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TOWARDS ESTABLISHING A RESEARCH
TRADITION IN ACCOUNTING

A THESIS PRESENTED IN PARTIAL FULFILMENT OF THE
REQUIREMENTS FOR THE DEGREE OF MASTER OF BUSINESS
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"Men have always had to discover by ceaseless trial and error - and will always have to do so - the nature and conditions of their power to act. The contradiction between choice and cause, the clash of the images of creative man and of deterministic nature, is, so to speak, the way we represent to ourselves the endless task of sorting out the forms of action; and the philosopher, by first bringing this clash into the forefronts of our minds, then showing the absurdity of adopting either side, and then dangling before us the possibility of accepting both while denying the possibility of doing so at once, quickens our determination to unravel our causality a little further, and so to act with more effect."

Corbett, Patrick

Ideologies

Hutchinson of London, 1965, pp. 220.

ABSTRACT

Perhaps the most significant event to have affected accounting since the beginning of the 20th century was the stock market crash of 1929. Although the event took place in the U.S.A. and so only directly affected the American accounting profession the subsequent influence of the American profession on the professions in many other countries (England, Australia, Canada etc.) can be traced to this event. The significance of this event lies in the fact that it witnessed the beginning of a new era in the accounting discipline in which the general attitude toward the profession was transformed from one of disinterest to one quite the opposite. Although the attitude fostered was one of extreme disdain, the immediate consequence of which was widespread criticism of accounting practitioners and their practices, accounting was to its advantage never again to rest on its laurels.

Notwithstanding the fact that the profession may have received a disproportionate amount of the blame for the events which took place at that time, the heavy criticism which the profession endured pointed unmistakably to many previously ignored responsibilities and inadequacies. Perhaps the most significant inadequacy was the fact that the profession was at a loss to answer or explain to its critics the rationale for the practices they were then using. Unable to successfully answer its critics and prompted by the threat of legislative sanctions and controls, accountants for the first time began to see the need for a critical examination of the practices which made up their discipline.

Apart from the work of one or two individuals (e.g. Sprague, Paton) this amounted to the first recognition by accountants en masse of the need for research in accounting. This need for introspection was recognised by most professional bodies by the mid 1940's.

The method of research adopted to facilitate this critical investigation centred around the wholesale gathering of data on what accountants were doing. The expected objective of this research was that one would from the collected information be able to establish common ways of doing things which could then, by the authoritative decree of the profession, be established as rules. This method of research continued largely unchallenged up until the 1950's and even today continues to dominate the way accountants conduct research. Concern had been expressed that research was not "organised" but this was not concerned with an organised plan of matters to be investigated or ways in which answers were to be sought but rather the questions of whether research should be an individual or group effort and whether certain issues deserved discussion with others (arguments of grouping).

Since the 1950's however, concern has been expressed among the academic branch of the profession that the sort of research referred to above was not only ad hoc in nature, but hardly even deserving of the term 'research'. Sir Alexander Fitzgerald was one to come to this conclusion after considerable personal endeavour along these lines. He commented "*Research is not merely an exposition of practices currently followed.*"

(Fitzgerald, 1957, 2) Supporting Fitzgerald and also

attacking the notion of authoritative rule-making, Chambers commented "*We [must] question the formulation of rules before adequate analysis of the matters in respect of which they are made*" (Chambers, 1966, 353).

In efforts to provide a solution to the now wide open question - What is the best research method for accounting purposes?, reference has subsequently been made to the physical sciences which are thought, by virtue of the fact that they have the only well-developed research tradition, to have the only research method of any consequence. Evidencing this belief, Chambers, referring to his own particular efforts to outline a methodology for accounting, commented "*...there was no pattern to follow except that of the well-developed sciences*" (Chambers, 1966, 4). Following further investigation of the physical sciences however, what is now apparent is that there appears to be more than one explanation for their success and so more than just one finite method of research than that implied by the phrase 'scientific method'.

In this thesis the intention is to investigate these latest developments with a view to deciding whether any or all of the explanations referred to have some relevance for accounting.

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INTRODUCTION

Development in Accounting

While it has been customary for many accounting writers to imply, by omission to specify otherwise, that developments in accounting have had a uniform and international significance for the whole of the discipline, three points must be made clear about the developments with which this thesis is concerned i.e. developments in accounting theory.

The first point is that these developments belong to but one of three seemingly disparate approaches to the subject of accounting theory. Although the basic goal of each of the approaches remains the same i.e. to search for a foundation upon which to build sound accounting practice, the ideas being discussed under the various approaches have differed markedly. Whereas at one extreme one finds oneself considering real and nominal or stative and transitive accounts¹ at the other extreme one finds oneself considering the significance of external research methodologies in accounting² and somewhere in the middle one finds reference to Generally Accepted Accounting Principles (G.A.A.P.'s), postulates and standards.³ While such variation tends to beg several questions including - Are these complementary or mutually

1. Refer Whyman, P. "In search of a satisfactory accounting theory October 1979, 76-77.

2. Refer Tilley, I. "Accounting as a Scientific Endeavour..." Accounting and Business Research, Autumn, 1972, pp. 287-297.

3. Reference most of the publications by professional accounting bodies.

exclusive approaches to developing an accounting theory?⁴ - suffice to say at this point that there is this variation and that this thesis concerns itself with the second approach i.e. the application of external research methods in accounting.

A second point to be cognizant of, is that developments associated with the above approach are the product of academic research originating primarily in the United States. As such, comments and conclusions are immediately relevant only to research being undertaken in that country. They are however relevant to New Zealand and other countries in as much as developments in these countries normally follow some years behind those in America. Further, it is the contention of the author that serious consideration needs to be given to this approach now, for if it is quashed (which it is in danger of so being), for the want of a little forward thinking by men of insight outside of America, then the chance for early resolution of accounting's foundational problems may effectively be denied it.

The third point of concern, hinted at above, concerns the dual nature of accounting theory developments in general. These have proceeded on two quite separate fronts - practitioner and academic.⁵ Although both groups have sought and currently

4. For a discussion of this problem refer to Aitken, M.J. 'What is Accounting Theory?' (Unpublished).

5. Because the American experience gives the best example of this, the two major professional or academic groups in this country will be cited. These are the American Institute of Certified Public Accountants (A.I.C.P.A.) and the American Accounting Association (A.A.A.), respectively.

maintain that they deal with the fundamental problems affecting the accounting discipline, the approach each has taken has differed markedly. Whereas the practitioner bodies have dealt with accounting by attempting to solve specific problems, the academic body has adopted a 'fire prevention' rather than 'fire fighting' role - they have sought to understand and solve accounting problems using a comprehensive 'back to basics' approach. Although one practitioner body (A.I.C.P.A) attempted at one time (1959) to adopt this sort of approach it had little success. The first works of this new approach were rejected by its then newly constructed Accounting Principles Board (A.P.B.) as being "*...too radically different from present generally accepted accounting principles for acceptance at this time*". (A.P.B., 1962).

Recognising these two quite different concepts of development it is the intention of this thesis to give primary consideration to academic developments.

Developments of Particular Concern

With the above three points in mind, one should not be surprised to learn that under the direction of accounting academia, the study of accounting theory has to some degree shifted away from the consideration of purely accounting matters to the

consideration of matters of a more general nature; away from considering accounting theory per se to considering theory in its most abstract sense as the vehicle by which knowledge is increased.⁶ The implication of this move was that the resolution of accounting problems was not entirely possible at the ground or common-sense level; that solutions necessitated reference to much deeper levels of thought, in the nature of philosophical discussion. In support of this move in accounting and heralding the possible benefits accruing to professional bodies who adopt this approach Chambers commented *"More recently, however, there are signs that ... professional associations sense a commitment not merely to pronouncement but to more rigorous inquiry. Whether as previously this will issue in debate without impact on practice remains to be seen. But the very inquisitive attitude, it may be supposed, will tend to spread an appreciation of the fact the practices are debatable at a deeper level than their surface effects, and will condition attitudes towards a more comprehensive and consistent system of thought"* (1966, 356).

Concern for the establishment of a comprehensive and consistent base in accounting began in earnest around the mid 50's.

Commenting at the very beginning of the period of academic awareness, Chambers noted that *"Even though the subject had been rescued from declining into dogma, it is still difficult*

6. *Because the increases in knowledge were thought to be a direct result of adherence to scientific practices and methods this move necessarily brought about a consideration of the accounting/science interface.*

to trace the elements of logical and methodical inquiry"

(Chambers, 1955, 429) thought to be the essential characteristics of research, vital to the resolution of accounting's problems. Chambers, although not the only theorist to respond to the call to make accounting more research orientated or scientific, was perhaps the most notable. A long debate with A.C. Littleton concerning a suitable structure for an accounting theory and a succession of publications beginning in 1955 with a 'Blueprint for a Theory of Accounting,' and culminating in 1961 with 'Towards a General Theory of Accounting' were indicative of his important contribution to accounting literature of the time. Although it was left to Carl Devine to popularise this topic with an article in the 1960 Accounting Review entitled 'Research Methodology and Accounting Theory Formulation,' the decades of debate which have followed, contemplating a suitable structure for a scientific accounting theory, are in no small way attributable to Chambers' inspiration.

However, although Chambers and many of his adherents believed that this move in favour of scientific methodology was in response to the quest for a more rational plan of action, evidence will subsequently be presented in support of the idea that this move was nothing more than the result of a cultural bias; a bias which mistakenly attributes to scientific methodology the status of a sufficient rather than simply a necessary factor in the advancement of knowledge.

In need of something to re-establish accounting's credibility in the public eye, however, (lost in a myriad of inconsistencies), accounting bodies have since sought to apply accepted scientific methodology in their research programmes and theory construction. Almost without exception, the philosophical base upon which this scientific methodology has rested is that of Logical Positivism. Unfortunately however, having adopted this particular approach when its dominance, in terms of providing a sufficient description of how knowledge progresses, was fast receding, the accounting discipline, with its tendency to follow rather than lead, has once again fallen behind in its development. Unable it seems, to look to the official accounting bodies for any lead in this area, individual theorists have continued with their endeavours. Through reading literature from the philosophy of science and undertaking personal associations with scientific endeavours, as Sterling did by spending some time in a scientific environment, many have begun to notice the inconsistencies in the scientific method of logical positivism and have begun to investigate ways of developing knowledge based on alternative philosophical bases.

However, because of the extremely abstract nature of the philosophy of science, many academics, skilled in traditional accounting methods, seem to have found it more than a little confusing and often frustrating trying to grapple with concepts associated with this field. Indeed, this state-of-affairs could very well be responsible for the 'don't want to know' attitude of many

accountants. Almost certainly it has been one factor which has been responsible for many of the superficial works which have so far dealt with the application of scientific methods in accounting. In all, there are three major problems affecting this association.

Problems affecting the Accounting/Science Interface

First, and perhaps foremost, accounting theorists have failed as a group to come to any consensus about the nature of their own discipline in terms of its goals and objectives. That this is so is witnessed by the large amount of disagreement which surrounds the debate as to whether accounting is a science or not. This debate only arose because theorists adopted different stances as to the nature of their discipline. Commenting about this lack of objectives Henderson and Peirson state that "*The absence of an objective is probably the greatest single problem of accounting*" (Henderson & Peirson, 1975, 3). The essence of this problem lies in the fact that if theorists do not know what it is they are seeking, then they have no firm basis for choosing between competing options. As one would expect, with no agreement about objectives and hence no criteria for rejecting outdated practices in favour of new ones, an almost crippling, or at the very least embarrassing number of inconsistencies have arisen in accounting. A consequence of this indecision is that academics seldom agree among themselves and accordingly face a decidedly difficult task in trying to enlist the support of accounting practitioners, especially when it comes to

rallying support for normative propositions. It was this sort of problem which led Sterling to comment "*We accountants do not resolve issues, we abandon them*" (Sterling, 1975, 28) and further "... *we move from one unresolved issue to another, while the stock of unresolved issues continues to increase*" (Sterling, 1975, 34).

The second major problem, although allied to the first, is sufficiently different and important to warrant separate attention. It concerns the failure of theorists to identify and deal with problems associated with this interface. It has a micro and a macro component. Dealing with the micro component first, there is a tendency of those arguing the pros and cons of the philosophy of science/accounting interface to argue superficially, failing to address themselves to the major problems, and often even when addressing the major problems, failing to recognise it.

At the macro level it can be seen that if this work does nothing else except identify this and the other two problems it will have been a success, for the key to any solutions lies in adequately defining and posing the problem(s). In support of this idea, Peter Drucker, more than twenty years ago, identified, as the most common mistake in management decision making, the tendency to find the right answer rather than the right problem. This message appears to have application to accounting. In a recent re-emphasis of Drucker's message Pike, commenting in an accounting context stated that "*The first step in any decision process is the perception of the problem or opportunity. Unless we start here all our strivings for accurate, relevant, complete and unbiased information may be useless*" (Pike, 1979, 20).

The third major problem faced by those contemplating the application of external research methods in accounting (already touched on) is apparent in the simplistic knowledge which most theorists appear to have of ideas from the philosophy of science. Many have failed to see (either deliberately or through ignorance) that the field of knowledge associated with the philosophy of science is itself in a state of confusion. Forces are at work which are currently undermining the very basis of time honoured beliefs on how knowledge is attained. Consequently, although the acceptance by academics of the dominant paradigm i.e. the scientific method of logical positivism, is understandable, this move was described by Gaffikin as regrettable "...for this dominance was fast receding and having accepted it, accountants have not only to justify their attainment to it, they have had to justify a philosophical point of view which is increasingly unable to withstand critical attacks." (Gaffikin, 1978, 3).

With few exceptions, accounting academics appear to have remained largely unaware of this state of affairs, and have continued through to the seventies with this association, some twenty years after this type of analysis began to be the subject of critical attack in the domain of the philosophy of science (Suppe, 1973, pp 4-5). To reiterate an earlier point, failure to take account of this factor has meant that much of the work completed since the beginning of the 70's has been little short of a waste of time.

With these three factors well in mind it should be evident that one of the major purposes of this work is perspective-

gathering in the form of problem identification. However, because the three problems mentioned could all equally well serve as research topics in themselves, of necessity some order of priority has to be adopted.

The second (next) section justifies choice of the third problem as the major focus of attention of this research. It briefly traces, using a metaphorical analysis, a series of alternative strategies open to accounting and shows the third research problem (the use of external research methods in accounting) to be at the centre of the approach offering the most potential and therefore most in need of resolution.

The third section endeavours to provide a preliminary understanding of the natures of both science and accounting.⁷ Although to some extent the author is loath to do this, for fear of falling into the trap of becoming preoccupied with a distinction between the two - a preoccupation, which it will be shown, has led to much unnecessary debate in the past - for the sake of perspective, some consideration is deemed necessary. This consideration however, is more in the nature of a review of this debate and aims to show, as simply as possible, that it is an entirely arbitrary decision as to how one views the natures of science and accounting and so their comparability.

7. *Because of the extended treatment accorded science in section four, this section will concentrate primarily on the nature of accounting.*

Consequently it will be shown that the implicit view of this thesis, that they are comparable, is logically indisputable. The only sense in which the two will appear to be at odds will be shown to be attributable to a cultural bias on the part of most people in the West who have been willing to believe that psychological and sociological explanation have little relevance in understanding the scientific theory construction process. That this sort of analysis has a part to play in both science and accounting will point us in the direction of particular theses in the philosophy of science which consider the traditional view of science (based around purely logical reasoning) a necessary but certainly not a sufficient condition for understanding how scientific knowledge progresses.

Section four will constitute the main part of the literature review. In this section, an endeavour will be made to present as clearly as possible a selection of ideas from the philosophy of science. The intention is not to provide extensive detail but simply to enable one to gauge the general direction in which the philosophy of science has been moving since its beginnings in the late nineteenth century. Four theses will be discussed with a view to establishing where accounting does or does not fit into the scheme of things. To facilitate some organisation of the various ideas, the section is divided into two parts. The first part will describe and analyse theses holding the objectivist view

of science, the view that science is rational and progressive, and the second, the subjective view of science, the view that science is neither objective nor necessarily progressive.

Section five will then detail the research method adopted and a directional hypothesis. It should perhaps be noted here, however, that this study deviates from what might be considered the norm insofar as it does not rely on an empirically based method of research. It seeks in fact to establish the possibility that there may be other research methods available which are perhaps better suited to the task of research in that they allow for a deeper meaning and understanding than that provided by empirically based methods. Implicitly espousing the possibility for such research and the ineffectiveness of empirical information in comparison, Rappaport, a noted anthropologist commented "*If meaning lies in the perception of deep similarities underlying apparent differences, then paradoxically, meaning is diminished as empirical information is increased.*" (Rappaport, 1976, 72).

Section six will then consider the extent to which accounting has become aware of, and is using the ideas associated with these theses. The works of several individuals and groups will be discussed with a view to establishing which of the theses in section four has received most attention in accounting theory construction literature to date. The section will be divided into two. In the first part a number of theorists and their works will be considered. However, emphasis will be primarily devoted to the exposition of the works of two theorists, Chambers

and Sterling. Chambers work was chosen because it was one of the very first in the field, appearing as it did in the mid fifties, and the work of the latter, because it is thought to be "...fairly representative of the notions of theory construction (held by) accountants" (Gaffikin, 1979, 10). In the second part, reference will be made to works which follow a different path than the majority. Although several of these will be mentioned, three will be taken as indicative of current trends and discussed in some detail. In the third section, some discussion of the fact that academics are ignoring their own implications for further epistemological study in accounting will be demonstrated. In addition, some implications of current academic developments to practitioner conceptualising will be drawn.

The seventh and final section, the conclusion, will bring together sections three, four and six. This will be done with a view to highlighting points of interest but more especially with the intention of inspiring and providing direction for future research.

PROBLEM STATEMENT

Nature of the Problem

When one considers building a structure of some sort, of necessity one must attribute primary importance to the sorts of foundations which one lays, for these will dictate the extent to which the structure can stand up to the stresses of the external environment. Although it is true that one lays foundations having given consideration to factors in the physical environment far and above that which is considered normal, often it is found that initial considerations were not adequate leading to catastrophic outcomes; e.g. the collapse of the structure due to abnormal, unaccounted for weather conditions or the knowledge that no more space is available because the foundations will not support any further highrise development. Such a situation is now apparent in accounting. The makeshift foundations are now beginning to give way because of the unforeseen extent to which the accounting discipline has and is still growing. There is now imminent danger that the shaky structure in which accountants are working will fall down around their ears.

The Alternatives

Faced with this situation accountants have three options available. To continue with the metaphor, first, they can opt to build a completely new building, this time making doubly sure of the foundations. Secondly, they can opt to remain in the old building and have a new interconnecting one built alongside the old or thirdly, they can opt to simply make efforts to strengthen the existing building.

The Most Acceptable Alternative

Because of the inconvenience involved in shifting between buildings, accountants, especially practitioners, have been reluctant to shift from their traditional surroundings. They have opted instead to have any interested architects (theorists) called in to attempt to shore up the structure. As a result there has been a series of attempts to prop up the accounting discipline. These attempts fall into the third approach to accounting theory referred to earlier.⁸ Under the guise of this approach, accounting bodies in the

8. *This approach revolved around the development of generally accepted accounting principles, postulates and standards.*

United States, Britain, Australia and New Zealand have set about determining a theoretical structure for the discipline based on the development of authoritative accounting standards. However, as Gaffikin put it, "*As an academic endeavour, accounting on this basis would involve little more than rote learning of mechanical skills and be devoid of any intellectual rigour*" (Gaffikin, 1978b, 9) and further "*In setting guidelines for practitioners they (the authoritative statements) are little more than the result of political ploys to placate the majority to the public*" (Gaffikin, 1978b, 8). Indicative of the fact that authoritative statements can never resolve issues in the long-run and the fact that they can never form the basis of a secure foundation, one need only look to the continual revision of such statements which has taken place since this task began. Commenting on the futility of this approach a recent American Accounting Association committee stated as one of its four major conclusions about accounting that "*... theory closure cannot be dictated*" (A.A.A. 1977, 49). Accordingly, attempts to shore up the existing structure have proved valueless because the materials used for the foundations do not constitute a solid enough base - they allow a dangerous rather than acceptable degree of movement.

A Second Best Alternative

In the face of these problems an increasing amount of support has been offered in favour of the second option

i.e. building an interconnecting structure alongside the present one. However, although a considerable amount of work has been done, in the form of having architects draw up possible plans (inflation accounting methods), this option is faced with two problems. First, there is something inherently contradictory, at the very least unnecessary, about having two sets of foundations for a discipline. That these alternative foundations must in some way be contradictory is easily discernable by the fact that if they were not they would be subject to the very same criticisms as above. In the event of them being the same however, a second problem becomes apparent, and that is the unnecessary waste of resources involved in maintaining two structures. The obvious parallel to this in accounting lies in the fact that currently there are suggestions that accountants should prepare, on behalf of their clients, two or even three sets of accounts. However, the more than obvious criticism of this (in line with the second problem mentioned above) is that clients are simply not willing to pay a multiple of their normal fee when according to present law one method of accounting will suffice.

