A CRITICAL ANALYSIS OF THE METHODOLOGY OF SELECTED MAJOR ACCOUNTING THEORISTS SINCE 1960

A THESIS PRESENTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS FOR THE DEGREE OF MASTER OF AGRICULTURAL BUSINESS AND ADMINISTRATION IN ACCOUNTING AT MASSEY UNIVERSITY

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Analysis and synthesis in philosophy are most fertile when they lead to a better understanding of knowledge, and knowledge is a clue to the understanding of both nature and culture

Bunge, Mario, The Myth of Simplicity:
Problems of Scientific Philosophy
Prentice Hall, 1963

ABSTRACT

There has recently been a significant amount of effort and space in the accounting theory literature devoted to attempts to determine a suitable methodological base for accounting. While some may take this to indicate that accounting is intellectually immature, it can also be taken to mean that accounting thinkers are aware of the need for a sound structure on which to develop the discipline: they have attempted to apply accepted scientific methods for research programmes and theory construction. It is important that the scientific methods chosen are themselves sufficiently rigorous and accepted as meeting these needs.

This thesis is concerned with examining the work of four accounting writers who appear to have had a significant influence on accounting thought - Raymond Chambers, Yuji Ijiri, Richard Mattessich and Robert Sterling. Although no one year can be held to be more significant than others, 1960, as the beginning of the decade in which most of the work was carried out, has been selected as the boundary of this analysis. The four selected theorists have all produced major works since that date.

In order to provide a perspective from which to assess the methodology of the theorists some space is devoted to tracing the thought on scientific theory construction over the last seventy years. It has included a brief survey of the major characteristics of logical positivism, and the work of philosophers who have reacted against that movement - Popper, Kuhn, Feyerabend and others.

It is shown that accounting theorists have tended to rely too heavily on a logical positivist position to determine the process by which research is undertaken and theories constructed. If the work of those philosophers of science who have reacted against the positivist position can be accepted as providing a more suitable expression of how knowledge is attained then accounting theorists have erred. With the possible exception of Chambers, from an analysis of theoretical works since 1960, this appears to be so.

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I INTRODUCTION

The noted philosopher, Sir Karl Popper, opens one of his "conjectures" on the growth of scientific knowledge (Popper, 1956, 357) with:

> "Once upon a time there was a famous scientist whose name was Galileo Galilei. He was tried by the Inquisition, and forced to recant his teaching. This caused a great stir; and for well over two hundred and fifty years the case continued to arouse indignation and excitement - long after public opinion had won its victory, and the Church had become tolerant of science."

The main concern of this particular paper by Popper is how knowledge is attained. He discusses three viewpoints: first, the essentialist view, second, the instrumentalist view; and third his preferred "conjectures, truth and reality" view. Because the Galileo episode is so well known and well documented Popper can use it to illustrate many aspects of his argument. His subject is that which is referred to as the philosophy of science: one that since its beginnings in the work of Aristotle has been subject to considerable debate due in no small way to its lack of definition. As its nature suggests, it is concerned with philosophy and science and as such its study merges with those disciplines. An "introductory" work has "sketched" four viewpoints (Losee, 1972, 1-3):

1. That it is the formulation of world-views that are consistent with and even based on important scientific theories and can therefore range from speculation about ontological classification to pronouncements regarding the implications of theories evaluating human behaviour as, for example in social Darwinism or ethical relativity.

2. That it follows scientists' predilections for some simple regularities in nature: it is mechanistic rather than teleological.

3. That it is a discipline in which the concepts and theories of science are analysed and clarified.

4. That the philosopher of science seeks to answer questions concerning the peculiar characteristics of scientific inquiry, procedures followed by scientists investigating nature, necessary conditions for correct scientific explanation and the cognitive status of scientific laws and principles.

This fourth view having such a wide scope almost includes the second and third views and is therefore useful in its generality. However herein lies the defect - it is not sufficient to specify the idea of the philosophy of science - it is, as was indicated, a "sketch". Nor does a "dictionary" provide a full answer: Runes (1974, 284-5) argues in favour of a working division of its subject matter into three fields which very basically are:

1. The "critical study of the method or methods of the sciences, on the nature of scientific symbols, and of the logical structure of scientific symbolic systems" (a study of method which includes logic and theory of knowledge).

2. "The attempted classification of the basic concepts, presuppositions and postulates of the sciences and the revelation of the empirical rational, or pragmatic grounds upon which they are to work" (including therefore the metaphysical aspects).

3. Ascertaining the limits of special sciences and their

interrelation and relation to a theory of the universe (teleological justification).

It includes, therefore, epistemology and Popper argues that this can be approached from two sides: first as the problem of common-sense knowledge, or secondly as the problem of scientific knowledge (1959, 18). However, he continues, "the most important way in which common-sense knowledge grows is, precisely, by turning into scientific knowledge" (1959, 19).

This research work is directed toward the problem of knowledge in accounting, or, more specifically, how some theories in accounting emerged. The above discussion therefore is relevant for it is into this broad background the research fits: in order to understand how knowledge of accounting grows it is necessary, as Popper implies, to determine how scientific knowledge grows. Nevertheless this approach begs several questions, the most obvious of which concerns the relationship between accounting and "science". It is in no way radical to state the connection as many others have done so before. For example, Spencer (1963), Tilley (1972), Kam (1973), Sterling (1975) and Flamholtz (1976) all allude to it in their titles. However this does not preclude the possibility of a misunderstanding of the notion of science; in fact this research will show this to be true but not only amongst accountants but by many "scientific thinkers". In order to conform, the work is divided into sections which roughly correspond to similar work at this level of study. While

the majority of these are empirical analyses of specific problem areas which lend themselves to easy sectionalisation, this theoretical analysis can be divided along similar lines. The sections are of widely disparate sizes.

The second (next) section justifies the research: it is what in empirical studies is referred to as the statement of the problem. The purpose of the study is explicated.

The third section examines the literature that has been devoted to the broad subject area. In an empirical study this would entail determining the results of past studies on the same or closely related subject matter. In a theoretical study the literature tends to be the subject of the study. A distinction is drawn in this work between the theoretical literature (the subject matter) and the literature examining or reviewing the theoretical literature. It is this latter type which forms the major part of the third section. Unfortunately there have been very few studies made which bear directly on the specific subject matter. Other work is therefore examined and the problems involved in relating it to the research are fully discussed in that section.

The next section of an empirical study would involve detailing the research method adopted. Research methodologies need not be restricted to empirically based techniques. It is, in fact part of the purpose of this research to demonstrate the error in this notion. This research has elected to study the work of only <u>some</u>

accounting theorists since 1960. In addition to describing the research methodology adopted in the work, section four will also justify the choice of the time frame and the theorists. This will be followed by the brief section corresponding to what in an empirical study would be termed the "hypothesis".

By far the greater part of this study is devoted to the next two sections. These form a two-pronged analysis of the substance of the work: first what are the current ideas in the philosophy of science which relate to theory construction, and, secondly, an examination of the work of the selected accounting theorists.

The eighth section, the conclusion, will relate the analysis of sections six and seven to section five. Because of the scope and nature of the study, the references will be necessarily many.

One of the most significant features of twentieth century intellectual activity is the efforts in many disciplines to attempt to formalise the structures on which these disciplines are built. In general terms this effort has been described in books on research methodology as the adoption of a "scientific approach". The speed with which scholars in the various disciplines have accepted these efforts has varied as has the time to make the initial explanations. Whereas by the middle of this century the great majority of the social sciences had undergone significant epistemological shifts in emphasis, some disciplines (e.g. accounting) were slow to appreciate the possible advantages.

In accounting there had been, what is now regarded as the fairly futile, debate as to whether what accountants did was in fact what some called an "art" (perhaps in its Grand Victorian sense) or whether it was a "science" and subject to fairly formalized rules and procedures. It appears that under the notion of some sort of professional independence of judgement, those who favoured the former view maintained the largest following. Whereas its most closely related field of study, economics, had made significant progress towards establishing what was regarded as a theoretical framework, it was not until the 1950's that serious consideration was given to the notion of "scientific"study.

This however did not prevent attempts being made at providing a theory for accounting: one which would "provide

a coherent set of logical principles that form the general frame of reference for the evaluation and development of sound accounting practices." (Hendriksen, 1977, 1). Although, given that accounting, as some form of recording - reporting activity dealing with economically revealing data, has been around for several centuries, and that there would have been some logical principles on which this activity was based, it is only within the last forty years that conscious efforts to develop a "theory" have had widespread acceptance. Caplan (1972, 45) states that "one of the most interesting aspects of accounting evolution in the past few decades relates to attempts to develop basic accounting theory". It is probably for reasons similar to this that these events have become the most documented in accounting literature. [1] The greater part of the movement took place in the United States and mainly since 1930 as one of the consequences of the uncertaintly created by the depression of that time. It was undertaken by professional bodies (academic and practitioners) and paralleled by government regulation. This movement lasted until 1959 when the major professional body - the AICPA [2] - disbanded their "theory-making" body - the Committee on Accounting Procedures - out of sheer frustration at the lack of progress. In its place they

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1. General texts have summarized the process, e.g. Most (1977) and Hendriksen (1977), however one of the best accounts is provided by Zeff (1971b). As stated there are several accounts, another good one is Beford (1969) and a more abbreviated one Moonitz (1975).

2. Abbreviations are used for most professional and other bodies, viz AICPA - American Institute of Certified Public Accountants; AAA - The American Accounting Association; SEC - Securities and Exchange Commission; APB - Accounting Principles Board; FASB - Financial Accounting Standards Board.

instituted the APB which was to last only until 1973 when it too was disbanded for similar reasons. Created to "carry on continuous examination and re-examination of basic accounting assumptions and .... develop authoritative statements for the guidance of both industry and our profession" (Alvin Jennings the then president of the AICPA, as it is now called, quoted in Zeff, 1971b, 167) it became a political "pawn", insulating the SEC from pressures and criticisms" finally "...failing to define the objectives of accounting, failing to construct a comprehensive and internally consistent theory of accounting and finally to understand the interelatedness and repercussions of its decisions" (Most, 1977, 77). The FASB was born out of the death of the APB. One of its major objectives is establishing rules and standards and it is currently attempting just that (FASB, 1976).

The accounting profession outside the United States has also had little success in providing a theoretical structure for accounting. Professional bodies in Britain, New Zealand and Australia have imposed upon themselves the obligation to produce standards and are in fact doing so. In addition to these an international body - the International Accounting Standards Committee (IASC) - has been set up also to issue statements of standard accounting practice.

There is no way that these efforts can be described as scientific attempts to develop theories. In setting guidelines for practioners they are little more than the result of political ploys to placate the majority of members and present a seemingly responsible countenance to

the public. 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. Fortunately this is not the case for, although the above describes the general background in which the more perceptive have to work, it also highlights the need for a deeper evaluation of the structural framework. Authoritative pronouncements will never resolve issues in the long run and the continued need for the revision of such statements (described above) indicates this. While accounting may seem to be, "as a logical and empirical science in too primitive a stage" for the development of a single general theory (Hendrikson, 1977, 1), several attempts have been made and they form the subject of this research work. In analysing some of these works, in determining their strengths and weaknesses, in questioning their acceptance or rejection, the whole issue of the process of knowledge accumulation is raised. If, as suggested earlier, the process is the subject of philosophers of science then it is necessary to examine the works in relation to current thinking in that area.

Accounting, as a social science, (Mautz, R K in, "Accounting as a Social Science", Accounting Review 38, April 1963, 317-25, describes it so), is faced with the same problems these other sciences are. In adopting a scientific approach for constructing theories they have relied on what appeared to be the majority opinion of philosophers of science in respect of theory construction. This subject however has been the most important problem of philosophers of science, for as Suppe (1972, 3) says:

> "If any problem in the philosophy of science can justifiably be claimed the most central or important, it is that of the nature or structure of scientific theories."

The problem of this analysis would therefore appear to be two-tiered: first to isolate what would seem to be the mainstream ideas in the philosophy of science and, secondly, to examine the accounting theories in relation to them.

In a work of this nature a solution to the first problem would appear to be overly ambitious. It is therefore further stressed that it is the mainstream ideas in the literature that will be examined. This will not be purely superficial, for such a treatment would be self defeating in not providing sufficient understanding to relate the accounting works to them [3]. Nevertheless a complete appreciation of all current thinking is not possible: for example a complete survey of ideas developed and/or developing in systems theory is not possible.

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3. In fact it will be demonstrated that a superficial understanding of one writer, T.S. Kuhn, has clouded rather than clarified the understanding of theories in many disciplines.

III LITERATURE SURVEY

THE TEXTBOOK TRADITION

A narrow interpretation of this research problem is that it examines the work of four prominent accounting thinkers in relation to contemporary views in the philosophy of science. Accepting this only leads to the conclusion that a literature survey would be fairly brief. In fact, strictly speaking, there do not appear to be any works on the subject. In respect to the works of the selected authors the most obvious source of comment thereon are the reviews (including book reviews) of them. In addition to these there are interpretations found in textbooks but these vary widely in content, coverage and appreciation.

A more general interpretation however would appear to be more appropriate. As indicated, some of the more specific material is contained within general texts. An awareness of their generality would assist in understanding their specification. Accepting this as a most suitable starting point is McDonald (1972) who attempts, as his title suggests, a comparative analysis of accounting theory. Apart from the non-specification of a time period McDonald then corresponds to this "broader interpretation" of his research work.

McDonald (1972, 2) opens with the statement that, "All theories and systems approaches are to a greater or less extent concerned with explanation and prediction. Even explanation in the abstract is rare. Explanation facilitates and makes more certain the predictive process. In the physical sciences, the laws of physics explain various

types of physical phenomena, but such explanation is ultimately used to predict what will happen under certain defined conditions. In the social sciences the same holds true". He then examines accounting using two approaches - a theory approach ("concerned with prediction") and a systems approach ("a complex of interacting and inter-dependent sets of phenomena").

"Theories are derived by organising the results of past experience" (5). These "past experiences" travel through three aspects before emerging as workable theories. There is, first, the translation process of encoding to symbolic representation which is necessary to enable the second process to be performed: manipulation or combination according to rules. It is here that constructs are developed and very often (e.g. income) they are not directly observable. The next step involves further translation processes - the decoding of the constructs back into real world phenomena. McDonald's view is, therefore, little more than a highly simplified logical positivist stance.

"Accounting is utilitarian in purpose and disruptive in nature" (13). There is, therefore, a need for workable goals. Some theorists have observed accounting and "deduced" (?) the underlying rationale of accounting; they produce descriptive accounting theories or theories of accounting. On the other hand there are those who attempt to "evolve a theory of what accounting ought to be; they produce normative accounting or a theory for accounting.

As examples of the former he suggests Sanders, Hatfield and Moore, Grady and Ijiri [4] ; the latter, MacNeal, ASOBAT and Chambers [5] . McDonald then provides a framework for analysis which, after a brief discussion of induction and deduction, is presented from a systems viewpoint. Like many other writers using the term, systems is used to describe little more than a flow chart schema on the theories of and for ideas. An expanded treatment of some examples follows and in theories of accounting these are Grady (1965) and Ijiri (1967) again to which is added AICPA (1970). Examples of theories for accounting now include Chambers (1966) and ASOBAT (1966) as before and Edwards and Bell (1961) and Moonitz and Sprouse (1963) and Moonitz (1961). Although the analysis provided by McDonald is largely unsatisfactory, it is important as one of the first attempts to analyse theory methodology in accounting in text book form. The preface indicates that it is a "short introduction to accounting theory" and it is little more than that. It does, however, appear to represent the first, widely available "text" on theory methodology.

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4. Their major (or only) works, viz; Sanders, TH, Hatfield, HR and Moore, V, A Statement of Accounting Principles, AICPA, 1938; Grady, Paul, Inventory of Generally Accepted Accounting Principles for Business Enterprises, Accounting Research Study 7, AICPA, 1965; and Ijiri (1967). It is difficult to see Grady as "a theory", using any approach, and Sanders, Hatfield and Moore as more than "a statement".

5. MacNeal, Kenneth, Truth in Accounting, University of Pennsylvania Press, 1939; ASOBAT is conventional abbreviation for, A Statement of Basic Accounting Theory, AAA Committee to Prepare a Statement of Basic Accounting Theory, (1966); Chambers (1966)

The most widely used standard text on accounting theory has been Hendriksen (1977). It has been used for most courses in accounting theory for over twelve years in its three editions [6] but probably through default more than any necessary intrinsic worth. It is interesting to note that the one chapter that has had to be extensively revised throughout its three editions has been that on the methodology of accounting theory. The author confesses his personal view of theory methodology is eclectic and consequently his work is little more than a synthesis. However herein lies its major attraction, for it has elucidated currently held views of theory construction appearing in the literature. By the same token this also gives rise to a major criticism: it has presented many so called "approaches" to theory construction as if they were all of equal merit or relevance. While earlier editions generated the myth that there can be various types of theories such as inductive, deductive, pragmatic or socio-economic, the latest edition (Hendriksen, 1977) perpetuates it (slightly tempered) and even adds more [7]. What results is a mixture of processes common to a positivist stance though in most instances insufficiently rigorous. Verification is seen as a very necessary element.

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6. 1965, First Edition, 1971, Revised Edition. It has been one of the very few textbooks published and is continually referred to in the literature.

7. For example: theories of investment valuation, predictive indications, communication theory ethical, events and macroeconomic approaches and non-specific behavioural objectives.

The most recent addition to the textbook presentations is Most (1977). Like Hendriksen, Most devotes the first chapter to considering methodological questions but, unlike Hendriksen, it relates the theory construction process to what is seen to be scientific method. While acknowledging that no satisfactory definition of accounting exists, a brief historical perspective lends meaning to the need for accounting, largely in terms of accountability. In this perspective Most points out that there was little criticism of accounting until the last forty years [8] yet acknowledges the work of a recent author [9] who showed that the "revolt against accountants has been brewing for nearly 100 years and certain problems appear to be perennial and impervious to regulation and legislation". (11).

However it is within the context of the growing accountability and criticisms of accounting that Most sees the need for accounting theory.

> "A theory is a systematic statement of the rules or principles which underlie or govern a set of phenomena. A theory may be viewed as a framework permitting the organisation of ideas, the explanation of phenomena and the prediction of future behaviour....."
>
> "A theory is above all an explanation. There is a widespread misconception that a theory must aid in prediction, but not all theories do. The theory of evolution, for example, has no predictive ability whatsoever.....

8. The first major criticisms coming from MacNeal, in his Truth in Accounting (University of Pennsylvania Press, 1939) who attacked accounting with what Most quotes the author as claiming "ill temper and sweeping denunciation". (10)

9. Brief, Richard H, "The Accountant's Responsibility in Historical Perspective", Accounting Review, 50, 1975, 285-97

> Similarly, a political theory may explain revolution in socio-economic terms, but could not predict that Chile would become the first country to vote a communist government into power ..."(11)

Most sees three views of theories: reductionism, instrumentalism and realism (extremely diverse!). He also makes the distinction between theories which are positive (the more common name for what McDonald, 1972, above, referred to as 'descriptive') and normative. He then turns to research methodology: referred to by many, under several different notions, as scientific method. This leads Most to his conclusion:

> "In summary: a theory is a complex set of rules or principles based upon knowledge preferably derived from research. Research is characterised by a certain methodology, which is a reliable set of methods. A method is a family of models which have been found useful for hypothesis testing. A theory therefore is essentially a set of acceptable hypotheses". (15)

ANALYTICAL WORKS

The three works discussed above are, as mentioned, primarily designed as textbooks. An awareness of their content is a useful guide to current accepted thinking: in Kuhn's terms, they "aim to communicate the vocabulary and syntax of a contemporary scientific language" (1970a, 136). There are some works which have attempted to analyze the structure of accounting theories. The most ambitious of these appears to be Yu (1976) the title of which is The Structure of Accounting Theory. It differs from the major works which form the subject of this analysis in that whereas they are

actually constructing a theory of (or for) accounting the purpose of this book is "to present a methodological exposition of theory construction and verification in accounting. It does not deal with accounting theory per se" (2)

The book is perhaps best summed up by a reviewer [10]:

> "Much of the book is devoted to discussions of the rudiments of philosophy ... hence, the more sophisticated reader may become impatient with the pace of the exposition. Nevertheless, the book does catalogue a large number of philosophical issues in the area of theory construction and indicates, in general terms, their relevance to accounting".

It is not certain what level of "sophistication" is suggested by the reviewer but it certainly is easy to become impatient with the book: for one that promises so much very little is actually proffered. The method which is given as scientific is again a weakened form of logical positivism and it overlooks entirely the Weltanschauungen analyses of philosophers of science such as Toulmin, Kuhn, Hanson, Feyerabend and Popper. Its scope is limited and so is its consequent appeal. In an appendix Yu summarizes "the several modes of ethical philosophy" and concludes with a short section on logical positivism doubting its usefulness to ethical statements. This is obviously true in relation to ethical philosophy but it is a little strange to find the greater part of the book devoted to describing a theory construction process which "is the product of logical positivism and cannot be understood or divorced from the tenets of that involvement" (Suppe, 1974, 6)

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10. Carlson, Mervin L, in Accounting Review, 52, 1977, 785-6

Two shorter but more rigorous works are Iselin (1971) and Penman (1973). Iselin is concerned with determining objectives of accounting in order that a "deductive theory" of accounting can be established. It accepts Hendriksen's (1977: but earlier edition obviously) different methodologies (and concentrates on one) and for that reason should probably be dismissed as being irrelevant in the light of contemporary views on theories. However it is interesting in that it represents an early example of methodological analysis and the conclusions appear to be fairly true for accounting; that is, the objectives of accounting established give a good indication of the scope of the discipline.

