms Structuralism and Structuralist will refer specifically to Structuralist literary theory. I will, however, from time to time, use the term Structuralist literary theory to distinguish between the work in literature and other areas where Structuralism has been applied.

2 This reference is to paragraph 367 on page 72. References to this subject are numerous, some being, 147 (p.22), 161 (25), and 692 (214). See also C6 (p.xxvii).

3 Most of the early impressive work on Structuralism was done by theorists working in languages other than English, and, as a result, most of the writings quoted are translations. Because of necessary liberties taken by the translators, especially in the punctuation, and because some of the expressions are now key expressions in these languages, when I have quoted from the English translation, I have sometimes felt it necessary to supply the original wording. Rather than clutter the main text with this, I have retained the English in the text (page references to the English edition in round brackets), and kept the French to the asterisked footnotes (e.g. [*1]). In these "Original Text Footnotes," page references to the French editions are in square brackets []. See list of works cited for bibliographical information.

Some English translations of work are unavailable. Where I have used only the original foreign title in the text, the translation is mine.

4 <u>Key to Figure 1</u>, explaining the terms as used by Ferdinand de Saussure.

<u>Langage, Langue and Parole</u>. Langue is a part of human speech. "It is both a social product of the faculty of speech and a collection of necessary conventions that have been adopted by a social body to permit individuals to exercise that faculty.... [It] is a self-contained whole and a principle of classification[*3] (9). Within this system of signs, the only essential thing is the "union of meanings and sound images..."[*4] (15).

It is therefore distinct from the act of speaking (Parole) which is the executive function. "Execution is always individual, and the individual is its master"[*5] (13). Although one might "apply the term linguistics to each of the two disciplines and speak of a linguistics of speaking...[,] that science must not be confused with linguistics proper, whose sole object is language"[*6] (19-20).

<u>Signe</u>. A linguistic sign (signe) is not an abstraction, but

is a reality that has its seat in the brain. It is a whole that unites a concept or signified (signifié) and sound image or signifier (signifiant), that is, the impression that the sound makes on our senses (15, 66-67). "Because the "bond between the signifier and the signified is arbitrary....I can simply say: the linguistic sign is arbitrary"*7 (67). Although Saussure emphasises the distinction between langue and parole, he also realises that the expression of the sign is achieved because of its auditory nature (70). Thus the expression of language, in fact the very evolution of language, is achieved by the speech act. He does make it quite clear, however, that speech, as a motor activity, is simply one method of communication (17-20, 66).

"Synchronic linguistics will be concerned with the logical and psychological relations that bind together coexisting terms and form a system in the collective mind of speakers" (99-100).

"Diachronic linguistics, on the contrary, will study relations that bind together successive terms not perceived by the collective mind but substituted for each other without forming a system" (100).

Syntagmatic relations are acquired because language is linear, and we can only pronounce one word at a time. "In the syntagm a term acquires its value only because it stands in opposition to everything that precedes it" (123).

Associative (Paradigmatic) Relations: "Outside discourse, on the other hand, words acquire relations of a different kind. Those that have something in common are associated in the memory, resulting in groups marked by diverse relations" (123).

The syntagmatic relation is in praesentia. It is based on two or more terms that occur in an effective series. Against this, the associative relation unites terms in absentia in a potential mnemonic series" (123).

5 Many of Jakobson's other essays were republished (although sometimes with extensive revisions) in his Language and Literature (1987). (See list of works cited for bibliographical details).

Original Text Footnotes

*1 véritable et unique objet [13]

*2 L'objet concrete de notre étude est donc le produit social déposé dans le cervaeu, c'est-à-dire la langue. [44]

*3 Mais qu'est-ce que la langue? Pour nous elle ne se confond pas avec le langage; elle n'en est qu'une partie déterminée essentielle, il est vrai. C'est à la fois un produit social de la faculté du langage et un ensemble de conventions nécessaires, adoptées par le corps social pour permettre l'exercice de cette faculté chez les individus....[E]st un tout en sou et un prinpe de classification. [25]

*4 C'est un systme de signes où il n'y a d'essential que l'union du sens et de l'image acoustique....[32]