The Most Promising Alternative

With the ever increasing recognition that both the above options are little more than stop-gap measures, an increasing amount of attention is being devoted to the first option. This option contemplates the complete replacement of the old

structure and the construction of a new one, better able to provide for a steady and ordered growth because of the more secure foundations upon which it is built. Making one last use of our metaphorical comparison it must be made clear that the only way of ensuring that the foundations of this new structure will cope with the demands placed upon it, is through research; not only of estimated stress requirements but of the foundation components themselves. In this sense there is little use in using more and more of a mix e.g. concrete, when we are worried about earthquakes. What we need is an entirely new foundations design, an example of which is the use of rubber shock-absorbers in the foundations of large structures subject to earth movements.

Accounting is in need of just such a foundation redesign. Accordingly by implication it needs to step up its research commitment and re-orientate its thinking so as to regard itself not as an isolated, peculiar brand of knowledge but as part of the development of knowledge in general. As one author commented "*The idea of accounting does not exist in a vacuum, it rests within the fold of a cultural and philosophical framework.*" (Glautier, 1973, 442). Contingent upon this idea it must seek to have a thorough understanding of how knowledge, in particular, scientific knowledge progresses. Such is the research task of this thesis; to attempt to come to grips with the problems of knowledge (theory development) in accounting by investigating the development of scientific knowledge. That it is necessary to understand the growth of scientific knowledge in order to facilitate the growth in

accounting knowledge is implied by Karl Popper, a renowned philosopher of science who argues that knowledge can be approached from two sides: first as the problem of common-sense knowledge or secondly as the problem of scientific knowledge (1959, 18), but that *"the most important way in which common sense knowledge grows is, precisely, by turning into scientific knowledge"* (Popper, 1959, 19).

The Practicality of the Chosen Alternative

While few will deny the intuitive appeal of this exercise, many will question its practicality, for in terms of solving the specific problems of accounting it makes little progress. In fact, if anything it does the complete opposite, in that it exposes accountants to yet another Pandora's box. Explaining the basis of this predicament Gaffikin commented *"In adopting a scientific approach for constructing theories they (accountants) have relied on what appeared to be the majority of opinion of philosophers of science in respect of theory construction. This subject however has been the most important problem of the philosophy of science"* (Gaffikin, 1978b, 9). Establishing the importance of this problem to philosophers, Suppe commented that *"If any problem in the philosophy of science can justifiably be claimed the most central or important, it is that of the nature and structure of scientific theories"* (Suppe, 1972, 3). Thus our intuitively appealing approach which at first seemed quite straight forward encounters a major obstacle, one which to date few have recognised, let

alone attempted to tackle.

Tackling this obstacle will be the major research problem of this work. More specifically the task is to investigate some of the lesser known theses from the philosophy of science as alternatives to logical positivism, the theory construction view which is now losing favour (see Suppe, 1973, 4-5), despite the fact that accounting has just begun to adopt it (see Sterling, 1976). In terms of the series of three problems referred to in the introduction, it only represents one of them, i.e. the third. However, in as much as we are considering the very basis of knowledge and research, answers to problems one and two will be aided by discussing the third. An additional, more practical reason for making this problem the focus of attention stems from the fact that of all three, this has been given least attention.

The intention in discussing ideas from the philosophy of science is not to give a complete historical profile, but rather to isolate the mainstream ideas. This is necessary both because of the limited scope of this work and because the mainstream ideas are considered too important to risk burying them in a voluminous list of interesting but, for the purpose of this study, insignificant details.

A PERSPECTIVE OF ACCOUNTING AND SCIENCE

Difficulties with the Accounting/Science Relationship

If one were to ask a group of persons having no substantive knowledge of the disciplines of chemistry and accounting, to make some verbal observations concerning the nature and scope of each of these disciplines, it seems likely that the individuals in this group would be more willing to presume something about accounting than they would chemistry. Several explanations may be advanced to explain this occurrence, but perhaps the most significant is the fact that people perceive accounting to be a far less specialised, more public, undertaking than they do chemistry. Whereas chemistry deals with complex properties which have little meaning to most people, accounting purports to bring order into everyday economic affairs, something which continues to remain dear to the hearts of most people. Because of the importance with which economic affairs are held in all countries of the world, there should be little reason for doubt or surprise that the accounting discipline and its practices and procedures are the subject of a considerable amount of scrutiny, often leading to criticism. Arguably the most recent and significant of these criticisms is the inability of the accounting discipline to come to some agreement about an acceptable, formalised approach to account for change i.e. a formal approach to identification,

measurement and communication of economic change.⁹

Referring back to the association previously made between chemistry and accounting, if one were to ask the same detached group of people another question regarding which was the more advanced of the disciplines, most would probably pick chemistry; not because they have any rational criteria for making such a decision, but simply because traditional sciences such as chemistry have risen in stature¹⁰ because of the formalised procedure (loosely labelled the scientific method) which they have adopted to place change in perspective. Although accounting, or rather the technical side of accounting, i.e. bookkeeping, has made elementary attempts since the thirteenth century to formalise practice and procedure, these attempts while having some success for bookkeeping, (because of its procedural orientation), have been totally inadequate and fraught with difficulties for the accounting discipline as a whole. These difficulties are best understood by considering something of the accepted (though not necessarily correct) nature of accounting. For this purpose, this work uses as a base a work by Tricker¹¹ which introduces three theses adequate

9. This would coincide with a relatively well accepted definition as the "... process of identifying, measuring and communicating economic information to permit informed judgements and decisions by users of the information" (American Accounting Association, 1966, 1).

10. This stature or prestige has, I believe, been responsible in a direct sense for the slow rate of progress in accounting and business thought and practice in general. In enrolments at Massey University it has been unquestionably the higher scoring entering students who have opted for science, and the rather lesser qualified, (comparatively speaking) who have been attracted to business. It has only been over the last two or three years, through recognition of career limitations in science, that things have changed. Perhaps all goes well for our business/accounting achievements a generation from now.

11 Refer 'Research in Accounting' R.I. Tricker (1978) Arthur Young Lecture. University of Glasgow Press.

for highlighting some important characteristics of accounting.

Contrasting Growth Patterns

The first of these characteristics is, "... that accounting has developed in response to changing needs in the field, under pressure from external stimuli, and not on the basis of erudite theory or the application of research findings" (1978, 6).

(emphasis added).

Not surprisingly this thesis brings accounting into immediate contrast with traditional scientific disciplines whose research traditions and theoretical foundations are thought, by virtue of the fact they they are labelled scientific, to be well advanced. Additionally, unlike science, accounting is seen to be experiencing change not according to any established procedure but simply according to the dictates of various influential groups. As a result, such change is quite often found to be inconsistent with the rights and needs of many minority users and indeed sometimes, financial report users as a whole. Chambers described the situation with the following words. "*The diffuse nature of interests of members of society at large is ineffective against the immediate coercion of circumstances; and whereas accountants, internal and external to firms, may have assumed a professional role, the immediate pressure of ownership and management groups have tended to make the accounting process subservient to those groups, and subservient in an inconsistent manner as the temporary exigencies of those groups have changed*

from time to time" (Chambers, 1966, 359).

Accordingly, although the following point should not be used to excuse the accountant from his obligations, it should nevertheless be kept in mind; that accounting problems are not of the accountant's making. This follows from the fact that the accountant is subservient to corporate goals and objectives, which themselves are subject to societal directives. This contrasts quite sharply with the scientist whose problems are very much of his own making once he decides to investigate some quirk of nature. He is not subject to anywhere near the same amount of direction as is the accountant. As compared to the scientist who may be said to follow his nose while conforming to a very loosely defined set of guidelines, the accountant is totally constrained by user needs and their consequent demands which are often explicit, uncompromising and inconsistent at the same time. This can be seen to contrast sharply with a particular scientific ideal which seeks to substitute individual direction biases for a disciplined and unbiased pattern of instructions, which although allowing a certain amount of freedom ensures the overall objectivity of the results.¹²

Stability of Growth Patterns

The second thesis stems in a large part from the first, but remains sufficiently different to warrant separate

12. It should perhaps be mentioned though that what constitutes disciplined and unbiased development appears ironically to be a matter itself, of individual evaluation, a point which does tend to temper the above scientific ideal and accordingly reduce the perceived gap between accounting & science.

attention. It follows from the necessity of the accountant to adapt to change. Commenting on the accountant's general inability to cope with change, Tricker states, *"Proposed changes in the basis of accounting measurement have been so profligate in the past two or three years that the jocular tag "flavour of the month accounting" does not seem out of place"* (Tricker, 1978, 7).

As evidence of this, one need only glance at the number of alternatives put forward to account for inflation. Following from what was said earlier, regarding the instigators of change, it is also significant to note that the reaction to it, and change itself are often extensive and carried out at a considerable rate.¹³ Given no other complicating factor, these two are sufficient to cause considerable anxiety in any discipline. If however, we add the devastating admission that accounting encounters change with no unified evaluative criteria, in the form of goals and objectives¹⁴ for accepting new and rejecting old ideas, then one may begin to gauge the dilemma accounting faces. Indeed, this is the situation which led Sterling to comment, *"We accountants do not resolve issues, we abandon them. I do not mean to imply that we ignore issues. Quite the contrary, we debate them long and loud. However, the debate, instead of coming to resolution, continues until another issue comes along that is more current and controversial and then we forget the former issue ... We move from unresolved issue to unresolved issue"*. (Sterling, 1975, 28).

13. See Tricker (1978, 7).

14. See Most (1977, Chapt. 6) for a summary of attempts to date.

Looking briefly at the history of this proliferation, reference is made to the early years of the twentieth century when following on from severe post-war inflation and speculation in the 20's, came the stock market crash of 1929. Following this economic disaster there was much soul searching on the part of businessmen and accountants alike. The notion of a dynamic accounting which could keep pace with economic affairs was consequently sacrificed and replaced by the cost doctrine which was subsequently and quite significantly embodied within the law. This was an unfortunate occurrence in accounting, for not only did it signal capitulation in the face of adversity, but also the beginning of proliferation. Clear examples of this complete capitulation and proliferation are available in the large numbers of modifications which have been made to the cost doctrine since its enshrining. However, despite its obvious inability to cope with the growing complexities of business, people have not been willing to let it go.

Substantiation of these two ideas is once again provided by Chambers who commented in relation to modification that *"There can be little doubt that the pressure brought to bear to introduce L.I.F.O., methods of accelerated depreciation, and initial investment allowances, from time to time and place to place, have all stemmed from the basic deficiencies of the cost doctrine"* (Chambers, 1966, 354) and with regard

to the premature, regulatory alleviation of the historic cost doctrine "We do not protest the value of regulation to secure uniformity of a practice in a field of wide public practice. We do question the formulation of rules before adequate analysis of the matters in respect of which they are made" (Chambers, 1966, 352). This led Chambers to conclude that "The cost doctrine in fact, disregards one of the most important features of an adaptive society, and condemns an accounting based on it to being a sterile half-history" (Chambers, 1966, 353).

Adding to this problem was the second period of rampant inflation in the 40's. While countries in Europe and South America acted to sanction alternatives, the response by England and the United States was one of restraint. Caution was adopted in considering alternatives to practices which had been the subject of so much recent rationalisation.¹⁵ In any case, following the thinking at that time, which unfortunately is still with us, it was considered better or more prudent to stay with something they knew than support something they did not.

Unfortunately, as a result of this, professional bodies have felt the need because of the increasing inadequacy of the method, to make extensive statements specifying rules and procedures.¹⁶ Combine this with a previous statement, that they have been doing so with no clear statement about the accounting function by which they might substitute old practices for new, and we have as a consequence "... a vast proliferation of alternative possible rules, having demonstrably contradictory effects on the determination

15. Chambers (1966, Chapt. 14)

16. For evidence of this refer to Accounting Trends and Techniques, published annually by the American Institute of Certified Public Accountants (A.I.C.P.A.)

of income and financial position" (Chambers, 1966, 355).

Chambers commented further that although "... *all have some common sense, practical justification ... they are not regarded as the occasion for jettisoning less acceptable rules, consequently the whole body of rules lacks the disciplined and orderly quality which characterises systematic development*" (1966, 355).

Extent of Cultural Influence

The third and final thesis which Tricker expounds which gives further insight into the difficulties which accounting faces in attempting to adopt formalised procedure is "... *that accounting is dependent on the ideology and mores of society. It is not value free. It is a child of its culture. Any claim to report objective facts has to be set into the context of the beliefs of that particular culture*" (1978, 8).

Although, broadly speaking, this is so in every discipline, including science, it is contended that the extent of this influence is much more apparent in accounting. This follows from an earlier argument which indicated the importance of the subject matter of accounting to the public at large. Stemming from this, there is consequently a greater tendency in accounting to disassociate oneself from earlier ideas and put forward one's own. Nowhere has this been more obvious than where accounting academics have been concerned. Their works have seldom taken any aspect established from previous debate as given. This was particularly so during the period 1955-1970,

which is regarded as the normative theory period.¹⁷ There are three reasons for this. First, because of differences in the size, structure, technology, environment and management of the organisation for which we are accounting. Secondly, because of the tendency to identify and solve quite different problems, to the extent that what is a problem for a creditor need not be one for an owner, and thirdly, perhaps more fundamentally, because of the vast differences in the 'values, ideology and culture' of the problem presenters and the problem solvers. Tricker provides a good example of this third thesis, indicating precisely how the orientation towards formalised procedure is proving difficult for accounting when he stated "*In Britain the broad thrust in accounting has been to improve the methodology and practice - providing clear answers to the question - 'What are the costs and the profits of this or that enterprise?' It has not been necessary until now to pose the more difficult question, 'What is cost, what is profit?' The answers to such questions are culturally dependent*" (Tricker, 1978, 8). As direct evidence of the growing importance or impedimental power of cultural influences in the creation of formalised procedure, one need look no further than the current attempts by accounting bodies, world-wide, to set up an international accounting body to foster international comparability in financial reporting.¹⁸

17. Henderson and Peirson (1977, Chpt 3) divide accounting theory development into five distinct periods; pre 1494, the development period: 1494-1800, the pre-theory period: 1800-1955, the general explanatory theory period: 1955-1970, the general normative theory period: 1970- present, the specific empirical theory period.

18. For details of the various International bodies, their sub-committees, their organisation, objectives, power of enforcement extent of influence and problems see Mueller G.G. 'International Accounting Standards and Problems The Accountant, Vol. 177, Oct 1977, 446-449.

A Tempering of Negative Arguments

Although the picture just painted portrays a very bleak future for accounting in so far as adopting any degree of formalisation is concerned, in fact the situation is over dramatised and over simplified. The reason for this treatment was to present, without qualification, the arguments put forward to counter the notion that accounting could adopt a formalised methodology along the lines of that adopted by a scientific discipline. Those favouring the opposing viewpoint will doubtless have already thought of answers to counter the implications following from the three theses.

In countering the first thesis the obvious solution is to shift the focus of attention away from the various pressure groups and concentrate on the entity. One of the few to do this has been Chambers. Commenting in the epilogue of his 1966 book, Chambers states *"The effects of the arguments of this book is to shift the focus of attention from the parties of interest (creditors, investors, managers) to the entity under consideration ... The shift does not ignore the participants; (instead) it recognises them all"* (Chambers, 1966, 375).

Taking this a little further, if one were to take a depersonalised or operational view of accounting, whereby 'accounting' signifies certain activities carried out by people, then as

Goldberg, another prominent accounting theorist, stated "... These activities can be observed, classified and otherwise examined. That is they can be subjected to scientific treatment" (Goldberg, 1977, 31). That this is possible in accounting is evident from a well received technically based definition of accounting as the "... process of identification, measurement and communication of economic information ..." (A.A.A., 1966, 1). One should be cautioned though, that this does not make the activities themselves scientific, nor does it mean that accounting is a science "But it does mean that the writing and thinking about accounting can be scientific, if it is aimed at arriving at generalisations relating to those activities and eliciting new knowledge about them" (Goldberg, 1977, 31).

Countering the second thesis necessarily brings us in contact with another eminent accountant, Professor Robert Sterling. Following from his earlier comment, which revealed how in the past, accountants have failed to resolve any issue satisfactorily, Sterling stated that the reason for this was the way in which we defined our problems. He commented "Most accounting issues are not 'researchable' in the scientific sense of subjecting them to tests of empirical and theoretical import. Instead accounting issues are defined as matters of taste as in art and the 'research' consists of arguments whether one person's tastes are better than another's" (Sterling, 1975, 34). It would be Sterling's contention that countering Tricker's second thesis demands redefinition of many of

accounting's problems and objectives and that such a move, although not guaranteed of success, had the potential to make accounting's problems simply difficult rather than unsolvable.

While in the comment above, Sterling seems to assume the applicability of scientific investigation to accounting, there has been considerable speculation among accountants (Tilley, 1972) about whether in fact a scientific approach is valid. Although we saw above how this was possible, one must be careful about the depersonalisation or mechanisation of a discipline which has such a vital interaction with people. Business phenomena can not be viewed from a totally mechanistic outlook. Agreeing with this basic sentiment Tricker commented "*The ever changing flux conceals an organic, not mechanistic system*" (Tricker, 1978, 15). Another problem which one must be wary of, is the tendency to become engrossed in the methodology itself to the detriment of the results achieved from applying the methodology. This is quite common in a discipline foundering in new ideas, given the fact that academics must play the 'publish or perish' game.

Contingent upon some of the arguments raised above, attempting to counter the third thesis is a rather more difficult task. This is so because unlike the first two, where one assimilated science and accounting by changing ones view or approach to accounting, countering the third thesis involves changing ones conception of science. It involves showing that socio-psychological, historical and geographical factors

play just as important a role in science as they do in accounting although this is by no means an accepted position. Arguing that science is indeed influenced by these factors are Williams (1975), Kuhn (1962) and Feyerabend (1975) to name but three of the more well-known advocates.

Summary

In bringing this section to an end, it should by now be apparent from arguments put forward above that there are no absolutely watertight arguments for or against the notion that accounting is a science. Arguments typically end up being decided as matters of personal preference based inevitably around differences of opinion, and simplistic views of particular concepts associated with science. Commenting with regard to the latter Laudan, a noted philosopher of science, stated *"What has stood in the way of a recognition of the cognitive parity of sciences and non-sciences has been a simplistic identification of (scientific) rationality, with experimental control and quantitative precision. Because 'humanistic' theories usually lack both it has been easy for some thinkers to dismiss their rational credentials. But the essence of rationality of science does not depend on such characteristics"* (Laudan, 1977, 191).

In spite of this line of argument however, few would be willing to admit that there is not something about science which makes it stand apart from accounting. The question that must be asked is whether this difference is of critical importance.

Laudan would appear to answer in the negative. He comments *"If there is any truth at all in the (positivist) claim about the differences between sciences and non-sciences, and I suspect there is some truth to it, it will be found not in the exclusive exhibitions of progress by science, but rather the higher rate of progress exhibited by them"* (Laudan, 1977, 192).

Expressing a similar point of view in the specific context of accounting Boulding has stated *"It is unnecessary to label fields of study as more or less "scientific" and absurd to judge them by this label. Fields of Study are more or less mathematical, more or less empirical, more or less subjective, and...each field must find the character suited to its own. The important question is not whether any field is "scientific", but whether its activities contribute usefully to the improvement of knowledge and whether these activities might themselves be improved in any ways"* (Boulding - from Green, 1966, 57).

If we apply these sorts of arguments to accounting then it seems that we are validly able to contemplate the use of any thinking technique (including scientific thinking techniques) in accounting, since accounting, no less than any other discipline, is interested in progress. Whether, in fact, the use of scientific techniques will issue in debate without having any practical effect on accounting should be of little concern. In the progression of knowledge all avenues must be explored to ascertain their usefulness. To presuppose that certain techniques are valueless without at least contemplating them, or without fully investigating them, as appears to be the case with the application of scientific research methods in

accounting (see, for example, Peasnell, 1978) is a far worse mistake than to investigate their potential and find them to be useless.

The implications of all this for accounting is that while the application of scientific research methods offers many potential benefits and pitfalls for accounting, accountants interested in this application are certainly in no position at this point to recognise and benefit from this knowledge because their knowledge of science is far too elementary.