Penman's (1973) compass is larger: it considers a framework suitable for accounting theory. After a brief introduction, ontological questions in accounting are raised. The next section discusses construct formulation as the basis of theory construction and the final sections "answer" the ontological questions with recourse to the construct formulation process. The work relies heavily (though not solely), for its analysis of scientific theories, on the efforts of Carnap, Hempel and Margenau and as such is open to all the criticism levelled against these writers.[11] Constructs exist on a higher, theory plane and are

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11. Which form the basis of the first part of section VI of this research work. Penman lists three works by each of these writers, including Carnap, R, The Logical Structure of the World, Routledge and Kegan Paul, 1967; Hempel, Carl, Aspects of Scientific Explanation, Free Press, 1965 and Philosophy of Natural Science, Prentice Hall, 1966; and Margenau, Henry, The Nature of Physical Reality: A Philosophy of Modern Physics, McGraw Hill, 1950

developed from the, lower, real world (empirical) plane through the process of refinement of perception to conception to construction. Constructs are subject to deductive processes using syntactical rules and these emerge after a process of verification, real world conclusions which through the media of statistical inferences and experimentation can be used to describe and/or predict real world events. This is very much the ideas of Henry Margenau [12] and with one or two minor modifications is the view most encountered in the literature on accounting theory construction. Penman (1973) thus avoids the misguided oversimplification of Hendriksen, the generality of Most and presents a more sophisticated version of McDonald.

A paper delivered at a seminar in 1966 forms the basis of Deinzer (1968). However seventy pages of notes to this paper form the bulk of the work (with an appendix excerpt from John Dewey). The result is a disjointed work, difficult to follow and it appears to be heavily influenced by the so called rigours of economic science presented in the Krupp book in which Margenau (1968) appears (The Structure of Economic Science, 1960).

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12. See Margenau (1960) for an outline, Margenau (1961) for the detail

PRESENTED PAPERS

There have been several symposia in accounting since 1960 which have had accounting theory as a major, if not the only, theme. The proceedings of these symposia have contributed significantly to the literature on theory methodology. A good example is Stone (1971) which is refered to below. Goldberg (1971) opens with a quote from William James and this sets the theme for his paper although "common sense" rather than "pragmatism" would be a more suitable description. Goldberg suggests that there are three varieties of accounting theory; one a normative - deductive approach and the other two inductive, the distinction in the latter being on the basis of predictability. The argument recalls Goldberg (1965) and tends to favour an empiricist approach. However it is a logically impossible empiricism, one that appears to deny positive theorizing!

In commenting on Goldberg's, paper Zeff (1971a) provides an excellent, if too brief, summary of much research-theorizing in accounting. Its main usefulness is in its championing the cause for soundly based scientific theorizing in place of authoritative professional pronouncements.

There are four significant symposia which had as their theme accounting research. Sterling in the Introduction to Sterling (1972a) shows that "there have been a number of attempts over the years to provide a concise definition of the "scientific method." All of these attempts have failed. They have failed because, as the historians of science have

shown, sometimes science progresses in one way and sometimes in another" (3) and this is one of the earliest acknowledgements by accountants of a broader view of scientific progress. Caplan's (1972) subject is "accounting research as an information source for theory construction", though the content is little more than an attempt to justify empirical research on the basis of it providing the information for minor theories:

> "Theory construction based on good empirical research .... will move us forward very slowly .... But in the long run this kind of research appears more likely to win the race in building a basic theory of accounting". (48)

Williams (1972) takes Caplan to task on eight counts: his major criticism is the complete lack of rigour in Caplan (1972). On this point there is little doubt that Williams is entirely justified but the preferred method of Williams for theory construction is the Penman (1973) - type discussed above. (There are reasons for this suggested a little later). Caws (1972) takes a similar pose on this matter in his critique of Ijiri (1972b).

Dopuch and Revsine (1973) in evaluating accounting research from 1960 - 1970, required Nelson (1973) to examine "A Priori" research in accounting and it is little wonder that he was not sure of the full meaning of his task as the term appears to have been coined for him. They seem to have had in mind what Iselin (1971) did: Theory constructed along normative - deductive lines. Nelson, accepting this, suggests that, "the 1960s was a golden age in the history of a priori research in accounting", (4) yet devotes half

of his paper to "a priori research" undertaken outside this time frame. He does not discuss philosophical considerations in theory construction. Given the topic, it is an extremely disappointing paper which left open the way to the original research in this present analysis.

The three other sections of Dopuch and Revsine are research in management accounting, behavioural science and accounting research, and empirical research in accounting. It is only the last of these that could contribute to the debate on theory construction but discussion tended to be directed towards practical problems in research.

Previts (1975) contains only one paper with direct relevance to this work - that by Sterling (1975). Discussion of this is left to later in this work. So too is Chambers (1976) in Previts (1976).

INSTITUTIONAL EFFORTS

There exists in accounting a body of literature on theory which could best be classified as institutional, represented on one extreme by Grady (1965) and at the other by ASOBAT (1966). Grady (1965) epitomizes the worst in such an approach: it is little more than a list of current procedures and while it may be a useful guide for further theorizing it can itself in no way be the "theory" it was supposed to be.

It has already been stated that frustration with the lack of progress in forming a theoretical structure had reached

a peak in 1959 in the USA. Therefore, as also mentioned, the APB was set up by the AICPA with the aim of solving the basic structural problem. To assist it a full time Accounting Research Division (ARD) was also set up and had by 1 July 1960 its first full time director and eight research studies with authors scheduled. The first two of these were, basic postulates and broad principles. The first research study was written by Moonitz (1961). "In 55 pages of broad ranging discussion, the author developed a framework of three tiers of postulates, comprehending the environment, the field of accounting itself, and imperatives. Since a study of this sort had few precedents in the accounting literature, it is probably fair to say that many readers, not to exclude the Board, did not know what to make of it." (Zeff, 1971b, 174) Needless to say the study was rejected as too academic. While the argument developed is in fact philosophically tame, its rejection by practioners highlighted the general framework for what they saw as being a theory - probably a set of working rules to be followed in particular circumstances. In fact, Zeff (1971b, 174) continues with, "Seldom had accountants formalized their conceptual schemata in terms of postulates, whether or not rigorously derived". It is ironic that these same expressions (called postulates by Moonitz) are today being used as the basis of professional pronouncements in the USA, UK, Australia, New Zealand and by the I.A.S.C. Needless to say also, the APB did not follow up with "the issuance of a pronouncements" as had been their original frame of reference. Nor did they issue any pronouncement following the third research study,

Sprouse and Moonitz (1962). In fact the rules for publication of future studies were changed to require the approval of the majority of the Board. Not only does this serve to illustrate the innate conservatism of a body supposedly serving the public interest, it exemplifies the most pernicious professional politicing.[13] The Board's response was to commission Grady (1965). Whereas Yu (1976) stated that he was not concerned with "accounting theory per se" (above) these last four works are and therefore could on those grounds be excluded from a survey on written exposition of theoretical method. However because they indicate attitudes of a large number of accountants to theory as such they are useful in appreciating the problems of theory acceptance. They represent the practitioner - professional - institutional approach to theory. It is also useful to examine the academic-professional - institutional approach. Because of its apparently highly significant influence on writers subsequent to its publication, ASOBAT (1966) is an important work.

The charge of the Committee was:

> "To develop an integrated statement of basic accounting theory which

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13 At least two points concerning these matters are worthy of note. First, it appears the profession (in the USA) has "reaped what it sowed" with all the troubles facing it at the moment with Congressional investigations and the like. Secondly, a criticism of Sprouse and Moonitz was their suggestion of replacement cost as a suitable recording "cost": in the USA certain replacement cost information is now mandatorily required to be reported by companies per SEC regulation.

> will serve as a guide to educators, practitioners and others interested in accounting."

The effort is not the first by the AAA but it does represent a significant change in direction. As Sterling (1967a) points out, a committee effort has the added constraint (over an individual) in that it must achieve general agreement between members but he does concede that the document is "revolutionary" when compared to contemporary practice and education. One of the reasons for this is its methodology which, Sterling (1967a, 96) says is "self-consciously antithetical to the previous inductive or empirical methods". Despite this it appears to have been well read: by at least March 1970, Deinzer had identified more than fifty articles citing it (1971, 114).

ASOBAT defines accounting in terms of identifying, measuring and communicating economic information, then proceeds to develop the theory on this basis. To do so it first examines the objectives of accounting which would then permit an identification of the scope of accounting and then the methods used. These methods - the methodology - includes "the various techniques and procedures used by accountants" and the purpose in developing a theory of accounting "is to establish standards for judging the acceptability of accounting methods" (6). The Committee then turned to presenting standards that would "provide the means of accepting or rejecting accounting methods" (6, emphasis in original). Unlike earlier efforts the Committee prescribes these: they are deduced from the preceding definitions and statements.

The American Accounting Association has since 1969 issued as a supplement to its journal - The Accounting Review [14] - the reports of its various committees. The Committee on Accounting Theory Construction and Verification (1969 - 70) Report is contained in AAA (1971b). The charge of the Committee was:

> "...to search out the sources of accounting theory; suggest criteria for the verification of accounting theory; and recommend theory construction methods appropriate for thought. Emphasis should be placed on empirical verification of accounting theories". (1971b, 50)

The title of the Committee, let alone its charge, presumes the theory construction methodology: verification, for many, is an untenable methodological presupposition. [15] There are at least two reasons, however, why this work is significant in examining accounting theories: first, it draws attention to the fact that Accountants were aware of the need for some basic work on theory construction, and, secondly, the members of the Committee were, what could be described as, "significant contributors" to the literature in accounting theory. To continue this latter point, in fact their recent contributions have shown that either the report was heavily influenced by their ideas or their work in producing the report greatly assisted in formulating their ideas. The allusion to the significance of the author, Williams (1972), mentioned earlier refers

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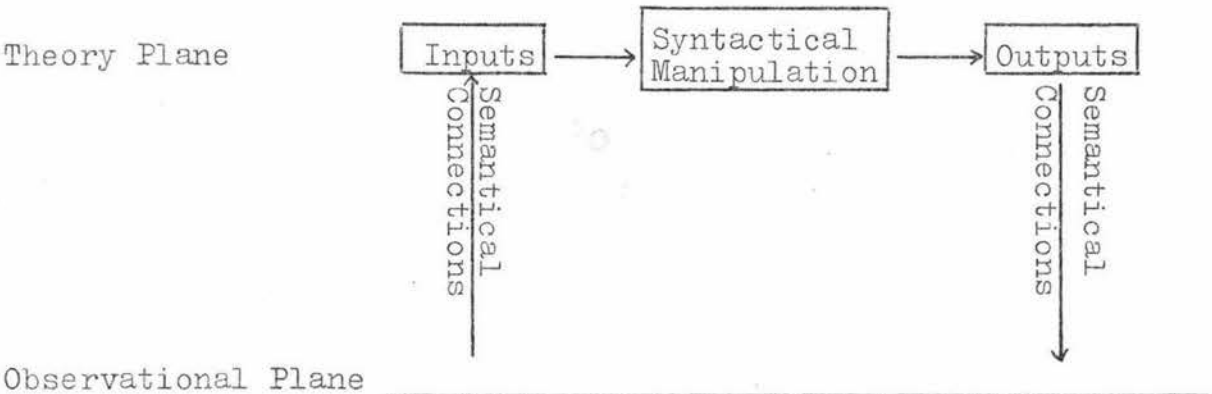
14. Actually as from 1978 this practice has been abandoned.

15. This point is taken up in Section VI of this analysis.

to this point. [16]

The method supported by the committee is positivistic - axiomatic calculi to which partial observational interpretation is given: it is the method commonly found in most of the less recent literature in the physical and social sciences. The Carnap - Hempel type of analysis is followed.

The introduction takes an over view and, given their overall subsequent analysis, a rather paradoxical allusion to Kuhn is made. Theories are first of all "a set of sentences" so some space is devoted to explaining some principles of semiotics: syntactics, semantics and pragmatics. The importance of verification - a principle with which even the scientific empiricists are not entirely happy - is stressed as is the idea of planes mentioned above:



Source: AAA(1971b) 58

16. The members of the committee were, Thomas H Williams (Chairman), William H Beaven, Daniel L McDonald, K Fred Skousen and Robert R Sterling.

A tentative metatheory for accounting is suggested: different researchers could, as, it is suggested they have already, concentrate on sub-areas.

> "In the syntactics area, we have the entire history of trying to develop 'concepts' or 'principles'. The idea behind stating such concepts or principles is to allow one to deduce the correct procedures in a specific case". (58)

and:

> "The pragmatics area includes the whole of the 'behavioural' research in accounting". (59)

however:

> "The semantic connections on the output side are even more neglected. The extant theory of accounting ... has not been confirmed. It is difficult to know just what one would mean by 'confirmation' in connection with the extant theory". (59)

This last point is of considerable significance to the committee for it indicates lines open to future accounting theoretical thinking. First they suggest that accounting could claim to be non-empirical in which case confirmation loses meaning. To the uninitiated this may seem so but, they argue, there is little future in this line of argument. Secondly, they could "attempt to alter the theory so that the outputs would be subject to direct verification" (60). This is rejected in favour of the third alternative of expanding the "Theory of Accounting" to include decision theories: accounting becomes "the measurement - communication function of the decision process".

To suggest, to those clinging, as fervently as a religious fanatic, to the theory construction process described that

it may be wrong is heresy. Ironically, the report of another committee of the same period (on Foundation of Accounting Measurements: AAA 1971a) opens with a discussion of the decision theory and its relevance for accounting and concludes with a discussion of theory construction. The process described in this instance follows the modified (semantical emphasis) logical empiricm of Mario Bunge [17].

The most recent institutional or committee discussion of methodological aspects of accounting is the AAA's Committee on Standards for External Financial Reports, Statement on Accounting Theory and Theory Acceptance (AAA, 1977). There had, however been several other publications dealing with matters of "theoretical concern" prior to this and subsequent to ASOBAT (1966). One such document is AICPA (1970): APB Statement No 4, "Basic Concepts and Accounting Principles Underlying Financial Statements of Business Enterprises". The AICPA were still searching for the elusive definitive statement on principles; Moonitz (1961) and Sprouse and Moonitz (1962) had been emphatically rejected and there was ambivalence towards Grady (1965). While this statement shows the influence of ASOBAT it readily admits to being

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17. This is probably a gross oversimplification in the appreciation of Bunge's work and may possibly be not entirely accurate: some philosophers change their views more than others. The report however presents that interpretation of Bunge. This influence is likely due in no small part to the fact that one committee member, Mattessich, was at the time heavily influenced by Bunge. Another member, Ijiri, was deeply interested in measurement theory which would further indicate a natural proclivity to Bunge's work on semantical aspects of theory construction.

descriptive and not prescriptive. While it was not intended to provide a theoretical framework the statement is deficient as a theory, according to Hendriksen (1977), for several reasons. His first criticism concerns the semantic descriptions of certain concepts: they are synthetic. Secondly there is no logical relationship between objectives and the detailed principles. Thirdly the lack of verifiability by which Hendriksen seems to be implying semantical connection - his first criticism. In short, to Hendriksen, "Statement No 4 is not a theory of current accounting practice nor a clear statement of generally accepted accounting principles" (84).

The AAA Committee on Research Methodology report is contained in AAA (1972a). However prior to its report being published, a colloquium had been held and the papers presented there were in fact the reports of individual committee members with a few extra commentaries. The proceedings of the colloquium are contained in Sterling (1972a), discussed above.

Although it is difficult to pinpoint a specific time, there was around about 1970 a change in emphasis in works on accounting theory. While some may see it as the maturing of a science, the cynic may well be moved to comment that it is the final admission of failure. Whereas 1960 [18] closed the curtain on "bushfire" [19]

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18. Approximately: Establishment of APB, publication of Moonitz (1961) amongst other things.

19. Solutions devised to extinguish the flames of trouble areas.

official pronouncements on theoretical issues and wholesale "inducing" of theories, and opened the season on broad methodological debate, 1970 introduced a new style of performance: theorizing has become more specifically related to sub-areas of the accounting arena. The emphasis is no longer on general methodological matters but is related to aspects of accounting such as financial reporting, behavioural theories, communication theories, information theories, systems concepts, measurement theory, international accounting, social performance measurement and other matters.

Whereas in previous discussions financial reporting was the end result of an overall financial accounting paradigm, it now became a subject in its own right: accounting as it was previously recognized to be has become fragmented with groups of researchers all working towards their own minor goals. It is as if, in Kuhn's terms, there had been the "period of pronounced professional insecurity generated by the persistent failure of the puzzles of normal science to come out as they should" (1970a) (pre 1960), followed by "the proliferation of articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals" (91) (1960s) followed by the return to the puzzle solving of normal science.

Whether this is in fact so is obviously dependent on many factors not mentioned and it is part of the purpose of this research to examine just those. However a pre-requisite would be the substitution of a new paradigm yet in accounting there is none. This tends to lead to the pessimistic conclusion described above.

To illustrate this movement away from the general aims it is only ncessary to look at the AAA committees reporting in 1976 and 1977:

> 1976 professional examination, accounting for social performance, international accounting in developing countries and financial accounting standards(on FASB Discussion Memorandum on Materiality).
>
> 1977 (includes) editorial policy of the Accounting Review, accounting in the public sector, managerial planning and control, international accounting operations and education, accounting by debtors and creditors, accounting treatment of changing prices, and other administrative - education oriented committees.

A similar trend is noticeable in the practioner professional bodies. The report of the Trueblood Committee (AICPA, 1973) is a good example: the title is "Objectives of Financial Statements" and that is exactly what the Committee investigated. The basis of their research was empirical and descriptive; it, in their own words, "solicited the views of more than 5,000 corporations, professional firms, unions, public interest groups, national and international accounting organisations, and financial publications ... conducted more than 50 interviews with executives ... 35 meetings ... a review of pertinent writings in accounting, economics and financial analysis" (10).

The profession in the United States is currently preoccupied with two matters. First, there is the question of survival in light of two Federal Government (Senate) investigations (The Metcalf Report, "The Accounting Establishment", US Senate Document, 95-34; and the forthcoming Moss Report).

Secondly, through the FASB, it is attempting to establish a conceptual framework for financial accounting and reporting.

> "A conceptual framework is a constitution, a coherent system of interrelated objectives and fundamentals that can lead to consistent standards and that prescribes the nature, function and limits of financial accounting and financial statements" (FASB, 1976, "Scope", 2)

A little further on it is stated:

> "concepts cannot be expected automatically and unequivocally to resolve specific problems. Like a constitution, a conceptual framework ... will require interpretation and, occasionally, ammendment."

and

> "Thus a conceptual framework is a guide for predictable analysis and judgement". (FASB, 1976, "Scope", 6).

To some this may sound very much as if they are attempting to provide a theory of accounting. However the work itself is unfinished (FASB) and research has not yet established the meaning of "theory": the question therefore is open-ended, left unanswered.

This is the environment in which, the latest institutional or committee discussion of methodological aspects of accounting was published: (AAA, 1977). For those interested in methodological aspects of accounting theories the report provides little over which to enthuse. It consists of five chapters:

1. Introduction
2. Alternative Theory Approaches
3. Criticisms of Present Theory Approaches
4. Difficulties in Achieving Consensus: A general view
5. Implications.

While chapter 1 provides some accurate observations (most of which have already been noted in this report), chapters 2, 3 and 5 are fairly predictable of accounting theory discussions.

The Introduction provides an overview of accounting and the need for theories:

> "In the view of this Committee, a single universally accepted basic accounting theory does not exist at this time. Instead, a multiplicity of theories has been - and continues to be - proposed. Therefore, this statement cannot provide accounting with an unequivocally acceptable conceptual superstructure when the underlying foundation has not yet settled". (1)

The need for theories is then discussed in relation to users and environment. The first part of chapter 2 involves a brief survey of the ideas of "classical approaches to theory development". Most of those chosen were written prior to 1960; of the four that were not two are, Moonitz (1961) and Sprouse and Moonitz (1962), one is Ijiri (1975) which is to be discussed later in this analysis and the last is one to which it seems every respectable work on accounting at a conceptual level must make reference. Edwards and Bell (1961) has been excluded from this research effort on the grounds of its specificity - it is related to income measurement and while it adds useful ideas and concepts to practical accounting considerations as a methodological study it is fairly sterile.

The analysis of these "classical approaches" is along the lines suggested by Hendriksen (1977) and is consequently fairly unsatisfactory. Distinction is made on the basis of

deductive and inductive emphases. A second classification, which includes many more recent studies, is termed "The Decision - Usefulness Approach" which could well be taken as a euphimistic description for practical approaches. The range of emphases in this group is extremely wide and includes such works as ASOBAT (1966), AICPA (1970), AICPA (1973), all the behavioural theories and the investment market approaches. No doubt the authors of many of the "classical approaches" would have felt their work should have been included! Equally Edwards and Bell (1961) would seem to fit the requirements of the third classification: information economics approaches. In fact most of the works in this third classification are by non accountants and appear in traditionally non accounting sources.

Criticisms of present theory approaches are considered under six headings, all of which are practical problem areas. The fourth chapter takes a broader view of theory construction and in fact considers the work of Kuhn (1970a) in relation to accounting theory. Unfortunately it is all too brief and they acknowledge that a Kuhnian analysis may very well not prove satisfactory. In fact they go even further in suggesting that "the existing state of theoretical conflict in accounting" may not need to be understood in terms of a philosophical perspective of the history of science.[20] The Committee concludes, like so

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20. The earlier allusion was made to Lakatos' work as an alternative to Kuhn and hence the reference to "history" rather than "philosophy " of science.

many others working in other social sciences, that there is a multiplicity of paradigms. This present work will, subsequently suggest that this may be an oversimplification of Kuhn's work arising out of a misunderstanding of the difficult notion of paradigm.

