Division of Research
Graduate School of Business Administration
The University of Michigan

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MANAGEMENT ACCOUNTING LITERATURE:

PAST, PRESENT, AND FUTURE

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Michael W. Maher

The University of Michigan

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Abstract

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The University of Michigan

Purpose. The purpose of this paper is (1) to identify major trends in management accounting research; (2) to evaluate the impact of these trends; and (3) to predict future directions of management accounting research. I hope that this paper, coupled with the companion piece, "Management Accounting Research: A Review and Annotated Bibliography" (by Klemstine and Maher), will provide a sense of our history, identify false starts and promising leads, and provide some insight into what we can expect from the management accounting research literature.

Evaluation of impact. A recurring question is: What impact has research had on practice? I assume that a primary impact of management accounting research on practice occurs through teaching; thus, my primary criterion for evaluating the impact of research is to assess its influence on "mainstream" cost/managerial accounting textbooks.

Major trends. Three major categories of literature, based on the general types of methods used, can be identified.

(1) It is well known that our early research literature (i.e., that published before 1960) was predominantly a priori conceptual (primarily seeking definitions, or attempting to find means of measuring "true" costs) and ad hoc descriptive. Much of this work has either found its way into mainstream textbooks or been rejected (e.g., many textbooks downplay attempts to find "true" cost measures).

(2) The 1960s saw the advent of modeling literature, which is loosely partitioned into (a) the application of quantitative methods to management accounting and (b) the economics of information choice. The former has been incorporated into mainstream textbooks at a rather simple level. The latter, which includes the developing agency and incentive contracting models, holds some promise of providing an economics-based management accounting theory.

(3) The 1960s also saw the advent of a limited amount of empirical work in accounting, which has been almost totally behavioral. This literature has moved from an emphasis on (a) motivation and organizational issues to (b) information processing and decision making. Except for occasional references to motivational and organizational issues, this literature is little reflected in mainstream management accounting textbooks.

Notable absences. Notable by its absence is a theory-building and empirical testing literature. We know very little about the properties of accounting systems used in organizations, and why particular accounting methods have evolved while others have not.

Obstacles to future research. Obstacles to developing an empirically tested theory in management accounting are primarily (1) the elementary level of theory; (2) the absence of testable hypotheses; (3) the cost of obtaining data; and (4) perhaps most important, the economics of research (i.e., what are the incentives to allocate research effort to management accounting theory development and testing?).

Predictions. What will have changed by 1992? 2002? An optimistic forecast is that we will better understand what organizations do and why (i.e., "positive" theory development). Mainstream management accounting textbooks will deemphasize descriptive/normative treatments of management accounting, and emphasize the relationship between organizational characteristics and the nature of accounting systems, from which normative models can be derived that are based on more realism than current models. Management accounting will become more institutional in characterizing the management demand for accounting information. This suggests that contingency theory and the recent work in industrial organization (e.g., Spence, Williamson) may usefully extend the theoretical foundation provided by agency models.

Comparative analysis. Management accounting research is often criticized because it has been little applied. However, a comparison of mainstream management accounting textbooks suggests that research has had at least as great an influence on textbooks in management accounting as on those in financial accounting and auditing.

MANAGEMENT ACCOUNTING LITERATURE:
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I. PREFACE

When faced with the task of surveying and evaluating the state of the art of an area as large and amorphous as managerial accounting, one becomes quite modest about his or her ability to perform such a task adequately. As a first approximation, how does one differentiate between "managerial accounting" and financial accounting, information economics, behavioral research, science, auditing, and so forth? Second, after defining the boundaries of the set, what are its elements? Does it include articles in professional journals, working papers, articles related to accounting in nonaccounting journals such as Management Science and the American Economic Review? Are all articles in the academic accounting journals to be considered research?

Third, how can the state of the art of management accounting be captured, summarized, and expressed? An obvious option is to write a review paper. But I rejected that alternative because we have many excellent papers reviewing subsets of the literature, including recent contributions by Baiman, Demski and Kreps, Sundem, Tiessen, and Waterhouse, Spicer and Ballew, and of course the (classic) review of quantitative models in managerial accounting by Kaplan. (A list of these and other review papers and bibliographies is presented at the end of this paper.) I rejected the review paper idea also because, frankly, the task of developing a good, comprehensive review of the entire management accounting literature seems formidable, to say the least.