In the next section an attempt will be made to rectify this state of affairs and so enable some meaningful evaluation of the uses of scientific research methods in accounting (a subject of concern for a subsequent section). In the course of this rectification the arguments for and against four of the best known theses from the philosophy of science will be put forward. Although the treatment will necessitate some degree of complexity in terms of considering hitherto unheard of ideas and be compounded even further by the length of the section, it is hoped that the reader will bear with it, for the sake of the subsequent section in which the uses (past, present and future) of such ideas in accounting will be established.

TWO VIEWS OF SCIENCE

Objectivist Views of Science

Establishing the Link Between Philosophy and the Scientific Method

Before proceeding with an analysis of the mainstream ideas from the philosophy of science, it is as well that we consider the link between philosophy and what has been commonly referred to as the "Scientific Method". To begin with, it is important to realise that the term "method" is a misnomer. This is so because it mistakenly implies a set of formal, procedurally orientated, research steps. As Buckley et al state "*Such is not the case*" (Buckley, et al, 1976, 29). Continuing in explanation and also establishing the link between the so-called scientific method and philosophy, Buckley et al remark "*Rather the scientific method refers to an attitude and philosophy which guide the researcher in his efforts to abstract essential elements from the overwhelming phenomena and sensations in the environment, and encourages him to see and examine the relationships among these elements*" (Buckley, et al, 1976, 29). (Emphasis added).

Following Buckley's et al statement, one can see that the scientific method may differ considerably depending on which philosophical base one adopts. This is something which few contemplating the accounting/science interface have realised. Most have generally accepted the traditional philosophical base (to be described shortly) in apparent ignorance of any alternative and have thus been understandably but regrettably narrow in their outlook. In condemnation of this ignorance it must now be proffered

that there are several other philosophical bases (the three most important of which will also be discussed in this section) which not only offer better explanations of scientific progress but also throw new light on the accounting/science interface. Using ideas from these alternative philosophical bases, some are now able to establish important similarities between science and accounting, similarities which have intuitive appeal even to those who previously rejected any such comparison.

As previously indicated, four philosophical explanations will be discussed. These are the philosophies of the Vienna Circle (commonly known as the Received View, and referred to earlier as the traditional analysis), Karl Popper, Thomas Kuhn and Paul Feyerabend. It will be shown that, although concentration on the earlier two, particularly the first, has been understandable to date given cultural biases and the relatively short time which the latter two have been in existence, the latter two cannot now be disregarded - any attempt to do so condemns a philosophy of science not encompassing them to being a sterile half-history.

To date, this appears to have been the unfortunate state-of-affairs. Such has been the concentration on the first of the above philosophies that in modified form it has become known as the 'Received View', a term perhaps more meaningfully interpreted as 'the accepted view of how scientific theories are constructed'. Evidencing the dominance of this view of scientific development Suppe commented "*It is little exaggeration to say that virtually every significant result obtained in the philosophy of science between the 1920's and 1950*

either employed or tacitly assumed The Received View" (Suppe, 1974, 3). The following paragraphs describe and analyse this view.

The Received View

Although in an earlier paragraph development of the Received View was attributed to a philosophical group in Vienna, this attribution was undertaken more for the sake of identifying the ideas with a particular personality or group as is evident in the other three philosophies, rather than having any special significance. In fact of all four it is the least able to be associated with any one person or group. Modified versions of this philosophy have been appearing regularly for the better part of one hundred years and are continually being forwarded by different individuals and groups to cater for new inconsistencies.

The Received View arises out of, or rather, is a product of, logical positivism and though distinct in its own right, cannot be divorced from the tenets of that movement. In fact, according to Suppe the Received View is simply logical positivism accepted over a restricted range of empirical knowledge, that is, scientific knowledge. Consequently an understanding of the Received View necessitates some knowledge and understanding of logical positivism.

Logical positivism is the name given to a philosophical movement which originated in Germany in the latter part of the nineteenth century. It originated in response to the inadequacies of

three main schools of scientific philosophy. These were mechanistic materialism ("*science presents a picture of the world firmly based on empirical enquiry*", (Suppe, 1974, 8) in which observation is immediate and there is no call for a priori or conceptual mediation), neo-Kantianism (science is the search for the general structure of sensations - scientific laws - by which we comprehend phenomena of an ideal world "*Every scientific theory contains a priori element ... as far as its fundamental principles are concerned,*" (Suppe, 1974, 9) and Machian neo-positivism ("*science is no more than a conceptual reflection upon facts whose elements are contents of a consciousness given to us by sensation*", Suppe, 1974, 9. There is no part for an a priori element).

At the turn of the century all three held dominant positions as philosophical explanations. This however, was not to last long. With the publication of Einstein's special theory of relativity in 1905, a philosophical crisis began to take place. None of the three German schools could reconcile the new physics with their philosophies. In fact, only one made the attempt - Machian neo-positivism - and only then accomplished its task by considerable modification of its original thesis (Suppe, 1974).

Such was the growing acceptance of the new physics that such modification became inevitable. Although there were attempts to modify another of the three theses - neo-Kantianism - the modification of Machian neo-positivism, which essentially comprised a tempering of its philosophical position, became the most acceptable. This approach was adopted for serious

consideration by two well-known schools of philosophers - the Berlin school and the Vienna school. *"Both groups agreed that Mach was correct in insisting on verifiability as a criterion of meaningfulness, but concluded he was mistaken for not allowing a place for mathematics"* (Suppe, 1974, 11).

One further development which sprung from the Vienna Circle led to what is generally considered to be the original version of the Received View proper (hence the association between the Vienna Circle and the Received View). This concerned the introduction of mathematical logic. The success with which mathematics was beginning to be axiomatised in logic led those of the Vienna Circle to conclude that mathematical statements of scientific laws and definitions of theoretical terms could also be so axiomatised. Thus in its original version the Received View held that *"A scientific theory is to be axiomatised in mathematical logic (first order predicate calculus with equality). The terms of the logical axiomatisation are to be divided into three sorts:-*

1. *Logical and mathematical terms;*
2. *theoretical terms, and;*
3. *observational terms which are given a phenomenal or observational interpretation.*

... the axiomatisations must include various explicit definitions for the theoretical terms of the form $Tx = Ox$ where 'T' is a theoretical term and 'O' is an observation. Such explicit

definitions are called correspondence rules ..." (Suppe, 1974, 12).

Expressed another way, physical phenomena are given conceptual significance by way of a set of rules e.g. rules of measurement, and are manipulated in accordance with the rules of mathematical logic so as to develop scientific laws (laws relating theoretical terms) which constitute general statements about phenomena which bring a certain amount of order to our world and thus aid present and future understanding of it.

Since this original version there has been a host of alternatives and although Suppe (1974, 50-53) talks of a final version, it is doubtful whether in fact the one he gives is the final one or simply a convenient stopping-off point. Reasons for suspecting that this was not the final version will be discussed shortly when considering some of the major responses to aspects of the scientific enterprise (e.g. explanation, truth) which have been erected upon the Received View. Among these various responses will be shown to be one which simply aims to find new evidence in the nature of a modification to rescue the Received View from criticism.

A Re-emphasis of Purpose

Before proceeding with this task however a re-emphasis of the purpose of this discussion is considered necessary. Accounting theorists have lately begun to become more aware of the

positivist philosophy and accordingly orientate accounting development with it in mind. Some would argue that this awareness has been latent within these theorists for some time owing to a general cultural bias - that developments have been proceeding along these lines for some time and that the only change is that theorists are now beginning to be conscious of it. Regardless, however, the point to be made is that if accountants have adopted a philosophical base which is untenable, then in terms of achieving objectives, solving problems and generally making progress towards better understanding the development of accounting knowledge such tasks are in vain. With this in mind, and with something of the development of the positivist creed in mind, reference is now made to some of the major aspects of this view.

Major Aspects of the 'Received View'

Already evident from their association with logical positivism, is the fact that the Received View theorists entertained a radically empiricist view of science. Following from this view was their belief that nothing was meaningful, and hence, scientific, unless it could be described in observational terms.

A second point to note is that their view of science was extremely simplistic in that they saw no problems with their idea that observational evidence could be theory-neutral, that is it could remain free of observer bias.

Another important feature of their epistemology was, what Lakatos referred to as, their justificationist thesis - a thesis which espoused that knowledge meant proven knowledge. Evidence of the positivist belief in the justificationist approach is seen in their attempts to build some foundations for scientific knowledge. They sought at once to banish metaphysics from science and develop positive arguments in support of the view that science provided the only path to certain knowledge. This latter point gave rise to what has possibly become the most debated issue, not only in the philosophy of science but in every discipline seeking the benefits of scientific development, namely, how to distinguish between sciences and non-sciences. It was this debate which led to the problem-bound Principle of Verifiability (sometimes referred to as a 'demarcation criterion'). Although like most philosophical notions there were many versions of this principle "... common to them all was the claim that a genuine statement must be capable of conclusive verification" (Schlick, cited in Williams, 1975, 21).

It is this concentration on verification which in turn points to another well recognised feature of the positivist dogma, namely, the reasoning process of induction. Statements were considered to be verifiable or provable in two ways. First, they could be deduced from the laws of logic, or secondly, they could be empirically demonstrated. Explaining the process by which one went about proving some statement, Williams remarked that "A proof, by definition, consisted of a finite number of

propositions which contained both premises and a conclusion. The conclusion was derived by using a finite number of strict logical transformations on one or more of the premises, the result of each such transformation constituting a line of proof" (Williams, 1975, 21). Depending on whether one was concerned with proving analytical or empirical statements, the premises of these statements comprised laws of logic or primary statements of experience respectively. With regard to the latter it was accepted that the 'truth-value' of these primary statements could be determined by simple observation.

Seeds of Decay

Although these notions were readily accepted to begin with, they subsequently led to many embarrassing inconsistencies and oversights in the positivist argument. The most significant of these concerned the positivists' criterion of demarcation which effectively excluded science itself from having any meaningfulness. Establishing the basis for this criticism Popper remarked that "... positivists in their anxiety to annihilate metaphysics, annihilate natural science along with it. For scientific laws, too, cannot be logically reduced to elementary statements of experience" (Popper, 1959, 36).

Criticism also surrounded the positivists' use of induction because according to philosophers such as Hume and Popper it was not rationally justifiable. Although positivists made at least three attempts to justify induction, including an appeal to

logic, an appeal to experience and, a retreat to probability, all three failed miserably.

Taking each attempt in order, for the first to succeed, that is, for the principle of induction to be logically justified, it must follow the pattern "... *if the premise of an argument is true, then the conclusion must be true*" (Chalmers, 1976, 12). This however need not be the case with inductive arguments. It is conceivable for the premise of an inductive argument to be true and the conclusion false. For examples of this, refer to Chalmers (1976, 13).

The second attempt to justify induction, namely, the appeal to experience also fails but for a much more simple reason. It fails because one cannot justify induction (appeal to experience) by another appeal to experience. To say that one saw the process of induction working on occasion one and occasion two does not imply that it will always work. Once again confirmation of this is provided by Chalmers at (1976, 14).

The third attempt to justify induction involves a retreat to probability. Instead of saying that a statement is absolutely true, one substitutes the phrase probably true. However, despite this innovation, this argument continues to encounter problems because the argument is still inductively based, that is, there is still a universal statement implied by a finite number of observations. Though it may sound intuitively plausible that if the sun rose each day for the last two thousand years and it rose again today, that it will probably

rise again tomorrow, this need not be the case. Making the point crystal clear Chalmers commented "... any observational evidence will consist of a finite number of observation statements, whereas a universal statement makes a claim about an infinite number of possible situations. The probability of the universal generalisation being true is thus a finite number divided by an infinite number, which remains zero however much the finite number of observation statements constituting the evidence is increased" (Chalmers, 1976, 17).

Confronted continually by these sorts of stop-gap measures, which were inevitably invalidated, many philosophers began to accept that the traditional analysis failed to shed sufficient light on the rationality of knowledge; that the major tenets of logical positivism which were that

- " (i) the empirical (observational) base of scientific theories is itself theory-neutral and therefore unproblematic,
that
- (ii) scientific theories are erected on this base with the help of inductive logic and,
that
- (iii) any statement that cannot be verified is meaningless and, therefore, where a theory yields an unprovable prediction it must be ruled that the theory is non-scientific" (Williams, 1975, 24).

were too simplistic to offer a full and complete understanding

of scientific knowledge.¹⁹ Faced by this situation, philosophers have opted for one of three alternative strategies. Some have continued to place their faith in the traditional analysis²⁰ whilst at the same time hoping that some as yet undiscovered variation (minor in significance) will eventually countenance their belief in it as a worthy model of rationality. Others have opted to abandon the search for a model of rationality as a lost cause, thereby supporting Hume's thesis that science is quite irrational. A third group have preferred to continue to support the notion that science is a rationally explicable process but, unlike the supporters of the first option, have sought to begin afresh, avoiding and/or seeking alternative explanations for many of the key concepts of the traditional analysis.

As one might expect, given the fact that 'old methods die hard', and tend, when dead, to invoke a movement from one extreme to another, the first and second options have received a considerable amount of attention. Categorising the three contrasting philosophers and their philosophies, two of them, Kuhn and Feyerabend, are easily placed within the bounds of the second option. Popper, however, is not so easily categorised.

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19. *Criticism to date of the traditional analysis has been purposefully preliminary. More detailed criticisms will be made when comparing the traditional analysis with the philosophies of Popper, Kuhn and Feyerabend.*
20. *Although this would include the philosophy of the Received View, this phrase has a more general meaning and refers to the notion that science is a rational and progressive discipline.*

Many philosophers, including Lakatos and Laudan (1977, 4), place him within the confines of option one because of his belief in the rational and progressive character of science. However, some doubt may be expressed about his placement alongside the positivists for whom he has shown vehement criticism, the like of which will be demonstrated in the following pages, enabling the reader to make a judgement for him/her self.

Popper's View

Popper's views - which were the first to significantly challenge the Received View (Williams, 1975) - represent a definite improvement on those of the positivists. If it were not for the fact that both Popper and the positivists believe in the objectivity of knowledge the contrast between their ideas would be complete. Unfortunately, the point on which they agree however, serves as a major source of division between philosophers, as is evidenced by the division of this section into objectivist and subjectivist views of science. Because Popper falls into the former (with the positivists) his work is often thought of as a mere modification of positivist doctrine. However, this is not the case. To establish this point, one need only look at the totally different approach which Popper took to the problem of knowledge. Whereas the positivists were primarily concerned whether a theory was meaningful and true, Popper was concerned more with whether there was a unique distinguishing characteristic of science which set it apart from other forms of enquiry. Commenting in this regard Popper stated "*The problem which troubled me was*

neither, 'When a theory is true?' nor, 'When is a theory acceptable?' My problem was different. I wished to distinguish between science and pseudo-science; knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth." (Popper, 1963, 33).

The problem which Popper set himself revolved particularly around the works of Einstein, Marx, Freud and Adler, Popper suspected and set out to prove that the works of Marx, Freud and Adler were not scientific in the sense that Einstein's work was. For this purpose he introduced into his thesis 'falsificationism'. Using this as his criterion of demarcation he was able to dispute the scientific claims of Marx, Freud and Adler on the grounds that their works were not subject to falsification. For a falsificationist a good scientific theory is one which makes definite testable claims about the world and survives such tests. As Chalmers put it "A very good theory will be one which makes very wide-ranging claims about the world, and which is consequently highly falsifiable, and is one that resists falsification whenever it is put to the test" (Chalmers, 1976, 39). Popper's consequent claim was that the theories of Marx, Freud and Adler were not scientific because "... insofar as they could be reconciled with any observation whatsoever, they could not be falsified" (Williams, 1975, 30). An additional, attractive consequence of this need for a theory to be highly falsifiable is that it must be consequently clear and concise. That is, it must run the risk of falsification.

Quickly summarising the falsificationists position, they see science as a rational and progressive enterprise. Science begins with problems (which are only problematic in the light of some theory) about the world. Falsifiable hypotheses are then formulated and tested leading to either temporary acceptance²¹ or falsification. When a hypothesis which has stood a wide range of tests is eventually falsified, the hope is that it has given birth to another problem and the process just described repeats itself. Two additional points to remember are that any prevailing theory is never true, just better than its predecessors and that content increasing auxiliary hypotheses (though not ad hoc hypotheses) often feature in this process.

Countering the tenets of Logical Positivism

In support of a statement made in the last paragraph - that problems are only problematic in the light of some theory - and thus taking task with the first of the tenets of Logical Positivism referred to earlier, (p46) Popper remarked "*My point of view is, briefly, that our ordinary language is full of theories; that observation is always observation in the light of theories; that it is only the inductivist prejudice which leads people to think that there could be a phenomenal language, free of theories, and distinguishable from a 'theoretical language'*" (Popper, 1959, 59). In countering the second tenet, (p46) Popper was of the opinion that there was no such thing as inductive logic for empirical sciences (1963, 40) and as regards the third tenet (p46), that theories were

21. *Knowledge according to the falsificationist is always temporary.*

never empirically verifiable and that rather than rule out any and all statements which were not empirically verifiable (a practice which was ridiculous since it negated every statement in natural science as well) that "... we must choose a criterion which allows us to admit to the domain of empirical science even statements which cannot be verified" (Popper, 1963, 40).

Popper's Answer to the Problem of Induction

It was with these notions in mind that Popper set about what has been described by Magee (1973) as his 'seminal achievement' namely, solving the problem of induction. One of his first steps in this endeavour was to point to a logical asymmetry between verifiability and falsifiability. Expressed in more elementary terms: although no number of observations reporting white swans allows one to derive the positive universal statement ALL SWANS ARE WHITE, one observation reporting a black swan allows one to logically derive the negative universal statement NOT ALL SWANS ARE WHITE. Thus while Popper negated the positivist conception of proof he nevertheless admitted that a theory was testable and further, that it was this testability which was the essence of the criterion of scientific status. In Popper (1959, 40-41) he commented "... I shall require (of a scientific system) that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience."

Here a particular emphasis should be placed on the word 'possible' for Popper did not say that every scientific statement must have been tested before it is accepted, rather that it must be capable of being tested. This was Popper's answer to the criticism, that since all statements in science were in principle falsifiable, that testing would lead to infinite regress since no scientific statement was justifiable, a criticism which Popper himself had used against the positivist notion of induction (1963, 19).

Popper's View about Conclusive Falsification

Taking up the point that no scientific statement is justifiable, either in terms of conclusive verification or conclusive falsification, it is important to note the distinction Popper drew between falsification at the logical level and at the methodological level. In his view, a scientific statement is conclusively falsifiable at the logical level, but not at the methodological level (Magee, 1973, 23) for it is always possible that an observation is false due for example to not taking into account all relevant conditions or alternatively making an error in measurement of these conditions. Commenting about the possibility of recalcitrant observations Popper remarked *"It might be said that ... it is ... impossible for various reasons, that any theoretical system should ever be conclusively falsified. For it is always possible to find some way of evading falsification, for example, by introducing ad hoc an auxiliary hypothesis, or by changing ad hoc a definition. It is even possible without logical inconsistency*

to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever." (Popper, 1959, 42).

Although Popper rightly proffered, on the basis of the above comment, that conclusive falsification was not possible at the methodological level, he has continued to take the view that conclusive falsification is possible at the logical level, a view which has earned him the term of 'naive falsificationist'. Conclusive falsification is only possible at the logical level if one can guarantee the truth of observation statements forming the premises of the logical argument. Popper's own work disputes this possibility. This has led Kuhn to comment "*Though he is not a naive falsificationist, Sir Karl may, I suggest, be legitimately treated as one*" (Kuhn, 1970, 14).

Falsification in Trouble

Although from Popper's work one would expect that following a falsifying instance a theory would drop from sight or at least be modified by a series of content increasingly auxiliary hypotheses, this view does not appear to be exemplified in history. Perhaps the best known example of this is apparent in Newton's theory of gravitation. Even after fifty years of unsuccessful predictions the idea that it could be false was only one of five alternative explanations (Williams, 1975, 37).

In spite of this apparently contradictory example however,

Popper maintains that it does not present a serious problem for his falsification thesis because he never intended that a theory be falsified by just any observation but only by those agreed upon by scientists as representing potential falsifiers. Additionally, he argued that while legitimate scientific activity did not permit the defence of a theory by auxiliary hypotheses which were ad hoc, it did allow for the use of auxiliary hypotheses which were content increasing²², that is, they led to new and novel predictions and/or more falsifiable problems.