*5 [E]lle est toujours individuelle, et l'individu en est toujours le maître.... [30]

*6 On peut à la rigueur conserver le nom de linguistique à chacune de ces deux disciplines et parler d'une linguistique de la parole. Mais il ne faudra pas la confondre avec la linguistique proprement dite, celle dont la langue est l'unique objet. [38-39]

*7 [L]e signe linguitique est arbitraire. [100]

*8 Par exemple, la linguistique doit être soigneusement distinguée de l'ethnographie et de la préhistorie, où la langue n'intervient qu'à titre de document; distinguée aussi de l'anthroplogie, qui n'étudie l'homme qu'au point de vue de l'espéce, tandis que le langage est un fait social. [21]

*9 à formuler une méthode positive et à connaître la nature des faits soumis é son analyse. [37]

*10 Une analyse véritablement scientifique doit être réelle, simplificatrice et explicative. [43]

*11 L'erreur de la sociologie traditionnelle, comme de la linguistique traditionelle, est d'avoir considéré les termes, et non les relations entre les termes. [57]

*12 Un systéme de parenté ne consiste pas dans les liens objectifs de filiation ou de consanguinité donnés entre les individus; il n'extise quedans la conscience des hommes, il est un systéme erbitraire de représentations, non le développement spontané 'une situation de fait. [61]

*13 Le but de tout activité structuraliste, qu'elle soit reflexive ou poétique, est de reconstituer un «objet», de façon à maifester dans cette reconstitution les rgles de fonctionnement (les «fonctions») de cet objet. [214]

*14 On dit qu'à force d'ascse certains bouddhistes parviennent à voir tout un paysage dans une fe. C'est ce qu'auraient bien voulu les premiers analystes du récit : voir tout les récits du monde (il y en a tant et tant eu) dans une seule structure : nous allons, pensaient-ils, extraire de chaque conte son modle, puis de ces modles nous ferons une grande structure narrative, que nous reverserons (pour vérification) sur n'importe quel récit: tâche épuisante («Science avec patience, Le supplice est sûr») et finalement indésirable, car le texte y perd sa différence. [(S/Z) 9]

*15 Et précisément, parce que toute pensée sur l'intelligible historique est aussi participation à l'homme structural de durer : il sait que le structuralisme est lui aussi une certaine forme du monde, qui changera avec le monde; et de même qu'il éprouve sa validité (mais non sa vérité) dans son pouvoir à parler les anciens langages du monde d'une manire nouvelle, de même il sait qu'il suffra que surgisse de l'histoire un nouveau qui le parle à son tour, pour que sa tâche soit terminée. [219-220]

*16 Il s'ensuit que le péché majeur, en critique, n'est pas l'idéologie, mais le silence dont on la couvre... [254]

CHAPTER THREE - SCIENCE

1 Although Margaret Masterman notes that there are at least 21 different senses of the word "paradigm" as used by Kuhn (Masterman 1970, 61-65), I have chosen this definition of a paradigm as used by Kuhn in the reference, for it is this to which he most commonly refers. We should also note that the idea of a scientific revolution, analogous to a political or social revolution has, of course, been used for over two centuries (Cohen 1976), but I am using it strictly as defined according to Kuhn's model. Although Kuhn also uses the term "paradigm shift" for revolution, I shall not refer to it in the thesis, as it may easily become confused with some of the terminology I intend to use in Chapter Four.

2 Huxley replied that "He was not ashamed to have a monkey for his ancestor; but he would be ashamed to be connected with a man who used great gifts to obscure the truth" (qtd. in Leonard Huxley, 184). (Although we should note that even Huxley, along with Whewell and others, had initially been reluctant to accept Robert Chambers's developmental ideas as given in his Vestiges of Creation because of religious ideas (Barber 599). The 1925 `Scopes Monkey Trial,' in which John T. Scopes was tried for teaching evolution in high school, again became a playground for prejudice and misinterpretation, and demonstrated that little had changed in 65 years (Fay-Cooper Cole, 120-130; Grabiner and Miller 832-837; Lear; Morris, 63-68).

3 I do believe that Snow's question is perhaps more complicated: it is probably the equivalent of What are your views on Hamlet's insanity? - difficult to answer, but one would expect an opinion from an "educated, cultured" person.