The committee sees three implications of their investigations - implications for further research - first, "theory closure cannot be dictated", meaning that an accounting theory will achieve "dominant acceptance" because of its proven long run efficacy. Secondly, "external reporting theory has a wider scope than that which has been generally perceived", which has already been indicated in this, as yet incomplete, literature survey. Thirdly, "all theory approaches are flawed when viewed from the perspective of some alternative approach", which sounds remarkably like, Paul Feyerabend's comment:

> "....it will become clear that there is only <u>one</u> principle that can be defended under <u>all</u> circumstances and in <u>all</u> stages of human development. It is the principle: <u>anything goes</u>" (1975, 28).

JOURNAL ARTICLES

This literature survey has so far examined accounting literature which has been concerned with accounting theory methodology; it has covered the field in groups. First is examined the general textbook approaches, secondly the major works of individuals (books), thirdly the "symposia approach" (group discussions on papers by individuals) then fourthly the institutional approach. This leaves at least

one source; one in which by far the greatest diversity exists - journal articles. These are now analysed.

Although not strictly speaking a seminal work, one article which because of its enormous subsequent discussion and influence can be seen to be a major motivating force in the accounting theory methodology debate since 1960 is Devine (1960). The paper expresses the intention of examining "some methods of inquiry" and suggesting "certain types of investigation that seem to be appropriate for the study of accounting" (387). It examines the (then) current thinking by social scientists on what constituted the scientific method and relating this to accounting on the basis for further research and theorizing. The emphasis is the necessary pre-requisite of the establishing of the purpose or objective of accounting:

> "The most urgent field for accounting research may not be related to problems concerned with the efficient measurement of transaction flaws unless efficiency itself is defined in terms of the accomplishment of socially worthy objectives" (399).

The method is "scientific method" which, Devine argues, is"composed primarily of the interaction of deductive methods and the philosophical doctrine of empiricism."(342)

As indicated the work is not the first to call for a scientific approach to accounting.[21] However the call

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21. The work of the two major authors examined in this research had long before made such appeal - Chambers (especially : 1955) and Mattessich.

was taken up in response to Devine (1960) and the first was Bedford and Dopuch (1961). Whereas Devine had argued on the basis of logical structure and deductive systems, measurement and induction, behavioural research and welfare and normative responsibilities, Bedford and Dopuch contend that the last three areas are not independent areas of study and an inappropriate perspective had been created by Devine (1960). The methodology suggested however is that seen to exist in the social sciences (which in turn are seen to mirror, or rather follow, those in the physical sciences), that is the logical empiricism of people such as Woodger's (The Technique of Theory Construction, (1939).

Given this entry cue, the stage was set for what was described above as, a decade of methodological debate. Philosophical concepts and questions were introduced freely. Gordon (1960), although concerned specifically with income and wealth measurement sees this as the basic task of accounting: "Accounting theory in the measurement of an economic entity's income and wealth involves the discovery and in some sense the verification of principles or rules which will make the practice of accounting in the actual tasks of measurement more effective" (603). Research provides the basis of theory construction (and verification) and he is perceptive enough to note that "accounting theory is largely deductive in nature" and "there appears to be little room for empirical research" (607, emphasis in original) "A formal methodology is a sign of measuring maturity in any discipline", asserts

Flanders (1961) who demonstrates the relationships between philosophy, history, arts and science through the use of "tools" (grammar, mathematics and logic) in a schema called "systematized learning" and then discusses the similarities between economics and accountancy. The "methodological ferment in accountancy reflects the search for a broad gauge 'Newtonian' theory of accountancy" (574) which, given that Einstein had worked some sixty years earlier, is not a very flattering prospect for accounting.

It is significant to Dopuch (1962) that "studies in accountancy have not been related explicitly to philosophical schools of thought which might possibly bear upon the discipline" (251). By showing that certain metaphysical assumptions are common in both areas Dopuch relates pragmatism to accountancy. The argument is conjecture and the result hardly eugenic. While the motive is indeed grand the connection is tenuous and the work, regrettably perhaps has had little perceptible influence on subsequent writings.

Another economist attempting to lend shape to the amorphous mass of the accounting methodology debate is Milton Spencer. In Spencer (1963) he attempts to present a formal axiomatisation of accounting "in a manner paralleling the kind of work done in economic theory during the last forty years". (310) His interest therefore is axiomatic method as a means for imparting logical structure to "accounting science". To anyone interested in theory methodology the question of

demarcation for science, social science and non science is inextricably bound up with the question of method. Mautz (1963) in asking the question, "is accounting a social science?" appears to recognize that but relies on what are probably outmoded sources for describing the method which could account for such statements as "If accounting is to be a social science, it must also accept more responsibility for value judgements" (1963, 321).

It was noted earlier that Hendriksen (1977) in the chapter entitled, "The Methodology of Accounting Theory" describes several approaches to theory; this is especially so of earlier editions, and it would appear that this is due to the nature of the work. It is a synthesis, and as such represents the literature. In fact there are several "approaches" advanced in the literature, for example, Schrader provides a so called "inductive approach", for, it is felt that "inductive method clarifies the nature and limitations of accounting data and of the interpretive reports reflecting those data" (649). Hendriksen cites Fremgen (1967) and Cowan (1968) as examples of a pragmatic approach. In fact, strictly speaking, such an approach would imply rigid behavioural objectives. Fremgen, however concentrates on defining usefulness or, to use his expression, "utility". He then proceeds to outline methods by which useful principles may be achieved and in so doing moves away from users to a concept similar to that described in ASOBAT (1966) as "relevance".

Cowan (1965, 1968) is also concerned

with usefulness: "Accounting is utilitarian", he opens. However he concentrates on needs and in so doing foreshadows the behaviourists and the current debate on objectives of financial reports. Although methodologically it is not easy to agree with his argument it is an important work for the reason stated above.

In a short article, Bedford (1967) notes the expanding boundaries of accounting. This is also the subject of Birkett (1970) who assumes a systemic viewpoint. These discussions are obviously significant in examining theoretical literature as very often this very problem - the establishment of disciplinary parameters - contributes in a major way to the "success" or "failure" of the theory description (if not the theory itself). In an historical perspective Baladouni (1966) also looks at accounting. In doing so he makes several points concerning the question of methodology. The "accounting universe" is important, therefore what is it? It is a "grand behavioural process. It is behavioural because it is related to human conduct; it is a process because it is a series of interactions conducing to an end" (216). Its identifying features are universality, prehensivenss, substance and methodology. This last feature is described as bringing "about a necessary awareness concerning the special methods, techniques and procedures utilized by revealing their merits as well as their deficiences" (217). Buckley, Kircher and Mathews (1968) attempt to provide a review of progress towards establishing a methodology on which to develop accounting theories:

> "If a general methodology in accounting could be agreed upon, theories developed within this framework would stand a better chance of gaining widespread approval. As it is now, both the ends and the means are constantly in dispute.
>
> Adherence to a general framework or methodology is the hallmark of theory formulation in many other disciplines" (1968, 374)

However although being capable of recognizing the symptoms the authors are unable to prescribe a cure and the article falls far short of what it promises to be. No definite guidelines for methodology are stated; merely possibilities are suggested.

The same can be said for Beams (1969) which follows the pattern of Dopuch (1962). Wheeler (1970) also adds little new information.

It was earlier suggested that there was some significance in the membership of the AAA committee producing AAA (1971b). Professors T. Williams and C. Griffin were colleagues at the University of Texas, Austin, when the former was a member of the committee. A series of articles by both or one of these two authors contributed to the accounting theory literature at that time. The major emphasis is on verification aspects of theories and early versions follow very similarly the line of argument in AAA (1971b). The first of these was Williams and Griffin (1969). In an earlier work the same two authors had investigated the relationship between mathematics and accountancy (Williams and Griffin, 1964). The first part of this article draws heavily on ideas of mathematical verification, and especially

the work of Hempel. Verification is essential in establishing the correspondence between the theory and the phenomena experienced or observed (146). Recognizing that this may allow a "set of theories purporting to explain the same physical phenomena" to exist, verification is used in its more general sense - of determining truth. In mathematics truth is synonymous with consistent logical relations, in the physical sciences it is correspondence with observables, but in the social sciences the answer is not so easy. The authors turn first to economic theory in an attempt to discern some pattern. While no single empirical verification concept in eceqnomics exists it does, the authors suggest, approach that in the physical sciences. If accounting theory has a real world frame of reference then it too must seek verification in terms of observables. What then are these real world referents? The authors suggest a few. An extensive appendix to the article examines recent empirical research studies on the basis of certain areas of practical significance for verification in accounting.

Griffin (1971) extends the argument substituting the word validation for verification the economic "model" is again described and related to accounting. The development of positive (cf normative) accounting theory, as in economics, is encouraged. Griffin (1972) opens with a much more general discussion of philosophical concepts which are then extended to accounting. Accounting is viewed as having developed first inductively then deductively (a priori). Evidence provided shows that "an appropriate methodology should be dictated by the nature

of the discipline: in accounting, apparently, evidence continues to accumulate showing that some theories can be "validated a priori, theories which appeal to reason and do not require resort to empirical verification. The cogency of other propositions call for empirical testing to establish their verifiability" (15). With such a conclusion there seems little point in having written the article! Griffin and Deskins (1972) although appealing to theory methodologists with its title contains nothing relevant to the field.

While this survey has examined only works relevant to the general methodological debate there have been works which have examined aspects of accounting theory formulations. If accounting is a process of measurement and communication then both these aspects must feature in methodological debate: they do. However it would not be practicable to include a survey of all these other areas in addition to the lengthy analysis of general methodological aspects. Professor Norton Bedford has made communication theory his area of speciality and has developed his income model (Bedford, 1965) on this basis (together with much of his other work). Ijiri is probably one author who has contributed most to the discussion of measurement theory and accounting: in fact two of his major works are directed to this subject and they form part of the major analysis of this research effort. Similarly Sterling (1970) devotes much of its space to measurement theory. The subject becomes at times an integral part of methodological argument as in Mattessich (1964) and Sterling (1970).

The reverse is also often true; for example, Larson (1967) and especially Larson (1969). The latter has contributed significantly to familiarizing many accountants with the work of Henry Margenau: The construct formulation process described by Penman (1973) is an extension of the Margenau description expounded by Larson (1969).

If recent attempts to determine users of financial reports, along the lines earlier suggested by Cowan (1968) and carried forward in FASB (1976) is significant, then behavioural theories will become an integral part of accounting theory. An early example is Prince (1963) but considerably more sophistication has been subsequently built in. The concept of systems and systems theories are so all embracing it would be difficult to know where to begin such a survey: The subject is inextricably linked to developments in all areas of business economics. Some work is easy to cite as being directly relevant, for example, Hines (1975). More importantly the work of Mattessich has developed into the more highly esoteric aspects of information systems theory in an attempt to determine a suitable accounting epistemological basis.

Considerable effort has been recently devoted to examining the relationship of modern portfolio theories to accounting and although they may liberally be included under the heading of behavioural theories they form such a significant subgroup as to stand on their own (many of the theories do not bear much resemblance to behavioural theories anyway).

Defining accounting in the context of its broad social goals obviously has implications for the development of accounting theories. In fact two works have developed highly significant theories along these lines: Gambling (1974) and Enthoven (1973). Interestingly however the starting point for both appears to be the more general theory work of Mattessich.[22]

Most of the discussion has concerned work carried out in the United States because that is where a large portion of it has taken place (if not written there, published there). However the one writer who has contributed most to the advancement of accounting theory is significantly non American - Australian Raymond Chambers. There have been other significant contributions from Australia. The British have tended to avoid considerable methodological debate and many concepts are authoritatively proclaimed. Practical problems have assumed a greater proportion of the efforts of accounting thinking which is understandable in light of the major economic problems facing or which have faced the British economy. Although some effort has been devoted to theoretical issues New Zealand thinkers have tended to be less concerned with philosophical issues and more concerned with interpreting or devising the numerous professional pronouncements.

This survey has included an examination of United States

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22. More especially Gambling

doctoral dissertation abstracts and an exhaustive search has failed to indicate any which investigate the philosophical aspects of accounting theory methodologies since 1960.

TWO SIGNIFICANT ARTICLES

The literature surveyed concludes with a review of two contributions by Australians; one published in Britain, one in the United States.

In Kam (1973) the author states that "the accounting profession appears to be moving in the direction of science." Science, to the author, is similar to that described by the logical empiricists. Tilley (1972) is also conscious of the scientific trend and his article, "Accounting as a Scientific Endeavour: Some Questions the American Theorists Tend to Leave Unanswered" he raises questions which appear to have been overlooked in the mainstream of accounting literature: "The unstated assumptions which the current 'scientific accountants' bring to <u>their</u> work" (288)

His first question concerns the overall aim of these accountants and he shows that in fact the notion of "scientific method" is not new and has "been in and out of fashion since the turn of the century" (288). However in the past it failed to produce useful insight into accounting and did not, Tilley asserts, because of the failure to relate theoretical propositions to empirical

evidence. "The result is that the field of accounting is littered with a host of untested speculations which no science could ever tolerate" (290); which presupposes an understanding of science as some unified body of knowledge supported by sets of facts which are all confirmable by reference to the empirical phenomena they represent. This is a pure vision of science which no doubt many "scientists" would like to hold. However it is a logical empiricist view of scientific knowledge which has in recent years been subject to considerable pressure because it has been shown by many researchers that taken to its extreme it is doubtful whether anything could be considered "scientific". Given his conception of science, Tilley finds the decision - oriented approaches to establish scientific accounting theory quite unsatisfactory for they do not justify either their faith in or their meaning of specific method.

To Tilley science is, as mentioned above, that presented by Cohen, Nagel, Ackoff and Russell [23] . An important question is whether science is identified by content or method. In coming down on the side of method he is supported by the work of the "philosophers of social science" and especially

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23. Cohen MR and Nagel, An Introduction to Logic and Scientific Method, Routledge and Kegan Paul, 1943; Ackoff RL, Scientific Method Optimizing Applied Research Decisions, Willey, 1962; Russell, Bertrand, The Scientific Outlook, George Allen and Unwin, 1949 and later, Woolf A, Essentials of Scientific Method 2nd Ed, George Allen and Unwin, 1928; and Campbell NR, Foundations of Science: The Philosophy of Theory and Experiment, Dover Publications, 1957; and others.

Richard S Rudner (Philosophy of Social Science, Prentice Hall, 1966).

Nevertheless science also has other characteristics which Tilley lists as:

> "(a) General discrimination
> (b) Generality and system
> (c) Empirical verification" (293)

Tilley (1972) is important in a survey of extant accounting theoretical writing for it presents ideas, which around the time of publication, were dominant. It summarizes (not intentionally) the theoretical articles in leading journals. It presents ideas found in AAA (1971a), AAA (1971b), Penman (1973), Iselin (1971) and Yu (1976). It presents the ideas ten years after the first publication of Kuhn (1970a). Kuhn's ideas had already been alluded to (for example, AAA, 1971a) but it was not until Wells (1976) that, in the mainstream theoretical literature, a full attempt was made to relate them to accounting developments.

The first part of Wells (1976) is devoted to a fairly literal summary of Kuhn's general thesis (Kuhn 1970a and Kuhn 1970b are the main references used). Accounting schools of thought are suggested and a possible accounting disciplinary matrix is outlined. The notion of professional insecurity is then illustrated with the methodological debates in accounting. In these Wells notes two themes, first the search for assumptions (examples given include, Moonitz, 1961, ASOBAT, 1966, and Ijiri, 1967) and secondly, the concern with principles and theory construction generally

(examples given include, Chambers, 1955), Mattessich, 1957, Devine, 1960, Chambers, 1963 and Sterling 1970b). The main thrust of the paper is to provide a rationale for the different views on accounting for inflation. The schools of thought are shown to exist in relation to the various notions of inflation accounting. Flamholtz (1976) also provides a rationale for Kuhnian model in accounting. Her discussion, however, is not restricted to notions of inflation accounting. Instead she examines financial accounting in whole and it is probably for this reason that her argument is unconvincing - her scope is too broad. This is a trap Wells (1976) does not fall into but for this research effort one particular comment is of particular significance:

> "If the analogy presented above is correct i.e., if Kuhn's notion of a revolution can be applied to accounting, then it appears that accounting is emerging from a state of crisis" (480).

CONCLUSIONS

This lengthy literature survey has been necessary to demonstrate that there has existed, at least since 1960 if not before, considerable concern over the theoretical structure of accounting. Intellectual dissatisfaction has existed because of the lack of methodological guidelines for research. Professional dissatisfaction has existed because of the lack of methodological guidelines for practice. Professional concern has arisen not so much from the failure of accounting to predict, prevent or otherwise indicate the numerous business failures but more

from the challenge from outside. Non accountants are requiring accountants to be accountable: (The Metcalf and Moss Reports, above) are they fulfilling their roles as effectively as possible.

Accounting literature has always had a strong practical emphasis because of the nature of the discipline. Attempts to develop a theoretical structure have always reflected this concern. This is borne out in the literature surveyed above. The survey grouped the literature into various categories. These were textbook presentations, major single author works, symposia or conference papers, institutional or group presentations and finally journal articles. The common theme has been concern for methodological explanations for accounting. In developing ideas most authors have agreed that a scientific approach is necessary. It is in their conception of science that most authors have differed. They are probably also agreed that in deciding a desirable framework recourse to other disciplines is a necessary prerequisite. This same soul searching has occurred in these other disciplines. The discipline most often associated with accounting is economic and it is interesting to observe that the process as having continued for some time: JN Keynes (father of the more well known J M Keynes) in fact was the author of such a book as early as 1891 (The Scope and Method of Political Economy) [24]. Accounting, as an intellectually

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24. Mark Blaug's, Economic Theory in Retrospect (Rev Ed, Richard D Irwin 1968) provides a good overall survey, S. Krupp's The Structure of Economic Science (Prentice Hall, 1960) an influential example (several papers).

rigorous and demanding subject, has only recently emerged from a fairly primitive set of technical procedures. It is only natural therefore that the ideas and concepts of the more academically respected disciplines be investigated and observed in order to determine the necessary methodological prerequisites. Most of the early work surveyed reflected this - the comparison was continually made with the more established social sciences. The next phase developed from this and soon recourse was made to the "primary sources" of methodological preconditions: the epistemological discourses in the philosophy of science. As with their counterparts in the other newer disciplines the accounting theorists opted for the most apparently accepted paradigm. The literature at this stage therefore reflected the ideas of the logical empiricists whose views dominated not only the literature of the philosophy of science but also the theoretical discussion in the so called social sciences.

By 1970 challenges to the logical empiricist view of theories were being noted in many of these social sciences. By far the most popular of these challenges were the ideas of Popper and Kuhn. Whereas the work of Popper requires a fairly extensive understanding of many philosophical aspects Kuhn presented something radically different and <u>on the surface</u> easily grasped. Where it is difficult to trace the development of some theoretical construct an early explanation is to say it arrived like "a shot out of the blue": a revolutionary idea. It is this aspect of Kuhn's work that has achieved considerable popular appeal and it

is found in the work of many disciplines. Early allusions to it in the accounting literature were promises of what was to come and by 1976 Kuhnian development had become a regular part of accounting curricula and theoretical literature.

Other themes have persisted in the literature and a not easily explainable one is the systems viewpoint. It has in fact existed seemingly distinctly from the mainstream described above. Systems is a ubiquitous concept and it is this very characteristic which makes description difficult. While many writers have maintained an overall teleological-like stance others have concentrated on sub-areas. These have taken many forms and descriptions. Still others have presented theories of sub areas of accounting: theories developed with sufficient rigour to warrant mention in any general survey of accounting thought (but regretably not in a survey of general accounting thought).

However this survey has considered only the major themes in accounting theory construction. The phases have appeared to follow that described above - pre-methodological, general methodological concern, specific methodological concern, and innovative methodological concern. A significant section has been omitted - the work of four very influential theorists who form the main subject of this analysis.

Buckley, Buckley and Chiang (1976) give four different research strategies:

1. Opinion Research
2. Empirical Research
3. Archival Research
4. Analytic Research

According to the descriptions they give, this present analysis belongs to the domain of neither opinion nor empirical research. "Archival research is concerned with the examination of recorded facts" and can be further classified as primary, secondary or physical. Analytic research involves "breaking down the problem into its component parts so as to discover its true nature and the causal relationships among its variables".

This research effort falls between these two descriptions. To the extent that it examines extant ideas it is archival. It does not involve the use of formal techniques found in archival research such as, for example, content analysis. Nor does it formally utilize sampling techniques in a strict technical sense: works selected are a priori. In this sense it fits more closely the analytic mode. The major deficiency, as Buckley, Buckley and Chiang (1976) see it, of analytic research is its inability or unwillingness "to apply the scientific method". As a major thesis of this analysis is that no one really knows what the "scientific method" is, it perhaps circumvents this problem.

This research effort then involves an historical examination of some theories of accounting in an attempt to determine their relation to the differing views of theory construction

in the philosophy of science. Models for this approach do exist. The initial conception was a result of reading Suppe (1974). Suppe is in two parts and it is the first section that inspired this work. It describes existing thought on theories then follows this by an examination of contemporary thinkers in an attempt to discern a true structure of scientific theories. The second part contains the proceedings of a symposium.

Since commencement on this research effort two accounting models have been published: the first is Anderson(1976) and the second the AAA Committee on Concepts and Standards for External Financial Reports (AAA, 1977). Reference has already been made to the latter. Anderson (1976) is specifically concerned with selected income measurement theories but his method is very similar to that conceived for this work.

The theorists selected for this study are:

Raymond Chambers
Yuji Ijiri
Richard Mattessich
Robert Sterling

Anderson (1976, 2) says "no group of authors can be proven to be definitely more appropriate for examination than any other". Nevertheless reasons can be given to support a particular choice.

Anderson's first choice - criterion is that the authors be "modern contemporaries". They would therefore be more likely to address themselves to the same sort of problems.

Anderson also suggests that "it helps if all participants in the project have a common basis for understanding each other". In this work the selected authors do obviously have a common basis but they certainly do not understand each other as the criticism of each other's works described later bear out. That they can provide differing solutions for the same problem is the major motivating force for this work.