Both of these defences have not found much sympathy among many contemporary philosophers of science. With regard to the latter, apart from the contrary historical evidence which in the case of the Copernican theory shows ad hoc hypotheses being used in both the defence and prosecution of this theory and imperfectly formulated novel conceptions being persevered with long after their apparent falsification, one is led to ask - Is it not an entirely retrospective decision in the light of a discovery which makes something novel, hence he cannot tell at the outset the potential of one auxiliary hypotheses from another and it is only by testing them all that we can establish some notion of novelty and even then we may never determine the novelty of some ideas because of our limited technology. It was with these sorts of ideas in mind that Kuhn

22. *Popper would presumably label the other four explanations associated with Newton's theory as content-increasing. This move would however be debatable.*

stated that "All theories can be modified by a variety of ad hoc adjustments ... It is important furthermore that this be so, for it is only by challenging observations or adjusting theories that scientific knowledge grows" (Kuhn, 1970, 13) (Emphasis added).

With regard to the former of the two Popperian defences - that potential falsifiers are a product of agreement among scientists - it is argued that because of this admission Popper's attempted distinction between a logical and psychological dimension of knowledge is invalidated. Popper maintains that the act of conceiving or inventing a theory is of little use to the logical analysis of scientific knowledge. Consequently he argues the knowledge is objective insofar as "It can be argued about, attacked, defended and used, without reference to the man who put it forward" (Magee, 1973, 71).²³

Leaving aside the possibility that they may be arguing at cross-purposes, Williams claims as a result of three factors that Popper's thesis is incomplete because he fails to take account of "... the importance of subjective elements as basic determinants featuring in the fight for the survival of theories" (Williams, 1975, 44). These factors are first that "... in terms of Popper's analysis of falsifiability, in the absence of decisions made by scientists, no theory can be refuted. Second, decisions made by scientists determine the class of basic statements, hence the potential falsifiers, and therefore ultimately what theory is open to falsification and hence scientific. Third, an attempt to rescue a theory

from refutation by introducing an *ad hoc* or content increasing auxiliary hypothesis, is merely collateral to the verisimilitude of the theory in question. It has a lot more to do with the scientists making the attempt - in particular, his intelligence, his professional skills, his motivation, etc." (Williams, 1975, 42). With these three factors in mind Williams was of the opinion that "It follows that the theory which survives in this struggle is largely determined by not only logical considerations but also historical, geographical, economic, sociological, psychological factors, etc." (Williams, 1975, 43).

In short, disagreement surrounds the question as to whether scientific knowledge is objective knowledge. The remaining two philosophers of science argue against this notion. Their arguments along with an analysis of their work will be highlighted shortly after a comparison of the Popperian and Positivist positions in the next paragraph.

Popper and the Positivist in contrast

The views of the positivists and Popper coincide at few points. However, on what some have determined to be a point of major significance they are in agreement. That is, they both accept the fact that scientific activity is rational and progressive and accordingly, that science is the source of objective knowledge. As to their points of deviation, there are four major ones. First, whereas the positivist's criterion of

demarcation was intended not only to demarcate between science and non-science, but also with respect to meaningful and non-meaningful statements, Popper was solely concerned with the problem of distinguishing between science and non-science. Second, whereas the positivists' theories were arrived at by induction, Popper's theories were arrived at by a product of guesswork, intuitive leaping etc. Third, whereas it was the positivist's belief that observations clearly mirrored reality, Popper believed that observations are affected by the observer and his environment. Fourth, whereas the theories of the positivists (despite claiming generality of scope), applied only to a restricted domain and therefore were verifiable, Popper's theories had no restricted domain, were accordingly not verifiable but were falsifiable.

SUBJECTIVIST VIEWS OF SCIENCE

Background

Implicit in the work of the positivists and the falsificationists is that progress demands amongst other things, a systematic framework of criteria and requirements, a coherent body of knowledge and a set of common perspectives. The theses about to be considered, argue against these notions to the extent of proposing that it is only in their absence that progress can really be made.

Kuhn's View of Science

Of all the philosophers adopting subjectivist views of science, Thomas Kuhn would appear to be the most notable and influential. Such is the appeal of his thesis that researchers in many wide ranging fields (including accounting) have sought to draw on his work. Indicative of the extreme to which this has been taken is the fact that it is now considered fashionable, indeed academically elevating, to simply mention his work.²⁴

In the manner of Popper, Kuhn insists that "... *an analysis of the development of scientific knowledge must take account of the way science has actually been practised.*" (Kuhn, 1970 4)²⁵ Consequently he adopts an historical approach to the development of knowledge, a feature which is seen by Gaffikin to be "... *a first point of attraction for many would-be-Kuhnists*" (Gaffikin, 1978b, 77). It is worth noting that this so-called point of attraction also serves to highlight an

24. *The best accounting example of this is in the 1977 Committee report of the American Accounting Association.*

25. *It will be evident in section six, however, that the extent to which they delve into consequent historical study is rather difficult.*

important distinction between the positivist and the Weltanschauungen theorist.²⁶ Whereas the positivists were mainly concerned with the logical form of theories, a preoccupation which consequently led them to accept a static view of science, the Weltanschauungen theorists were concerned with the dynamic aspects of scientific knowledge. This was attainable only by looking at a developmental (historical) perspective of science.

Although having undergone some surgery since its introduction in 1962 (especially with regard to Kuhn's notion of a paradigm),²⁷ lying unchanged at the centre of Kuhn's historical perspective of science is a description consisting of a series of revolutionary and non-revolutionary periods - the revolutionary periods he saw as being discontinuous. More specifically, in his analysis of scientific development he sees a continuous five stage cycle.

- Preparadigm (speculation about appropriate methods, rules forming foundations, etc.)²⁸
- Normal Science (acceptance of a dominant paradigm - start of puzzle solving)
- Crisis (consequence of anomalies in present paradigm)
- Extraordinary Science (search for new paradigm)
- Normal Science (acceptance of new, dominant paradigm - continuation of puzzle solving)

26. *Persons who philosophise a view of the world from a personal perspective.*

27. *This has now been replaced by the more tightly defined terms disciplinary matrix (overall structure of that which is held in common) and exemplars (concrete problem solutions accepted by a scientific community) (Gaffikin, 1978b, 78).*

28. *This first step is only relevant for disciplines traversing the gap from non-science to science.*

The preparadigmatic stage will be evident whenever there is found to be deep and frequent debates between opposing groups over what constitutes valid problems, methods of action and standards of solution. This debate will continue until a sufficient amount of agreement has been attained with respect to some particular school. The ideas central to this school will then become the basis of a 'normal science' in which scientists can puzzle solve without continually questioning the fundamentals of research. This will continue until a time when anomalies which have been discovered in the process of puzzle solving become insurmountable and precipitate a stage of crisis - nowhere specifically detailed by Kuhn. This state of affairs can end in any of three ways: first, the pre-crisis paradigm is able to solve the crisis provoking problems; second, the problem continues to resist attempts at resolution and is subsequently set aside for further generations, or third, a new paradigm (disciplinary matrix) emerges to face the battle of acceptance. One of these is accepted and the puzzle solving tradition of 'normal science' starts over. Should the third alternative be the method adopted in resolving the crisis then a scientific revolution may be said to have taken place.

Two noteworthy features about this process are first, that the old disciplinary matrix is kept until and unless a new one arises to replace it, and second, the process is non-cumulative. This second feature is of considerable importance because it points in the direction of what is considered to be Kuhn's most radical thesis - the thesis of

'incommensurability'. According to this thesis it is impossible to decide which of two competing theories better approximates the truth. There is therefore no rational justification for choosing one theory in preference to another. It is at this point that another sharp division exists between the thinking of Kuhn and Popper. Whereas it is Popper's belief that the progress of science is guaranteed by its rational character, Kuhn insists that in the absence of rational justification there is no way of determining how much closer one theory approximates the truth than another. *"Hence scientific revolutions do not necessarily entail any progress in man's search for truth"* (Williams, 1975, 55).

Kuhn's Major Thesis in Trouble

Despite the intuitive appeal associated with Kuhn's incommensurability thesis, many writers including Scheffler (1972) and Shapere (1964) have taken exception to it and have detailed what appear to be logical inconsistencies in Kuhn's thinking. For the purpose of defending Kuhn's thesis, there is thus a need to look at and dispute these criticisms. Looking first at Kuhn's understanding of incommensurability, Doppelt has observed that Kuhn argues in four different contexts that rival paradigms are incommensurable. These are:

- " (1) *because they do not speak the same scientific language,*
 (2) *because they do not address, acknowledge, or perceive the same observational data,*

- (3) *because they are not concerned to answer the same questions or resolve the same problems and*
- (4) *because they do not constitute what counts as an adequate, or even legitimate, explanation in the same way"* (Doppelt, 1976, 35).

Doppelt maintains that the arguments in favour of and against the incommensurability thesis depend on which of these four contexts one takes as the most significant. He argues that Scheffler and Shapere take the first context as the key one and attribute two, three and four to the fact that a paradigm is imprisoned within its own unique and untranslatable language, or conceptual framework. They accordingly argue that because rival paradigms lack a common language (which the positivists' insist acts as a link between observational and conceptual data) they cannot be meaningfully compared. Thus, on this interpretation incommensurable theories must also be incomparable.²⁹

29. Unfortunately, this is not aided by Kuhnian statements of the nature "... both (referring to himself and Popper) emphasise instead the revolutionary process by which an older theory is rejected and replaced by an incomparable new one" (Kuhn, 1970, 2) (emphasis added). However despite this statement which was unqualified in context we will shortly show that qualification has been made elsewhere by Kuhn. This qualification aims to show that a theory can be sufficiently incomparable to evoke the notion of relativism and yet remain short of total incompatibility.

Having skillfully established this apparent incomparability, Scheffler and Shapere ask several apparently embarrassing questions. *"First of all, if rival scientific paradigms are as insular, self-enclosed, and imprisoned within their own language as Kuhn maintains, in what sense can they be rivals or compete? If they cannot communicate or argue how and on what can they disagree?"* (Doppelt, 1976, 37). The implication is that Kuhn's incommensurability does not appear to mirror the obvious theoretical conflict in evidence in scientific life, and further, that it appears to be at odds with his own view of 'anomalies' which, supposedly established a point of contact between rival paradigms.

The arguments of Scheffler and Shapere, despite being logically appealing, rest on a particular interpretation of Kuhn's thesis, one which understandably springs from their belief in a common scientific language. In this sense they needed to pick the first context as the most important, because it served the purposes of positivism. However, there is another, more satisfactory interpretation of Kuhn's incommensurability. This interpretation rests on the second context - the incommensurability of scientific problems between rival paradigms. In a series of quotes taken from the writings of Kuhn, Doppelt establishes the importance of the incommensurability of problems and standards of adequacy between paradigms (Doppelt, 1976, 40). At no time though does he adopt the absolutist position adopted by Scheffler and Shapere with regard to the incomparability of paradigms. He comments *"On our view, incommensurable paradigms can and do share some observational data, problems, and language but that ...*

Rival paradigms can share this much and none-the-less exhibit fundamental disagreements irresolvable by scientific argument concerning the set of problems and data that any adequate theory must treat (only some of which they share)..." (Doppelt, 1976, 50)³⁰

Doppelt develops many more fascinating arguments in his fifty three page article, but perhaps the most interesting of all of these is his support for the positivist conception of a cumulative progress of science in the long run. Wishing, however, to discredit the positivist model of scientific development in favour of Kuhn's model he commented "... regardless of whether there is cumulative progress of knowledge in the longrun, the fact of shortrun loss-of-data poses a powerful challenge to the positivist model of scientific development as a gradual, continuously cumulative progress in scientific knowledge" (Doppelt, 1976, 72).³¹

A Point of Contrast Between Kuhn and Popper

Having become justifiably carried away with considering the thesis of incommensurability, it is nevertheless time to consider another aspect of Kuhn's thesis. This concerns Kuhn's rejection of

³⁰.The reader is strongly encouraged to read for him/herself the full text of Doppelt's argument because the summary of the work exhibited in this thesis does it little credit.

³¹.The loss-of-data notion referred to in this quote follows from Kuhn's views "...That every new paradigm ... involves losses as well as gains with respect to its predecessor(s) in terms of the kind of data and problems it is concerned to handle, and can in fact handle" (Doppelt, 1976, 44).

Popper's characterisation of scientific theories as 'falsifiable' theories. On this aspect Kuhn is committed to rejecting Popper's falsification thesis for the same reason that he rejects Popper's thesis that scientific theories follow a rational development. Developing the substance of this rejection Williams remarked "A theory can only be falsified if it can be demonstrated that, in at least some respects, it is not true. Yet such a demonstration always rests on certain theoretical assumptions. Consequently the end product of any demonstration is open to question" (Williams, 1975, 56). Considered in the light of incommensurability, we are accordingly left with the dilemma of deciding whether the demonstration challenges the theory or vice-versa.

Furthermore, disputing Popper's claim that the former is the more probable, Kuhn took up Popper's claim that a theory is scientific if and only if observation statements can be logically deduced from it. Kuhn, in challenging the implication that a scientific decision could be based on exclusively syntactic criterion remarked "... I doubt that scientific theories can without decisive change be cast in a form which permits the purely syntactic judgements which this version of Sir Karl's criterion requires. But even if they could, these reconstructed theories would provide a basis only for his demarcation criterion and not for the logic of knowledge so closely associated with it" (Kuhn, 1970, 15). Sir Karl writes that "The logic of knowledge consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained" (Popper, 1959, 31).

From this, he maintains that rules and conventions akin to the following develop: *"Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop without 'good reason'"* (Popper, 1959, 53). Kuhn argues however that *"Rules like these ... are no longer simply syntactic in their import. They require that both the epistemological investigator and the research scientist be able to relate sentences derived from a theory not only to sentences but to actual observations and experiments"* (Kuhn, 1970, 15). Kuhn accuses Popper's notion of falsificationism of failing to account for this perspective and consequently attributes to Popper the construction of an ideology rather than a logic; of procedural maxims rather than methodological rules.

A Problematic Aspect of Kuhn's Thesis

One aspect of Kuhn's thesis which has continued to remain problematical in spite of efforts to rationalise it, is his explanation of how it is that anomalies come to subvert a prevailing paradigm. In contrast to the acceptance of a new paradigm, where Kuhn maintains that an act of faith in the nature of a religious conversion or gestalt switch is involved, his explanation of the former, which presumably is a forerunner to acceptance of a new paradigm, appears entirely unsatisfactory. According to Kuhn, crises follow when *"...the (scientific) profession can no longer evade anomalies that subvert the existing tradition of scientific practice"* (Kuhn, 1962, 6). However, in a statement describing the activities of scientists in periods of normal science Kuhn explained that when a puzzle evades resolution *"... only the practitioner is blamed, not his tools"* (Kuhn, 1962, 79).

One is led on the basis of these statements to ask - Just what makes an anomaly subversive? Kuhn fails to account for this and is criticised by Feyerabend as a result. The basis of this particular criticism as it welds into Feyerabend's overall thesis will be considered shortly.

Kuhnian and Popperian Fundamentals in Contrast

Just before discussing Feyerabend's thesis though, it is important to recognise the fundamental similarities and differences in the works of Popper and Kuhn. The fundamental similarity is contained in the fact that both Popper and Kuhn believe that knowledge has no foundations, neither in experience nor reason. This contrasts them both with the Received View theorists who entertained a justificationist theory of knowledge. The fundamental difference between Kuhn and Popper, and, for that matter, the Received View theorists, is in their view of whether scientific knowledge is objective knowledge.³² Kuhn argues against this notion claiming that "*... scientific knowledge, arising out of research in periods of normal science, always possesses subjective elements*" (Williams, 1975, 50). An allied concept over which they disagree concerns the notion of scientific progress. Whereas Popper maintained that progress was guaranteed by the rational character of science, Kuhn argued that because of the incommensurability of paradigms there was no concept of rational justification in favour of one theory over another and thus no way of ensuring scientific progress.

32. *In section six however, this thesis will investigate more fully Popper's view of objectivity and show that contrary to the belief expressed above (a common belief) this is not a feature which separates Kuhn and Popper but rather Popper and the positivists.*

Feyerabend's Dadist Philosophy

For the same reason that an analysis of ideas from the philosophy of science became the major focus of attention in this thesis, namely, because little work had been done in this area, this thesis accords an extra degree of attention to Feyerabend's thesis. This additional attention is also given because it is the author's contention that Feyerabend's work shows potential for being the next logical step (following on from the A.A.A.'s use of Kuhnian analysis) in an association between the philosophy of science and accounting. Yet another reason for this extended treatment, which follows from the second (above) is that we need to determine whether there is any substance to Feyerabend's work. Many have ridiculed his work as jocular and one (Hattiangadi) even termed him a 'Play Philosopher'. As a result of these attacks, the majority have been led to disregard his work. To determine whether his work should be so treated, an examination of the works of three major critics will be undertaken in an attempt to come to some conclusion about the validity of their criticisms and hence the substance of Feyerabend's work.

Appearing, as it does, at the opposite extreme to logical positivism and Popper's revolutionary positivism it is with very little reservation that Feyerabend is looked upon as the most controversial of the modern day philosophers.³³ The way in which his major work 'Against Method' has caused the defense mechanisms of traditionalists such as Lakatos (1974), Hattiangadi (1977), Kulka (1977) and Tibbetts (1977) to spring into action, is ample evidence of his controversial style. In his book, Feyerabend includes many of the familiar themes which have become associated with his name. These include: an 'anything goes' approach with respect to methodology (Chapter 1); the denial

33. *It should be noted, however, that there is much on which Popper and Feyerabend are in agreement. For confirmation, refer to Suppe (1974, 170).*

of the distinction between a context of discovery and a context of confirmation (Chapter 14); the importance of non-rational factors in the development of science (Chapters 8, 12); the rejection of critical rationalism and logical empiricism (Chapter 15); the development of counter-inductive procedures and the proliferation of theories (Chapter 23); the denial of the empiricist criterion of cognitive significance (Chapter 5); a rejection of the current notions of intellectual standards, facts, and reason (Chapters 1, 2 & 5); the incommensurability of scientific theories (Chapter 17); and finally, the over intellectualism and superficiality of modern science in contrast to magic and witchcraft (Chapter 18), (Tibbetts, 1977).

Feyerabend's book is written with the conviction that anarchism, while not the most attractive political philosophy, provides an excellent remedy for epistemology and the philosophy of science. Features of the last statement which should be noted are; first, that Feyerabend considers anarchism a 'remedy' and not 'a certain cure' for the ailments which confound epistemology and the philosophy of science; and second, that although clearly distinguishing between the philosophy of science and epistemology (the theory of knowledge), Feyerabend infers the applicability of his thesis to both. This is something which Kuhn failed to do and which consequently caused his thesis to become so associated with the philosophy of science that when other disciplines, such as accounting (not normally classified as a traditional science), attempted to make use of it their attempts were criticised purely over the science /non-science distinction, which, in no small way can be attributed to both the narrow mindedness of those associated with traditional sciences, and the cultural bias of the great majority, who as Laudan said, have tended to place so much faith in the words and deeds of these people.

Like Kuhn, Toulmin, Lakatos and others, Feyerabend makes use of history to aid in demonstrating the applicability of his thesis. He comments *"One of the most striking features of recent discussions in the history and philosophy of science is the realisations that events and developments, such as the invention of atomism in antiquity, the Copernican Revolution, the rise of modern atomism ... and the gradual emergence of the wave theory of light, occurred only because some thinkers either decided not to be bound by certain "obvious" methodological rules, or because they unwittingly broke them"* (Feyerabend, 1975, 23). Having cited his aversion to set methodologies, the remainder of his work sets about the task of validating this stance and entertaining, as a possible solution, the notion of methodological pluralism, using the other themes already mentioned as examples to round out his thesis.

Criticisms of Feyerabend's Philosophy

In singling out works representative of the response which Feyerabend's work has had, one has a decidedly difficult task in discovering favourable responses, for he is criticised on a wholesale basis by virtually every modern day philosopher. Even Lakatos, who became closely allied with Feyerabend, as two of the most outspoken critics of Popper's work (Kulka, 1977) adamantly rejects Feyerabend's recourse to anarchism, commenting in one of his works *"I view the third approach (referring to Feyerabend's thesis) with horror"* (Lakatos, 1974, 323).

Mindful of this predominantly critical response, care must be taken against selecting criticisms which are simply unfavourable observations of particular ideas, in favour of those which have as their sole purpose the questioning of the central messages of Feyerabend's thesis. To these ends, reference will be made to three review articles appearing in a review symposium on Feyerabend's thesis, published in the Philosophy of the Social Sciences journal (1977)³⁴ Further references will be made to a reply to these reviews by Feyerabend (1978a), two more rejoinders by Hattiangadi (1978) and Tibbetts (1978) and a further reply to the rejoinders by Feyerabend (1978b).