The option that I chose was to develop two papers representing the extremes of the choices available to me. The first is a reasonably exhaustive data base titled "Management Accounting Research: A Review and Annotated

Bibliography" (Klemstine and Maher). This is a purely descriptive piece that traces the development of management accounting from the beginning of time (which I define as 1926, when Bill Paton started The Accounting Review in the basement of the Economics Building at The University of Michigan) through 1982. More than 600 pieces are reviewed and categorized by source discipline (e.g., economics, psychology, statistics), topic (e.g., CVP, transfer pricing, direct vs. absorption costing), and method of analysis (e.g., experimental, survey, modeling). (I resisted the temptation to run an ANOVA or factor analysis on this set and report the results!)

The second paper is this one, which is very subjective, and more superficial than a review paper. The purpose of this paper is to comment on some of the past major trends in management accounting research, to evaluate the impact of these trends, and to predict some future directions for research in this area. I hope that this paper, coupled with the companion bibliography, will provide a sense of history, identify false starts and promising leads, and provide some insight into what we can expect from management accounting research in future years. (Of course, I make the standard accountant's disclaimer that in my review of the data I am not liable for errors, irregularities, fraud, etc.)

II. A BIBLIOGRAPHY

My comments on the literature depend critically on my specifications of what is included in the set of "management accounting literature," so I shall take a moment to describe that set.

There were two criteria for selecting pieces for the set. (1) The paper should deal with a management accounting topic. Generally, we included any paper dealing with internal decision making or product costing as discussed in cost and managerial accounting textbooks. Thus, for example, we included work on the direct vs. absorption costing debate, and some work on internal control if it dealt with managerial rather than auditing issues. In addition, our set is broad enough to include some of the work on human resource accounting. (2) All pieces must have appeared in one of the major research journals, which we have defined as including the Journal of Accounting Research, The Accounting Review, the Journal of Accounting and Economics, Abacus, Accounting, Organizations and Society, and Accounting Research (which was published from 1948 to 1958). We have also included books, monographs, and management accounting articles in research journals outside of accounting that have been extensively referenced in the academic accounting literature (for example, Hirschleifer's [1956] classic transfer pricing paper), and articles in professional journals that have been extensively referenced in the academic literature. We have obviously made both Type I and Type II errors in selecting elements for the set of management accounting literature. However, we see the bibliography as an evolving document, and with sufficient input from academic colleagues, these errors will be minimized.

III. DEVELOPMENTS IN THE LITERATURE

Reviewing the literature from 1926 until the present provided some new insights and reinforced some old ones. (I confess that my academic career has focused on the post-1970 era. I had read very little original source literature published before 1960, and less than one-half of the literature published in the 1960s, before undertaking this review.) Most important, there has been a significant change in the character and quantity of the literature since the early 1960s. (I have partitioned the literature into pre-1960 and post-1960 for the sake of convenience, and hope the reader does not attribute some other significance to that breaking point.)

The pre-1960 literature has the characteristics of olde time religion; that is, it represents a search for fundamental concepts and truths. Horngren [1975] and others have referred to this period as one in which accountants searched for true costs (i.e., the one true religion), while in later periods we looked at accounting in terms of user decision models (i.e., pick the religion that works) and the economics of information. Of course, the preoccupation with finding True Costs paralleled the search for True Income for financial reporting. A key difference between the pre-1960 and post-1960 literature is a decreased emphasis in the literature on financial reporting implications.

Many academics of my vintage believe that the pre-1960 literature was devoted mainly to the examination of direct vs. absorption costing and other attempts to define the difference between period and product costs. While that was a main emphasis, I was surprised to find that the literature also contained many definitional pieces, observations about practices, and other works that have become an established part of our textbook folklore. Much of the material in mainstream cost accounting textbooks was originally discussed in the literature. For example, a 1928 article on differential costs that could be the basis for any "modern" cost or managerial accounting textbook's chapter(s) on relevant or differential costs included descriptions of differences between differential, average, and "sunk" costs, and had a clear user orientation: "Accounting is useful to business mainly to the extent that it provides data which can be used in the formation of business judgments" (Rorem [1928], p. 335).