These authors are among the most prolific in the accounting literature. While this extends the research effort it does provide a greater frame of reference on which to deduce the writers' particular predilection for theory construction. Anderson's third choice-criterion is that the authors have expressed their views" in a major work." This too was the case in this effort and these major works were all published since 1960.

Anderson's fourth choice-criterion was that "authors whose theory construction was thought conducive to the type of analysis of interest were selected". This is also true of this work in the context of some points made above, especially the fact that authors selected are the major contributors to accounting theory on the basis of output and apparent influence on other writers.

It was mentioned in the proposal for this research that "it is highly improbable that one year, viz 1960, would mark any turning point: to consider, for example, the theoretical writings of Chambers and Mattessich only since 1960 would be to preclude some important methodological

exploratory work". However the literature has borne out the fact that there was a perceptable change in emphasis around 1960. The APB was set up and the Accounting Research Studies (Moonitz, 1961, Sprouse and Moonitz 1963, etc) were commissioned. Devine's call for greater attention to methodological detail also appeared at this time (Devine, 1960). In addition, as mentioned above, the major works of the selected authors have appeared since that date.

Anderson (1976) further states that "the development of a useful methodology for theory comparison is a major potential benefit (of the study)". He then lists five characteristics of the desirable methodology all of which seem fairly obvious to a critical reader. The fifth is that "the methodology had to provide a basis for comparing the authors" (5). His methodology is a cross comparison technique based on a rectangular matrix structure. Because there are insufficient common features which could serve as elements this sort of approach is deemed inappropriate for this research effort and it is the models provided by Suppe and AAA (1977) that are followed. That is, a descriptive but logical analysis highlighting the main features in relation to dominant themes in the philosophy of science. This then requires a preliminary detailed discussion of these dominant themes found in the literature of the philosophy of science. The comparison will follow that.

It is considered highly likely that this analysis will show that there is no one methodology in accounting that is more suitable for developing theories than others. The authors selected for study have claimed a certain methodology but this has not affected their theories, although it has influenced the way in which they attempted to work. This, it will be shown, is due to a lack of knowledge of the way theories are constructed. This is not peculiar to accounting theorists alone, for as Suppe says:

> "For more than fifty years philosophy of science has been engaged in a search for philosophic understanding of scientific theories; today it is still searching" (4).

The next section will briefly follow the progress of this searching. It may well be that there is a distinction between philosophy and empirical disciplines which as Law (1975) argues makes epistemological issues, as conceived by philosophers of science, unhelpful. This does not account for the popularity of Kuhnian thought the influence of which is felt throughout all the sciences. Why other ideas have not been as readily accepted is probably due more to what Castells and de Ipola (1976) call "epistemological obstacles" than to carefully considered philosophies. As with many fields of study there are different levels of interpretation of notions in the philosophy of science. Kuhnian theory is an ideal example. Its general notion is attractive because it is simple, and, like distance, simplicity lends enchantment. In this case a more literal interpretation may be afforded enchantment for it is as if he (Kuhn) had cast a spell over the theorists

in many fields. Writers such as Baumberger (1977) have argued that much of the misinterpretation is due to Kuhn himself. That there is probably some truth in this is evidenced by the effort that has been devoted to Kuhn and his supporters to further explain some of his notions (not least of which is the notion of"Paradigm").

Other exponents of epistemological processes have not been free from criticism; for example Popper and his work have been the subject of many a debate. The latest writer to provide the science philosophers with traumatic, cognitively dissonant experiences is Paul Feyerabend whose dadaist sentiments have resulted in a general stirring in the academic bowels.

The literature surveyed showed that accounting theorists have, in the main, not kept pace with developments in the philosophy of theories. This is understandable for the survey also showed that in respect to philosophical development accounting emerged in 1960 from a fairly primitive state. It may still be fairly philosophically primitive but the sort of thought development outlined by the survey would tend to suggest not. It would hopefully suggest that, inspired by the work in other disciplines, accounting has worked towards providing a methodological structure for itself. Although developing very late accounting is in no worse position methodologically than many other disciplines: all are reliant on developments in the philosophy of science which is itself in disarray. The methodological uncertainly is apparent in other disciplines as it is in accounting. An analysis of four substantial theorists in accounting will demonstrate this.

THE RECEIVED VIEW

At the beginning of the century there existed three main German scientific philosophy schools. These were mechanistic materialism ("A blend of Comptean positivism, materialism and mechanism" ; Suppe, 1974, 8), Machian neo-positivism ("Science is no more a conceptual reflection upon facts whose elements are contents of consciousness given to us by sensations", Suppe, 1974, 9) and neo-Kantism (there are sensations which are in fact structures of phenomena of an ideal world structure and scientific laws exist to describe this structure). All three denied, or were incompatible with relativity theory and quantum theory - the new physics. This was so even though the Machian positivists were the most sympathetic and the result was a philosophical crisis - what new philosophy of science needed to be adopted to explain the nature of scientific inquiry. Several attempts were made and the two most acceptable were a modified neo-Kantism (under Ernst Cassirer) and "a weakened version of Mach's neo-positivism" in which the leaders were Hans Reichenbach (Berlin School) and Moritz Schlick (Vienna Circle) (Suppe, 1974, 11). Both these latter groups firmly affirmed Mach's verifiability criterion but also admitted mathematics and they were to dominate the physical and social sciences for approximately fifty years. The subject matter of scientific theories was phenomenal regularities which could, nevertheless, be characterized in theoretical terms. These theoretical terms would be explicitly defined in terms of phenomena and although they may be in mathematical

terms these were in reality nothing more than conventions with increased mathematical influence (in the Vienna Circle) it was finally considered that mathematical logic would be the most satisfactory medium for scientific theories: a theory is "to be axiomatized in mathematical logic (first order predicate calculus with equality)" (Suppe, 1974, 12). The axiomatizations had to include various explicit definitions for the theoretical terms of the form:

$$Tx \equiv Ox,$$

Where T is a theoretical term, O an observation term and the relationship called a correspondence rule. This was to be what Suppe (1974) calls the Received View [25].

The initial version of the Received View is summarised by Suppe (16-17) as a "mathematical logic L meeting the following conditions:

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

(ii) The nonlogic terms or constants of L are divided into three disjoint classes called vocabularies:
(a) The logical vocabulary consisting of logical constants (including mathematical terms).
(b) The observation vocabulary, Vo, containing observation terms.
(c) The theoretical vocabulary, V_t, containing theoretical terms

25. Much of the earlier parts of this section are strongly influenced by - in fact follow - Suppe (1974, 1 -232). The Received View was central in the logical positivism developed by the Vienna Circle and the subsequent developments. The actual naming is attributed to H. Putman in 1962

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

(iv) There is a set of theoretical postulates T whose only non-logical terms are from V_t

(v) The terms in V_t are given an explicit definition in terms of Vo by correspondence rules C-that is, for every term 'F' in V_t, there must be given a definition for it of the following form:

$$(x)\ (Fx \equiv Ox),$$

where 'Ox' is an expression of L containing symbols only from V_o and possibly the logical vocabulary."

The Received View underwent an evolution over the next fifty years. Although it is interesting to trace this transformation it is not within the scope of this research effort to do so: Suppe (1974) does and ends with a final version of the Received View (50 - 53). It is mainly the items (ii), (iii) and (v) which change most.

Carnap in 1956 [26] gives the final version of (i):

"There is a first-order language L (possibly augmented by modal operators) in terms of which the theory is formulated and a logical calculus K defined in terms of L" (Suppe, 1974, 50).

The second part, (ii) is worded a little more precisely in light of the changes and becomes:

"The nonlogical or descriptive primitive constants (that is, the 'terms') of L are bifurcated into two disjoint classes:

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26. Carnap, Rudolf, "The Methodological characteristics of theoretical concepts "in Feigl, H and Scrivans, M (Eds) Minnesota Studies in the Philosophy of Science, Vol III University of Minnesota Press, 1956

> Vo which contains just the observation terms;
>
> V_t which contains the nonobservation on theoretical terms;
>
> Vo must contain at least one individual constant" (Suppe, 1974, 50)

The question of interpretation is of vital concern and it was the subject of considerable debate and consequent reworking in the Received View framework. The matter is closely interrelated with correspondence rules. The final version is greatly extended and given by Suppe (50-51) as:

> "The language L is divided into the following sublanguages, and the calculus K is divided into the following subcalculi:
>
> (a) The observation language, Lo, is a sublanguage of L which contains no quantifiers or modalities, and contains the terms of Vo but none from Vt. The associated calculus Ko is the restriction of K to Lo and must be such that any non-Vo terms (that is, nonprimitive terms) in Lo are explicitly defined in Ko: furthermore, Ko must admit of at least one finite model.
>
> (b) The logically extended observation language, Lo, contains no V_t terms and may be regarded as being formed from Lo and adding the quantifiers, modalities, and so on, of L. Its associated calculus Ko is the restriction of K to Lo.
>
> (c) The theoretical language, L_t, is that sublanguage of L which does not contain Vo terms; its associated calculus K_t, is the restriction of K to L_t.
>
> These sublanguages together do not exhaust L, for L also contains mixed sentences - that is, those in which at least one V_t and one Vo term occur. In addition it is assumed that each of the sublanguages above has its own stock of predicate and/or functional variables, and that Lo and Lo' have the same stock which is distinct from that of L_t"

As with (ii), (iv) is modified to be consistent with other changes:

> "Lo and its associated calculi are given a semantic interpretation which meets the following conditions:
>
> (a) The domain of interpretation consists of concrete observable events, things, or things-moments; the relations and properties of the interpretation must be directly observable.
>
> (b) Every value of any variable in Lo must be designated by an expression in Lo.
>
> It follows that any such interpretation of Lo and Ko, when augmented by appropriate additional rules of truth, will become an interpretation of Lo and Ko. We may construe interpretations of Lo and Ko as being partial semantic interpretations of L and K, and we require that L and K be given no observational semantic interpretation other than that provided by such partial semantic interpretations."

The final part is the most detailed part of the Received View. Along with interpretation it received considerable attention. Suppe notes that no single comprehensive account of the main architects of the Received View, Carnap and Hempel exists. He has extracted his final version from most of their recent writings: other versions exist. Given this the fifth element becomes, in Suppe's words (1974, 51-2):

> "A partial interpretation of the theoretical terms and of the sentences of L containing them is provided by the following two kinds of postulates: the theoretical postulates T (that is, the axioms of the theory) in which only terms of V_t occur, and the correspondence rules or postulates C which are mixed sentences. The correspondence rules C must satisy the following conditions:
>
> (a) The set of rules C must be finite.
>
> (b) The set of rules C must be logically compatible with T.
>
> (c) C contains no extralogical term that does not belong to Vo or V_t.

> (d) Each rule in C must contain at least one Vo term and at least one V_t term essentially or non-vacuously.
>
> Let T be the conjunction of the theoretical postulates and C be the conjunction of the correspondence rules. Then the scientific theory based on L, T and C consists of the conjunction of T and C and is designated by 'TC' ".

Suppe readily admits he is making a value judgement in "making the Hempel's and Carnap's version the final version". He claims it is "the most sophisticated and least vulnerable version".

Adherents of the Received View held it to be an adequate explanation of all scientific theories and if a theory did not meet the conditions above then it was not scientific. However it has come under serious attack.

The conditions laid down in the final version are seldom achieved. It can only be held to be an ideal which is rarely achieved. Logical positivism is a very restrictive and even simplistic philosophy. Not all scientific theories can be axiomatized as has been demonstrated in the theories which have been held and contributed to the development of many disciplines (for example, anthropology, sociology, paleontology and many others). This is adequately demonstrated by Suppe (1974) even accepting the qualification of "fruitful": fruitful axiomatization because "any theory can be axiomatized in a trivial fashion" (63).

Suppe (and others, for example Naess, 1972) have also shown that the Received View has been severely attacked for its

reliance on the observational - theoretical distinction. There have been three sorts of attacks: first, it has been shown that the analytic - synthetic distinction, derived from Kant, is untenable. Secondly "the observational - theoretical distinction cannot be drawn for scientific languages" (Suppe, 1974, 67), and thirdly the history of scientific progress has shown observations incompatible with the observational - theoretical distinction. In fact Suppe (1972) argues that"the observation - theoretical distinction obscures much that is epistemologically important and revealing about how theories relate to or connect with phenomena" (18).

The notion of partial interpretation is axial to the Received View as it is involved in the analytical-synthetic distinction, the observational - theoretical distinction and doctrines about meaning. However it is, as writers have shown (Suppe, 1974 and Suppe, 1972), "notoriously unclear".

The Received View described above is the formal notion of scientific theory development held by philosophers of science for some time. It is important to all disciplines where there have been advocates of scientific theory construction. Just as a detailed description of it is without the scope of this research effort, so is a detailed analysis of arguments levelled against it. In general terms it is the ideal form of logical positivism or scientific empiricism described by many writers. Attacks on it have been sufficiently common for it to be rejected

by most philosophers of science (according to Suppe). Criticism varies with what critics feel is the greatest source of inadequacy:

> "Philosophy of science is not a monolithic enterprise and there is wide diversity of opinion among philosophers of science as to the effect of these various criticisms on the Received View" (Suppe, 1974, 116).

Advocates of scientific theory construction have relied on such notions held by the philosophers. In so far as accounting is concerned it has been shown in the literature survey that what was for the most part described as proper theory construction was in fact significantly derivative of the Received View. For example, Yu (1976), AAA (1971b) and Penman (1973) [27].

If the Received View is not accepted by philosophers of science, what is? Unfortunately there is, as yet, no clear-cut answer, so much so that the methodological turmoil found in other disciplines is no less apparent in the philosophy of science. Various suggestions have been made from positions of modified positivism to complete methodological anarchy. If the "experts" cannot find an answer, it is highly unlikely that others relying on them will. This in fact is the central proposition of this research effort: there is no <u>one</u> theory methodology that is accepted as being more correct than others. Accepting this, this present work analyses extant theories from different viewpoints and find

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27. Diagrammatic presentations are afforded: See Yu (1976), 23, 24 and 92; AAA, (1971b), 58 and 60; and Penman, (1973), 206, 212, 217 and 219. All make the theoretical-observational distinction and support the strict formalisation.

perhaps support for one methodology as seeming to be able to explain theory construction better than others. Attention is now turned to briefly reviewing some of the alternatives suggested. Suppe (1974) in rejecting the Received View in fact suggests that an adequate analysis of theories must possess the following nine characteristics (117):

1. The analytic-synthetic distinction must not be presumed.
2. No distinction between direct-observation and nondirect observation terms may be assumed.
3. Theoretical terms must be construed as being antecedently meaningful, though their incorporation into a theory may alter their meanings to an extent.
4. The meaning of theoretical terms may incorporate, or be modified by, recourse to analogies and iconic models.
5. The procedures for correlating theories with phenomena must not all be viewed as integral components of theories; at least some of them must involve auxiliary hypotheses and theories.
6. The procedures for correlating theories with phenomena must allow for causal sequence correlations and for experimental ones; the experimental correlations must be spelled out in full methodological detail.
7. The analysis cannot view the entire content of theories as being axiomatizable or formalizable.
8. Whatever formalization is involved must be semantic, not syntactical.
9. The analysis of theories must include the evolutionary or developmental aspects of scientific theorizing, and not limit itself to providing canonical formulations of theories at fixed stages of development.

ALTERNATIVES TO THE RECEIVED VIEW: SKEPTICAL ANALYSES

There are three groups of alternatives to the Received View of theories according to Suppe:

Skeptical Descriptive Analyses
Weltanschauungen Analyses
Semantic Apprcaches

Suppe describes skeptical descriptive analyses as being similar to the above mentioned primary proposition of this effort:

> "Rather than presenting an analysis how theories ideally should be formulated (an ideal that theories in practice fail to meet), it is held that an adequate analysis of theories should characterize theories as they are actually employed in science." (1974, 120)

Suppe sees Peter Achinstein [28] as a central figure in the skeptical analyses. Feyerabend is included in the Weltanschauungen analyses approaches but his work published since Suppe went to press would no doubt make him a leading contender for Achinstein's "position". Essentially these philosophers despair of there ever being analysis which will indicate deep properties common to all theories because of the variety of extant theories. Suppe is critical of such a stance because although there are many sorts of theories (see Rapoport, 1958), this does not mean that "theories are merely possibly entertained systems of propositions". The fact that there are many different theories (along the lines of Rapoport, 1958) suggests that there is indeed no general structural analyis of theories possible but there are reasons to suppose deep structural

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28. See, for example, Achinstein, P, Concepts of Science, Johns Hopkins Press, 1968

properties are common to all theories.

WELTANSCHAUUNGEN ANALYSES

Those adopting Weltanschauungen Analyses are Toulmin, Kuhn, Hanson, Popper and Feyerabend. There are others but these represent the more important ones. Not everyone would agree with this general "grouping" because of the widely disparate analyses offered by these philosophers. However their similarity is in their viewing scientific progress as being from within a conceptual perspective which is shaped from the way scientists experience the world: science is an ongoing cultural activity with all its dynamical, dialectical and normative aspects.

To Toulmin, theories are laws, hypotheses and ideals of natural order. Although they are used to predict, their main function is to provide explanations of those recognised regularities. Prediction or forecasting "is a craft or technology, an application of science rather than the kernel of science itself" (Toulmin, per Suppe, 1974, 128). His account is instrumentalistic and his theories contain two components: first, ideals of natural order, and secondly, other laws used to account for phenomenal deviations from the ideals. "Scientific theories are formulated, judged, maintained and developed relative to a Weltanschauungen which includes the changed meanings attached to terms after they undergo a language shift resulting from their incorporation into the theory" (Suppe, 132). This denies the Received View's theory

reduction assumption and theoretical terms become theory dependent while retaining part of their pretheoretic meaning.

The Received View's doctrine of neutral observation language is discredited by Hanson (1958), who early demonstrates that observations are in fact, "theory laden". In these there lies a logic of discovery. "Theories put phenomena into systems" (Hanson, 1958, 90) and this means that they are not discovered by induction but by retroductive inference. Hanson does not, however, present any detailed analysis of theorizing and his retroductive reasoning depends heavily on his doctrines of theory laden observation and meaning dependence.

For most people interested in the development of scientific knowledge the work of Popper, Kuhn and Feyerabend is both interesting and stimulating. They are this in themselves and in their interrelation: Kulka (1977, 325) suggests that what was "the Carnap-Popper controversy has been overshadowed by the Popper-Kuhn controversy".[29] To do their work justice in the context of such a research effort is obviously not possible. However it has been claimed, earlier, that these philosophers, and most especially Kuhn, have been interpreted superficially and it is hoped that this work does not fall into the same mode.

A popular view of Popper's work is that for him science cannot progress through induction. Consequently the verification criterion of cognitive significance is rejected.

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29. A fascinating account of the former is contained in Michalos, Alex C, The Popper - Carnap Controversy, Martinus-Nijhoff, 1971

Scientific theories cannot be verified by observational evidence but they can be observationally falsified. Hence, observational evidence is important only for its ability to falsify hypotheses and "only those general statements which are falsifiable are to be classified as genuine scientific statements" (Harre, 1972, 60). The main problem of philosophy of science is the growth of knowledge and this cannot be reduced to a study of artifical languages (artifical logical calculi) and herein lies his second criticism of the Received View.

In Popper (1956) he describes three views of knowing - the essentialist, the instrumentalist and one involving conjectures. He rejects the first two in favour of the third and it is from here that the often quoted statement is taken - that scientific theories are "...genuine conjectures - highly informative guesses about the world which although not verifiable (i.e. capable of being shown true) can be submitted to severe critical tests. They are serious attempts to discover the truth" (1956, 382). Because theories are conjectures, science ought to proliferate theories as much as possible subjecting them to possible empirical falsification. It is through this process that scientific knowledge progresses - the conjectures are continually revised to answer additional refutations - "we learn from our mistakes". He is, as Law (1975, 318) calls him, "a revolutionary conventionalist". This is an allusion to the work of others such as Kuhn and Feyerabend for, while Kuhn holds the view that explanation for scientific change is within the realms of the social

psychology of discovery, for Popper, "scientific change is rational or at least rationally reconstructible and falls in the realm of the logic of discovery" (Lakatos in Lakatos and Musgrave, 1970, 93).

Suppe contends that the work of Paul Feyerabend develops out of Popper's philosophy: he accepts "that theories are falsifiable but not confirmable, that theoretical and observation terms are dispositional and theory-laden, that science ought to proliferate theories, and the growth of scientific knowledge comes via that proliferation of theories" (1974, 170). Feyerabend is said to have taken Popper's neo-rationalism and carried it to its ultimate conclusion and in so doing has attracted such descriptions as methodological dadaist (Tibbets, 1977), "egalitarian romantic", "against intellectual standards" (Hattiangadi, 1977) and epistemological anarchist (Kulka, 1977a). He would prefer the designation "theoretical pluralism". This theory pluralism is the core of his work. He sees the Received View version of scientific theorizing revolving around two self imposed conditions. The first of these is the consistency condition (derivability condition) and the condition of meaning invariance (stability thesis). Like Hanson, he believes factual observations are context-dependent and his attack on the Received View is to show the two conditions described untenable.

His alternative is "that it is possible to have a philosophy

of science wherein theories are testable on the basis of observation" (Suppe, 1974, 177).

Since Suppe (1974), Feyerabend's book, Against Method has been published (Feyerabend, 1975) in which his ideas are a little more advanced (or extreme). In this it is apparent that he has taken the Weltanschauungen analysis to its limits. Recently three reviews of the work attempted to assess its contribution for understanding the epistemological processes of scientific progress (Hattiangadi, 1977; Kulka, 1977; Tibbetts, 1977). Feyerabend's response (1978) is caustic, bitter and at times destructively ad hominem. So much so in fact, that in reply, one of the reviewers (Hattiangadi) has asked, if it is acceptable for Feyerabend to argue for anything goes why cannot a reviewer of his work be granted the same prerogative.