Although a well accomplished and at times, amusing writer, it seems that Feyerabend abuses his talent by recourse to over zealous and arrogant, adhominem arguments which might well be justified in an 'anything goes' philosophy, but do little to gain him the support he needs among conventionalists. A counter argument which Hattiangadi puts to good use to point to the ridiculousness of Feyerabend's arrogant stance is that if it is acceptable for Feyerabend to argue for 'anything goes' then why is the privilege not granted to his reviewers. He comments *"It is, moreover, astonishing to find the author of 'anything goes' cavilling over a mere interpretation of his words. Does anything not go, then, in reading Feyerabend? Can we not proceed counterinductively in interpretation? Does Feyerabend (but no one else) mean something more than he says?..."* (Hattiangadi, 1978b, 59). In addition to his aggressive style, two further factors which likewise

³⁴. As it happens, it will be apparent from the discussion that these works are also subject to the criticism of superficiality. However, they are cited because they are apparently regarded as generally indicative of the response which Feyerabend's work has engendered.

do little to endear him to the masses, are first, that he has what many consider to be, an annoying habit of changing his mind (Tibbetts, 1977, 272), and second, he has a definite tendency to indulge in extreme points of view.

Kulka's Critique of Feyerabend

Regarding the former, both Tibbetts (1977, 272) and Hattiangadi (1978, 60) accuse Feyerabend of contradiction and frequent changes in point of view, in the latter case, using as example Feyerabend's apparent about face in determining the role of explanation in theory. Feyerabend's reply to this is a good one in that he admits to being fallible. He states, in reply to Hattiangadi, *"Well, thinking is a difficult business and I have not yet found the secret of at once penetrating to the very core of truth. Has he?"* (Feyerabend, 1978b, 186).

On the subject of contradictions, as opposed to complete point of view changes, it is interesting to note that many of the so called contradictions which Feyerabend is accused of, stem from what are essentially terminology debates. Now, while there is a natural hesitation to talk about any aspect of impression in terminology for the very same reason as was suggested earlier when the science / non-science controversy was discussed, namely, the inability to come to any definite conclusions, one may at least make the observation that, it does seem that Feyerabend, in using terminology open to debate (perhaps

purposely), has given rationalists the rope with which to hang themselves. This is particularly evident in Kulka's case. He is severely criticised by Feyerabend (1978a) for reading into Feyerabend's words that which is not there. Some of Kulka's statements which display a rather fanciful interpretation of Feyerabend's ideas include, *"Since there can be no perfect methodology, all methods are useless and therefore, 'anything goes'.* (Page 279) *"... all methodologies are equally bad..."* (Page 279) and *"For he makes it plain there is no such thing as progress"* (Page 280).

In all three statements Kulka is far from justified in making the statements that he does. In the first statement, to infer that as a result of no methodology being perfect, that they are all useless, reflects both his personal bias against Feyerabend and his inability to read Feyerabend's work, which clearly shows that while Feyerabend criticises some procedures, he defends and recommends others. The second statement, by using the word 'bad' completely misconstrues the meaning of the statement. It is quite obvious from Feyerabend's work that the way in which he uses the term 'anything goes', signifies simply that no methodology is necessarily better than another and not that all methodologies are useless as the term 'bad' suggests. In chapter two of his book, Feyerabend states *"My intention is not to replace one set of rules with another set, my intention is rather to*

convince the readers that all methodologies, even the most obvious ones, have their limits." (1975a). Having noted what is a tendency to deliberately misconstrue Feyerabend's statements (in a far from convincing manner) the trend is confirmed with the third statement which has taken one of Feyerabend's statements which denies knowledge of what progress is (1975, 27) and asserts from this that Feyerabend does not believe in progress, which is a particularly amusing assertion in view of the fact that Kulka is supposedly trained in logic. His assertion is certainly no logical assertion. Rather, a subjective judgement, which tends to prove Feyerabend's point that logical rules are seldom, if ever, kept (1975, 22).

Unable to dispute Kulka's right to make the claims he does, based on the notion that if 'anything goes' then anything must be received, one may be justified however in disputing his, and other rationalist claims if they argue in an irrational manner. To argue that all methodologies are useless if they are not perfect, and that a lack of understanding of progress implies no such thing, is surely to argue irrationally. In addition to arguing irrationally, Kulka's review also appears to suffer from superficiality in that first, he has obviously not read the book thoroughly and second, he has failed to consider many of the central themes of the work, including the major one; that one must keep one's options open. This, however, is quite understandable. The way in which Kulka misconstrued Feyerabend's

statements left him no chance of interpreting the main theme in the manner just described. In essence, his superficial treatment gave rise to his irrational comments.

Tibbetts' Critique of Feyerabend

Tibbetts, in order to avoid being subject to the above criticism, specifically restricts his critique to four areas. He comments *"Rather than attempt the impossible and comment on all the themes noted above, I will in this paper limit my remarks to Feyerabend's defense of methodological pluralism, the empiricist criterion of meaning, the all important issue of evidence, and finally the relationship between scientific methodology and the alternative forms of inquiry and explanation, particularly magic and myth"*. (Tibbetts, 1977, 266).

On the first of these issues, Tibbetts claims that *"... Feyerabend's denial of a set of privileged and universally binding principles of inquiry is not incompatible with the claim that scientific methodology provides the most reliable and credible knowledge we have to date of the world around us"* (Tibbetts, 1977, 267), and develops an argument based on a distinction between a context of discovery and a context of justification, preferring to put disciplines, such as magic and witchcraft in the former, and the traditional sciences in the latter, based on the further supposition that science is more 'self-critical' and 'self-correcting'. In reply to this, Feyerabend questions the basis of such suppositions. He asks *"... but where, it must be asked, did he get his*

information concerning witchcraft? What particular witchcraft doctrine has led him to the conclusion that witchcraft is not self-corrective? (Feyerabend, 1978a, 47). In response, Tibbetts admits his lack of knowledge in the area, but in similar fashion questions Feyerabend's knowledge. He comments *"Rather than tell us he is content to darkly allude to literature and information he is in possession of..."* (Tibbetts, 1978, 55).

Tibbetts then attempts to take this one step further by claiming that Feyerabend sees only that which he wants to and that others might be less inclined to do so. However, this argument is rather academic, as it seems is the entire recourse to the science-magic controversy. It remains a further example of what was described earlier as one of Feyerabend's major support losers, that is, the tendency to indulge in extreme points of view. When one gets right down to it, the concept of methodological pluralism is hardly discussed, and when done so, only in the narrow sense of the science-magic debate, which tends to validate a comment by Gaffikin that *"... it is, however, some of his apparently more extravagant claims that have attracted most criticism"* (Gaffikin, 1978b, 74). It seems ironic, somehow, that Tibbetts recognises the wider sense of methodological pluralism, *"Personally, I prefer the less spectacular but more accessible hypothesis of methodological pluralism, where, in no area of human inquiry and experience - including science - is inevitably pernicious and "ideologically petrified"* (Tibbetts, 1977, 275), but only does so as a final comment after an

entirely inconclusive argument on one very narrow aspect of the debate. Consequently, much of this debate is periferal and little short of a complete waste of time. This becomes even more apparent in Feyerabend's reply to Tibbetts' initial response. Feyerabend comments *"If he is satisfied with his statements then, then why is he not satisfied with mine"* (1978b, 184) thus stalemating the argument, and further - *"I can assure him that I would look to my "witchdoctor friends", to use his somewhat imprecise terminology, and so would many other people in California whose experience with scientific medicine has been anything but encouraging"* (1978b, 184) a statement which all but sidesteps Tibbetts' thoughts about a separate context of justification in favour of a subjective preference for witchdoctors, which can not be verified one way or the other, but which is, nevertheless, just as the former statement is, in line with Feyerabend's thoughts about the subjectivity of knowledge. In view of Feyerabend's support of subjectivity of knowledge, the arguments put forward are quite useless.

The only hope of a successful argument obliging Feyerabend to change his mind is a convincing argument about historical development, which counters Feyerabend's own view. To overcome a *Weltschauungen* analyst, especially Feyerabend, one must play him at his own game. This, Tibbetts has failed to do. He has put forward arguments with little or no historical justification, preferring it seems to rely on the reader's

(predominantly rationalists) personal bias, a fact Feyerabend is quick to point out, "*Is it that he just repeats rationalist gossip without having examined its validity*". (1978a, 47).

Having made a statement at the outset, exempting Tibbetts from the criticism of superficiality, we must now retract this exemption. It is withdrawn for two reasons. First, because Tibbetts becomes too concerned with the science versus magic debate, in attempting to counter methodological pluralism, and second, because in doing so he fails to adequately resolve this dispute in that he does not supply sufficient, contrary historical evidence.

One further example from Tibbett's work, which points to its demise is apparent in the following comment '*Feyerabend's partiality towards non-science is as arbitrary as the supposed arbitrariness of my own preference for scientific forms of inquiry*'. (1978b, 57). In this statement, Tibbetts makes the claim that Feyerabend's decision in favour of non-sciences is just as arbitrary as his own. Aware of a similar argument in Feyerabend's work one wonders just what Tibbetts was doing when he was reading Feyerabend's work. His statement is obviously intended as a criticism, but only succeeds in restating one of Feyerabend's own messages - that choice between alternatives is often based on opinion; this opinion arises as a result of our cultural bias or social conditioning. In rejecting Feyerabend's claim about arbitrariness, Tibbetts also sought to go one step further and show that choice among alternatives is not '*...merely a subjective, personal matter*' (1977, 269). He continues with the

message of explanation 'On the contrary my argument hinges on the assumption that what determines the degree of reliability we attach to a set of empirical assertions is determined by verifying events' (1977, 269). However, one is led to ask - is it not an entirely subjective, even arbitrary decision as to what a person witnesses in verifying an event? Even if the majority of people verify an event in similar fashion, this does not lend support to Tibbetts' argument because these decisions could well be the result of social conditioning. For example, the majority of people reading Tibbetts' work are likely to agree with his observations, because the majority, like Tibbetts, are likely to be complex rationalists.

Hattiangadi's Critique of Feyerabend

Of all three reviewers, Hattiangadi is the one who attracts least attention from Feyerabend, something which seems indicative of the fact that he has least to say. His review consists of a synopsis of what Hattiangadi terms as Feyerabend's three main salvos, namely, Feyerabend's attacks against intellectual standards, facts, and reason. This is followed by a discussion of the Romanticist era in an attempt to show Feyerabend's work to be old hat, and finally, a solitary criticism which, as shall be shown is not even relevant to the book under review, a point which does tend to make one wonder why his was the longest of the three reviews. Arguing in the same manner as Tibbetts did, that unless specifically excluded, something must be accepted, Hattiangadi proffers that despite Feyerabend's rejection of a formal account of explanation 'presumably, he

still believes that theories in science are explanatory" (1977, 295). Referring to an earlier work of Feyerabend's, 'Explanation, Empiricism and Reduction', Hattiangadi has Feyerabend first defending a Popperian point of view, and then refuting it (1977, 295-298) which Hattiangadi claims is inconsistent with Feyerabend's assumed view that a theoretical system is both explanatory and deductive in nature. In his initial response, Feyerabend comments that explanations play no role in his book (1978a, 45). However, Hattiangadi, not content to let the matter rest, replies to Feyerabend in a vicious, ad hominem style, entirely it seems, without cause, Hattiangadi being the least of the three to incur the wrath of Feyerabend's own ad hominem arguments. Totally overlooking the fact that Feyerabend, when making the earlier comment, was referring to his book, which Hattiangadi was also supposed to be concerned with, Hattiangadi, in his reply preferred to refer to another quite separate article, entitled 'Changing Patterns of Reconstruction' written in 1977, in which Feyerabend declares his support for disjunctive disjointness. Leaving aside the fact that Feyerabend answers this criticism, it does seem that Hattiangadi's style of review is rather odd. In what is his only real criticism, he fails to link it to the book he is supposed to be reviewing. Consequently, despite the fact that Feyerabend admits a change in his point of view, he (Feyerabend) specifically divorces his criticism from his book, a point which Hattiangadi does not question. Thus, it gives Hattiangadi's work the appearance of a mere resumé, and in view of the corrections

which Feyerabend makes, not a very good one at that.

Summarising these three works, there is an inclination to regard two of them as a complete waste of time. These are the reviews by Hattiangadi and Kulka. They succeed in disputing and resolving nothing. Tibbetts' work does have the basis of some valid criticisms, especially in regard to Feyerabend's view, which denies a distinction between a context of discovery and a context of justification, however, Tibbetts' argument fails to attract much attention. This was due to two factors. First, an overwillingness to be sidetracked by Feyerabend's use of extreme examples, and second, a superficiality which manifested itself, both in terms of overall shallow treatments and the tendency to make statements (as did Kulka) which were quite illogical.

Having undergone an extensive review of Feyerabend's thesis, a review which has seen his thesis largely unaffected, one is thus able to proclaim his work one of substance and compare it with among others, Kuhn's thesis.

The Fundamental Difference Between Kuhn and Feyerabend

Earlier, when discussing Kuhn's ideas it was noted that Kuhn's inability to show how a crisis eventuated represented a major flaw in his work. It was also noted that it was on this point that Feyerabend sought to be critical of Kuhn. This criticism turns out to be a major point of contrast between

the two. The following quote, although somewhat lengthy, is nevertheless justifiable on the grounds that it clearly establishes this contrast. Feyerabend comments "According to Kuhn mature science is a succession of normal periods and of revolutions. Normal periods are monistic; scientists try to solve puzzles resulting from the attempt to see the world in terms of a single paradigm. Revolutions are pluralistic until a new paradigm emerges that gains sufficient support to serve as a basis for a new normal period. This leaves unanswered the problem how the transition from a normal period to a revolution is brought about ... we have indicated how the transition could be achieved in a reasonable manner; one compares the central paradigm with alternative theories. Professor Kuhn seems to be of the same opinion. Moreover he points out that this is what actually happens. Proliferation sets in already before a revolution and is instrumental in bringing it about. But this means that the original account is faulty. Proliferation does not start with a revolution; it precedes it. A little imagination and a little more historical research then shows that proliferation not only immediately precedes revolutions, but that it is there all the time. Science as we know it is not a temporal succession of normal periods and periods of proliferation; it is their juxtaposition". (Feyerabend cited in: Williams, 1975, 62).

Feyerabend's best known example for supporting his consequent contention: that a pluralistic science is likely to be more progressive than a Kuhnian monistic normal science, is demonstrated with respect to Brownian motion.³⁵ Feyerabend's basic argument is that the refutation of a theory can only be aided with the help of alternatives, so that the Kuhnian advice of postponing alternatives until the first anomaly has occurred, effectively serves to insulate the theory from criticism rather than lead to its overthrow.

However, despite agreeing with Feyerabend's preference for a pluralistic rather than a monistic science, doubt still remains as to whether Feyerabend really provided a solution to Kuhn's problem. For example, Williams writes "...

Feyerabend's idea that proliferation of theories solves Kuhn's problem in that alternative theories bring to light new facts that refute more established theories cannot be supported as it stands" (Williams, 1975, 72).³⁶

SUMMARY

In bringing this extended (but by no means comprehensive) section to a close, it is hoped that the reader has, at the very least, a basic awareness that there is much more to understanding science and the development of knowledge than

35. For a detailed account of this argument refer (Feyerabend, 1965, 175-176).

36. For a detailed account of the argument associated with this claim refer to (Williams, 1975, Chap. 6).

that which has formed the basis of traditional views.

Perhaps the most important point of all is that gaining an understanding of the development of knowledge requires that an understanding of the progress of traditional scientific theorising be a necessary rather than sufficient condition for this purpose. Evident also from above is that a complete and acceptable understanding of science has not yet been reached, which among other things does tend to make any debate over whether accounting is a science or not somewhat pointless, at the very least premature.

In the following section, discussion will centre around where accounting fits into the above scheme. Whether it is or should be based on the objectivist or subjectivist view of knowledge and whether within the appropriate one it is or should be based on a particular conception of that view, for example, the positivists justificationalism.

One point which must be made clear to sceptics who would caution against a task of this sort (because of the realisation that neither of the four theses referred to above offers a complete and satisfactory explanation of progress), is that a delay would be self-defeating. Had no one gone ahead with research (which by its very definition presumes a problem) until everything was settled the world would be little further advanced than when it began. In fact, it is doubtful whether we would still 'be' because no research would have been directed towards curing and preventing disease.

RESEARCH METHOD/HYPOTHESIS

According to Buckley et al (1976) research works may be distinguished by the extent to which they follow one of four independently recognizable research strategies.

These are: opinion research, archival research, empirical research and analytical research. For the purpose of categorising the current work however, none of the above strategies gives a completely adequate account on its own. Indeed in terms of the definitions given by Buckley et al, the work clearly falls outside the bounds of two of the strategies, that is, opinion and empirical research. This was to be expected in the latter case for as this work seeks among other things to criticise the extensive use of empirical research in accounting it would clearly be a 'reductio ad absurdum' to contemplate its use. Although the definitions of analytical and archival research do not individually constitute a complete description either, a research method which falls somewhere between them is sufficiently indicative of the method adopted by the current research effort.

To the extent that this work makes use of existing ideas it is archival, although it should be noted that the work does not make use of the formal techniques which Buckley et al ascribe to archival research. Nor indeed does it make use of formal sampling techniques. The works selected are selected specifically to support a particular hypothesis, the substance of which is that - if one accepts the proposition that 'if something is worth doing it's worth doing well' in connection with the perceived need to

conduct "scientific" research in accounting then one cannot but support the need for more epistemological study in accounting for it will be apparent that most accounting works have not kept pace with developments in the philosophy of science. Given the fact then that the works selected are selected arbitrarily - that the work deals with "... *the use of internal logic on the part of the research*" (Buckley et al, 1976) and assumes further that "*the researcher has the resources required for solving the problem within himself*" (Buckley et al, 1976) it is apparent that the work although having an archival component has a closer link with the analytical research strategy.

This research effort then involves an examination of ideas arising out of a logical extension of the perceived need to establish a research tradition in accounting. More specifically, it takes up what appears to be a general call for scientific investigation of accounting phenomena, having particular regard for the theory construction process, and to begin with, seeks to correct the erroneous view which most interested accountants seem to hold regarding the nature of scientific development. Accordingly, it attempts in the first major section of the work to develop a perspective of important aspects of new views of science, contrasting them with the traditional 'Received' view.

In the second of the two major sections of the work an attempt is made to determine where and how these ideas have and maybe related to accounting. Using the works of Chambers and Sterling an attempt will be made to establish the traditional view of

scientific research. Chambers' work was chosen because his work was one of the first of significance in the field and Sterling's because his is recognised by some as being generally indicative of the notions of theory construction held by accountants (Gaffikin, 1979, 10). Although there were a number of other theorists whose views on scientific theory construction could have been cited in order to round out the traditional view of scientific development, such analysis has already been admirably treated by Gaffikin (1978b) on whom this work relies as an authority on these matters. The space made available by relying on Gaffikin's work has been used to look at the incidence and scope of works, implicitly or explicitly favouring some of the more complex views of science. One work in particular is singled out as being supportive of the logical extension attempted in the current work. This is the work of Richard Mattessich. An analysis of his work and reference to many others provides justification for the explication of ideas in section four of this thesis.

Some Accounting Experience with Philosophy

Background

Based upon the work completed to this point, two ideas stand out as being of primary importance. The first concerns the disputing of the traditional, well-accepted notion of science, and the second, a qualification of this dispute. Regarding the former it has been demonstrated that reference to scientific discourse or, as it is more commonly referred to, the "scientific method" does not imply some finite set of formal, procedurally orientated research steps, but rather an attitude or philosophy which conditions the researcher. Regarding the latter it has further been demonstrated that just as there was no finite set of research steps associated with the "scientific method" there is no one philosophical explanation of the progress which, in our new understanding of science, conditions the researcher. Instead there are a whole range which make up a field of knowledge known as the "Philosophy of Science".

This thesis chose to consider a representative sample of four of these philosophical explanations. In the current section the intention is to consider the works of a range of accounting theorists over a thirty year period (1950-1980) to get some idea of the philosophical position, implicit or explicit, on which their works have been premised. The implication following from this move (recited earlier) is that if accounting theorists have accepted a philosophical position which is untenable, then all their efforts to build a solid theoretical foundation on which

the accounting discipline may be expected to cope with present and future problems may be in vain.

To provide a certain amount of structure when dealing with the works of the various accounting theorists, the works will be divided into two selections. The first will establish the common philosophical line or stance adopted by accounting theorists when considering the use of scientific theory construction methods in accounting. A second selection will serve to illustrate some of the more controversial philosophical lines which some authors have adopted. Together they should provide some insight into the association to the present time between the philosophy of science and accounting.