The point can be summarized by stating that nearly all of the topics and issues discussed in cost and managerial textbooks of the 1960s vintage had been discussed in the pre-1960 academic literature. Perhaps this point was not previously apparent to me because cost and managerial accounting textbooks are not replete with references, particularly not to the pre-1960 literature.

The pre-1960 literature was also included an abundance of "case studies" which describe cost and managerial practices in particular firms or industries. Examples of titles include "Cost Accounting for Motor Freight Lines" (Lehnberg [1950]), "Costs and Inventory Values in the Glue Industry" (Pape [1959]), and (my favorite) "Packing House Accounting" (Zraick [1947]). These papers were descriptive -- they made no attempt to theorize about why particular accounting practices developed in particular firms or industries.[1]

While these descriptive papers would probably not make the "cut" today at the academic journals, papers dealing with actual practices could make a valuable contribution to our current literature. Kaplan [1977] stated: "I would like to see some studies on the problems of estimating and implementing these [quantitative] models in actual situations with some estimate of the perceived benefits and acceptance of these models" (p. 62).[2]

The pre-1960 descriptive papers, if published today, would be criticized for their lack of generalizability. They were, in effect, case studies characterized by considerable depth compared to current surveys and field studies.[3] While the number of observations was small, we learned a lot about each observation. However, very little systematic work on the "why" of managerial accounting was done. Such questions as, Why are particular accounting methods developed as they are? What characteristics of companies lead to the use of

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[1]For readers interested in more information about these papers, they are listed chronologically under the heading "Genera Descriptions of Firm/Industry Practices" in the companion bibliography: "Management Accounting Research: A Review and Annotated Bibliography," by Klemstine and Maher [1982].

[2]See Kaplan's [1974] study of cost behavior in a hospital, for example.

[3]This depth was shared by Solomon's [1965] study of decentralization.

flexible budgets? Variable costing? Cost-based transfer pricing? Why and how are cost allocations used? were not systematically dealt with then, nor have they been since. The answers to these questions -- which could lead to a "positive" theory of management accounting -- might be found in a more sophisticated application of the technologies in the early descriptive literature.

Post-1960 literature

In the 1960s, the quantity and nature of the literature changed dramatically. First, the emphasis changed from "theory" construction and product costing for income measurement to decision making, planning, and control. Users of accounting data were explicitly incorporated into much of the behavioral and contracting literature (e.g., agency theory), and were implicit in many others (e.g., cost-volume-profit). Ideas and methods were more extensively imported from other disciplines. The 1960s saw the advent of modeling in management accounting, which can be loosely partitioned into (a) the application of quantitative methods to management accounting and (b) the modeling of accounting decisions, including the economics of information choice, and contracting (e.g., agency theory). The 1960s also saw the advent of limited behavioral work.

While importing ideas from other disciplines has been a major feature of our literature from the beginning, there is an important difference in what was imported before and after 1960. The former was limited to general concepts; examples include Rorem's [1928] import of differential cost notions from economics and Matz's [1946] report on the development of the computer at the University of Pennsylvania, in which he suggests that it might someday be a useful tool for accountants. After 1960, the ideas and methods were incorporated into research designs. A pre-1960 paper on cost estimation would suggest that accountants consider using statistical methods, while a post-1960 paper would use the method and discuss its properties.

Three examples

To provide a flavor of how the character of the literature changed over time, I summarize the chronological developments of three topics: (1) direct vs. absorption costing, (2) cost allocation, and (3) information economics. The issue of direct vs. absorption costing received considerable attention from about 1950 to about 1965, but very little after that. The second, cost allocation, has been with us since the beginning of accounting and probably will be until the end. The seminal work in the third topic, information economics, appeared in the post-1960 literature.[4]

Example 1: Direct vs. absorption

The direct vs. absorption cost controversy presents an excellent example of how the literature on a topic would develop quickly into a heated debate, in which the writers would use argumentative methods to persuade readers about the inherent properties of particular costing methods. We found only one article on the topic before 1951 (Schlatter [1945]); more than twenty between 1951 and 1965; and only a few since 1965. This literature has only three modeling papers and one empirical (field) study. The rest of the literature is based on a priori (i.e., argumentative) methods. This literature has provided some definitional groundwork, but we learned very little about whether and why firms choose direct and/or absorption costing, and we have made only modest progress in demonstrating decision sensitivity to the choice of each

[4]More details about the literature in each of these categories are presented in the companion bibliography by Kleimstine and Maher [1982].

system. (Bailey [1973], Demski [1970], and Ijiri, Jaedicke, and Livingstone [1965] could be considered exceptions.)