This is in no way presented to suggest that Feyerabend's contributions be dismissed. Although, as mentioned above, the present analysis can do little more than present the "essences" of recent work in the philosophy of science, it is important to note Feyerabends' main thesis for it is an interesting and valuable analysis for understanding scientific progress. According to him options must at all costs be kept open: a single methodological or epistemological prescription must not be exhorted. It is however some of his apparently more extravagant claims that have attracted most criticism. While it is not unusual for radical viewpoints to attract followers, extremism is likely to frighten off sympathisers. Such is the case with

Feyerabend's insistence on the inclusion of certain areas of study, which most people have considered pseudo-science, as subjects worthy of the nomenclature of science; for example, magic and astrology. Little wonder then that reviewers are led to make statements such as:

> "I agree with Feyerabend that to deny a priori that non-scientific techniques are effective is dogmatism. But to assign equal or greater success to nonscientific procedures, as does Feyerabend is in my opinion socially irresponsible, for it only reinforces the beliefs of that segment of society who retain a stone-age mentality towards anything beyond their comprehension." (Tibbetts, 1977, 26a).

And, when confronted with several entries under the heading "wicked remarks" in the index, it is hard to know just how serious he is. (His response, Feyerabend, 1978, could perhaps be an indication of his intention!).

Hattiangadi (1977) sees Feyerabend's main arguments as being grouped around into three attacks, or "salvoes" on traditional thinking on methodology. The first of these is "a general one against all intellectual standards", the second, "against the particular intellectual standards associated with respect for facts" and the third, against, "methodological rules". Following on from these is an analysis of Lakatos's work and amongst other things the "incommensurability" argument so central in Kuhn's work. His conclusion is that "science is much closer to myth than scientific philosophy is prepared to admit". His answer lies in methodological pluralism or anything goes or methological anarchy or what ever euphemistic description is suitable.

Of all the philosophers of science in recent years the one whose work has attracted the most attention of writers and researchers outside that field is Thomas Kuhn. He in fact is the only one of the "new" scientific philosophers whose analysis has been related to accounting: see, for example, Wells (1976), AAA (1977) and Flamhotz (1976). It was mentioned above, however that this wide appeal is partly due to an oversimplified interpretation of the full implications of his work. It is now apparently fashionable to drop his name in somewhere, as for example in AAA (1971b). Of the accounting writers making reference to Kuhn's work, Wells (1976) provides the best summary. The notion of revolutionary progress in any discipline is undoubtedly an attractive way to describe the development of thought. However it often seems that something very basic is overlooked and that is the notion of incommensurability.

The Received View theorists (or, more generally, the logical empiricists) sought an explanation for all theories in terms of <u>form</u>. Their tool was modern logic and if all theories or theoretical statements can be reduced to a logical form then it follows they <u>must</u> succeed. A major danger in such analysis is that it would be concerned with a static view of science : a cross section of science at some stage of development. Those proposing alteration have argued for an understanding of the development of scientific thought: the dynamical aspects. Logical method must then be exchanged for historical method and that is what the Weltanschauunger analysts (most) have been concerned with.

None more so than Kuhn and it would seem that this historical approach is a first point of attraction for many would-be-Kuhnists. This is despite the claims by some that his analysis is suitable for the study of the history of science but not for a novel new metatheoretic conception. It is also despite claims by Popper that it is an approach concerned with "the psychology of research."

There are also apparent "stages" in the development of the Kuhn hypothesis, first of which was the publication in 1962 of his The Structure of Scientific Revolutions. Certain areas were found by critics to be unsatisfactory, so a post-script was added to a second edition of the book (1970a). This included some of the ideas developed in some work written slightly prior to this but published later: Kuhn (1970b; 1974). The "third stage" is found in Kuhn (1976) and has not been fully appreciated by the scientific community (or found to be utterly unsatisfactory, see Feyerabend, 1977).

Given the historical emphasis of Kuhn's work, he has tried to show that all science consists of revolutionary discontinuities and nonrevolutionary periods. His analysis sees disciplines passing through several stages:

Preparadigm (unscientific chaos)
Acceptance of a dominant paradigm (normal science)
Crisis due to anomalies in accepted paradigm
Extraordinary or revolutionary science (search for a new paradigm - scientific chaos).
Acceptance of dominant paradigm and normal science (puzzle solving).

The greatest dissatisfaction with his notion as presented in his first edition was his use of the term "paradigm" and consequently in his second stage (Kuhn, 1970a etc) he distinguishes between a disciplinary matrix and exemplars. The former represents the overall structure of that which scientists hold common to their discipline and the latter concrete problem solutions accepted by the scientific community. A disciplinary matrix contains shared exemplars.

There are two other very important (but related) notions in the Kuhn thesis. The first is that scientific knowledge progresses discontinuously; it does not, as previously held, progress through accummulation. Toulmin had, much earlier implied it but it becomes central to Kuhn's notion of progress through revolution. The second notion is that of incommensurability. One paradigm replaces a previous one and cannot be explained in terms of it. Kuhn differs from Popper in that with this progress through revolution there is no method of strict theory history of falsification and corroboration. The thesis is sometimes called, as Stegmuller (1976, 137) points out, "Kuhn's irrationalism", a name which is"however, quite misleading, because it makes one think of an irrational philosophical position on Kuhn's part". (137).

Kuhn himself has recently clarified his idea of his use of the term incommensurability:

> "...when I spoke of theories as incommensurable, I meant that they could not be compared. .....In applying the term incommensurability to theories, I had intended only to insist that there was no common language

> within which both could be fully expressed and which could therefore be used in a point-by-point comparison between them". (1976, 191)

Kuhn's position is both attractive and alarming in its apparent simplicity. The former is borne out by the acceptance afforded it in many disciplines, in economics (e.g. Stanfield, 1974) in sociological study (e.g. Ford, 1975), and accounting (e.g. Wells, 1976) to name but a very few. The latter is evidenced in the considerable criticism it has been subjected to from other philosophers of science (e.g. Lakatos and Musgrave, 1970). It is suggested that its appeal is often unwarranted and its use as providing an explanation for epistemological questions in many areas has been based on an oversimplified appreciation of it. This is not so much a criticism of Kuhn's thesis as so much of the people who have "used" it. Even so, Kuhn's early use of the term paradigm was severely criticised for its vagueness (Lakatos and Musgrave, 1970) and he has been forced to qualify it (Kuhn, 1970b; 1974). One writer has even suggested that much of Kuhn's appeal lies in the fact that in the environment of strict logical empiricism Kuhn humanised scientific work:

> "After decades during which science had been viewed as a series of disconnected logical decision calculi (which may ultimately be performed by computers, and thereby by totally rationalized), there comes a fellow who dares to say that, after all, <u>science is genuinely and basically human</u> .... Kuhn was the author who brought man back into the picture.." Baumberger, 1977, 3).

More than just this however, there is, in Kuhn's work, much that is esoteric and as yet unresolved. Many questions

need to be answered and much more detail given to some aspects. The wholesale adoption of the overall superficial characteristics is premature to say the least. Many of these attempts themselves lack the detail for they cannot provide the answers. For example to simply outline the thesis then fit convenient historical "facts" to the overall framework and offer that as explanation for theory development (e.g. AAA, 1977; Stanfield, 1974) is not acceptable.

Ironically, formalism against which Kuhn was reacting has recently contributed to the methodological rigour of his thesis: a set theoretical presentation. It has arisen largely from the work of Joseph Sneed (Sneed, 1971) and has been contumeliously referred to by Feyerabend (1977) as "Kuhn Sneedified". Most of the work has subsequently carried out by Wolfgang Stegmuller,[30] Sneed having developed his view on the basis of particle mechanics only. However Kuhn (1976) says "the new formalism makes important new territory accessible to analytic philosophy of science" (179) and "...even its elementary structural form captures significant features of scientific theory and practice notably absent from the earlier formalisms ..." (180). Stegmuller claims even more advantages (1976, 10-14).

Sneed's formalism requires the specification of a theory (T) which demands specification of not only a core (K) but also

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30. Most of Stegmuller's work has been in the German language which has necessarily restricted its reading in the English speaking world. However works are now being printed in English and although Stegmuller (1976) is the translation of volume II of an extensive treatise on the philosophy of science it contains most of his ideas on matters relevant to this work.

a set of intended applications (I). The theory may be presented as an ordered pair:

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

The core, K, is made up of M, a set of models, Mp, a set of possible models, Mpp, a set of possible partial models, a set of possible constraints, C on these models and a set of variables (Sneed, 1971, Chpt 7)

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

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

A student of a scientific theory, familiar with Mpp will attempt to determine Mp and "..it is a large part of what scientific, or at least physics, education is about" (Kuhn 1976, 181). In addition to this "creative imagination required to find an Mp corresponding to a non standard Mpp .. is among the criteria by which great scientists may sometimes be distinguished from mediocre" (Kuhn, 1976, 181). Herein lies a question, for neither Sneed nor Stegmuller have much to say about this process outside fully mathematized theories.

Essentially Sneed's theory is a set of distinct applications: "learning a theory is learning successive applications in some appropriate order and (that) using it is designing still others" (Kuhn, 1976, 182). The second part of the quotation refers to the notion that applying a theory means discovering <u>new</u> applications which are found extensionally (as members of I) or by expanding the core K to get E. To Sneed and Stegmuller this expansion occurs mostly within normal science. However they also recognise

that in some instances this would represent what Kuhn calls a scientific revolution and Kuhn (1975) is concerned at the difficulties of identifying these.

The expanded core would contain new constraints (C_a), a set of additional functions (v_a) and a set of special laws for special applications (L), and:

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

and therefore

$$T_E = \langle M, Mp, Mpp, C, v, L, Ca, va, I \rangle$$

"Two men who subscribe to different cores ipso facto hold different theories. If, however, they share belief in a common core and in certain of its exemplary applications they are adherents of the same theory even though their beliefs about its permissible expansions differ widely."

"A core, in short, is a structure that cannot, unlike an expanded core, be abandoned without abandoning the corresponding theory" (Kuhn, 1976, 187).

The above has necessarily been an oversimplification of those new ideas, however the value to understanding Kuhn's notion of theory dynamics should be obvious: a formal analysis to understand the distinction between normal and revolutionary progress.

Returning to Suppe's (1974) analysis he discusses criticism of the Weltanschauunger analysis under which the headings corresponding to recurrent themes in the works:

- Observation is theory laden
- Meanings are theory dependent
- Facts are theory laden;

all of which leads him to enquire as to "How plausible is it to suppose that each person working with a theory possesses a Weltanschauung?". (218) At least two substantial developments have taken place since the question was asked which could indicate answers in the affirmative.[31] It is nevertheless interesting to note that Suppe concludes:

> "....the Weltanschauung is an exceedingly complex entity, being approximately the whole of one's background, training, experience, knowledge, beliefs, and intellectual profile which is of possible relevance to working with a theory. Once this is realized, it becomes exceedingly doubtful whether a Weltanschauung can be the joint possession of a group of scientists - as, for example, Kuhn's analysis requires". (218)

SEMANTIC APPROACHES

Suppe refers to the third group of alternatives to the Received View as, "Semantic Approaches". Theories are seen as extralinguistic entities describable by their linguistic formulation. The notion of models plays an important part. The idea that scientific theories have as their subject matter a class of phenomena is also important. (Suppe, 1974, 223; Suppe 1972, 12-13). These phenomena are its intended scope and, "A central task of a theory, then,

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31. The Sneed-Stegmuller analysis of Kuhn and the extreme methodological anarchism of Feyerabend, discussed above.

is to present descriptive, predictive, and possibly explanatory accounts of the behaviour of physical systems which correspond to phenomena" (Suppe, 1972, 13).

Suppe's own work serves to exemplify the semantic approach. The approach is complex (especially as much of the work has been directed towards mathematical physics) and, as Suppe himself readily admits (1974, 230) is still in the process of development. The development has he claims, been sufficient to indicate "a promising approach to analyzing theories".

SUMMARY

This section has traced the main ideas appearing in the philosophy of science relating to the process of theory construction. That described by many authors in many disciplines as the "scientific method" has been a reflection of the majority view: in almost every case this has been along the lines of the Received View. Frustration and dissatisfaction with many unsatisfactory aspects of that analysis has led some writers to cast serious doubts on it. According to these "new" writers theories are heavily influenced, or even dependent on, the theorists'own personal view of the world. To support this view writers have drawn examples fromthe history of science.

The work of Thomas Kuhn has been popular to the extent of being a fashion. Kuhn's own analysis provides a justification for his argument in the sense that it represents a significant

break in past notions full of ambiguity and anomaly. The acceptance afforded his work however appears to stem from its apparent simplicity: it is very easy to see sudden changes in commonly held beliefs within a discipline but this does not necessarily explain how the change came about. Realizing this many philosophers of science, including Kuhn himself, have been developing a more rigorous analysis. It is from this work that most advances in understanding the theory process are most likely to eventuate. This particular analysis has necessarily been simplified and synoptic. It does provide, however, the basis for the analysis in the next section.

It was mentioned above (in section IV) that works do exist which provide a good basic framework also suited for this research effort. One such work is Anderson (1976) who also discusses the work of Sterling and Ijiri. While recognising that the discussion in the previous section suggests that it is not possible to make such a distinction, it is stressed that this analysis is concerned with the form and not the content of the work of some accounting theorists. Therefore unlike Anderson (1976) which has its central subject, the notions of income measurement, this work examines the manner in which notions such as those were derived. There are three basic sources of such information: first, there are the works of the particular theorists which are directed to the presentation of their theories, secondly there are the works of the theorists which describes their own views of the theory construction process, and thirdly, there are the works of others analysing these theorists' works (mainly in the form of reviews). The first two sources are most important to this work - the theorists' works in light of the developments sketched in the last section.

BRIEF CHRONOLOGICAL SURVEY

If it were possible to devise scales to measure written work and relevant contribution to accounting thought then it would be possible to graph the work of Chambers, Ijiri, Mattessich and Sterling over the last thirty years. In such circumstances the graph line for Chambers would probably be a smooth parabolic curve with its highest point

representing Chambers (1966a). Ijiri's would be a much steeper bimodal curve the peaks indicating Ijiri (1967) and Ijiri (1975). Although Mattessich's curve would be gently rising to a high point representing Mattessich (1964) it would soon leave the graph and enter another dimension: although still concerned with theory his later work has not been restricted to accounting theory. Sterling's curve would be as steep as Ijiri's though with only one high point, representing Sterling (1970b), and a significant right skew.

There is little doubt that the above description would be rejected as being wildly speculative. It was included to provide a more dramatic visual image of the differences in the contributions made by the four theorists to accounting thought.

(a) Raymond Chambers

Chambers' contribution to the accounting theory literature has been both extremely substantial and extremely significant. For the theory - analysist the work of no other accounting theorist is so complete. His work prior to his Autobiography (1966a) demonstrates the build up to that work and his work subsequent to it has reinforced the theory contained therein. His autobibliography (1977) while not including all his work is sufficient to demonstrate that his contributions to accounting thought, even though they are considerable have been consistent. It is not as easy to say the same for his views on theory construction which is perhaps quite natural given developments in the philosophy of science.

Chambers' article (1955a) has been regarded as a seminal work in modern scientific accounting. To Yu (1976, 99) it is one which "has since rendered a tremendous impact upon accounting thinking". In AAA (1977, 12) it is held to be "the starting point for a decision - usefulness theory of financial accounting". His articles (1955b), (1957) and (1960) are extensions of the first mentioned work and the process culminates in his (1961) and is reinforced in his (1962).

Even if Chambers himself had not stated as much (1974c, 10) there would be little doubt that "Towards a General Theory of Accounting" (1961) contributed significantly to his book (1966a): "Much of this part of the paper (i.e. 1961) developed into the earlier chapter of Accounting Evaluation and Economic Behaviour..." As mentioned (1966a) represents the high point in the Chambers curve. His work subsequent to this has been of four main types. First there is the rigorous defence of the theory in Accounting Evaluation and Economic Behaviour (AEEB) (1966a). Secondly there is the extension of that work or, more correctly, the minor amendment to it; for example, (1970a). There has in most Western countries been considerable interest in accounting in inflationary times, for obvious reasons. Chambers (1966a) provides the most satisfactory theoretical answer and recognizing this as a good "selling point" of his theory, Chambers has devoted considerable effort in making concerned parties aware of his work: this is the third type. The labels "other work" or "miscellaneous" are never very satisfactory for they can never indicate the extent of the efforts included therein. However to avoid unnecessary detailed

classification this is the fourth type of his work. This classification is general and most works overlap considerably because behind all of them is the omnipresence of the theory in AEEB (1966a). Therefore there is purpose in a book such as Securities and Obscurities (1973a) for, while demonstrating the commonality of anomalies in accounting practice, the notions in AEEB (1966a) are noted as being the only possible solution.

(b) Yuji Ijiri

In suggesting that the curve for Ijiri's work would be bimodal, allusion was being made to his two major works on (financial) accounting theory: (1967) and (1975). These are by no means his only works and in fact he is a very significant contributor to an area of accounting and management knowledge well outside the scope of this research effort - management information and control systems. Ijiri (1967) was not his first major work and developed around some previous work - more especially, Ijiri (1965), Ijiri (1966), Ijiri and Jaedicke (1966) and Ijiri, Jaedicke and Knight (1966). It "inquires into the measurement aspects of accounting . Measurement is the core of accounting" and an understanding of it is a necessary prerequisite for understanding accounting. Ijiri (1967) is an attempt to provide a theory of accounting "as it is and not as someone thinks it ought to be" (x)

In attempting to make his book (1967) readily accessible to the comprehension of a wide audience Ijiri had simplified many of his expressions. This no doubt caused a little

frustration and in his second book (1975) the previous analysis is condensed into two chapters and presented more vigorously. There appear to have been only a few papers published between 1967 and 1975 and they, often the result of conference - symposia contributions, are also incorporated into the latter book. The remainder of the argument is new. There does not appear to have been any further, readily available material published by Ijiri since his (1975).

(c) Richard Mattessich

The pattern of the theoretical contributions of Mattessich comes closest to that of Chambers, initially. His first major work is his Accountancy and Analytic Method (AAM) (1964) but the ground work had been laid earlier. In fact his article of 1957 is to his AAM (1964) as Chambers' article of 1961 is to Chambers' AEEB (1966a). The titles of Mattessich's works are usually indicative of their contents. His early work therefore is primarily concerned with two aspects of accounting. His first concern was with devising a far more rigorous theoretical base to the discipline. This he argued would be achieved through greater use of mathematics. His second concern was in relating the then much more theoretically structured discipline of economics to accounting. Mattessich's cultural background is probably significant in respect of this second matter for he is a German by birth and the accounting discipline is there (Europe) much more imbued with theoretical economic notions.[32]

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32. See Choi, DS and Mueller, Gerhard, An Introduction to Multinational Accounting, Prentice Hall, 1978

His first major work is entitled, Accounting and Analytical Methods. Apart from some intellectual sparring with Chambers, starting first with his comments on Chambers' review of his AAM (1964)[33] and continued in his articles of 1970(a) and 1971, his work begins to move rapidly away from accounting. He first concerns himself with epistemological questions in accounting: as witnessed by his working paper (1968), his paper to the International Conference of Accounting Educators (1970b) and the award-winning (AAA Significant Contribution to Accounting Literature, 1972) article of 1972. The title of every subsequent work alludes to systems and it can safely be assumed that this difficult to define subject area becomes his major interest. Although not available at time of writing this research effort, his latest work is entitled, Instrumental Reasoning and Systems Methodology.

(d) Robert Sterling

Sterling has produced only one major theoretical work, (1970b). This is claimed to be basically his doctoral thesis

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33. The opening remarks are well worth reproducing in full: "The great Sphinx of Giza is more than evidence of the achievements of ancient Egypt; its mutilated face is symbolic of man's reaction to novel or foreign ways. Superstition and intolerance tempted a Mameluke ruler to order the destruction of this unique cultural monument in 1380. Mamelukes' cannon shots partially destroyed the nose and the beard of the Sphinx but imparted a new meaning to it. For nearly six centuries the mysterious smile of this recumbent figure bears witness to people's behaviour towards things they refuse to comprehend. For this the Mamelukes may even be praised. Similarly Chambers ought to be praised for finding my book worth bombarding with eighteen pages of piercing criticism". (Mattessich, 1967, 119)

(adapted) written ten years earlier. Almost all his other, shorter, work is concerned with establishing some basis for accounting theory. In addition he has edited several collections of papers on theoretical aspects or instigated their publication. Further, he has been involved in at least two committees which have produced significant theoretical contributions: AAA (1971b) and AAA (1977). Unlike the other three theorists there is no discernible published development leading to Sterling (1970b). Nevertheless a pattern of his other work does emerge; one in which there is an awareness of some developments in the philosophy of science. As with Mattessich's work the titles are fairly indicative of contents and mostly allude to theory. Whether these works actually "develop" is questionable and discussed more fully below.

THE MAJOR WORKS

It was mentioned above that the first source for analysis of the theoretical viewpoints was the major works. These are the theories themselves represented by Chambers (1966a), Ijiri (1967) and Ijiri (1975), Mattessich (1964) and Sterling (1970b). A useful starting point for analysis is the authors' statement of purpose.

According to Chambers (1977, 18), his (1964) "presents the theory of what was subsequently called continuously contemporary accounting". "It is straight exposition" concerned not with analysing or criticizing extant practice but prescribing the theory.

It has already been mentioned that Ijiri (1967) has as its primary aim the probing "into the foundation of accounting" in particular measurement aspects. "The objects of this study are the accounting theories and practices that are now socially accepted". (1967, x) That Ijiri (1975) is entitled Theory of Accounting Measurement is sufficient to conclude that he is attempting to theorize. The assumption of accounting as a "system designed to facilitate the smooth functioning of accountability relationships among interested parties" is made early on and Ijiri then proceeds to develop a theory on this basis. Mattessich (1964) states clearly that, "The purpose of this study is to present a unified frame of accounting ..." (1964, ix). Similar conclusions to those derived from the title of Ijiri (1975) can be derived from the title of Sterling (1970b): Theory of the Measurement of Enterprise Income (TMEI).