4 (See Glossary for detailed information). General Relativity deals with alterations caused by accelerated motion, as opposed to the uniform motion dealt with by Special Relativity.

5 The most dramatic confirmation of time changes predicted by the theory came in 1971 using atomic clocks in jet aircraft (Hafele, 1971; Hafner, 1971; Hafele and Keating 1972a; 1972b). Work of this kind has also been done on sub-atomic particles: the most recent impressive recent work was done on neutrinos from a super nova in 1987, which indicated that the predictions are accurate to a ratio of $1/1x10^{11}$ (Asimov 1990, 7).

6 (Similar to the initial rejection of Faraday's work ([Agassi 1971, 8, 165-6; Preyer 60]). This "slip" was in their description of Einstein's work which they trusted the Prussian Academy of Science would not hold against him in the consideration of his admission to the Academy (Popper 1981, 97). Popper continues:

> Indeed, the wording of the apology for Einstein's slip is most interesting and enlightening. The relevant passage of the petition says of Einstein: "That he may sometimes have gone too far in his speculations, as for example in his hypothesis of light quanta [i.e. photons], should not weigh too heavily against him. For nobody can introduce, even into the most exact of the natural sciences, ideas which are really new, without sometimes taking a risk" [square parenthesis in Popper's translation]. (98)

In addition, Kuhn has proposed, in his Black-Body Theory and the Quantum Discontinuity, 1894-1912 that Einstein and Paul Ehrenfest, and not Planck were first responsible for the concept of quantum discontinuity (Horgan, 15).

7 At least many prominent people had read his thesis. In 1879, Planck had found greater problems with his doctoral thesis in which he worked on the second law of thermodynamics: Kirchoff and Carl Neumann refused to discuss it with Planck, and Planck doubted whether Helmholtz had even read it (Barber 596).

8 This is the basis for Joyce's "pornosophical philotheology" (Ulysses, "Circe," 933). Other areas of science had also had to take cognizance of the fact that a fair number of "certainties" are arbitrary delineations, based to a large extent on language. For instance the arbitrariness of the delineation in the colour spectrum is shown by different designations of colour between English and Welsh (Hjelmslev 1969) and Zulu (Visser 1982b). As Visser notes, the "colour spectrum has no boundaries apart from those established by languages" (54).

9 The very fact that Born's statistics were opposed by such prominent scientists delayed his receiving a Nobel Prize for twenty-eight years (Born 1971, 228-229).

10 Einstein often used the expression `old one' to refer to God, (Bernstein, 20; Pagels 45), and this saying of his was repeated several times in various forms (Bernstein 175; Ronald Clark, 327; A.P. French, 275; Pagels 148). It brings to mind Spinoza's rejection of the teleological standpoint in his Ethics (30-36). In addition, we should not reject the importance of Einstein's philosophical importance in his physics, for, as Reichenbach recounts: "When I, on occasion, asked Professor Einstein how he found his theory of Relativity, he answered that he found it because he was so strongly convinced of the harmony of the universe" (1951, 292)

Nevertheless, on this comment by Einstein, Born noted with regret that the rejection was based purely on a "basic difference of philosophical attitude" (91).

He said that the de Broglie-Schrödinger wave fields,

> were not to be interpreted as a mathematical description of how an event actually takes place in time and space, though, of course, they have referred to such an event. Rather [,] they are a mathematical description of what we can actually know about the system. (1940a, 491)

11 Some scientists, still insisting on the autonomy of science, declare this a waste of time; James Watson, co-discoverer of the DNA structure, writes "In England, if not every where, most botanists [in the 1950s] were a muddled lot....some actually wasted their efforts on useless polemics about the origin of life or how we know that a scientific fact is really correct" (63), although the discussion continues to the present (Timpane).

In a discussion with Karl Popper, Philipp Frank noted with dismay that his Engineering students "merely wanted to `know the facts'.... These students wanted to know only those things, those facts, which they might apply with a good conscience, and without heart-searching" (Popper 1970, 53). In addition, Toulmin uses the statement by Einstein "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds." Toulmin uses this as a reason for saying that we should look at "not so much what the scientists say, as to see what sort of things they do with the words they employ" (1967, 15). At the risk of arguing against Einstein, I would propose that what has been shown above, is that, of necessity, part of their deeds are their words, especially their words on the philosophy of science.