Example 2: Cost allocation

Although early textbooks gave considerable attention to cost allocation, we were surprised to see how recently the topic was introduced into the research literature. An early article on cost allocation was written by Vatter [1945], who set forth the limitations of cost allocation and called for research in the area. This was followed by several definitional and conceptual articles until 1964, when two modeling papers (Churchill [1964] and Williams and Griffin [1964]) moved the literature from a definitional orientation to one focusing on the application of models.

In recent years, many authors of modeling papers have downplayed the normative tone of the cost allocation literature. As Kaplan [1977] concluded: according to these authors, "We're not telling you to allocate, but if you must, use one of the models described here" (p. 59). In short, cost allocation papers are schizophrenic -- on the one hand, they display reasonably sophisticated methods for allocating costs; on the other, there remain reservations about the propriety of allocating costs in the first place. The topic calls for some positive theory (e.g., Zimmerman [1979]) and empirical work that will help us to specify the use of cost allocations in organizations. While we are beginning to understand that the demand for allocation is derived from the properties of decentralized organizations, it is still a mystery why costs, particularly accounting costs, are allocated.

Clearly, we have reached a major turning point in this literature. We appear to have reached the point of diminishing returns to argumentative discussions and modeling (except for Kaplan's advice to research the costs and benefits of implementation), and helpful answers to the "why do firms allocate" questions could come from positive economic theory and behavioral research. The potential for breakthroughs in this area is both exciting and, in my view, important.

Example 3: Information Economics/Value of Information[5]

It would be amazing to find that the conditions of supply and demand for our product had not been considered prior to 1968 -- but that is very nearly the case. Some of the early work on cost concepts implicitly considered the usefulness of cost data,[6] but there was no explicit consideration of the value of information in the accounting literature until Feltham [1968] and LaValle and Rappaport [1968]. Information economics became a growth industry in a remarkably short period of time, possibly because the market "demanded" an explicit evaluation of information value, possibly because the "supply" of this product became newly available.

While readers of the information economics literature have come to expect it to consist of abstract modeling, that is not necessarily so (e.g., see Crandell [1969] and Mock [1969]). It is interesting to speculate how the literature might have differed as to both its character and its application if a significant subset of the early literature had been less abstract.

Observations on the Literature

After reviewing more than fifty years of literature, one develops many hypotheses about why the literature has evolved as it has. There were many

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[5]My review and discussion of information economics are limited because the topic has its own forum at this conference.

[6]For example, see the passage from Rorem's [1928] article on differential costing quoted earlier.

obvious external factors -- for example, the literature from 1941 to 1945 was determined by issues related to government contracting. Also, as the training of researchers changed, the influence of modeling and, to a lesser degree, experimental methods became apparent. Of the two factors -- external influences and training -- training has clearly been the major factor affecting changes in the literature. (So current training should be a good predictor of future literature.)

Perhaps the most important influence on our research has been the computer. Research is a function of researchers' tools, and the major new tool available to researchers in recent years is the computer. While very little of our literature deals directly with the computer, its indirect effect is apparent; namely, the ability to run large data bases and deal efficiently with complex algorithms, which has clearly had an impact on the literature in the past twenty to thirty years.

<u>Impact of Managerial Accounting Research</u>

Accounting research is often criticized for having little impact on practice. Since our research deals more with concepts and methods than with well-specified techniques, very little of it can be expected to have a direct impact on practice. More likely, the impact is as follows:

Research → Teaching → Students → Practice.

To evaluate impact, we should look at teaching materials (e.g., readings, books, textbooks, handouts) as the means of filtering, synthesizing, and communicating ideas to students, who then carry these ideas with them into practice.

For example, discounted cash flow techniques for capital budgeting were well known in the literature long before they became common in organizations.

It took a generation of students schooled in discounted cash flow methods to bring DCF to their jobs. An examination of the economics of using DCF should reveal that the on-the-job costs are lower for managers who had DCF training in school than for those who did not. Hence, DCF-trained managers are more likely to use it.