From the above it is obvious that all five works were presented as theories. The question now is how were these theories presented? An answer to this sort of question is rarely easily found and this case is no exception. An answer would require an acceptance of theory construction procedures which the last section showed does not exist. The question can therefore be modified to, how did the theorists perceive theory construction techniques as illustrated in their major works?

A scientific theory, according to the Received View, is a deductively connected bundle of laws which are applicable

to observable phenomena in ways specified by the correspondence rules. At this level of generality Chambers (1966a) and Sterling (1970b) would seem to be ideal candidates for such a classification. On a more formal level the Final Version of the Received View presented by Carnap and Hempel, according to Suppe (1974, 50) has been discussed above (in Section VI). In essence this is that there is an L (first order language) divided into L_o (observation language) and L_t (theoretical language) defined in terms of a logical calculus, K (and therefore K_o and K_t) and correspondence rules, C. There are however, other versions of the Received View though they "can be considered as being variations on stages in the development of the Received View sketched above" (Suppe, 1974, 50). Kaplan (1964), for example, distinguishes between concatenated and hierarchial theories. The former is one in which component laws are filled into a network of relations "so as to constitute an identifiable configuration or pattern", the latter, one in which "component lanes are presented as deductions from a small set of basic principles". However, Kaplan admits "they are often regarded as marking the earlier and later stages of theory formation" (1964, 298).

This sort of description could well be applied to the work of Mattessich (1964) and Ijiri (1967 and 1975). If it is, as Suppe states, a variation on the Received View description. Ijiri (1967) and (1975) concentrate on measurement. Mattessich (1964) "suggests that 'knowledge of the achievements of modern logic, the philosophy of science, measurement theory, management and behavioural science,

systems simulation, etc' is urgently needed. His objective is to attempt such a synthesis" (Chambers, 1966c, 102). To conjoin these different areas would obviously be concatenation and in providing a theoretical synthesis would be a concatenated theory in the Kaplan sense.

Mattessich also provides eighteen basic assumptions of accounting although he admits that they are "variously called accounting 'postulates', 'principles', 'standards' etc" (30). These provide "the foundations" of accounting and as such the analysis corresponds to Kaplan's hierarchial theory construction.

This sort of analysis, other than tending to be a little overly simplistic, does little more than provide support for the commonly held notion that "facts" can easily be produced to agree with any argument. Therefore all that can seriously be noted from the above is that all authors have suggested their works provide theories and that these theories could well have been constructed on methods proposed by logical positivists. A far more satisfactory approach is to examine briefly the views enounced by the theorists - in the works in question.

Chambers evinces his method of construction in the Introduction (1966a, 6-8). The following are quoted from there:

> "This method begins with observation that there is a class of objects, events or operations, possessing some similarities and distributed in some way

> through the universe of experience. Each of these notions may be more or less vague at the onset; each comes to be more clearly defined as inquiry proceeds".
>
> "We conjecture further..."
>
> "We select, .. a number of statements.."
>
> "The set of statements which define accounting ..."
>
> "But conjecture is not good enough for a process which has wide applications. The theory must be tested" (Emphasis added)

These comments tend to suggest an amalgam of Received View and Popperian positions and the Popper influence is most especially so regarding the last quoted statement. In fact Chambers' continues from that last quotation with: "The first stage in this testing process is deductive of all possible consequences of the theory" - a claim which echoes strongly Popper's (1959, 32-34) "Deductive Testing of Theories".

The point is made even more strongly by comparing "the objective of deduction is to develop fully the meaning of the theory "(Chambers,1966a, 7) with "the purpose of this last kind of (deductive) test is to find out how far the new consequences of the theory ... stand up to the demands of practice" (Popper, 1959, 33).

Chambers' insistence on the importance of observation suggests a Received View approach. His subsequent elaboration of the processes of reasoning bears this out even more (Chpt 1). His insistence on testing of propositions goes beyond Popper's requirement to the extent

of requiring verification. His separation of propositions into formal (through rules of correspondence) and empirical further supports a Received View position.

Ijiri's work (Ijiri, 1967; Ijiri, 1975) appears to be even more heavily influenced by the Received View viewpoint. His initial concern in (1967) is language. A theoretical language is seen as necessary for theorizing and axiomatic calculus is the most suitable language. Rules of correspondence are therefore crucial and modern measurement theory is the medium. He provides three axioms - the axiom of control, axiom of quantities and the axiom of exchange - and in doing so his purpose is heuristic rather than ostensive. The result is that Ijiri (1967) is more tentative than a formal general theory should be. It does not describe or explain every situation so in this sense fails as a general theory. The axiomatic approach has been criticized[34] by philosophers of science and it is doubtful whether it can ever establish theories - it can be little more than analytical.

The concern with language and the three axioms are retained in Ijiri (1975). This work could be described as a more rigorous extension of the earlier work, Ijiri (1967). It easily distinguishes between inductive and deductive models, descriptive and normative models and theories and policies. This last distinction is important for the terms are intermingled in extant accounting and it is important "because the lack of such a distinction is perhaps one of the most

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34. See Toulmin, Stephen, "The Structure of Scientific Theories" in Suppe (1974), 600 - 614

serious impediments to making accounting research scientific. The distinction needs to be made because issues of theory must be resolved by scientific means" (10).

Logic plays an essential role in his scientific means (Chpt 2) yet the central theme of the theory - accountability - is derived by "a reverse - inductive analysis" (AAA, 1977, 10). Popper has shown that inductive logic cannot exist!

Mattessich (1964) presents an axiomatic model of accounting. Much of the book, AAM, is analytical and descriptive: considerable space is devoted to examining measurement theory, and central themes in economics and management science. Techniques developed within these other disciplines are regarded as being useful to developing accounting as "the quantitative description and projection of income circulation and of wealth aggregates" (426). A set of eighteen basic assumptions of accounting forms the basis of the theory - that which is presented as an axiomatic model. As such, the criticisms of axiomatization alluded to above apply equally well to Mattessich's approach. Unlike Ijiri however, Mattessich does not have confidence in an inductive logic: "...as long as Hume's rejection of the induction principle cannot be refuted on better and firmer grounds than has been the case thus far ..." (233)

The term "model" has many uses and many varied meanings. Possibly the most common use is describing any mathematical representation of some complete notion. However it is used in the scientific - philosophic sense to indicate a model

of some things - as an icon: it is structurally isomorphic to what it models. Such models are used to explain certain notions in many disciplines and they are especially common in economics. Suppe (1974) states:

> "There is no doubt that the formalism of theories can be interpreted in terms of iconic models and that doing so often is heuristically fruitful in suggesting hypotheses, developing theories and so on". (99)

Much of the work on such models has been done by philosopher Mary Hesse [35], Although they were earlier conceived of by Nagel.[36] Sterling claims his TMEI (1970b) was not published for ten years. The book is very neatly organised and adopts the model form of explanation which owes much to Nagel. Accepting Sterling' claim, the work must have developed prior to Hesse's more rigorous model - analysis and Kuhn's work (although it is cited in the bibliography). The model is only useful in simplifying the semantic interpretation of the theory - the partial interpretation of TC (using the notation described above in showing the final version of the Received View). The following is adapted from Suppe (1974, 98) and shows more clearly the place of models according to Nagel:

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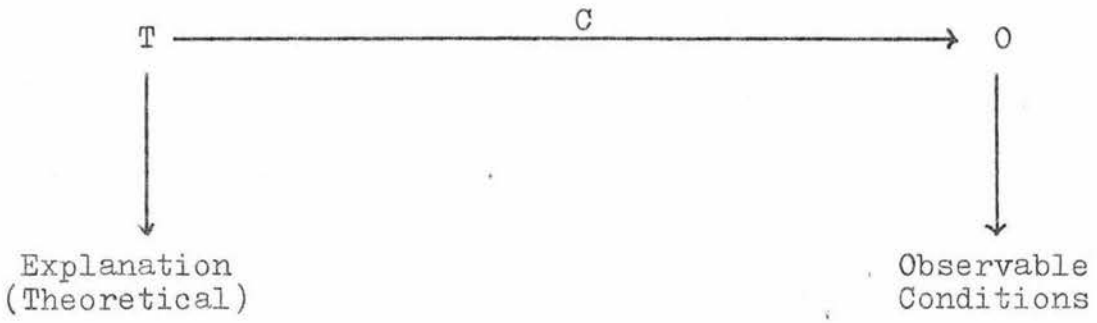
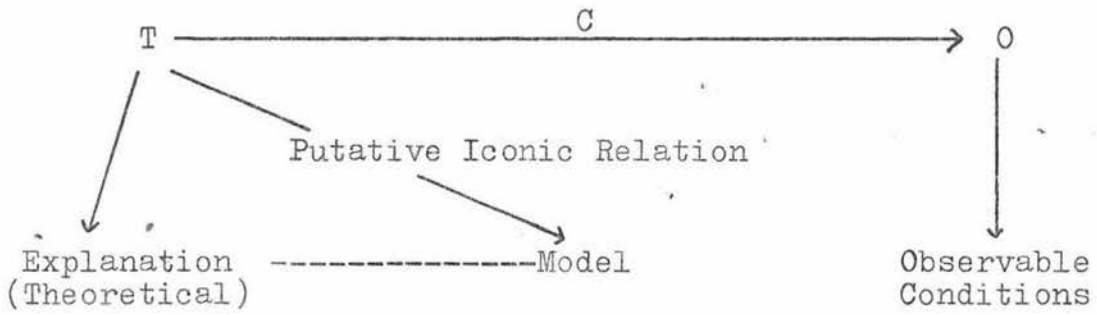
35. See, for example her Forces and Fields, Littlefield, 1965 and Models and Analogies, University of Notre Dame Press 1966

36. Especially, The Structure of Science, Harcourt Brace, 1961



(i) Interpretation of empirically true theories on Received View



(ii) (i) above in terms of Nagel's proposal

This model explanation is that to which Sterling (1970b) aspires. It is therefore to this extent that Sterling does not accept an unqualified Received View stance. Sterling even goes further for he acknowledges the work of one of the severe critics of the Received View which had been published at "time of writing": NR Hanson (1958). He accepts Hanson's notion of theory laden perceptions. (Sterling, 308 - 9)

It was stated in earlier sections of this research effort that the Received View has dominated thinking on theory construction in this century. It was also shown to adopt concepts developed in the philosophy of science. Its notions of theory construction have largely been derived through

other disciplines. If the Received View has been dominant it follows that accounting will also reflect this. An examination of the major works of Chambers, Ijiri, Mattessich and Sterling bears this out and although not the only influence the Received View approach is implicit in varying degrees of these works.

It was mentioned that these works are only one source area for determining the epistemological persuasions adopted by these theorists. They have all written on theory processes and these are now examined.

THE WORKS ON THEORY CONSTRUCTION

As most of Ijiri's work on theories has been incorporated into one of two major works, little more can be said of his view of or position on theory construction. Ijiri (1965) provides his early thoughts on axioms of accounting. The set of axioms (and the set of measurement rules they facilitate) is extracted from conventional accounting but is "sufficient to explain the majority of the principles and practices in conventional accounting" (36).

Inductive logic was "developed" by Received View theorists (more particularly Carnap and Reichenbach - see Suppe, 1974, Chapter 1) to enable verification of generalizations about experience. It is interesting to note Ijiri's comments in this respect (1965, 37):

> "...though the truth of the axioms cannot be proved (in the purely philosophical sense), they are empirically supported since the

> axioms are abstracted from what accountants have been doing in their daily practices".

These comments taken in conjunction with his statement that the axioms are "tied in logically and mathematically with the set of measurement rules" tend to indicate that his axioms are in fact only theoretical constructs (T Terms).

Ijiri (1971a) is contained within (1975) as the specification of the need for logical in accounting theory processes. (1972b) develops the (contrapositive) inductive method espoused in (1975) as does his (1973).

Sterling has devoted considerable effort to accounting theory construction. One of his earliest works was Sterling (1967a): a review of ASOBAT (AAA, 1966). This work has been briefly discussed in a previous section and Sterling's review highlights the lack of rigour and precision, or, to be more correct, the need for more precision and rigour. What is most significant however is that Sterling makes two allusions to Kuhn's, Structure of Scientific Revolution. The work is revolutionary in its departure from previous so-called theories and its adoption of a world view.

The purpose of Sterling (1967b) is "to abstract a general statement of present accounting theory". Of all the works discussed so far this work would come closest to fitting the Received View of theories appellation. In terms of The Final Version described by Suppe (infra)

Sterling (1967b) is a work which can fit those requirements.

Sterling (1970a) is even more directly concerned with accounting theory construction and that it had a strong influence on AAA (1971b) is immediately clear. As with many other of Sterling's works (or those in which he was involved) discussed so far, there is an early allusion to Kuhn with no subsequent Kuhnian analysis. In this case the allusion concerns Kuhn's notion of anomalies but the rest of the paper is devoted to explaining a logical positivist view which fits the Final Version (above) more closely than Sterling (1967b). Whereas the earlier work concentrated on the developing L (first order language) and K (logical calculus) greater stress in this instance is laid on verification and interpretation. Although because of its purpose it is understandable, it is unfortunate that Sterling (1972a) is not more detailed for it is in this work that the fact that there may be no one prescribed methodological basis for scientific theory construction is recognised. This is borne out by:

> "A major part of the scientific process is the scientist's creation of the method which allows him to carry out his investigation" (4)

and

> "Thus, the 'data grubbing' of the empiricist, the 'abstract meanderings' of the mathematician or logician, and the 'idle dreaming' of the theorist are all appropriate methods. The only thing that is inappropriate is the perjorative terms used to describe them." (5)

It may well be that recognition of this led Sterling to turn to a systemic approach in Sterling (1972c). It has

not been possible within the scope of this research effort to discuss systemic theorizing [37]. Baldwin (1967, 66) states that:

> "Whenever a theory explains an event solely in terms of its final result or its functional value without describing the mechanism by which that result is obtained the theory is called teleological".

Sterling (1972c) is concerned with teleological explanation and although it would be unfair to say that he is unconcerned with the mechanisms of accounting he is more interested in a decision model approach. All too often, he argues, operational constraints obscure teleological considerations.

Baldwin (1967, 66) also states that "whenever these teleological explanations have arisen in physics or biology, they have been eventually superseded by causal explanations". This may also be the case in accounting though it is tentatively suggested that the discipline is in a too primitive theoretical state for this to be of immediate concern. However, as noted, systemic theorizing, with which modern teleological notions are usually associated, is beyond the limited scope of this analysis.

Other than display an insensitivity to the paintings of Matisse and Piccaso, Sterling (1975a) achieves little. Methodologically he is primarily concerned with observation and rules of correspondence. "A basic tenet of science is that there be agreement among independent observers" (30). Added to this is his statement that "If accounting is to be an empirical science, then we must redefine our discipline

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37. See Churchman (1968) and Gigch (1974)

so that it is based upon laws instead of conventions" (30). These laws will be scientific - "testable empirical generalisations". An example is given. This sort of statement is similar to those used by advocates of the covering law model and Suppe (1974, 230) states that, "The covering law of scientific explanation tacitly assumes the Received View". Sterling's example definition (law?) is indeed derived from his income theory and in his insistence on empirical testing - would appear to be supporting Received View principles.

It is extremely difficult to follow the reversal in views from Sterling's "Introduction" (1972a) to his "Nature and Verification of Theories" (1976a). Whereas the former was declaiming methodological pluralism the latter appears to be rigidly expounding a Received View position. Given the present state of the philosophy of science the Received View does offer security in that its domination has resulted in it being freely encountered in the literature on theories. Sterling's apparent reversal can only be seriously explained on the basis of his dissatisfaction with the lack of methodological rigour of alternatives. This also implies, however, ignorance of the Sneed-Stegmuller formalization of Kuhn's work, for anyone seriously concerned with theory construction should at least acknowledge it. As explained earlier, Kuhn's work has been recognised in a very wide range of disciplines.

Mattessich's early work on accounting theory was, as noted above, largely incorporated into his AAM (1964). However his

interest in economics and philosophy was indicated early in his career. This has now become clear from the translation (in 1976) of his (1947). It may be that perceived limitations of accounting in satisfying these interests have resulted in his later work reflecting this early interest.

Only three of his numerous works since his AAM (1974) are directly (or indirectly in fact) concerned with accounting theory construction - They are (1968), (1970b) and (1972). Mattessich (1968) is an attempt to provide a basis for accounting theory construction. Many of the then extant problems of science philosophy are discussed and related to accounting. Early in the work the intention is expressed:

> "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" (3)
> (emphasis in original)

The terms used in the above quotation are fairly indicative of the emphasis of the work. Although Popper and Kuhn are alluded to, the work rests largely on a Received View stance.

Both Mattessich's (1970b) and (1972) are general calls for greater methodological rigour in accounting theory formulation. Nothing new is presented and emphasis is placed on matters such as systematic testing procedures to enable verification or refutation:

> "This testing of a general accounting theory by way of the empirical verification or refutation of its interpreted systems, seems to me the only way of conforming to the requirements of an empirical discipline".

It has already been mentioned that most of Mattessich's work has been devoted to management science and systems research and its applications. It is therefore well removed from this analysis.[38]

Like Ijiri and Mattessich, and as mentioned above, much of Chambers' early work has been"developed into "his major work. Chambers has been one of the (if not the) most prolific writers on accounting theory and as such it would be a difficult task to analyse all his work. As this analysis is attempting to establish his position on theory construction - methodology, only works directly relevant have been examined for this purpose.

Chambers (1955a) is, as its title suggests, a plan which a subsequent theory may follow. Although four basic premises are suggested it is largely a descriptive work. Accounting theory to this point is unscientific and unstructured. Before theory can be soundly constructed, some basic considerations need to be accounted for. This work suggests them. Chambers (1955b) continues these ideas. The distinction between rules and theories is explained. In 1953 AC Littleton published his Structure of Accounting Theory

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38. His "New Contributions to the Set - Theoretical Formalization of Accounting and Management information theory, "in Proceedings of the First Annual Conference of the Canadian Institute of Administrative Sciences, 1973, 1-19 to 1-53 footnoted in (1975c) is not available in Australasia.

(AAA). In his 1956 article Chambers rejects the so called inductive methodology of developing an accounting theory. Chambers (1957) is disappointing and does not provide the detail promised in the title. Chambers (1960) however, does: the conditions necessary for scientific research are set out.

While Chambers (1961) does not discuss methodological questions it does attempt a theoretical presentation. In fact it concludes with forty "foundations assumptions or postulates" and twenty one "derived principles". The former are perceived (observed) real world conditions and the principles are logically derived from them. Chambers (1962) is in a similar tenor to (1960). A futile though persistent theme in accounting literature to this time was the "debate" on whether accounting was an art or a science. In reality what appeared to be the bone of contention was the extent to which systematic theorizing could be utilized. If it was an "art", accounting could then assume a position similar to seventeenth century alchemy - supposedly analytical but in fact highly subject to "professional opinion". Abstract accounting theorizing was regarded as being impractical and unnecessary; and so it would be, Chambers (1965) argues, if it were done in vacuo. The paper presents some ideas on the nature of theories then discusses "some incidents in the development of accounting theory". (24)

"A theory is a well ordered set of statements about classes of things and classes of events which are in some way connected in an experience of them. The function of a theory is to explain how things and events are connected" (1971a, 138)

This statement is the most explicit notion of a theory discussed by Chambers. The paper is directed to theory construction and the view presented appears to be a modified Received View: events are to be converted to theoretical calculi which must then be related back to the real world through the actions of R, a receiver. There is no emphasis on verification but the concept of validation is taken up in (1971b): "to validate an accounting theory is to bring forward proof that an application of the theory has usable products or worthwhile consequences" (13). Correspondence between theoretical terms and the real world is the central theme of (1972a)

Stenhouse (1973) describes the importance the ethologist, N. Tinbergen, attached to observation in developing his theories. Chambers (1973a) is a working example of this sort of observation and (1973b) in fact argues for its importance in developing ideas. It is not the sort of observation from which theories are supposedly (according to some) induced. Rather it is a cathartic, purifying process which enables the construction process to be undertaken. It is therefore a prerequisite of theory construction and not part of the theory.

Few authors produce a paper describing their particular theory construction similar to Chambers (1974c). Kuhnian analysis has shown that the acceptance of a new paradigm (disciplinary matrix) is a matter of faith. The author of a new theory therefore has a "hard sell" and he must attempt to convince others of his theory's suitability. This may well have been part of the reason for Chambers (1974c),

granted that it was intended as an introduction for the reprint of his (1966a). The paper is largely descriptive and does not detail much of the formal construction process. The importance of observation is again stressed. This, as noted above, in no way suggests the importance of empiricism and much of contemporary empirical accounting research is classed "counter-productive" (22). The correspondence between the observable and the theoretical is a problem. While observation "cannot prove what is right or best or most useful" it can be suggestive: it may reveal flaws or faults in what is currently done.

CRITICAL WORKS

The literature survey above (Section III) discussed the works of accounting writers generally on theory construction. Some works not mentioned are those aimed specifically at critical analyses of the works of the four selected theorists. In the hope that these may throw some light on accounting theory construction these works are now briefly discussed. Reviews form the largest part of this group and regrettably they usually aim at specifics rather than consider the overall theoretical approaches. Consequently they add little to this research effort.