1950 was chosen as the starting point for the review because prior to this point very little effort had been devoted to the use of external research methodologies in accounting theory. Efforts were predominantly centred around the development of generally accepted accounting principles, postulates and standards. This particular research effort had sprung from the unfortunate state of affairs associated with the stock market crash of 1929. The laissez-faire attitude which had been adopted toward the accounting discipline up to this point by both the public and other professional bodies was quickly substituted for what became a crippling amount of expedient and often contradictory regulation.

Establishing the Traditional View of Scientific Development

Chambers' Perspective

One of the first to recognise the possible negative attributes associated with this regulation was Professor R.J. Chambers. In a book which echoed the sentiments of work he had begun in earnest around the 1950's Chambers commented that we must "*question the formulation of rules before adequate analysis of the matters in respect of which they are made*" (Chambers, 1966, 352).

Reflecting on the consequent purpose and direction which he took in the 1950's he further commented that "*By 1954 I believed it necessary...to set down the way in which a theory of accounting should be developed. In none of the important works on accounting was there a treatment of methodology. There was no pattern to follow except that of the well developed sciences.*" (Chambers, 1966, x) (emphasis added). His intention was clearly to come to some understanding regarding the methodology of science in an attempt to make a comparison between scientific and accounting progress.

For this purpose Chambers used among a number of 'formal guides' the works of Cohen and Nagel, Campbell, Hempel and Margenau. In this endeavour, that is, using philosophical works as guides, he was undoubtedly one of the first, if not the first accounting theorist to do so. For this he is to be accorded considerable praise. A point to be cognizant of however, is that in his endeavours he has made use of philosophical works which have espoused largely positivist ideals. Not surprisingly there are

clear references in his work to the sentiments of the Received View. One such reference is apparent in his stressing of the vital importance of observational evidence. Another is apparent in his separation of proposition into formal and empirical classifications, and a third is his use of correspondence rules as a bridging mechanism between formal and empirical propositions (1966, 34).

On the face of it one might thus have expected to find all of Chambers' works firmly rooted in positivist dogma, and yet this is not the case. If one had to establish Chambers' allegiance to any one philosophical position (a practice which Chambers himself is against) one would have a decidedly difficult task for while positivist sentiments are in evidence in much of his early works so too are Popperian notions. To complicate things even further, Chambers also makes reference to the possibility of accounting progress being explained in terms of Kuhnian analysis. This immediately raises the question - has Chambers' simultaneous reference to all three been contradictory? Based upon the discussion and classification of the various theses in section four, it appears that whereas simultaneous reference to the works of the Received View and Popper would not be contradictory, simultaneous references to objectivist and subjectivist views of knowledge would be. Knowing that Chambers refers simultaneously to the works of both Kuhn and Popper, and given no other evidence, this points unmistakably to a contradiction in Chambers' work. However, before accusing Chambers of contradiction, it is worthwhile to consider some new evidence which

clarifies and qualifies ideas from section four.

According to information presented in the fourth section, the philosophies of Kuhn and Popper differed primarily over whether scientific knowledge was objectively or subjectively based. In that section, little was said about this difference except that it existed. Now it is time to determine just what is meant by these two terms.

In arguing that scientific knowledge is objective knowledge, Magee, interpreting Popper's stance, proffered that objective knowledge meant "*... knowledge that could be argued about, attacked, defended and used without reference to the man who put it forward*" (Magee, 1973, 71)³⁷. For his part Kuhn argued that because a paradigm is largely socially constructed that knowledge is always grounded in man's social condition. He writes, "*Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of belief. An apparent arbitrary element compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time*". (Kuhn, 1962, 4).

37. This incidentally coincides with Chambers' view of objectivity expounded in the following quote. Whilst referring to ideal statements, Chambers commented that they must "*... individually capable of being tested by other persons than the propounder of the theory*". (Chambers, 1966, 7).

Looking closely for a contrast between these two conceptions of knowledge, one could be forgiven for being decidedly confused for one is looking for a contrast which does not exist, at least on the basis of the above statements. Kuhn is saying that we can never attain strictly objective knowledge which is accepted as a truism by all but those who follow a strictly positivist line, and Popper, on Magee's interpretation, that we can nevertheless attain a state of knowledge in which some idea(s) is(are) accepted as sufficiently verified by others to no longer be peculiar to one person. This is also an intuitively acceptable proposition. So where is the contrast? In fact there is none.

This is to be attributed to a modification of position by Popper. Though Magee correctly interprets Popper's current position, it seems evident that in some of Popper's earlier works, his position was considerably more extreme, that is, he believed, like the positivists, in strictly objective knowledge. Confirmation by implication that Popper had held such a position is offered by Williams who in contrasting Popper and Kuhn commented "*Essentially what distinguishes Popper's views from Kuhn's is that for Kuhn knowledge is never strictly objective*" (Williams, 1975, 50).

On this interpretation, therefore, the differences between the objectivist philosophy of Popper and subjectivist philosophy of Kuhn are not as extreme as first implied. Without becoming too involved with this comparison, it might just be mentioned that there is a sense in which Popper's thesis becomes but one aspect of a subjectivist's view. To see this, one must recognise an

important point of difference between the two philosophies.

Whereas the latter is concerned with a holistic and dynamic view of science, by contrast the thesis of Popper reflects a narrow decidedly static view of science. One might accordingly conjecture that Popper's thesis represents but a moment in time in a subjectivist model of scientific development.

To carry the metaphora little further, it is as if Popper has sought to analyse a piece of a fruitcake in an attempt to determine the existence of the fruitcake's major ingredients. He is able to speak "objectively" insofar as the slice is concerned but not the cake as a whole for it may be that a certain ingredient did not permeate in equal proportions in each slice of the cake. Similarly with scientific development. He may be able to speak objectively about the fact that scientific development may have occurred in a certain way over a defined slice of time but not about all of time.³⁸ Pointing among other things to the fact that Popper has chosen time periods convenient to his argument, Kuhn commented "*I suggest then that Sir Karl has characterised the entire scientific enterprise in terms that only apply to its occasional revolutionary parts*" (Kuhn, 1970, 6) and as a result "*Rather than a logic, Sir Karl has provided an ideology, rather than methodological rules, he has supplied procedural maxims*" (Kuhn, 1970, 15) both of which fall short of his proposed general

38. *To carry this a little further, if as Popper proposes his is a general explanation of scientific development, then Popper may be subject to the criticism which he himself used against those proposing an inductive logic, for he too cannot build a general explanation of scientific development using slices of time. In other words proceed from the specific to the general.*

explanation of scientific development.

To return to the original reason for re-comparing the philosophies of Popper and Kuhn, that is, to determine whether simultaneous reference to both by Chambers was contradictory, it is now apparent that it was not. However, in spite of this new evidence, Chambers does not get off the hook, so to speak, for establishing the compatibility of Kuhn's and Popper's views only serves to draw attention to the contradiction between the Received View and the new allies. Between these options there is no possible reconciliation of differences owing to the fact that the positivists continue to entertain what from the other point of view is a quite unrealistic assumption that knowledge can be strictly objective.

Chambers has recently attempted to skirt this problem however, by stating that references by him to empirical and formal propositions (1966, 34) were never meant to imply that these were mutually exclusive, that there was always an implicit assumption on his part that observations were not theory neutral and vice versa. Indeed several references to Popper, who was shown earlier to have altered his view regarding the attainment of strictly objective knowledge, might be interpreted as giving some substance to this rationalisation. However, one cannot but be sceptical of this present-day rationalisation given the fact that the model he produces in his 1966 work³⁹ clearly follows the logical positivist scheme in which knowledge is portrayed as being strictly

39. See Chambers (1966) page 34.

objective.

Notwithstanding the difficulty encountered in trying to establish Chambers' current philosophical position, owing to his reluctance to ally himself with any one school and a natural tendency to rationalise, one is nevertheless able to point to one thing about his work up to the 1970's, namely the predominant influence of the Received View on his view of theory construction. Given this fact and two others; first, that the view Chambers adopted was by the time he used it considered "... *inadequate as an analysis of scientific theories*" (Suppe, 1974, 4), and second, that he was aware of alternative explanations one may begin to comprehend one of the problems which this thesis is trying to demonstrate, namely, that research into this area has been half-hearted in the sense that it has been lacking in scope even by those who have showed most interest. As a consequence of this, two things become apparent. First, Chambers' work is in violation of what would be one of his own principles, namely "*if something is worth doing it's worth doing well*". Second, that accounting's experience with this area of research lags unnecessarily behind current thoughts on the matter.

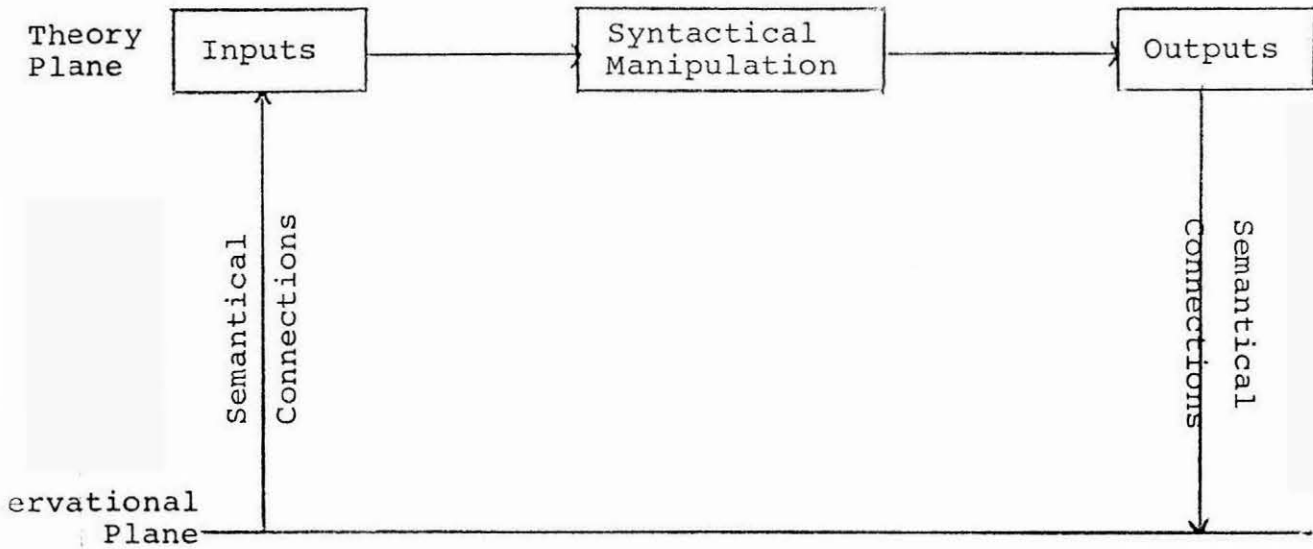
Sterling's Perspective

Further support for the lag idea is more acutely demonstrable with respect to the work of Professor R. Sterling. Like Chambers' work Sterling's work up to the 1970's can be shown to have been liberally distributed with positivist sentiments. However, after this point the works of Chambers and Sterling differ to the extent that Chambers' work contains few philosophical overtones

while Sterling's work continues to espouse support for the positivist model of theory construction. Why Chambers has not been disposed to continue this sort of investigation is a matter of conjecture which this work has not time to deal with. The point which is significant, however, is that given the fact that Chambers' reference to the Received View was out-of-date in 1964-1966 (the time during which he was writing and publishing his 1966 work), one can only wonder why Sterling, who had a background in philosophy, continued to use a model which was by the late 1970's acutely outdated. One rather uncomplimentary explanation which supports an earlier idea of half-heartedness on the part of people undertaking this sort of research is that Sterling was 'cashing in' (in the sense of publishing) on ideas which most people knew little about. Thus he was not disposed to correct the inadequacies which he himself saw in the system.

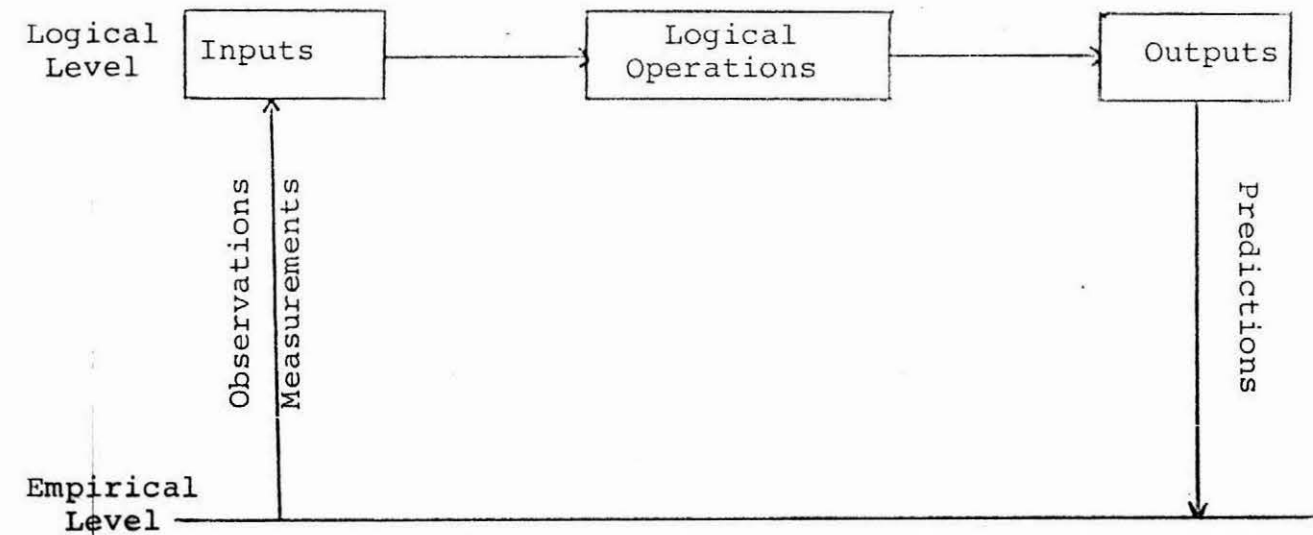
Although this explanation is admittedly scathing, it is not altogether unfounded. This is borne out by the fact that, although Sterling acknowledges in his (1970b) work the work of Hanson who showed observations to be theory-laden, he continued in his other publications in the same year and as long as six years later to make use of a model which by implication denied this notion.

The model in question is portrayed in the following diagrams:



(Sterling, 1970a, 448)

This diagram is but a simple variation of Margenau's model which reappears once again, slightly modified, in Sterling (1976).



(Sterling, 1976, 16)

An additional criticism which can be levelled at Sterling is his tendency to confuse, if not himself, then others, with allusions to, but no subsequent analysis of, the directly contradictory works of Kuhn and Popper. Confirming this, Gaffikin, referring specifically to Sterling's (1970a) work, commented "As with many other

of Sterling's works...there is an early allusion to Kuhn with no subsequent Kuhnian analysis...the rest of the paper is devoted to explaining a logical positivist view" (Gaffikin, 1978b, 103).

A General Perspective

Although it is true that Sterling's work differs little from Chambers' work in the sense that they both display an affection for the objectivist view of science,⁴⁰ Sterling's explicit style, despite exposing him to more criticism, seems to have won him more acclaim in the sense that his views are more in keeping with the majority view. Indicative of this fact are comments of the sort "*The theoretical work of Sterling is fairly representative of the notions of theory construction of accountants*" (Gaffikin, 1979, 10). This comment also indicates that the positivist sentiments which pervade Sterling's work also pervades the work of many others. Additional evidence of the wide acceptability of Sterling's work can be seen in the extent to which his work has been given quasi official status. On this count, it is not difficult to see that Sterling's work serves as the basis for three American Accounting Association committee reports: (1971), (1972) and (1977).

For his efforts in pioneering the consideration of external research (scientific) methods in accounting theory, Chambers has

40. *On the basis of Sterling's support for positivism we may however distinguish their works by assuming on the basis of this support that Sterling believes in strictly objective knowledge which we have tried to show that Chambers does not.*

been attributed little recognition. Despite the fact that Yu maintains that his (1955a) article "... *has since rendered a tremendous impact on accounting thinking*" (Yu, 1976, 99), the popularisation of methodological debate in accounting is generally accorded to an article by Carl Devine which appeared in the 1960 Accounting Review entitled 'Research Methodology and Accounting Theory Formation'. This work which proposed that the scientific method was a logical structure composed of an interaction between deductive systems and the philosophical doctrine of empiricism was to set the scene for wide-ranging methodological debate.

Bedford and Dopuch (1961) provided the first response to Devine's work. They criticised the way in which Devine segmentalised accounting research needs. They argued that Devine's four way segmentation into the logical structure and deductive systems, measurement and induction, behavioural research and welfare, and normative responsibilities, oversimplified the situation. They suggested that the latter three were not independent of each other. They did however, in a most significant silence, implicitly substantiate Devine's separation of the first area of concern from the others. This indicates that they supported the logical empiricist notion that formal aspects of theory could be separated from empirical aspects; in other words, that observations were free from theory bias. Further example of their logical empiricist stance is apparent in their references to the methodological prescription of Woodger (1939) which contained a strong positivist flavour.

Since these initial contributions, numerous articles and a handful of books have dealt in varying degrees with the subject of methodology⁴¹ but, as Gaffikin correctly observes, most of these have added "... *little new information*" (Gaffikin, 1978b, 42). They have been by and large entrenched, either explicitly or implicitly, within the philosophy of the Received View. Three works which are not subject to this stereotyping, however, are works by Tilley (1972), Wells (1976) and Mattessich (1978). By contrast, their works are revolutionary in character. This is especially so in the case of the latter for in his latest, perhaps most notable work he unpopularly implies that accounting theorists need to place more emphasis on epistemological research if they hope to be part of a progressive discipline. However, before considering the latest contribution of Mattessich, it is important to consider some of his early work.

This is important for two reasons. First, because it needs to be shown that his later work is but a follow-on from his earlier work. And secondly, that his early work differs little from the work of many others who now brand his work as radical and therefore undeserving of recognition for accounting purposes.

41. For brief discussion of the substance and philosophical lines of many of these works refer to Gaffikin (1978b). Gaffikin's work demonstrates, among other things, that the four best known and widely used texts on the subject of accounting theory by McDonald (1972), Yu (1976), Hendricksen (1977) and Most (1977) all support a positivist interpretation of theory.

Establishing a Contemporary View of Scientific Development

Mattessich's Perspective

Mattessich's work, like that of Chambers , spans a thirty-year period beginning around the 1950's. Whilst this passage of time has led Mattessich and Chambers in two apparently separate directions, at the outset the goals of both can be shown to have strong resemblances. Confirming this idea, Gaffikin writes, *"The pattern of the theoretical contributions of Mattessich comes closest to that of Chambers , initially"* (Gaffikin, 1978b, 90). Not surprisingly then, Mattessich was primarily interested in providing more rigorous theoretical foundations for the discipline. Toward this end, he insisted on the use of mathematical formalism and also sought to relate the theoretically more advanced discipline of economics to accounting. A series of articles dealing with those two aspects culminated in his major accounting-related work Accounting and Analytical Methods (A.A.M.) in 1964. Amidst a discussion of various themes, including empirical science, economics and management science, this work places particular emphasis on the subject of measurement theory.

In this endeavour he has not been alone. Both Sterling and Ijiri have made extensive recourse to the theory of measurement in developing their general theories of accounting. Using Ijiri's work which is by far the most extreme, in that it considers accounting to be merely a sub-field of measurement theory, this paragraph will deviate slightly from discussing Mattessich's contributions to point to the problems with this excessive concern

for measurement. Using the positivist theory construction techniques of axiomatisation, a separate theoretical and observational language and inductive logic, Ijiri (1967) has to his satisfaction developed a general theory of accounting. To this an objection must be registered, however, because with the exception of Ijiri himself theorists have not defined accounting in the way Ijiri presents it - as a sub-field of measurement. Rather, they have defined it as exactly the opposite. On this interpretation then, Ijiri has not developed a general theory of accounting at all and in terms of the Received View to which he shows great allegiance he has only concerned himself with their rules of correspondence. Similar sorts of criticisms are also applicable to the early works of Mattessich and Sterling.

Continuing with a description of Mattessich's early work, it is significant to note that Mattessich in A.A.M. develops an axiomatic model of accounting based around eighteen basic assumptions. Commenting on the use of axiomatic formalism, however, Suppe (1974) argued that if formalisation of theories was to play a part in their construction, then it would probably be a semantic formalisation rather than an axiomatisation. Accordingly, for the early works of Mattessich, Ijiri and many others, it seems doubtful whether in fact they were scientifically theorising or *"merely formally describing extant practice"* (Gaffikin, 1978b, 120). Contingent upon this idea, one finds difficulty in associating the title 'competent research effort' with these works for as one author noted, *"Research is not merely an exposition of practices currently followed, nor a description of techniques."*

It involves, for example, a philosophical examination of the hypotheses upon which practices are based, investigation of the validity of those hypotheses, experimentation with alternative methods, testing the validity of alternative hypotheses, and similar objectively critical enquires" (Fitzgerald from Zeff, 1973, 34).