I do not believe we should hold ourselves to the standard of immediate application of our work. We "invent" very few techniques that clearly have immediate high benefits to users. Most of our work leads to new ways, or modifies old ways, of thinking about problems. For example, the use of matrix algebra for reciprocal cost allocations may be a reasonable idea that will reshuffle cost and resource allocations, and it may have marginal motivational effects in some instances. But the cash flow implications of reciprocal cost allocations are not likely to be dramatic in a given firm. (I doubt that an events study would have revealed a stock price reaction to the first company that used it.) In short, a reasonable "standard" for implementation of research is inclusion in educational packages, not direct implementation in practice.

To examine the impact of research on teaching materials, I reviewed a number of textbooks from 1927 through the current year, and two trends in textbook evolution are apparent. First, textbooks do change with developments in the literature, although not with <u>all</u> developments. Second, the lag between literature and cost accounting textbooks are lengthened, but the gap is being filled by "advanced" books[7] that help communicate some of the advances in the literature to students (and instructors).

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[7]For example, Kaplan's [1982] <u>Advanced Management Accounting</u> and Dopuch, Birnbirg, and Demski's [1982] <u>Cost Accounting</u>.

I also observed some difficulties in incorporating the literature into textbooks. For example, in his review, Kaplan noted that quantitative methods have not been integrated into "standard" cost accounting textbooks. Why? First, it is not the comparative advantage of most accountants to deal with technical mathematical and statistical issues -- it would be pretentious of us to teach linear programming to MBA students, for example (although I confess that I have done so). Presumably, students become well versed in management science models, statistics, and mathematics in other courses. Our comparative advantage is in dealing with problems on the input side of models, particularly when the inputs are accounting data; and in dealing with problems on the output side, particularly when the model is applied to an accounting problem. Much of the literature focuses on technical rather than application issues; hence, considerable translation is needed to integrate quantitative techniques with managerial/cost accounting topics, without simply repeating what students have learned in other courses.

Second, given the incentives for textbook writing, we expect textbook authors to be risk-averse with respect to the incorporation of new materials. We know that basic research, by its nature, includes work that will ultimately be shown to be of little value. Textbook writers must be convinced that the literature has some merit before incorporating it in their textbooks. In this sense, textbook writers are both "filterers" and translators. They filter the literature for what they believe is promising, and they translate it. The lower the costs of filtering and translating, the greater the chance for the literature to be incorporated into textbooks.

Third, authors of "best seller" textbooks have little incentive to change their books at all, unless the market demands it. In short, they have little incentive to push the market. When all these factors are considered, it is not surprising that textbooks have been slow to adopt some of the advances in the literature of the 1960s and 1970s.

Modeling and Managerial Accounting: Are They Synonymous?

The greatest impact on managerial accounting literature in the past two decades has been modeling. But in the past few years, we have seen a decline in applications of management science models to management accounting. The growth industry in modeling — information economics and incentive contracting -- is populated by only a few researchers (and rookies are confronted with many barriers to entry). Thus, except for this small growth industry, I sense a concern about the future management research in general, and about our attractiveness vis-à-vis auditing, behavioral theory, financial accounting, and other research (see Sundem [1981], for example). This concern may be partially due to the assumption that modeling and management accounting are synonomous because, as Kaplan stated in 1977, "...it's hard to identify areas where new management science models are going to have a substantial impact on the way we think about these [managerial accounting] problems" (p. 61).

My review of the literature convinces me that the opportunity set is very large. For example, notable by its absense is a theory-building and empirical testing literature. We know very little about the properties of accounting methods in organizations and the determinants of the methods that have evolved. I see this "positive" research as leading to another round of normative research -- both modeling and behavioral/motivation -- based on less naive assumptions about how and why accounting is used.

There has been very little work in the use of internal controls and internal auditing to monitor agents' behavior. There has been very little research on behavioral decision making in managerial accounting. I do not believe we have exhausted the opportunities for applying management science models in managerial accounting. For example, we have virtually no literature on the use of simulation in planning, budgeting, cost-volume-profit analysis, and other activities. We know very little about regulatory influences on management accounting. Finally, we have done very little work on interaction of computers and management accounting on resource allocations in firms. These are only a few of the growth industry opportunities in management accounting, in addition to the work being done in the incentive contracting literature. In the next section of this paper, I will explore a few of these opportunities in more depth.