Solomons (1966) is a good example of a reviewer "missing the point" for in stating that he could have been "spared the first hundred pages of the book" he is overlooking that which is basic to Chambers' theory. Solomons is more concerned with the work's "wealth of quotable footnotes" and

other specifics. Cruse (1967) is concerned with the lack of reference to general systems theory as, he argues, "it underlies the very concept of the book". This may be so but it may equally demonstrate the ubiquitous nature of systems theory. Measurement is an important aspect of Chambers' theory (1974c). In criticizing this aspect Larson and Schattke (1966) were attacking an integral part of the theory. Measurement rules are Chambers' correspondence rules through which observation and theoretical constructs are related. Chambers (1967b) meets the criticisms re-emphasizing the importance of measurement theory to his theoretical method.

Iselin (1968) is another work which does not bear up to its title. Although promising to assess Chambers' theory, it is more concerned with specific features: "possible inadequacies and inconsistencies within Chambers' system.." (232)

Chambers' review of Mattessich (1964) has already been alluded to. It is one of the few analyses of his work (Chambers, 1966c). Of the several points Chambers takes issue with Mattessich those concerning the chapter on empirical hypotheses (1964, Chapter 7) are of interest here. Mattessich distinguishes between scientific and pragmatic hypotheses. The distinction, to Chambers, is empty: testing of a scientific hypothesis is the same as testing a pragmatic hypothesis. Mattessich also errs in distinguishing between rules and hypotheses. Rules are based on hypotheses and the two are therefore not distinct. The whole chapter is, suggests Chambers (117), "a tentative sally in the

direction of scientific rigor". The review reaffirms the views on theorizing held by Chambers and discussed above [40].

It is difficult to determine the extent to which Ijiri (1967) can be said to present a theory. While this was an expressed intention, other than suggest the three axioms basic to accounting, because of its emphasis on measurement it is likely such a work can only produce a sub-theory to be filled into a larger theoretical work. That this may be correct is borne out by the fact that Ijiri revised his work and produced his (1975). It is also suggested by default in the two major reviews of this (1967) - Bedford (1968) and Chambers (1972b). Neither work examines the book on its theoretical methodological basis other than to raise questions concerning the axioms. Actually Bedford goes further by stating that:

> "...The objectives are not well specified. One is not certain whether Ijiri's book should be measured by its new contributions to basic accounting concepts or in terms of the insights gained from a formal representation of the fundamental proportions of accounting". (Bedford, 1968, 270)

Clarke (1976) examines the work of both Mattessich and Ijiri. It does in fact describe a practical test of Ijiri's three axioms although its broader goal is research into management information systems (MIS), a subject which Clarke suggests is at present "quite unscientific". This work then (Clarke, 1976) "sought to apply a little of the understanding of the rigorous approach to theory construction

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40. Compare with the quotation from Most (1977, 15) early in Section III, concerning the idea of a theory being a set of acceptable hypotheses.

which is part of the essential methodology science" (2) The truth and utility of Ijiri's axiomatized accounting theory was laboratory (computer) tested. The analysis is formal and rests on the assumption that accounting is an instrumental science. Popper (1959) rejects this approach to theories as was indicated in Section VI above. Axiomitization is also regarded as a development of the Received View (in general that is). The testing of Ijiri's axioms was to determine their truth and adequacy and although Ijiri never intended the axiomatization to be so rigorous (Clarke, 9), no errors were found. However it "was not logically sufficient". Clarke substituted seventeen axioms. Although very interesting Clarke's (1976) relevance to his work is limited because, as stated, its primary concern is Intelligent MIS and its assumption on the nature of theories.

Mattessich (1971b) criticizes Sterling's TMEI (1970b) for its teleological approach in general. Given Mattessich's subsequent personal interest this is a little ironic. Valuation is a prime concern of Sterling (1970b) and Mattessich finds several areas of contention. No specific comments are directed to Sterling's (1970b) theoretical structure other than:

> "Whereas the structure of the latter (i.e. Edwards and Bell, 1961) may be compared to that of a Gothic cathedral in which every element fulfils an indisputable architectonic function, Sterling's work rather resembles a Baroque building, endowed with rich ornamentations, often fascinating, but not always contributing to the support of the major structure" (Mattessich, 1971b, 193)

BRIEF SUMMARY OF SIGNIFICANT THEORY METHODOLOGY ELEMENTS

	CHAMBERS	IJIRI	MATTESSICH	STERLING
(a) MAJOR WORKS	(1966a) The development of a theory along inductive processes is rejected. Observation is seen as a crucial feature and is the basis for conjectures from which theories will be deduced. Testing of the theory indispensible and although at first this will be Popperian-type deductive testing it is, finally verification in the Received View sense. It is a "theory of accounting in the present context of institutions" (1966a, 15)	(1967) ; (1975) A strong Received View position adopted: existing (conventional) accounting axiomatised "in order to explain complicated phenomena to a satisfactory degree" (1967, 88). Formal aspects stressed, especially measurement (1967). Largely devoted to prescribing formalised techniques on which to base extant accounting and therefore falls short of a strict theory.	(1964) Received View in Kaplan (1964) sense: axiomatisation on formal - "logical" basis of 18 assumptions.	(1970b) Primary concern is measurement of enterprise income. However in attempting such an understanding of accounting is important. The analysis is presented along the formal Received View lines although it uses the traditional economic simplified economic model approach. The theory is seen as a neat formal structure with measurement procedures an important characteristic.
(b) WORKS ON THEORY CONSTRUCTION	Significant works: (1971a), (1973b) (1974c). Theories seen as formal explanations of phenomena: scientific theories in the Received View manner, so rules of correspondence and verification actually articulated in (1971a). This position greatly modified in his (1973b) although the necessity for empirical testing was still held to be important. Observation seen as paramount precondition for theory construction. His (1974c) is largely descriptive of the development of his (1966a). He acknowledges influence of a Received View Source (8) and claims also his theory was derived from economics and measurement theory (21).	Most of his works relevant to theories have been incorporated is his major works. Axiomatization seen as a necessary formal precondition to theory construction. He also argues for logic and inductive processes for construction and verification (1971a ; 1972b)	Significant works; (1957), (1968), (1970b) and (1972). His early work concentrated on attempting to mathematize accounting and hence give it a positivistic formalism. His article of 1972 was seen as a significant contribution (prize winner, AAA) and it continued the plea for a scientific approach which prescribed a formal process for theory construction. His latest works have been directed towards systemic explanation but have also been more concerned with management science than accounting.	Significant works: (1970a) (1975a) and (1976a). Almost all his work appears committed to a strong logical positivist position. This is despite his acknowledgement of methodological pluralism (1972a) and his reference to Kuhn in several places (e.g. 1970b). It is also despite his membership of Committees relying on Kuhnian analysis, notably AAA (1977).
(c) CRITICAL WORKS	Attacked for his lack of positivist rigour (e.g. Larson and Schattke, 1966) and his non reference to systems theory (e.g. Cruse, 1967) although the need for this is not explained or justified.	Ijiri (1967) is described as a descriptive theory attempt (McDonald, 1972) and Ijiri (1975) is held out as an example of "inductive theorizing (AAA, 1977)	Mattessich's work has not attracted much critical attention other than Chambers' review (1966c). Despite this award winning article (1972) his work has been largely ignored by US theorists. He has exerted some influence, through acceptance, outside the USA (e.g. Gambling, 1974) Chambers' review criticizes his definitions primarily but does also deny much of Mattessich's uses of hypotheses - they are seen to be inconsistent and not satisfactorily demonstrated as relating to accounting.	Sterling's work has influenced much of US accounting theory as is evident from its continued references. Very little criticism has been levelled at his work. Mattessich's review (1971) of his TMEI (1970b) is one of the few critical assessments made. Even so it is the subject matter rather than the theory construction which is criticized, in fact, Mattessich says: "Even those disagreeing with the somewhat narrow frame of premises and the conclusions of Sterling's book, will recognize that it is a highly stimulating contribution in theory construction" (Mattessich, 1971, 176-77).

SUMMARY

The primary interest of this research effort is theory construction in accounting. In order to determine some general pattern the work of four significant theorists was examined and it was stated that the concern was more with form than content. After a brief chronological survey of the work of the selected theorists, their "major works" were discussed. It was expected that there would be a primary source in determining the theorists' concepts of accounting theory construction and it was contended that only their views on this were relevant to the present analysis. A second source of information on the theorists' views was thought to be their writings particularly directed to the subject. All had in fact published papers directly relevant. In most instances these ideas tended to be consistent with those contained in the major works. The third source of information proved to be the most disappointing. This was discussions of the theorists' work by others. Reviews were the most common and because of the significant extent of academic incestuousness, most reviews were by one of the others. Little new can be extracted from such a situation as, even with the best of intentions, such work can only hope to reinforce the writers particular predilections.

Naess (1972, 57) has suggested that:

> "The vigour and the confusion of the debate on the nature of theories are in no small measure due to the same factor: the vagueness and ambiguity of the term 'theory'."

As early as 1958, as noted earlier, Rapoport (1958) presented several meanings of "theory". It could be suggested that the argument of this research effort is reductio ad absurdum. In attempting to determine the common basis for theory construction in accounting it was shown that no single method of theory construction exists. It was in fact central to the analysis in Section VI that those who have argued that there is only one scientific theory construction process have erred.

Nevertheless there are countless references to a scientific method. Although different authors have varied their descriptions slightly, they have described essentially the same process. In technical terms this process has been described as logical positivism, scientific empiricism, logical empiricism or the unity of science movement.

Suppe (1974) has referred to the theory construction view espoused by advocates of these movements as the Received View and that term has been used in this analysis. The method was not constant and changed over its sixty years of philosophical dominance. Four features are essential to it:

1. The distinction between the theoretical and an observation language.
2. Rules of correspondence.
3. Empirical confirmation or verification of theories.
4. The reductability of theories.

In practical expositions the theoretical -

observational term distinction has been described in terms of two planes - a theoretical plane and a real world plane. Recent work has shown this distinction to be untenable for several reasons including that observation is theory-laden, meanings are theory dependent, and facts are theory-laden. The author's view of the world, his Weltanschauungen, will determine the full meanings of terms.

The subject of rules of correspondence is large in itself and changed substantially over the life of the Received View. Whereas they were initially conceived as fulfilling three functions - defining theoretical terms, giving those terms cognitive significance and specifying admissable experimental procedures for applying the theory to phenomena - the first declined in importance. Correspondence rules are strictly speaking not part of the theory; rather they are "auxillary hypotheses of procedures for applying the theory to phenomena" (Suppe, 1974, 103). There are very many ways of applying a theory to phenomena which means there will also be very many correspondence rules. Thirdly some writers have shown that the experimental connections between phenomena and theory is "over simple and epistemologically misleading".[40]

The untenability of the confirmation or verification of theories has been widely demonstrated by Popper's falsifiability criterion. Other criticisms have been levelled at the

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40. Especially the work of Patrick Suppes - see Suppe, 1974 106-109 and 221-230

principle and on the most basic level the question of validation is likely to become one of the reliability of measurements. Closely allied to the Received View notion of confirmation is the positivistic treatment of theory reduction. There are different ways in which theory reduction takes place but it is essentially to overcome the problem of apparently confirmed theories subsequently being proved false that the notion was developed.

Criticism and reaction to the formalized view of theories held by the advocates of the Received View are many and widespread. The situation had become serious: According to Baumberger (1972, 2),

> "The only admissible scientific study of science, the only science of science, the only metascience with any claim to scientificity was their own formal logic of science. In their hierachical conception of science this logic was the superstructure which logically controls the admissible 'object' sciences, the so-called formal and nomologically oriented 'real' (or 'special') sciences."

Reaction against this formalization has resulted in a variety of alternative views of the theory process. This is evident in the works of Kuhn, Popper, Hanson, Feyerabend and many others who view theories as deep conceptual systems which provide a perspective, for viewing the world - a Weltanschauung. As such, "theories are viewed as essentially dynamic, growing entities, and it is maintained that theories cannot be understood if divorced from the dynamics of their development" (Suppe, 1974, 114). On the other hand the more cynical commentators have suggested the appeal of Kuhn's

ideas is due to his having "brought man back into the picture and reminded people of a few conspicuous facts that tended to be overlooked by philosophers and scientists alike" (Baumberger, 1977, 3). Although this comment may reflect its authors' inherent conservatism, it does reflect a basic insecurity by those attempting to determine a proper basis on which to develop scientific theories. Do theories emerge as revolutionary new explanations of natural phenomena in the manner propounded by Kuhn? Is it necessary to, as Popper (1959) suggests, deny the "psychology of knowledge" as useful in understanding theory construction? Does Feyerabend's extremist notion of "anything goes" or methodological anarchy really provide the answer as to how knowledge of the world is acquired? Is the explanation found in a manner similar to Crick and Watson's discovery of the structure of DNA - "controlled accident"?

There being no one theme in the philosophy of science, it is little wonder that accounting theorists have not agreed on theory construction process. The literature survey of Section III showed that in fact a vast number of views exist. The work of four leading theorists was examined in Section VII with the intentions of discerning some indication of theory processes. The section opened and closed with comments to the effect that it was <u>form</u> and <u>not</u> <u>content</u> which was the prime concern. If, as suggested in Section VI, theories are not merely possibly entertained systems of propositions, what common basis exists for determining whether accounting theories can be said to be scientific and for deciding which is a more soundly constructed theory.

In the closing stages of Section III the work of Ian Tilley was discussed and the question as to whether science is identified by content or method was raised. In fact the subsequent analysis of the ideas of Weltanschauungen advocates showed that the distinction was arbitary. If Chambers' theory is determined by his world vision then his content also demonstrates this.
Although purposely not mentioned at the time the analysis of his work tended to bear this notion out. Ijiri in his concern for precision and on understanding of the process of communicating accounting information was in fact working with correspondence rules. Suppe (1974) has argued that if formalization of theories has a place in their construction then it will probably be as semantic formalization rather than axiomatization. Ijiri's work falls between these two ideas. As such it is doubtful whether he was in fact scientifically theorizing: he was merely formally describing extant practices.

Similar arguments hold for Mattessich and Sterling. All three have had measurement as a central concern. In any view of theories it is difficult to conceive of measurement as forming part of the theory-proper. This is so unless, of course, the theory is the theory of measurement. If accounting exists purely to provide measures of certain economic data then such concern may well be justified for a theory of accounting would merely be a sub-theory of measurement. This is not so because all of the theorists have not defined accounting as such.

There are at present considerable research efforts in accounting being directed to establishing the empirical foundations of certain ideas. This is very adequately demonstrated by the literature. The regrettable aspect is that the authors of these efforts are claiming to be assisting in the formulation of theoretical concepts. In most cases they are doing little other than provide statistical support for the likely common occurrence of a preconception of the behaviour of certain phenomena. This is very much the situation in research into the stock market to "determine" accounting needs. The above analysis has shown that this is not the process by which knowledge proceeds. At most these sort of research efforts are little more than puzzle solving in the Kuhnian sense.

The above analysis has also shown that of all the accounting theorists, Chambers is the most logically consistent and the most rigorous in his presentation. One possibly disturbing feature however is his insistance on the process of observation as the basis for inquiry. This problem is connected to the previous and also the process of testing. Popper, however has said that ".. it is clear that an account of experience - of an observation or the result of an experiment - can in the first place be only a singular statement and not a universal one" (1959, 28) He argues it is therefore not possible to develop a theory (universal statement) on this basis. It was suggested in Section VII that Chambers' observation can only be similar to that conceived by Tinbergen - as the

mind-clarifying, cathartic precondition for theory development.

The work of Chambers, Ijiri, Mattessich and Sterling was selected on the basis of their apparent influence on accounting thought. They were considered to be leaders in the field of accounting theory and all had produced major works purporting to be theories of or for accounting. No central principles of theory construction have been discerned. In science "the intensive study of the history of science by professional historians or scientists with genuine historical training, brought forth a wealth of astonishing details and also new points of view" (Naess, 1972, 10). It may well be that this is the way for those interested in understanding how ideas have developed have to work. This of course begs the question and it may in fact be seen that accounting has not developed sufficiently: its scientific history may well be too brief. If so, then there exists a "chicken and egg" situation - how can accounting thought develop. The solution lies in borrowing concepts from more developed disciplines. If there is uncertainty in those disciplines it will only be transposed into accounting. An analysis of recent accounting theoretical writings certainly tends to bear this out.

APPENDIX

Although it is normal to include all works cited or referred to in the bibliography because of the nature of this work the relevant works of the four major theorists examined have, for ease of reference, been listed separately in this appendix. These works are not all the works by these particular theorists - only the ones used in the research analysis.

(a) CHAMBERS, Raymond J

1955a "Blueprint for a Theory of Accounting", Accounting Research, 6, January 1955 , 17-25.

1955b "A Scientific Pattern for Accounting Theory", Australian Accountant, 25, October 1955 , 428-34.

1956 "Some observations on Structure of Accounting Theory", Accounting Review, 31, October 1956 , 584-92.

1957 "Detail for a Blueprint", Accounting Review, 32 April 1957 , 206-15.

1960 "The Conditions of Research in Accounting", Journal of Accountancy, 110, December 1960 , 33-9

1961 "Towards a General Theory of Accounting", in Sidebotham R, Introduction to the Theory and Context of Accounting, Pergaman Press, 1965 .

1962 "The Resolution of Some Paradoxes in Accounting", Occasional Paper No 2, Faculty of Commerce and Business Administration, University of British Columbia, 1963.

1963 "Why Bother with Postulates?", Journal of Accounting Research, 1, Spring 1963 , 3-15.

1964 "Conventions, Doctrines and Common Sense", Accountants' Journal 43, February 1964 , 182-7.

1965 "The Development of Accounting Theory", in Chambers, Goldberg and Mathews (eds), The Accounting Frontier, November 1965, 18-35.

1966a Accounting Evaluation and Economic Behaviour, Prentice-Hall, Englewood Cliffs, February 1966.

1966b "Prospective Adventures in Accounting Ideas", Accounting Review, 42, April 1967 , 241-53.

1966c "Accounting and Analytical Methods: A Review Article" Journal of Accounting Research, 4, 1966, 101-18.

1967a "The Foundations of Financial Accounting", Berkley Symposium on the Foundations of Financial Accounting January 1967 , University of California at Berkeley, 26-44.

1967b "Continuously Contemporary Accounting - Additivity and Action", Accounting Review, 42, October 1967 , 751-7.

1968 "Accepted, Better or Best? - The Goal of Inquiry in Accounting", Singapore Accountant, 3 1968, 27-33.

1970a "Second Thoughts on Continuously Contemporary Accounting", Abacus, 6, September 1970 , 39-55.

1970b "The Commercial Foundations of Accounting Theory", in Stone, Williard E (Ed), Foundations of Accounting Theory, 1971, 59-77.

1970c "Accounting - From a Logical Point of View", Singapore Accountant, 5, 1970 , 13-18.

1971a "Accounting Theory Construction", Proceedings of Third International Conference on Accounting Education, Sydney, October 1972, 138-51.

1971b "The Validation of an Accounting Theory", Waseda Business and Economic Studies, 7 (1971), 13-18

1972a "Accounting Theory, Practice and Policy", Singapore Accountant, 7, 1972 , 39-43.

1972b "Measurement in Current Accounting Practice", Accounting Review 47, July 1972 , 488-509.

1973a Securities and Obscurities, Gower Press, Melbourne, May 1973.

1973b "Observation as a Method of Inquiry - the Background of Securities and Obscurities", Abacus, 9, December 1973 , 156-75.

1974a "The 'Objectives' of Accounting", Singapore Accountant, 9, 1974 , 39-45.

1974b "Third Thoughts", Abacus, 10, December 1974 , 129-37.

1974c "The Development of Theory of Continuously Contemporary Accounting", Working Paper 13, Academy of Accounting Historians, December 1974 .

1976a "The Possibility of a Normative Accounting Standard", Accounting Review, 51 July 1976 , 646-52.

1976b "Accounting Principles and Practices - Negotiated or Dictated?", November 1976 , in Previts (1967) 1-22.

1977 An Autobibliography, International Centre for Research in Accounting, University of Lancaster, 1977

(b) IJIRI, Yuji

1965 "Axioms and Structures of Conventional Accounting Measurement", Accounting Review, 40, January 1965, 36-53

1966 "Physical Measures and Multi-Dimensional Accounting" in Jaedicke, RK, Ijiri and Nielson (Eds) Research in Accounting Measurements, AAA, 1966.

1967 The Foundation of Accounting Measurement: A Mathematical, Economic and Behavioural Inquiry, Prentice Hall, 1967.

1971a "Logic and Sanctions in Accounting", in Sterling, R and Bentz (Eds) Accounting in Perspective, South Western, 1971, 3-28.

1971b "Axioms for Historical Cost Valuation: A Reply" Journal of Accounting Research, Spring 1971, 181-87.

1971c "Critique of the APB Fundamentals Statement," Journal of Accountancy, November 1971, 43-50.

1972a "Measurement in Current Accounting Practice: A Reply," Accounting Review, 47, July 1972, 510-26

1972b "The Nature of Accounting Research," in Sterling, (1972a), 59-26.

1973 "A Priori Research in Accounting: A Critique "in Dopuch and Revsine, 1973 , 20-25

1975 Theory of Accounting Measurement, American Accounting Association, Studies in Accounting Research, No 10, 1975

(c) MATTESSICH, Richard

1956 "The Constellation of Accounting and Economics" Accounting Review, 31, October 1956, 551-64

1957 "Towards a General and Axiomatic Foundation of Accountancy, with an Introduction to the Matrix Formulation of Accounting Systems", Accounting Research, 8, October 1957, 325-55

1958 "Mathematical Models in Business Accounting", Accounting Review, 33, 1958, 472-81.

1964 Accountancy and Analytic Methods, Richard D Irwin, 1964.

1967 "Accounting and Analytical Methods: A Comment on Chambers' Review", Journal of Accounting Research 5, 1967, 119-23.

1968 On the Truth, Acceptability and Verification of Accounting Propositions, (Working Paper No 7) Faculty of Commerce and Business Administration (FCBA) University of British Columbia (UBC), October 1968.