Since the publication of A.A.M., Mattessich has published only three of his many works on the topic of accounting theory construction. They are his (1968), (1970) and (1972) works. Indicative of the philosophical stance adopted in his 1968 work is the comment "It is to be hoped that accounting contains a core of propositions that are open to refutation and that lend themselves to testing and verification". (Mattessich, 1968, 3).

Although there is a hint of Popper in the above quotation, and other references in the rest of his work to Kuhn, the idea of verification is indicative of the fact that the work is largely couched in the philosophy of the Received View. This stance is continued in his (1970) and (1972) works for which in the latter case he was awarded the American Accounting Association Significant Contribution to Accounting Literature Award. Both works stressed two aspects. First, the need for increased methodological rigour in the development of accounting theory, and second, the need for systematic testing to enable verification or refutation. In Mattessich (1972), he writes "*Systematic testing procedures are a crucial element in a scientific approach*" (479) and further that "*...testing of a general accounting theory by way of empirical verification of its interpreted systems, seems to me the*

only way of conforming to the requirements of an empirical discipline" (486).

Evident from the investigation so far is that Mattessich's work, although often overlooked because of its present direction, stands alongside the "best" of the theoretical works in accounting in the last thirty years. Although many discount his current work because it moves away from accounting per se, it is the present author's contention that Mattessich's move was not only a necessary one, but one which Mattessich himself shows has been called for for more than a decade by many accounting sources. Among these sources he lists Williams and Griffin (1969), Devine (1971), the Report of the Committee on Foundations of Accounting Measurement (1971) which stated at one point that the "...*practice of accounting ultimately belongs to the science of knowledge*" (1971, 40) and the Report of the Committee on Accounting Theory Construction and Verification. This latter group commented in 1971 that "*Until recently, accounting theory largely consisted of a set of a priori arguments about the relative merits of alternative accounting measurements. Currently there is a growing body of research that is trying to establish an empirical tradition in accounting theory development and verification. A full understanding of such research requires that the purpose of such research be viewed within the context of the history of the scientific method* (1971, 77) (Emphasis added).

Moving one step further and demonstrating where the philosophy of science fits into this picture, Lakatos, a well-known

philosopher of science held that the philosophy and history of science were inseparable. He commented that in constructing an historical case study *"one should adopt the following procedure: (1) one gives a rational reconstruction; (2) one tries to compare this rational reconstruction with actual history and to criticise both one's rational reconstruction for lack of historicity and the actual history for the lack of rationality"* (Lakatos, 1970, 138). Thus, in Lakatos' view, any historical study must be preceded by a heuristic study. Putting it more succinctly, the "history of science without philosophy of science is blind"⁴² (Emphasis added).

Following these but more importantly his own convictions in favour of *"...an integrated picture of the foundations on which...research must ultimately rest"*, Mattessich in his latter works, particularly his latest - Instrumental Reasoning and Systems Methodology (I.R.S.M.) - has moved right away from accounting (though it is still mentioned periodically) in favour of developing a systems methodology for all applied sciences of which accounting is but one. Developing this philosophy of applied sciences is the first of two central themes, the other being *"...to acquaint the adepts of these applied fields with major problems of the philosophy of science (epistemology) in general"* (Mattessich, 1978, 1). Mattessich implies that for too long

42. Given then from above that the history of the scientific method is important for accounting (which by itself suggests a view of science contrary to that of the logical positivists) and Lakatos' idea about the inseparability of historical and heuristic study, it is evident that philosophical (epistemological) study is called for in accounting.

short-run needs such as those of accounting practice have been favoured over long-run considerations e.g. securing accounting's foundations and that *"Only slowly are we willing to face the overwhelming evidence that long-run thinking is more urgently needed today than ever before"* (Mattessich, 1978, 3). In facilitating this later purpose, he maintains that the study of philosophy is eminently suited. Among the benefits of this study he cites: a disinclination to use shallow reasoning, the revealing of implied value judgements, separation of actual and intended goals and the ability to see the limitation of conceptual tools and methods.

Mattessich is not however, naive in his support for philosophical study. He recognises that for most scientists, whether they are physical or applied scientists, epistemology has little or no significance. He comments *"Scientists, even successful ones, who care little and know nothing about epistemology are legions..."* (Mattessich, 1978, 2). Mattessich contends however that the positive or negative recognition of the need for epistemological study will determine the sort of scientist one wishes to become. He maintains that the truly eminent scientists have been those who have devoted *"...considerable time to the exploration of connections between their own efforts and the overall scheme of scientific endeavour"* (Mattessich, 1978, 2).

Although it may be somewhat presumptuous to compare the eminence of accounting scholars to scholars in physics etc. e.g. Einstein, it is nevertheless interesting to note that three people who

have been touched with the brush of eminence in accounting have been Chambers, Sterling and Mattessich himself. Significantly all three have won distinguished awards⁴³ for their early efforts which among other things explored the connection between accounting and "scientific endeavours". Although they perhaps did not concern themselves with what Mattessich terms the 'overall scientific endeavour', the early works of these theorists show movement in that direction and as such bear witness to Mattessich's claim that truly eminent individuals will be necessarily drawn to the study of epistemology. Mattessich further comments "*Whoever chooses any scientific calling out of genuine curiosity, will find it difficult to stop his inquiry at the threshold of his own discipline. His inquisitiveness will lead him to at least to a rough understanding of neighbouring as well as foundational areas*" (Mattessich, 1978, 2) (Emphasis added).

One last salvo which Mattessich delivers in support of the need for epistemological study is one which strikes at the heart of the empirical scientific tradition. He comments "*...empirical science as a system of information and knowledge creation is still in a suspended state, it literally hovers above its foundation without being sufficiently tied to it*" (Mattessich, 1978, 4). Adding to this, Mattessich maintains that science's strongest justification has been that it is rational and consistent but questions where this rationality and consistency begins and ends. He then demonstrated by example (page 4) the

43. *Either distinguished lecturing or writing awards.*

shallow manner in which the term consistency is viewed to bring home the point that even the most accurate procedure is inadequate, if not consistently carried out to the very end.⁴⁴

With this reasoning base behind him, Mattessich launches into a discussion of a number of issues. These include a discussion of the use of systems analysis as a tool of philosophical investigation (Ch. 2), the evolution and integrity of deductive and inductive logic (Chs. 3, 4, 5), the methodological endeavours of economists (Ch. 6) and finally ideas from the philosophy of science (Ch. 7). Mattessich's last chapter, chapter seven, is not unlike the fourth section of this current thesis in that it presents a brief history and summary of a variety of philosophical ideas.

In this last chapter, Mattessich, in keeping with his goal to substitute a narrow perspective for a more holistic one, attempts a reconciliation of formalist philosophies with the philosophies of the Weltanschauungen analysts. It is at this point that his 1978 publication differs substantially from his earliest works. Rejecting the formalist approach of the Received View outright, a view which we saw him to be very much in favour of in his early works, he appears to have moved in favour of a formalistic interpretation of Kuhn's thesis. He uses the work of Sneed (1971) and Stegmuller to reconcile, in the manner described earlier by Lakatos, heuristic study with historical study. It is worth noting that a similar sort of endeavour was apparent in section four and the

44. *This was essentially the argument used to question the works of Chambers and Sterling.*

current section of this thesis when discussing the works of Doppelt and Chambers, respectively.

Examples of Reconciliation

With regard to the former, Doppelt demonstrated what while Kuhn's notion of revolutionary progress was a better short-run explanation of progress in science, the objectivist view (excluding the justificationalists) that knowledge was cumulative, though disputable in the short-run, was nevertheless an entirely adequate explanation for the long-run.⁴⁵ Explaining the basis of this difference Doppelt maintains that this in part results from the fact that the two explanations tackle different problems or aspects of science. Whereas the objectivists prefer to compare more or less finished theories, Kuhnian exponents prefer to investigate the complex structure of their theories' actual internal development. Having been acquainted with the benefits of Kuhn's thesis by Doppelt, benefits which briefly stated are that his thesis supplies powerful arguments against some objectivist models of scientific development as well as supplying a better explanation of short-run progress in science, the thing which has to be decided is whether there is any benefit to be gained from considering a heuristic component in theory development. Sneed argues in support of this dualism, (as Lakatos was seen to do earlier) but the formalistic presentation he has in

45. *There is an apparent contradiction in this statement in the sense that earlier it was reported that they were not reconcilable. However, the contradiction is only semantical and not substantive for the reconciliation above is such that it renders the formalistic notions of the positivists useless except in the illusive and undefined long-run.*

mind is based around what Suppe referred to earlier as the more acceptable semantic formalism rather than axiomatisation. Mattessich describes Sneed's formalism as "a rigorous set - theoretical presentation of theory".

Essentially Sneed's formalism requires the specification of three things. The theory (T), a core (K) and a set of intended applications (I). The theory presented as an ordered pair would be:

$$T = \langle K, I \rangle$$

The core, K, is comprised of a set of models, M, a set of possible models, Mp, a set of possible partial models, Mpp, a set of possible constraints on these models, C, and a set of variables such that

$$K = \langle M, Mp, Mpp, C, v \rangle$$

and $T = \langle M, Mp, Mpp, C, v, I \rangle$

(Sneed, 1971, Ch. 2)

Sneed argues that in order to get acquainted with a newly encountered theory one needs to get hold of some basics and learn a series of pertinent applications at the outset. Reconciling this with Kuhn's notions of normal and revolutionary science, Sneed maintains that in periods of normal science the hard core will simply be expanded by new applications to get E. The expanded core would also contain a set of new constraints, C_a , a set of additional functions, v_a , and a set of special laws for corresponding applications, L. In this case

$$E = \langle M, Mp, Mpp, C, v, L, C_a, v_a \rangle$$

and $T_E = \langle M, Mp, Mpp, C, v, L, C_a, v_a, I \rangle$

Alternatively, in times of revolutionary science the hard core will be exchanged for a new one. Commenting on the tie-up between Sneed's and his thesis, Kuhn stated "*A core, in short, is a structure that cannot, unlike an expanded core, be abandoned without abandoning the corresponding theory*" (Kuhn, 1976, 187).

Rounding out the rationale for his reconciliation, Mattessich in his turn comments "*In this way Sneed's structuralist approach permits a formalistic representation of the notion of revolutionary progress as well as Kuhn's distinction between normal versus revolutionary science*" (Mattessich, 1978, 267).

The second example in which, in the absence of knowledge about Mattessich's work, we attempted to reconcile formalist and historical explanations of science concerned the work of Chambers. Chambers in his 1966 work made reference to philosophical explanations from both an objectivist and subjectivist standpoint. At the outset this dual reference appeared to be contradictory. Indeed it was to the extent that Chambers concerned himself with the formalism of the Received View. However, it was pointed out that Chambers' view of objectivity was on his own interpretation more like that of Popper's than the positivist's. Accepting this as a modification of position, it was then demonstrated that the gulf between the objectivist view of Popper and the subjectivist view of Kuhn was not as acute as at first thought. Demonstrating his support for reconciliation based on a change of position, Mattessich commented "*It seems that in Feyerabend's later publications, the Weltanschauungen analysis reached its limits. Thus it is hardly surprising that the new synthesis* (referring to the

Sneed/Stegmuller structuralism) is found not in a continuation of this extreme subjectivism and philosophical nihilism, but in the quest for a balanced view about the interplay between the subjective and the objective" (Mattessich, 1978, 265) (Emphasis added).

On the basis of this limited review of Mattessich's latest work, there is no doubting the fact that, despite making only a few cursory references to accounting, it nevertheless presents accounting theorists with a significant challenge. The message is clearly to either retool with epistemological devices to try and facilitate some commongrounding or to continue to bear the brunt of professional scorn for failing to come to some agreement about what an accounting theory should look like. Contrary to a widely held belief though, the challenge which Mattessich puts out is not new, merely revitalised; consequences of which are that it is more obvious in its criticism and less polite in its respect for disciplinary and ideological boundaries.

Although as was stated earlier the writings of several individuals and groups had been pointing in this direction for some time before Mattessich's 1978 work, two publications stand out. The first stands out because it presents an example of the difficulties which Mattessich would contend accounting theorists will continue to face unless they adopt a holistic attitude to the study of their discipline. The second work stands out as an example of the sorts of insights and ideas which can be accomplished if one considers a wider epistemological frame of reference.

Two Supporting Works

The first work is by an Australian writer, Ian Tilley. The title of his (1972) work alone 'Accounting as a Scientific Endeavour: Some Questions the American Theorists tend to leave unanswered' is suggestive of the less than adequate approach which American accounting theorists, perhaps the most active theorists overall, have adopted toward the accounting/science controversy. Tilley lists a number of questions indicative of which are "*What distinguishes science from non-science? (and) Can a useful decision even be made between what is scientific and what is not?*"

(Tilley, 1972, 288). After discussion of the various questions, Tilley concludes in a long but significant statement that "*It is in the validation of propositions that the A.A.A. and a number of individual American theorists hope to see the benefits of science but strangely they have not inquired into the nature of scientific discourse - in particular how a scientific explanation differs from and is 'better' than a non-scientific one, what is the meaning of the 'scientific method' and whether it provides clear guidelines to base an accept/reject decision on the hypothesis generated by researchers... Exactly what 'science' means is still unclear in the philosophy of science. And yet many American academics proceed as though these questions had been adequately resolved...*" (Tilley, 1972, 297)⁴⁶

46. While Tilley speaks only about American theorists, it must be remembered that theorists other than Americans, at least in the opinion of the author, are equally subject to his criticisms.

Having noted in the title of his work that the questions Tilley raises are questions theorists have left unanswered, Tilley appears to support by implication Mattessich's contention that those in applied disciplines, in this case accounting, should involve themselves more directly in these questions and hence epistemological analysis. Indicative of the wide support with which the sentiments of Tilley are held, Gaffikin commented:

"It (Tilley's work) summarises (not intentionally) the theoretical articles in leading journals. It presents ideas found in A.A.A. (1971a), A.A.A. (1971b), Penman (1973), Iselin (1971) and Yu (1976)" (Gaffikin, 1978b, 49).

The second work also comes out of Australia but is written by a New Zealander, Professor Murray Wells. This work, which appeared in July 1976 was the first to discuss, in anything but a cursory manner, the association between Kuhnian analysis and accounting. The work appeared to be primarily designed to provide a rationale for the various inflation accounting methods but in the process succeeded in creating an intuitively appealing image of Kuhnian analysis. After summarising, in literal fashion, the details of Kuhn's thesis in the first section of the work, Wells likens the various inflation accounting methods to Kuhn's schools of thought. He then attempts to outline a possible disciplinary matrix and conjectures that accounting may be in Kuhn's crisis stage, resulting from the professional insecurity brought on by methodological debate. Although Wells comes to no conclusion (nor did it seem he intended to), he does present some interesting arguments. A similar work was produced by Diana Flamholtz in the

same year but her work has not been credited with the same success as Wells'. There seem to be two reasons for this. First, the fact that she was pipped at the post so to speak by Wells' publication and second, more importantly, because unlike Wells, Flamholtz's attempt to relate Kuhn's analysis to financial accounting as a whole was considered too ambitious, a fact which caused Gaffikin to state "...it is probably for this reason that her argument is unconvincing her scope is too broad" (Gaffikin, 1978b, 50).

Forgetting this difference however, both works appear to enter into the spirit of Mattessich's challenge - to carry out research on a front unrestricted by disciplinary or ideological boundaries. Unfortunately, since the publication of these two works, little has been done either by the authors themselves or by others to develop and explore these ideas in full. In figurative terms it is as if one had breached the opposition's defence but failed to take advantage of the opportunity to make headway. Perhaps even more damning is the fact since these works have been published, the comparative rate of these normative publications appear to have fallen off as compared to empirically based works which continue to flood accounting literature.

The suggestions here is not however that empirical research should be done away with; rather that people become aware of the inadequacies of this sort of research and the potential benefits of alternative research methods. To try and state the problem succinctly, empiricism may result in the refining of a given idea but once that refining process has been completed,

empiricism only succeeds in a continual self-confirming of the refined idea. No successful questioning of the system, of which the idea is part, can be made. Such is the situation with historic cost. One could say that refining has taken place to the extent that we now have adjusted historic cost but the notion of historic cost itself as a complete system has not been successfully questioned by empirical study nor is it ever likely to be because empirical research cannot directly give rise to, or support novel alternatives, a feature necessary before the relinquishing of the old method will take place. This is so because the alternatives are necessarily prescriptive as compared to historic cost i.e. they are not in use. This being the case, one must look for other sorts of explanations of these alternatives other than empirical explanations. Such is the objective of investigation into new research methods.⁴⁷

A Necessary Prerequisite for Change

On this interpretation then there is a definite need to shy away from uncritical empiricism.⁴⁸ However, the task suggested is not going to be an easy one, schooled, indeed indoctrinated, as many

47. *It is not appropriate here to become involved in a detailed discussion of the deficiencies of empiricism or the advantages of other research methods. For this purpose, the reader is advised to refer to Feyerabend (1962) (1965) and (1969) which cover this discussion admirably.*

48. *Uncritical not in the sense of willingness to argue against an empirical position for this has become a chronically absurd obsession which more often than not increases rather than decreases confusion. Rather, uncritical in the sense of coming to grips with the inadequacies and limitations of the empirical research method and being open-minded towards new research methods.*

people are in its method. It is not an impossible one however and in fact the answer to it is implied in the last sentence by the word 'schooled'. This word highlights the fact that education plays an important part in the establishing and entrenching of ideas. As Sir Alexander Fitzgerald wrote in 1940 *"The kind of research that can be done by accountants depends upon the kind of education they receive"* (Birkett, 1972, 45). Contingent upon these ideas change demands a redirection in the education system. In this particular case however, successful change demands re-education of the educators themselves. Teachers must be taught that in a dynamic world one must adapt to change and that a necessary prerequisite for adaption is the movement away from the teaching of dogma and perhaps more importantly dogmatic teaching (teaching which discourages a student from being critical). Implicitly advocating this solution, Chambers commented: *"The history of the development of knowledge cautions us against overrating the extent of our knowledge at any time, and against too strong a commitment to it"* (Chambers, 1966, 365).

Only when these notions have been successfully demonstrated to, and accepted by teachers, will accounting students, the academics and practitioners of tomorrow begin to reap the benefits; perhaps the most important of which is to learn to unlearn. For the purpose of facilitating this task epistemological research seems eminently suited. It will not only encourage the process of unlearning but aid in the foundations of new learning by simply showing how knowledge grows, in particular, giving some idea of what we can and cannot know. That this will prove extremely

valuable in terms of upgrading our system of education is implied by Sir Alexander Fitzgerald who wrote "...the kind of education that is possible depends upon the quality of the research that is done" (Fitzgerald, reported in Birkett et al, 1972, 45). What better way to improve the quality of research than by investigating its very nature; by making use of the field of knowledge (epistemology) which deals with ways of thinking (researching) about problems.

To ensure that such a change in our education system comes about quickly, effectively and with the smallest amount of resistance, it is essential that both the theoretical and practical branches of the discipline are united in their support of this move. However, before this is likely to eventuate, academics and practitioners need to become aware of what methods and techniques the other is using. Indicating that this mutual awareness is vital for the future of the profession, Zeff, as a conclusion to a work dealing with practitioner research, commented: "*The interests and capabilities of accounting academics today are vastly different from those of only ten years ago, and it is important for the future of the profession that academics and professionals understand each other's concerns and methodologies*" (Zeff, 1973, 63).

An Implication for Professional Research

Having familiarised ourselves with the latest endeavours of accounting academics, following Zeff's comment, it turns out to be useful to take a brief look at practitioner attempts along these same lines. This effort proves useful for two reasons.

First because it indicates just how practitioners go about conceptualising and secondly, whether current academic endeavours provide any useful source of information regarding the consistency of such endeavours. To answer these questions, this work will take as example the current attempts (methods) which the American Institute of Certified Practising Accountants (A.I.C.P.A.) through their Financial Accounting Standards Board are making (using) to aid this task.