IV. TOWARDS A THEORY OF MANAGERIAL ACCOUNTING

In this section I will discuss some of the recent attempts to develop a theory of managerial accounting.

Information Economics and Agency Theory[8]

In only eight years of published literature, from the seminal works by Feltham [1968] and others until the publication by Demski and Feltham of Cost Determination [1976], we reached the point of diminishing marginal returns to further research into single-person information system choice.[9] Extensions into multiperson contexts -- agency theory and game theory -- have been fruitful, but often make the problem quite complex. The basis for this work is game theory and social choice theory, which provide optimal solutions only under limiting assumptions (see Arrow [1963] and Sen [1970]).

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[8]It is potentially misleading to combine these topics -- they are actually quite different. I do so because the accounting researchers who contribute heavily to information economics in the accounting literature have also contributed to agency theory.

[9]See Sundem [1981] for a thorough discussion.

More recent work in agency theory has the potential to contribute to management accounting in a number of ways. First, it may be used as a conceptual framework for performing empirical studies of behavior within organizations (e.g., Maher, Ramanathan, and Peterson, [1979]). Second, it provides a theory for monitoring and performance evaluation that has already provided some important insights into trade-offs between risk sharing and motivation, problems raised when skills are not observable ("adverse selection") and when effort is not observable ("moral hazard") (e.g., Demski and Feltham [1978]).[10] Third, information system choice can be evaluated in the context of agency theory. Agency theory is concerned with establishing Pareto optimal contracts. Contracts must be based on observables; hence, the role of information systems is to provide signals upon which contracts can be based. Hence, the optimal information system is one that equates the marginal cost of information, including contracting costs, with the marginal agency cost if that information is not used.

Information economics and agency theory have clearly provided insights into the value of information and agency questions at a theoretical level, which alone is sufficient justification for the research. But will this work provide a much needed theoretical foundation for managerial accounting? Does it have potential for application to real problems? I am cautiously optimistic that the answer to both questions is yes. As to application, my criterion is that the research must become part of mainstream educational packages. Some influence of agency theory and information economics on recent cost and managerial accounting textbooks is apparent. However, much work needs to be done to translate the literature and to reduce barriers to entry for students (and instructors).

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[10] Baiman [1982] provides a thorough review of this literature.

As to theoretical foundations, the work has helped us to understand fundamental issues about the purpose of accounting, and the conditions under which more information is preferred to less. For example, the generally accepted notion that information should be evaluated on the basis of its decision impact rather than inherent properties of the information can be partly traced to this literature. (Academics of my vintage sometimes forget how recent and important this shift in emphasis has been.)

To become a theoretical foundation of an applied discipline like accounting, we expect the research to be empirically testable. At this time, empirically testable hypotheses are not apparent, except in carefully controlled settings -- e.g., when laboratory experimental and simulation methods are used. Why haven't we seen more attempts to test agency theory in managerial settings? First, the area is new and the literature so far has dealt only with primitive agency relationships. Second, if we consider the incentives of researchers themselves, we can explain a great deal about the development of a literature (i.e., the "economics of research"). The barriers to entry to the extant literature and to data are obvious. Researchers who are trained in and oriented toward empirical work find both testable hypotheses and data bases more accessible in capital markets, auditing, industrial organization, and organizational theory research. Only if researchers are sufficiently rewarded to reduce barriers to entry to the literature,[11] and if the costs and benefits of obtaining data are sufficiently altered, can we expect a spate of empirical tests of agency theory.

While some observers of research activity in this field are pessimistic about its potential, I am not. However, it would be a mistake to encourage a