1970a "On the Perennial Misunderstanding of Asset Measurement by means of 'Present Values'", Cost and Management, March-April 1970, 29-31.

1970b "Some Thoughts on the Epistemology of Accounting" 2nd International Conference of Accounting Educators, London, Proceedings, 46-53

1971a "On Further Misunderstanding about Asset 'Measurement' and Valuation: A Rejoinder to Chambers' Article", Cost and Management, March-April 1971, 36-42

1971b "The Market Value Method According to Sterling: A Review Article" Abacus, 7, 1971, 176-92.

1972 "Methodological Preconditions and Problems of A General Theory of Accounting", Accounting Review, 47, 1972, 469-87.

1974 "The Judgement and Reduction of Value Judgements in Systems", Management Science, 21, September 1974 1-9.

1975a Non-Verbal Propositions and Imperatives: On the Relationship Between Sentances and Physical Configurations of Systems, FCBA, UBS Working Paper No 294, January 1975.

1975c Fundamental Problems of a Teleological and Instrumental Theory of Social Accounting, FCBA, UBC, Working Paper 302, April 1975.

(1947) Information and Knowledge Economics: A Border Area of Philosophy and Economics, FCBA, UBC, Working Paper No 317 (translation of an earlier article in German language).

1976 Normative Versus Positive Systems: On the Relation Between Normative, Teleology and Mentalistic Aspects, FCBA, UBC, Working Paper No 390, May 1976.

Instrumental Reasoning and Systems Methdology (publication announced to be in 1977 but as yet unseen).

(d) STERLING, Robert R

1966 "Operational Analysis of Traditional Accounting" Abacus, 2, December 1966.

1967a "A Statement of Basic Accounting Theory: A Review Article", Journal of Accounting Research, Spring 1967, 95-112.

1967b "Elements of Pure Accounting Theory", Accounting Review, 42, Jan 1967, 61-73.

1968 "The Going Concern: An Examination", Accounting Review, 43, July 1968, 481-502.

1969 "A Test of the Uniformity Hypothesis" Abacus 5, September 1969, 37-47.

1970a "On Theory Construction and Verification", Accounting Review, 45, 1970, 444-57.

1970b The Theory of the Measurement of Enterprise Income, University Press of Kansas, 1970

1972a Research Methodology in Accounting, "Introduction," Scholars Book Company, 1972, 1-7.

1972b "Decision Oriented Financial Accounting", Accounting and Business Research, 2, Summer 1972, 198-208.

1972c "An Explication and Analysis of the Structure of Accounting", Part One, Abacus, 7, 1971, 137-152; Part Two, Abacus, 8, 1971, 145-162.

1975a "Towards a Science of Accounting", Financial Analysts Journal, September-October 1975, 28-36.

1976a "The Nature and Verification of Theories" in Previts, Gary J (Ed) Papers, Presented at the Accounting Research Convocation, University of Alabama, November 1975.

BIBLIOGRAPHY

AMERICAN ACCOUNTING ASSOCIATION

1971a "Report of the Committee on Foundations of Accounting Measurement", Accounting Review, 46, Suppl, 1971, 1-48

1971b "Report of the Committee on Accounting Theory Construction and Verification", The Accounting Review, 46, Suppl, 1971, 53 - 79

1972a "Report of the Committee on Research Methodology in Accounting", Accounting Review, 47, Suppl, 1972, 399 - 520.

1972b "Report of the Committee on Accounting Valuation Bases", Accounting Review, 47 Suppl, 1972, 535-573

AMERICAN ACCOUNTING ASSOCIATION, COMMITTEE ON CONCEPTS AND STANDARDS FOR EXTERNAL REPORTS

1977 Statement on Accounting Theory and Theory Acceptance, 1977, American Accounting Association

AMERICAN ACCOUNTING ASSOCIATION COMMITTEE TO PREPARE A STATEMENT OF BASIC ACCOUNTING THEORY

1966 A Statement of Basic Accounting Theory, AAA, 1966

AMERICAN INSTITUTE OF CERTIFIED PUBLIC ACCOUNTANTS, 1970, ACCOUNTING PRINCIPLES BOARD

1970 Basic Concepts and Accounting Principles Underlying Financial Statements, Statement 4 of the Accounting Principles Board, 1970

AMERICAN INSTITUTE OF CERTIFIED PUBLIC ACCOUNTANTS, 1973, STUDY GROUP ON OBJECTIVES OF FINANCIAL STATEMENTS

1973 Objectives of Financial Statements, American Institute of Certified Public Accountants, 1973.

ANDERSON, James A

1976 A Comparative Analysis of Selected Income Measurement Theories in Financial Accounting, Studies in Accounting Research 12, American Accounting Association, 1976

BALADOUNI, Vale

1966 "The Accounting Perspective Re-Examined", Accounting Review, 41, April 1966, 215-25

BALDWIN, Alfred L

1967 Theories of Child Development, John Wiley and Son, 1967

BAUMBERGER, Jorg

1977 "No Kuhnian Revolutions in Economics", Journal of Economic Issues, 11, March 1977, 1-20

BEAMS, Floyd A

1969 "Indications of Pragmatism and Empiricism in Accounting Thought", Accounting Review, 44, 1969, 382-88

BEDFORD, Norton M
1965 Income Determination Theory: An Accounting Framework Addison-Wesley 1965

1967 "The Nature of Future Accounting Theory", Accounting Review, 42, 1967, 82-5

1968 "The Foundations of Accounting Measurement", Journal of Accounting Research, 6, 1968, 270-82

1969 The Future of Accounting in a Changing Society, Stipes Publishing Co, 1970. Arthur Andersen & Co Lecture Series - 1969

BEDFORD, Norton M and DOPUCH, N
1961 "Research Methodology and Accounting Theory - Another Perspective", Accounting Review, 36, 3, 351-61

BIRKETT, W P
1970 "The Boundaries of Accounting", AAUTA 8th Convention Papers, Hollandi, 1970

BUCKLEY, John W, BUCKLEY MH and CHIANG, H
1976 Research Methodology and Business Decisions, National Association of Accountants and the Society of Industrial Accountants of Canada, 1976

BURKE, E J
1974 "Hypothesis Construction in Accounting", Accounting Education, Suppl V14, 1974

CAPLAN, Edwin H
1969 "Relevance - 'A Will-o'-the-Wisp'", Abacus, 5, 1969, 48-54

1972 "Accounting Research as an Information Source for Theory Construction", in Sterling (1972), 45-51

CASPARI, John A
1976 "Wherefore Accounting Data - Explanation, Prediction and Decisions", Accounting Review, 51, 4, October 1976, 739-46

CAWS, Peter
1972 "Accounting Research - Science or Methodology?", in Sterling (1972), 71-3

CASTELLS, Manuel and de IPOLA, Emilio
1976 "Epistemological Practice and the Social Sciences", Economy and Society, 5, No 2, May 1976, 111-144

CHAMBERS, Raymond J
See Appendix

CHURCHMAN, C West
1968 The Systems Approach, Dell Publishing, 1968

CLARKE, Beven J
1976 Improvements to the Ijiri-Mattessich Axiomatic Accounting Theories with a Practical Test and Application, Paper presented at Accounting Association of Australia and New Zealand, 1976 Conference. Commentary by John SHANAHAN

COWAN, T K
1965 "A Resources Theory of Accounting", Accounting Review, 40, 1965, 9-20

1968 "A Pragmatic Approach to Accounting Theory", Accounting Review, 43, 1968, 94-100

CRUSE, Rex
1967 "Accounting, Evaluation and Economic Behaviour" by R J Chambers: Review", Accounting Review, 43, January 1967, 207-8

DEINZER, Harvey T
1965 Development of Accounting Thought, Holt Rinchart and Winston, 1965

1968 Methodological Presuppositions in Financial Accounting Models, University of Florida, 1968

1971 "What is the Message of 'A Statement of Basic Accounting Theory'?" in Stone, (1971), 114-31

DEMSKI, Joel S
1973 "The General Impossibility of Normative Accounting Standards", Accounting Review, 48, 3, 1973, 718-23

DEVINE, Carl T
1960 "Research Methodology and Accounting Theory Formulation", Accounting Review, 35, 3, 1960, 387-99

DOPUCH, Nicholas
1962 "Metaphysics of Pragmatism and Accountancy", Accounting Review, 37, 1962, 251-62

DOPUCH, Nicholas and REVSINE, L (Eds)
1973 Accounting Research 1960-1970: A Critical Evaluation, Monograph 7 Center for International Education Research in Accounting 1973

EDWARDS, E O and BELL, P W
1961 The Theory and Measurement of Business Income, University of California Press, 1961

EDWARDS, Paul (Ed-in-Chief)
1967 The Encyclopedia of Philosophy, Macmillan Coy - Free Press, 1967

ENTHOVEN, Adolf J H
1973 Accounting and Economic Development Policy, North Holland Publishing Co, 1973

FEYERABEND, Paul K
1970 "Against Method: Outline of an Anarchist Theory of Knowledge", in RADNER, Michael and WINOKUR, Stephen (Eds), "Analyses of Theories and Methods of Physics and Psychology", Minnesota Studies in the Philosophy of Science, Volume IV, 1970, University of Minnesota Press

1975 Against Method, NLB, 1975

1977 "Changing Patterns of Reconstruction", British Journal for the Philosophy of Science, 28, 1977, 351-82

1978 From Incompetent Professionalism to Professionalized Incompetence - The Rise of a New Breed of Intellectuals, Philosophy of Social Science, 8, 1978, 37-53

FINANCIAL ACCOUNTING STANDARDS BOARD,
1976 An Analysis of Issues Related to Conceptual Framework for Financial Accounting and Reporting: Elements of Financial Statements and Their Measurement, FASB Discussion Memorandum, Financial Accounting Standards Board, 1976

FLAMHOLTZ, Diana
1976 "The Structure of Scientific Revolutions and its Implications for the Development of Accounting Policy", Working Paper 27, The Academy of Accounting Historians, August 1976

FLANDERS, Dwight P
1961 "Accountancy, Systemized Learning, and Economics", Accounting Review, 36, 3, 1961, 564-76

FORD, Julienne
1975 Paradigms and Fairy Tales, An Introduction to the Science of Meanings, Routledge and Kegan Paul, 1975, in 2 volumes

FREMGEN, James M
1967 "Utility and Accounting Principles", Accounting Review, 42, 1967, 457-67

GAMBLING, Trevor
1974 Societal Accounting , George Allen and Unwin

GIGCH, John P van
1974 Applied General Systems Theory, Harper and Row, 1974

GOLDBERG, Louis
1965 An Inquiry into the Nature of Accounting American Accounting Association, Monogrph No 7, 1965

1971 "Varieties of Accounting Thought", in Stone,(1971), 1-25, 31-49

GORDON, Myron J
1960 "Scope and Method of Theory and Research in the Measurement of Income and Wealth", Accounting Review, 35, 1960, 603-18.

GRADY, Paul
1965 Inventory of Generally Accepted Accounting Principles for Business Enterprises, Accounting Research Study 7, AICPA, 1965

GREEN, David
1966 "Absolutism and Accounting Theory," in Aspects of Contemporary Accounting, University of Florida Press, 1966, 1-11

GRIFFIN, Charles H
1971 "Validating Theoretical Constructs: Accounting and Economics", Singapore Accountant, 6, 1971, 27-33

1972 "Theory Verification: Accounting and Philosophy", Singapore Accountant, 7, 1972, 9-15

HANSON, Norwood R
1958 Patterns of Discovery, Cambridge Press, 1958

HARRE, R
1972 The Philosophies of Science, Oxford University Press, 1972

HATTIANGADI, J N
1977 "The Crisis in Methodology: Feyerabend", Philosophy of Social Science, 7, 1977, 289-302

HELMER, Olaf and RESCHER, Nicholas
1959 "On the Epistemology of the Inexact Sciences" Management Science, 6, October 1959, 26-52

HENDERSON, Scott and PEIRSON, Graham
1975 Issues in Financial Accounting, Cheshire Publishing Pty Ltd, 1975

HENDRIKSEN, Eldon S
1977 Accounting Theory, 3rd Ed, Richard D Irwin, 1977

HINES, Danny R
1975 "Systems Theory: A Framework for the Development of Accounting Theory", 27th Annual Meeting of Southeast Regional Group, AAA, Collected Papers, 1975, 149-153

IJIRI, Yuji
See Appendix

IJIRI, Yjui and JAEDICK, R K
1966 "Reliability and Objectivity of Accounting Measurement", Accounting Review, 41, July 1966, 474-83

IJIRI, Y, JAEDICKE, R K and KNIGHT, K E
1966 "The Effects of Accounting Alternatives on Management Decisions" in Jaedicke, Ijiri and Nielsen (1966), 186-99

ISELIN, Errol R
1968 "Chambers on Accounting Theory", Accounting Review 43, 1968, 231-38

ISELIN, Errol R
1971 The Objectives of Accounting in an Accounting Theory Based on Deductive Methodology, University of Queensland Press, 1971

KAM, Vernon
1973 "Judgement and the Scientific Trend in Accounting", Journal of Accountancy, 135, February 1973, 52-57

KAPLAN, Abraham
1964 The Conduct of Inquiry: Methodology for Behavioural Science, Chandler Publishing Company, 1964

KEAT, Russell and URRY, John
1975 Social Theory as Science, Routledge and Kegan Paul, 1975

KUHN, Thomas S
1970a The Structure of Scientific Revolutions, Second Enlarged Edition, University of Chicago, 1970

1970b "Reflections on My Critics", in Lakatos and Musgrave (1970), 231-78

1974 "Second Thoughts on Paradigms", in Suppe (1974), 459-82

1976 "Theory-Change as Structure-Change: Comments on the Sneed Formalism" Erkenntnis, 10, 1976, 179-99

KULKA, Thomas
1977a "How Far Does Anything Go? Comments on Feyerabend's Epistemological Anarchism", Philosophy of Social Sciences, 7, 1977, 277-89

1977b "Some Problems Concerning Rational Reconstruction: Comments on Elkana and Lakatos", British Journal for the Philosophy of Science, 28, 1977, 325-44

LAKATOS, Imre and MUSGRAVE, Alan (Eds)
1970 Criticism and the Growth of Knowledge, Cambridge University Press, 1970

LAMBERT, Samuel Joseph
1974 "Basic Assumptions in Accounting Theory Construction" Journal of Accountancy, February 1974, 41-48

LARSON, Kermit D
1967 "Descriptive Validity of Accounting Calculations", Accounting Review, 42, 1967, 480-88

1969 "Implications of Measurement Theory on Accounting Concept Formulation", Accounting Review, 44, 1969, 38-47

1971 "Are there Commercial Foundations of Accounting Theory", in Stone, (1971), 78-89

LARSON, Kermit and SCHATTKE, R W
1966 "Current Cash Equivalent, Additivity, and Financial Action", Accounting Review, 40, October 1966, 634-41

LAW, John
1975 "Is Epistemology Redundant? A Sociological View", Philosophy of Social Science, 5, 1975, 317-337

LOSEE, John
1972 A Historical Introduction to the Philosophy of Science, Oxford University Press, 1972

MARGENAU, Henry
1960 "What is a theory?", in KRUPP, S (Ed), The Structure of Economic Science, Prentice-Hall, 1960, 25-38

1961 Open Vistas, Yale University Press, 1961

MATTESSICH, Richard D
See Appendix

MAUTZ, R K
1963 "Accounting as a Social Science", Accounting Review, 38, 1963, 317-25

MAUTZ, R K and SKOUSEN, K Fred
1969 "Some Problems in Empirical Research in Accounting", The Accounting Review, 44, 3, 1969 447-55

McDONALD, D L
1972 Comparative Accounting Theory, Addison-Wesley Publishing Company, 1972

MOONITZ, M
1961 The Basic Postulates of Accounting, Accounting Research Study 1, American Institute of Certified Public Accountants, 1961

1974 Obtaining Agreement on Standards in the Accounting Profession, American Accounting Association, Studies in Accounting Research, 8, 1974

MOST, Kenneth S
1977 Accounting Theory, Grid Publishing Company, 1977

NAESS, Arne
1972 The Pluralist and Possibilist Aspect of the Scientific Enterprise, George Allen and Unwin Limited, 1972

NELSON, Carl L
1973 "A Priori Research in Accounting" in Dopuch and Revsine (1973), 3-19

PENMAN, Stephen H
1973 A Framework for the Development of Accounting Theory, University of Queensland Press, 1973

POPPER, Karl R
1956 "Three Views Concerning Human Knowledge", in Contemporary British Philosophy, 3rd Series, Lewis, H.D. (Ed) 357-388, Allen and Unwin, 1956 (2nd Ed 1961)

1959 The Logic of Scientific Discovery, Hutchinson of London, 1959, (Sixth Impression Revised, March 1972)

PREVITS, Gary J (Ed)
1975 Papers Presented at the Accounting Research Convocation, "Bridging the Gap", University of Alabama 1976 (date published)

1976 (Ed), Papers Presented at the Accounting Research Convocation on the subject of Emerging Issues, University of Alabama, 1976

PRINCE, Thomas R
1963 Extension of the Boundaries of Accounting Theory, (South-Western Publishing Company, 1963)

1970 "An Overview of Conceptual Measurement Issues in Financial Accounting Theory" in STONE, " (Ed) (1970), 45-84

RUDNER, Richard
1965 Philosophy of Social Science, Prentice Hall, 1965

RUNES, Dagobert D
1974 Dictionary of Philosophy, Littlefield, Adams and Company, 1974

SCHILPP, Paul Arthur
1974 The Philosopher of Karl Popper, in 2 Books, The Library of Living Philosophers, Volume XIV, Open Court Publishing Company, 1974

SCHRADER, William J
1962 "An Inductive Approach to Accounting Theory", Accounting Review, 37, 1962, 645-49

SMITH, James E
1975 "Communication Theory: A Methodology for Evaluating Accounting Theory and Its Relevance to Accounting Instruction", 27th Annual Meeting of Southeast Regional Group, AAA, Collected Papers, 1975, 154-58

SNEED, Joseph D
1971 The Logical Structure of Mathematical Physics, D. Reidel Publishing Company, (Synthese Library) 1971

SOLO, Robert A
1975 "What is Structuralism? Piaget's Genetic Epistemology and the Varieties of Structuralist Thought", Journal of Economic Issues 9 (No 4, December), 605-25

SOLOMONS, David
1966 "Accounting, Evaluation and Economic Behaviour by R J Chambers: Review", Abacus, 3, December 1966, 205-9

SPENCER, Milton H
1963 "Axiomatic Method and Accounting Science", Accounting Review, 38, 2, 1963, 310-16

SPROUSE, R T and MOONITZ, M
1962 A Tentative Set of Broad Accounting Principles for Business Enterprises, Accounting Research Study 3, American Institute of Certified Public Accountants

STANFIELD, Ron
1974 "Kuhnian Scientific Revolutions and the Keynesian Revolution", Journal of Economic Issues, 8, 1, 1974, 97-109

STAUBUS, George J
1967 "Current Cash Equivalent for Assets: A Dissent", Accounting Review, 42, 1967, 650-61

STEGMULLER, Wolfgang
1975 "Structure of Dynamics of Theories", Erkenntnis, 9, 1975, 75-100

1976a The Structure and Dynamics of Theories, Springer-Verlag 1976

1976b "Accidental ('Non - Substantial') Theory Change and Theory Dislodgement: To What Extent Logic can Contribute to a Better Understanding of Certain Phenomena in the Dynamics of Theories", Erkenntnis 10, 1976, 147-78

STENHOUSE, David
1973 The Evolution of Intelligence, George Allen and Unwin, 1973

STERLING, Robert R
See Appendix

STONE, Williad E (Ed)
1970 Theory Formulations, University of Florida Press, 1970

1971 Foundations of Accounting Theory, University of Florida Press (Accounting Series No 7), 1971

SUPPE, Frederick
1972 "Whats Wrong with the Received View on the Structure of Scientific Theories?" Philosophy of Science, 39, 1972, 1-19

SUPPE, Frederick
1973 "Theories, Their Formulations and the Operational Imperative Synthese, 25, 1963, 129-164

1974 The Structure of Scientific Theories, University of Illinois Press, 1974

TIBBETTS, Paul
1977 "Feyerabend's 'Against Method': The Case for Methodological Pluralism", Philosophy of Social Science, 7, 1977, 265-75

TILLEY, Ian
1972 "Accounting as a Scientific Endeavour: Some Questions the American Theorists tend to leave Unanswered", Accounting and Business Research, Autumn 1972, 287-97

TOULMIN, Stephen E
1967 "The Evolutionary Development of Natural Science", American Scientist, 455, 4, 1967, 456-471

WHEELER, John T
1970 "Accounting Theory and Research in Perspective", Accounting Review, 45, 1970, 1-10

WILLIAMS, Thomas H
1972 "The Relationship of Empirical Research to Accounting Theory", in Sterling (1972), 53-57

WILLIAMS, Thomas H and GRIFFIN, Charles H
1964 The Mathematical Dimension of Accountancy, South-Western Publishing Company, 1964

1969 "On the Nature of Empirical Verification in Accounting", Abacus, 5, 1969, 143-78

WOODGER, J H
1939 The Technique of Theory Construction, 1939, University of Chicago Press

YU, S C
1974 "The Several Modes of Normative Accounting Thought: A Critical Examination", The International Journal of Accounting, 9, 1974, 2, 83-104

1976 The Structure of Accounting Theory, University of Florida Press, 1976

ZEFF, Stephen A
1971a "Comments on 'Varieties of Accounting Theory'", in Stone, 1971, 50-58

1971b Forging Accounting Principles in Five Countries: and an Analysis of Trends, Stipes Publishing Company, 1972, (Arthur Andersen and Company Lecture Series - 1971)

ZEFF, Stephen A and HOFSTEDT, T R
1974 "The Communications Gap: The Researcher and the Practioner", The Accountant's Magazine, January 1974, 18-21