12 I have steered clear of other social issues that would also be seen as "corrupting" science. Some would echo Shklovsky's belief in the autonomy of art, and would say that science does not reflect "the color of the flag which waved over the fortress of the city." This attitude is expressed by Kuhn (1970c, 160ff; 1974, 460-462), when he says that scientists are unaffected by anything except the approval of their colleagues (1970c, 164) and have very little to do with social pressure or heads of state (168). Their research, he claims, is therefore unaffected by these outside issues.

Apart from the fact that approval from colleagues can spur the most heinous scientific crimes (Crane; Gould 1980; 1984; Mullins), these beliefs are a little naive. Apart from the cases already discussed concerning Galileo and Darwin, physicists and philosophers and historians of science have taken note of the fact that scientific work has been influenced by and has influenced their society and culture in various forms, especially their attitudes towards their nationality, racism and politics, (Timothy Beardsley, 14-18; Bernstein, 170-173, 177-183; Brown, 101-114; Cooper, 10-14; Einstein 1940b; Farely and Geison; Forman; Marilyn French; Gould 1981; Graham; Heisenberg 1958b, 27-29; Hendry; Heyl, 135-136; Hinshaw; Jones; Monod, 40-41; Morely; Morrison, Bethe and Panofsky; Pagels, 53-56; Orwell; Polanyi 58-60; Popper 1957b, vii; Rhodes; Shuttleworth, 269-70; Snow 1981, 100-103, 176-188), education (A.P. French, 317-318; Heisenberg 1958b, 62), ethics (Rescher), psychology (Freud, 140-144; Holton, 238; Maranto, 29-34; Northrop 1958, 1-3; von Franz; Young 1969; 1980b), gender (K.C. Cole; Doyle; Fee; Foucault 1980; Geddes and Thomas; Gould 1981; 1984; Greer; Jansen-Jarreit; Seltzer, 119-120; Wolkomir and Wolkomir;) social status (Gould 1980), religion (Brown, 145-185; Burchfield; Paul Davies; Frank; Pagels, 53-54) and the world in general (Einstein 1940b; A.P. French, 277; Marilyn French).

13 In his 1887 discussion, Huxley notes that the word atom is to be used lightly, and merely as a name for that which has not yet been divided, as opposed to the word molecule is used for that which has. He then makes the prediction, that the theories of science could quite conceivably move from the position of Dalton (a refinement of Democritus) to Aristotle (1893b, 75).

14 The search for an independent quark, with the knowledge that its finding would lead to "an enjoyable trip to Stockholm" (Polkinghorne 65) has never borne fruit, and it is unlikely that it ever will - see note on quarks in Glossary for details of this.

CHAPTER FOUR - FEYERABEND

1 Many researchers in "soft, or social sciences" also tried to apply the paradigm to their fields" (Horgan, 15). Ackerman also argues against the model's application to Fine Art for two other reasons: he says that older scientific practices (unlike artistic) are only of "historical significance" (372), and that physics is concerned primarily with what is "right or wrong" in the empirical sense.

Ackerman's view is accurate, but we note that he says that "the majority [not all] of the practising physicists" do not examine previous models - as we have seen in the previous chapter, those few physicists (such as Heisenberg) who have taken the trouble to examine older theories have found parallells and illuminations on the epistemologies of the present theories. As we saw further, Heisenberg feels that the search for ultimate particles is doomed to failure until the physicists realise that their ideas are formed according to Newtonian-type prejudices.

Furthermore, the weakness of physicists' ability to say what is absolutely "right or wrong" has been amply demonstrated in the previous chapter.