The method of research adopted by the A.I.C.P.A. is based upon two simple features. First, a synthesis of the main ideas from the literature and second, a reliance on public comment in the form of questionnaires, written submissions and participation in public debate. In these endeavours their efforts have been exemplary. Unfortunately however, it is apparent that these efforts are premised on an inductive logic. From the fourth section of this thesis, it was noted that Popper questioned the tenability of such an approach. He even went as far as to deny the existence of such logic. Additionally, Gaffikin has commented: *"In the sense of achieving some theoretical truth the F.A.S.B. efforts are in vain for it cannot be established by fiat"* (Gaffikin, 1979, 14). Commenting in similar fashion, the American Accounting Association Committee on Concepts and Standards for External Reports stated as one of its major conclusions that *"...theory closure cannot be dictated"* (A.A.A., 1977, 44). Both of these qualifying remarks result from academic attempts to come to an understanding of the theory construction process. Clearly they establish the problem with current practitioner undertakings

and in the process the usefulness of current academic endeavours as well as the usefulness of a comparison between the two. This in turn supports Zeff's notion that each side must make a concerted effort to get to know the other or else no comparison or, perhaps worse, misleading comparisons will be made.

Some Support for Due Consideration Being Given to Feyerabend's Thesis

One of the central themes to emerge from the current section has been the need to take a holistic but balanced view of the interaction between accounting and the philosophy of science. While there is evidence to suggest that accounting is heading in that direction, having given consideration (limited as it is) to ideas from three of the four theses discussed in section four, there is no evidence at all of attempts to determine the significance, if any, of Feyerabend's thesis in accounting.⁴⁹

In section four of this thesis, a significant amount of space was accorded Feyerabend's thesis in an attempt to determine whether there was any substance to it. After reviewing three significant attempts to discredit his thesis, it was concluded that Feyerabend's work was largely unscathed. Furthermore, it must be noted that all three of his critics were in fact complimentary of much of his work. To sum up the essence of their criticism, they were critical of the fact that having discovered several significant

49. *There are however indications that some authors have encountered problems with respect to accounting and its latest research endeavours of a similar type as that discovered by Feyerabend. For a discussion of this, refer to an explication of a work by Hakansson in Aitken, M.J. (1979).*

problems with traditional notions of scientific development, Feyerabend had mistaken his problems for a solution. As one critic writes: *"It is such a great pity to find that Feyerabend, having discovered a beautiful problem regarding the function of scientific theories, and about the use of intellectual standards in science, mistakes his problem for a solution to it, and pursues it no further"* (Hattiangadi, 1977, 302).

With the knowledge that Feyerabend's work has much to offer the philosophy of science, if not as a finite solution, then at the very least as a bona fide source of problem identification, one is thus left to decide its relevance in accounting. Given that Feyerabend's thesis is a theoretical alternative, and the fact that one cannot provide any practical support for this type of alternative, the only option open to one is to provide some rational support. However, while the term 'only' may suggest to some, particularly empiricists, that this is second best support, the message of this thesis and indeed the particular arguments to be put forward dispute this claim.

The first argument rests on the combination of two well-supported propositions. First, the fact that there has been a general recognition since the 1950's, at least among the academic branch of the discipline, that scientific research is needed in accounting and secondly, the old but nevertheless significant adage, that "if something is worth doing it's worth doing well". In the immediate case, if scientific research is worth considering at all, it is worth full and proper consideration. Anything less than this can

only succeed in defeating the purpose for which scientific research was desired in the first place, namely, to attempt a more rigorous study of accounting.

A second logical reason which may be advanced in support of considering Feyerabend's thesis in accounting becomes apparent in answer to what is a frequently voiced criticism of philosophical study in accounting. Namely, that accounting seems to have progressed to date in spite of its lack of concern for philosophical study. One may conjecture however that this progress is more a result of the natural vigour possessed by the patient than of the helpfulness of advice given by the doctor. Consequently, it does not seem unreasonable to posit that accounting might have been a lot further along the road of academic maturity than it is today, if explanations of scientific progress such as Feyerabend's were given due consideration.

Having tentatively established that Feyerabend's thesis may be worth some consideration in accounting, this does not however ensure that it will be considered for the arguments advanced were of a general rather than specific nature. As such, they could equally well be used in support of investigating any idea in accounting. While it is contended that such an outlook should be an accepted policy of all disciplines interested in progress, it should also be made clear to those who would prefer to believe that such a policy will result in the loss of accounting's core identity that this is not to be feared.

Providing a rationale for this, Erwin Schrodinger⁵⁰ wrote: "*No scientific structure falls entirely into ruin; what is worth preserving preserves itself and requires no protection*".

Accepting this proposition however, does not mean that an individual must contemplate any and every idea for this is clearly impossible. The contemplation of ideas clearly follows some pattern or order of priority. This order of priority is often decided on how well the implications of the particular idea or, in this case, thesis suit the desires of the person considering them. Just as Wells provided some policy implications of Kuhnian analysis in accounting, it seems relevant to do the same thing for Feyerabend. It will thus enable some consideration in one's priority listing.

Some Policy Implications of Feyerabend's Thesis

Similarly sceptical of the traditional notion of science, Feyerabend agrees with Kuhn that theories developed in accordance with the "scientific method" do not ensure that knowledge is rational or progressive. However, Feyerabend takes exception to what he describes as Kuhn's own inability to show how knowledge might progress. Specifically, Feyerabend argues that to contemplate as Kuhn does, the absence of alternatives until anomalies have occurred in the current "paradigm", is to put the cart before the horse. Against this Feyerabend argues that important anomalies in the prevailing paradigm will only arise if alternatives are

50. Taken from Schrodinger's Science and the Human Temperament. Translated with biographical introduction, by James Murphy, Allen & Unwin, London, 1935.

continually contemplated; that where a single theory stands unchallenged by "a different set of equally embracing principles", it unlikely to be confronted with evidence that could ever be seen as constituting a serious threat to its basic adequacy. Such is the simple but nonetheless significant argument for Feyerabend's methodological pluralism.

Prior to demonstrating the major implications of Feyerabend's notion of scientific progress in accounting, it should first be noted that the term 'methodology' does not pertain to methods of accounting per se in the sense that to contemplate a different methodology is to necessarily contemplate a distinct method of accounting. On the formalist interpretation of Kuhn's thesis developed earlier in this section, one may see that although there may be several theories with cores, K, the intended applications, I, might very well overlap such that one gets something like the diagram in figure 1.

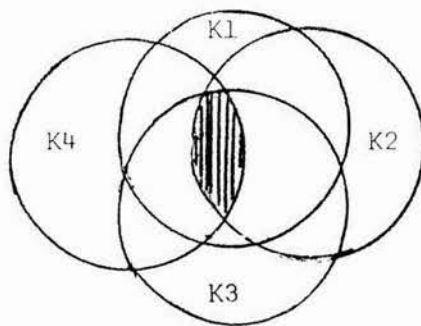


Figure 1

This overlap between the four different theories signifies that there is some overlap between the components of the various

theories. In an accounting context, the substance of this overlap might very well be the continuous historical record of a firm's transactions, a feature which appears to remain a constant in all the various theories. Thus, contemplating Feyerabend's thesis does not mean that one must necessarily contemplate no uniformity of method.

Given this preliminary remark, there are at least two major implications of Feyerabend's thesis for accounting. First, that contrary to current thought, the significant array of existing theories in accounting do not constitute a state of chaos in the discipline but rather a necessary prerequisite of scientific development. These various theories in accounting may be thought of as alternatives which constantly place major and minor aspects of the prevailing paradigm under scrutiny. Accordingly, it should be recognised that contemplating progress demands that one does not try and limit the number of alternatives but rather the reverse.

A second implication of Feyerabend's work is that accountants need to become more adventurous in their outlook. Specifically they need to be less protective of tradition and correspondingly more willing to thrust aside ideological boundaries and constraints.

For the sake of making this latter argument more plausible a qualification of the statement "accountants need to become more adventurous" is put forward. The substance of this qualification is that not every accountant needs to become actively involved in this research. Obviously this is an impossible ideal given the

fact that most accountants have an immediate practical responsibility. It is not however, an impossible ideal to hope that the efforts of those who do work in this field are understood, encouraged and supported by others, both from the standpoint of morale and financial support. To facilitate this understanding, once again it must be emphasised that the education system has a vital part to play.

Summary

This section has attempted to describe and evaluate ideas contained in various accounting works which have sought to establish a research tradition in accounting. Perhaps the point most apparent from this study has been that considerable concern and speculation has surrounded attempts to determine a suitable methodological base to aid the theoretical structure of accounting. With few exceptions however, the authors all appear to have agreed on two basic points. First, that a scientific approach was needed, and second, that development necessitated recourse to other theoretically more mature disciplines (e.g. Economics). Whilst agreeing about these general ideas however, there has been considerable differences between individuals with respect to their understanding of science and the extent to which they see the experiences of other disciplines being used in accounting.

Before the 1950's, references were frequently made to many of the more theoretically mature social sciences in establishing methodological prerequisites for theory development in accounting. After the 1950's however, references began to be made to more

primary sources of methodological prerequisites. This has involved many accounting theorists in the consideration of ideas from the philosophy of science. Again with few exceptions most accounting theorists appear to have followed in the footsteps of those in academically more mature disciplines who have opted for objectivist views of science, showing particular favouritism for the Received View.

Although the Received View continues to play a significant role in accounting theory literature, evidenced in particular by the work of Sterling (1976), the decade of the seventies has given rise to a number of works calling for change or, at the very least, justification of present research methods. Among a number of such works three were highlighted. In something of a controversial move, primary effort was directed toward discussion of a work which did not directly deal with accounting. The work was that of Richard Mattessich. However, in opposition to those who would argue against the use of Mattessich's work (and hence its relevance in this thesis) because of its non-accounting base, it can in fact be said to have been prompted by Mattessich's extensive accounting experience. Central to Mattessich's work was the implication that accounting theorists and teachers (often one and the same) need to free themselves from traditional ideological constraints and become actively involved in the study of epistemology.

The other two works discussed were shown to be representative of a number of works which had expressed, prior to Mattessich's

publications, support for the basic notions Mattessich subsequently expounds. Recognising the widespread opposition to this type of study, it was conjectured that the answer to breaking down this opposition was dependent upon the flexibility of the education system, in particular the willingness of all in the accounting discipline to cooperate toward mutual understanding of the need for research.

CONCLUSION

Accounting, according to Chambers "... has become the subject of theorising late in life" (Chambers, 1955b, 428). This fact has associated with it both advantages and disadvantages for accounting. One of the advantages is that accounting is able to learn from the mistakes of other disciplines, thereby saving itself a considerable amount of time and energy. A disadvantage, however, is that having not had the learning experience of the other disciplines, accounting to a large extent has its intellectual prestige decided by thinkers in other disciplines, who may or may not do potential accounting thinkers justice. Perhaps the most important disadvantage however is that if there is uncertainty in the discipline to which accounting looks for guidance, this will inevitably be reflected in accounting's development.

The evidence presented in this work bears witness to the fact that accounting's development has been anything but certain. From at least the 1960's when academics and practitioners en masse simultaneously began to voice their dissatisfaction at the lack of methodological guidelines for research and practice respectively, accounting can seldom be portrayed in a greater state of flux. And yet in spite of this flux, never before as close to a solution to its problems, for the two major factions in accounting (academic and practitioner) had at last begun to address the common problem, i.e. the lack of research.

The flux which has eventuated has resulted in particular from

the stepping up of academic efforts in the search for methodological prerequisites. This effort has involved a shift away from the methodological experience provided by the traditionally allied social sciences, in particular economics, to the more primary sources of methodological exposition. This in turn has involved theorists in the consideration of epistemological discourses from the philosophy of science.

Unfortunately however, in common fault with many more advanced disciplines, most accounting theorists who entered this field, adopted a view of science which had since the 1950's (ten to twenty years earlier) been the subject of severe criticism in the discipline from whence the original view came. This view was known as the Received View. Evidencing the extent of this criticism, one author commented *"These attacks were so successful that by the late 1960's a general consensus had been reached that the Received View was inadequate as an analysis of scientific theories"* (Suppe, 1974, 4). Although there have been several versions of this view, the essential characteristics of it were that -

1. it was generally thought that the observation (empirical) base of scientific theories was theory neutral and correspondingly unproblematic.
2. a link between the theory and observational planes was accomplished through a set of rules of correspondence which purported to give theoretical terms operational meaning.
3. theories were capable of and necessitated empirical confirmation or validation and
4. theories in order to qualify as scientific had to be

reducible to elementary statements of experience -
the reducibility thesis.

With regard to the first of these, recent work by a number of philosophers including Popper, Kuhn and Feyerabend has shown the untenability of this distinction. Reasons put forward to demonstrate this point include the fact that observations and facts are theory-laden and meanings theory-dependent. Commenting on this, one author stated "*Sir Karl [Popper] and I are united in opposition to a number of classical positivism's most characteristic theses. We both emphasize, for example, the inevitable entanglement of scientific observation with scientific theory; we are correspondingly sceptical of efforts to produce any neutral observation language*" (Kuhn, 1970, 2).

On the subject of rules of correspondence it has been commonly claimed that they are void of significance because they are not part of theory at all. Against the positivists who claim that these rules define and give cognitive significance to theoretical terms, as well as specifying rules by which theory can be applied to phenomena, one author commented that they are merely "... *auxiliary hypotheses of procedures for applying the theory to phenomena*" (Suppe, 1974, 103).

Popper's falsificationist thesis denies the third characteristic of theory verification and his arguments also defeat the positivist's fourth characteristic of theory reduction. Commenting about the untenability of this latter notion, Popper claimed that according to this notion, the positivists ruled out all science

including natural science *"For scientific laws, too, cannot be reduced to elementary statements of experience"* (Popper, 1959, 56).

With the increasing recognition that the Received View fails to provide an adequate account of the way in which scientific knowledge progresses, philosophers of science have been driven toward consideration of alternative explanations. These have ranged from the Popper's falsificationalist thesis through to the dadist philosophy of Paul Feyerabend.

The most notable of these alternatives was a thesis by Thomas Kuhn. Appearing as his thesis did between the two extremes is undoubtedly a feature which served to attract people to it. Other reasons for its widespread support include its dynamic rather than static view of science and its capitalisation on the introduction of the human element into philosophy. In this latter regard one author claimed that Kuhn's thesis *"brought man back into the picture and reminded people of a few conspicuous facts that tended to be overlooked by scientists and philosophers alike"* (Baumberger, 1977, 3).

In spite of the fact the Kuhn's thesis was shown to be a very attractive alternative among contemporary thinkers, it was evident that it had not and did not look like dominating philosophical thinking as the Received View had. Confirming this contention Suppe commented *"The situation today, then, in the philosophy of science is this: the Received View has been*

rejected, but no proposed alternative analysis of theories enjoys widespread acceptance" (Suppe, 1974, 4).⁴⁸ Given this state of affairs, it is little wonder that accounting theorists have not agreed about a suitable theory construction procedure or been willing to investigate the potential of these new epistemological explanations.

In spite of this situation however, a handful of theorists have begun to brave the study of epistemology. By and large though, these attempts have been understandably exploratory in nature. With the exception of one or two theorists, notably Wells and Flamholtz who entertained some policy implications of these theses for accounting, the works have been mainly concerned with suggesting that more effort needs to be devoted to coming to grips with the philosophy of science as a field of study.⁴⁹ Among those to implicitly or explicitly suggest this line (all of whom are assumed to be adherents of the notion that if something is worth doing, it is worth doing well) have been Chambers (1966), Sterling (1971), Tilley (1972),

48. *This bears remarkable resemblances to the current state of affairs in accounting as between historical cost and its alternatives. The major alternatives, in the philosophy of science however, conjecture explanations for this state of affairs and so for the reason that they may provide insights into this situation, the study of them seems worthwhile.*

49. *Until this is done, policy implications such as those entertained in this thesis cannot hope to be anything but novelties for the purpose of encouraging future epistemological training and analysis.*

the American Accounting Association (1971), Devine (1971), Williams and Griffin (1969) and Mattessich (1978).

Of these works, Mattessich's is undoubtedly the most extreme and understandably the most controversial. Part of this controversy originates from the fact that in his latest work Mattessich accords no special place for accounting. Accounting is just one of a number of social sciences to which Mattessich commends the study of epistemology. Accounting can however claim special allegiance to his work, for it springs from Mattessich's original experience and subsequent disillusionment with the state of accounting research, particularly as it affected accounting theory formation.

In response to these inadequacies, Mattessich's first response, like others, was to seek to establish an empirical scientific tradition in accounting along the lines of the physical sciences. However, after work in the area of methodological investigation, spanning three decades, Mattessich has now come to realise that the empirical research method which is now thought of by many as the only research methodology conducive to progress, is really only part of what was and is needed; that the current efforts by those adopting this methodology claiming that they are assisting in the formulation of theoretical concepts is illusory. At best they are providing some statistical support for a given proposition.

Mattessich claimed that to brush off the question *"By means of which criteria do I determine whether my hypothesis or theory*

is tenable?...by citing some statistical rules of acceptance (the limitations of which are often enough insufficiently understood) would be grossly superficial" (Mattessich, 1978, 2).

Mattessich concludes much later in his work that *"...the representation of reality makes sense only if all of it, also the realm of value judgements, is taken into account, and if atomistic philosophies, like logical positivism and critical rationalism are abandoned in favour of a more holistic outlook" (Mattessich, 1978, 323).* This sentiment, better than any other, expresses succinctly, the rationale for the current thesis.

As explained in the last section however, the task of popularising this most basic of all study is not going to be an easy one for there appears to be a strong aversion toward moving outside the traditional accounting boundaries. This aversion appears to spring from two primary causes. First a natural fear of the unknown and second an intellectual laziness and protectiveness. Countering these notions will ultimately depend upon the facilitating capacity of the education system, in particular, the willingness of educators themselves to be open-minded towards change. In justification of the teaching of epistemology, Mattessich commented, *"the basic epistemology problems are the same for all empirical sciences, but some aspects as for example the methods of testing and verification... degrees of involvement in value judgements, etc. vary greatly from discipline. Hence there is a justification for teaching epistemology in various academic faculties, or at least*

arranging with one department, philosophy of sciences courses for different areas" (Mattessich, 1978, 5).

It is evident from this thesis that problems abound not only in the discipline of accounting but also in the philosophy of science. Deciding whether theories progress as revolutionary episodes as Kuhn suggests, or according to the 'anything goes' philosophy of Paul Feyerabend and additionally to what extent subjective versus objective factors are relevant to the construction of theory, is still undecided. Given the problems in both fields, it is evident that research opportunities abound in both areas. It is to be hoped, as far as accounting is concerned, however, that more research is to be undertaken regarding the suitability of research methods themselves; that as a result of this, future researchers are taught, among other things, to recognise the serious deficiency of the empirical research method namely, entrenching one with a particular given system of ideas. For this purpose consideration of alternative theses from the philosophy of science seems eminently suited for they represent the most sophisticated attempts to date to develop alternatives to empiricism.

In contrast to past efforts however, it is to be hoped and encouraged that in the true spirit of cooperation (so necessary for a holistic approach) that future research work in accounting and other so-called social sciences will provide support and direction for research being undertaken in the philosophy of science, which in its turn should give guidance to the research studies being undertaken in the more applied

disciplines. Commenting with respect to the benefits of such an association Mattessich stated "*The communication between philosopher and scientist [he includes social scientists] is no one-way street; the more intensive the interchange becomes, the more significant will be the common endeavour*" (Mattessich, 1978,6)

It is important however, to make sure that each retains its individuality and is not totally dictated to by the other, for then efforts at confirmation will fall into the same trap as appears to have happened with the empirical and Kuhnian frameworks of progress i.e. they would become self-validating and perpetuating.

This problem is just one of a number of problems which have failed to be addressed or even recognised by the majority of those trying to encourage the development of a "scientific" research tradition in accounting. It has failed to be addressed for the simple reason that investigation of scientific research strategies has been half-hearted in the sense that though people want the benefits associated with the label "Science" they are not willing to undertake the corresponding rigorous investigation demanded of a scientific discipline.

In the specific situation with which this work has concerned itself, it is evident that the majority of those interested in scientific models of progress have violated the criteria of rigour by failing to pay attention first, to the extent to which the models which they have adopted have been accepted in their own field, and secondly, to the criticisms which have been directed against these models.

On this interpretation therefore accountants, academic or otherwise, are in no position at this point to assess the possible benefits of applying scientific research methods in accounting because their knowledge of science is far too elementary. This is not to suggest however that this sort of understanding is beyond them or irrelevant. Given the generally agreed propositions that accounting is desperately in need of a research tradition, that the "scientific" research tradition remains the only research tradition of any substance, and that the accountant's ability to understand, does not inhibit him from coming to grips with these areas of concern, what is called for is a much greater understanding of these ideas. To put the matter succinctly - a little philosophy and history of science may make people mentally rigid. Only if we go on learning more and more of it - go on "unlearning" it - will it correct its own deficiencies gradually and help us to reach the required elasticity of the mind.

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