2 A recent example is Peter Washington's Fraud (1989). Once a reader is able to fight through the personal and collective insults and cheap "illjudged sideswipes" (Ellis 7), however, one finds the problems Washington has tried to avoid. Washington makes a brave attempt to deny that he is attacking Literary theory per se by saying that he attacks the "fruit of [the] irregular union" of Deconstruction, Marxism and Feminism by institutions (Washington 11). He later says, however, "Academic life is a comedy shot through with moments of tragedy and farce. For the most part, theory belongs with the farce" (37), and devotes his book to individual attacks on Structuralism, Deconstruction, Marxism and Feminism. He criticises the theorists for their jargon (11-37) and then uses it far more than most of his opponents, demonstrating the need for jargon under certain circumstances. (See also reviews of Washington's work by Ellis (1990) and Sage (1990). See also Felperin).

3 Feyerabend says that Kuhn's work has encouraged "creeps and incompetents" (1981c, 160), and notes that if Kuhn's criteria were applied to other fields, Organised Crime could be classified a science, and [John] Dillinger, by refuting Andrew Carnegie's "Pioneering don't pay" would be regarded as an outstanding individual researcher (Feyerabend 1970, 200).

4 Unfortunately, this thesis cannot examine the important and related Feminist argument concerning the notion of science and related ideas of objectivity, power, structure, analysis (and even literary theory itself) as methods and symbols of male domination (in opposition to subjectivity, subjection, and synthesis) in our society, or at least that the notion of an unbiased objectivity perceived to be in science has been abused to advance male chauvinistic prejudices. An insight into this debate is given by Chernin (29-44); Capra (1982); Cixous (1988), Driver (1982), Fetterley (1978), Marilyn French; Friedan (1981), Jansen-Jarreit; Keller; Kolodny (1975; 1980), Kristeva (1980), Nancy Miller (1981), Russett; Showalter (1988) and Trilling (1978).

5 And one might say the same of Willis Lamb who questions the reality of photons (Hacking 1983, 29-30, 56). The attack on Velikovsky comes from all quarters. The copy of Velikovsky's Worlds in Collision (1972b) that I used, was borrowed from the Physics Library, Rhodes University. On the cover, some person, identified as a "Mr. Witt" has written: "This book would be classified under `Jokes' if such a category existed." At the bottom of page three of the copy of May 1972 (2.2) edition of Pensée (from the same library) someone has written: THIS BOOK [sic] IS IN THE LIBRARY FOR INTEREST ONLY - NOT TO BE TAKEN SERIOUSLY. SEE SAGAN['S] "BROCA'S BRAIN"...Ch 7 [,] GARDNER...p 379 and the Fall 1980 edition of Skeptical Inquirer.
(The references are to Sagan (1974) and Gardner [1957]).

Since Velikovsky's original work, new evidence has continued to fan the fire, and while some scientists see it as supportive (Juergens 1966a; Stecchini 1966a; Stove 7; Talbott 1972a; 1972b, Velikovsky 1966; 1972a), some deny it (Gardner 1957, 28-41; 1981, 379-390; Sagan 1974; 1977; York 1972), although even Carl Sagan warns against a "chauvinism" that can narrow our thinking on some scientific issues (1973, 41-49).

6 Foucault (1967; 1970, 344-387; 1975; 1980) has looked in some detail at the relationship between these "sciences of man" and "sciences proper" (1970, 345), and the manner in which human sciences have tried to adopt methods of science in order to gain the power or truth that is associated with science (1980, 86, 126-132) - unfortunately he does not adequately explore the extent to which these "certainties" of the "sciences proper" are fallacies, or at least, anthropomorphisms. His description of the hierarchic and exclusive practices of disciplines (1970) and his account of cultural history and its epistemes (1972) and his comments upon science and medicine, however, have their parallells with Feyerabend's views. Although aware of this aspect of Foucault, I have preferred to use Feyerabend's models because of their more exclusive reference to the topics with which I have been concerned.

7 We may note with interest, however, that Marx has little right to remove splinters, as he, too, abuses science in this way (Marx 168, 423-24).

An example (closer to home) of this is given by the circular sent to students wishing to register for Master's degree at Rhodes University. While the Faculties of Science and Pharmacy require that a thesis "must show that the candidate...as [sic] assessed the significance of his findings," this is not a requirement for a thesis delivered in the Faculties of Arts, Commerce, Divinity, Education, Law and Social Science, which are, presumably, insignificant faculties. Furthermore, in the proposal, the candidate is expected to give the goals of his research, which "should either set out the specific question(s) to which the candidate hopes to find an answer, or - in the case of open-ended topics in the humanities - outline the subject to be critically investigated [my emphasis]." The implication is very obvious: in the sciences, there are no open-ended topics (Rhodes University, 1989).

8 The survey into open-schools was dismissed by the South African Conservative Party in September 1990 on the grounds of its being "unscientific."

Part of the success of the `Pygmalion' experiment conducted by Rosenthal and Jacobson (1968) was the `scientific' nature of the initial testing, which was "being conducted at Harvard with the support of the National Science Foundation" (66). Even here, however, the importance of the emphasis on IQ testing as a "scientific" concept was not sufficiently evaluated.

More peripheral to the argument (and at the risk of offending the reader), but nevertheless an indication of the extremities to which some have gone in order to abuse the word science, is the rather ironic attachment of some religious groups to a notion of science. As Paul Davies writes, "No religion that bases its beliefs on [scientifically] demonstrably incorrect assumptions can expect to survive very long" (3). The most well-known is Mary Baker Eddy's Christian Science (1918) which recognised the need for Christians to find the scientific principles of Christianity (Eddy; Leishman; Peabody; Peel).

9 Frye v. United States, 293 F.1013 (D.C. Cir. 1923) (Giannelli) is set as precedent, as is demonstrated by this quote which is taken from a debate surrounding the use of voiceprint material in United States v. Addison, 498 F.2d 741, 745 (D.C. Cir. 1974) (Giannelli, 1209 ft. 82) and also with reference to People v. King, 266 Cal. App. 2d 437, 72 Cal. Rptr. 478 (1968) and People v. Williams, 164 Cal. App. 2d Supp. 858, 331 P.2d 251 (App. Dep't Super. Ct. 1958) (Giannelli, 1209-1210). One juror summed up the attitude of the public with: "You can't argue with science" (Neufeld and Coleman 18). This attitude is still shared by the some of the most most prominent of physicists and philosophers, like Stephen Hawking, who believe (as physicists at the beginning of this century had) that science will soon be able to explain everything, and will, in effect, come to an end (Appleyard; Hawking; Kneale.).

CONCLUSION

1 Nevertheless, the attraction that the word has is as great as the attraction of the mystery surrounding much of religion. This attitude continues today, as writers are elevated because of their insight into scientific concepts (Chapman), an operation that often rivals Johnson's metaphysical conceit by which the "most heterogeneous ideas are yoked by violence together" (23): an author who apparently merits this elevation is Virginia Woolf who receives it in articles such as Miriam Marty Clark's "Consciousness, Stream and Quanta in To The Lighthouse" (1989).

WORKS CITED

WORKS CITED

Note:

Works are listed according to the author and year published.

The date (and the dates listed in the text) are the dates of the publication of the edition used, and not necessarily the original date of publication. Where possible, this original date is supplied in bibliographical information.

Translations are listed chronologically; where an English translation has been used only, it has been listed in English, and some bibliographical information of the translation has been supplied.

When English translations of the text have been unavailable, I have supplied an approximate translation of the title and/or an indication of the main concern of the article in square brackets at the end of the reference.

Sometimes I have used numerous essays from a text by an author (e.g. Barthes, Feyerabend and Jakobson). Because these essays are well known, and have been published separately in many other texts, I have listed each essay individually, with its original date, and have simply mentioned the source by underlined date; its bibliographical information may be found under a separate entry for that text.

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INDEX

INDEX

Although some of the major issues have their own entries, this index concentrates on people, and most often movements or beliefs associated with people are to be found listed with those people (e.g entries for **Marxism** will be found under **Marx, Karl**). It should be used in conjunction with the entries in the contents pages (iii-vii).

Pages references that are inclusive (e.g. 98-203) do not necessarily indicate that the discussion of a topic is followed directly through in these pages, but rather that the topic/person is referred to in those pages inclusively. This has been done (in the interests of space) where a topic/person is referred to on more than two consecutive pages. A continuation between two pages is shown as two separate entries (e.g. 197,198).

Page references in **bold** refer to explanations of the subject in the Glossary.

References to pages after page 186 are in the list of works cited, and refer to works by and concerning authors.

Q

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