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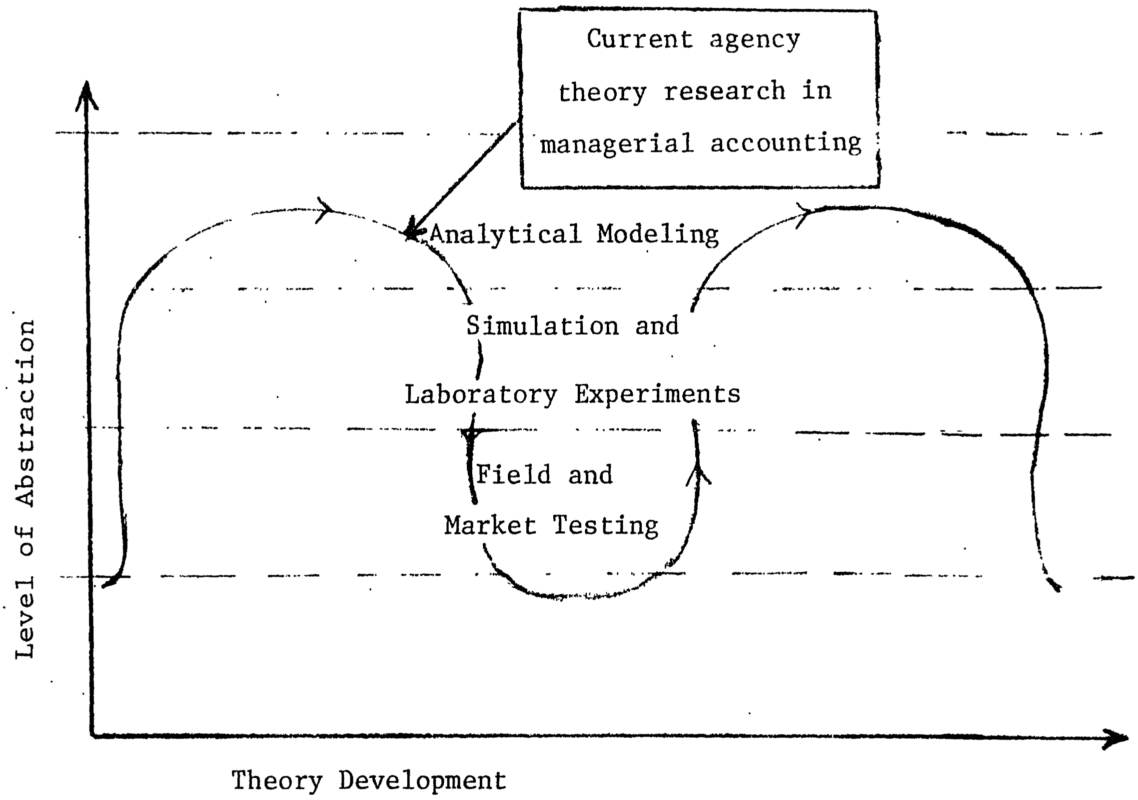
[11]Stan Baiman's [1982] review of analytical agency models is an excellent example.

great quantity of empirical research before we know what to look for! Ideally (in my opinion), we should encourage the deductive reasoners to continue their work, and we should encourage reduction of barriers to entry so potential testable hypotheses are more widely accessible. A natural evolution will be the development of testable hypotheses and empirical work, perhaps along the lines suggested by the graph below.[12]



In short, there are a number of potential avenues for future research in information economics and agency theory. I am particularly optimistic that agency theory will provide part of a much needed conceptual framework for management control and auditing. While it is premature to suggest that information economics and agency theory provide either normative guidelines for firms or testable hypothesis for a positive theory, the research certainly

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[12] I am grateful to Gary Sundem for pointing this out to me.

qualifies as part of the "insight literature," that is, literature providing useful insights to academic and practicing managerial accountants.

Toward an Economics-Based Theory of Management Accounting

As previously indicated, a positive theory of management accounting could make a major contribution. This is not to downplay the importance of behavioral work, including both organizational/motivational and human information processing, and of modeling. However, we know so little about why and how accounting affects resource allocations and vice versa that I expect the marginal value of economics-based positive research to be particularly high. Unfortunately, the economics of internal organizations has only recently received attention in economics, so we have little theory to draw on.[13] Further, the extant work does not provide testable hypotheses.

Accountants and other management researchers may have a comparative advantage in doing research on the economics of internal organization -- in fact, this may be an unusual opportunity to export theory.[14] Some attempts in this direction can be found in two "integrative" papers that compare and contrast agency theory, contingency theory from the organizational theory literature, and Williamson's [1973, 1975] work on markets and hierarchies (Tiessen and Waterhouse [1982] and Spicer and Ballew [1982]).

The work in markets and hierarchies shows some promise for linking organizational research to economic theory. Namely: Why and under what conditions do organizations replace markets? Why do particular firms vertically

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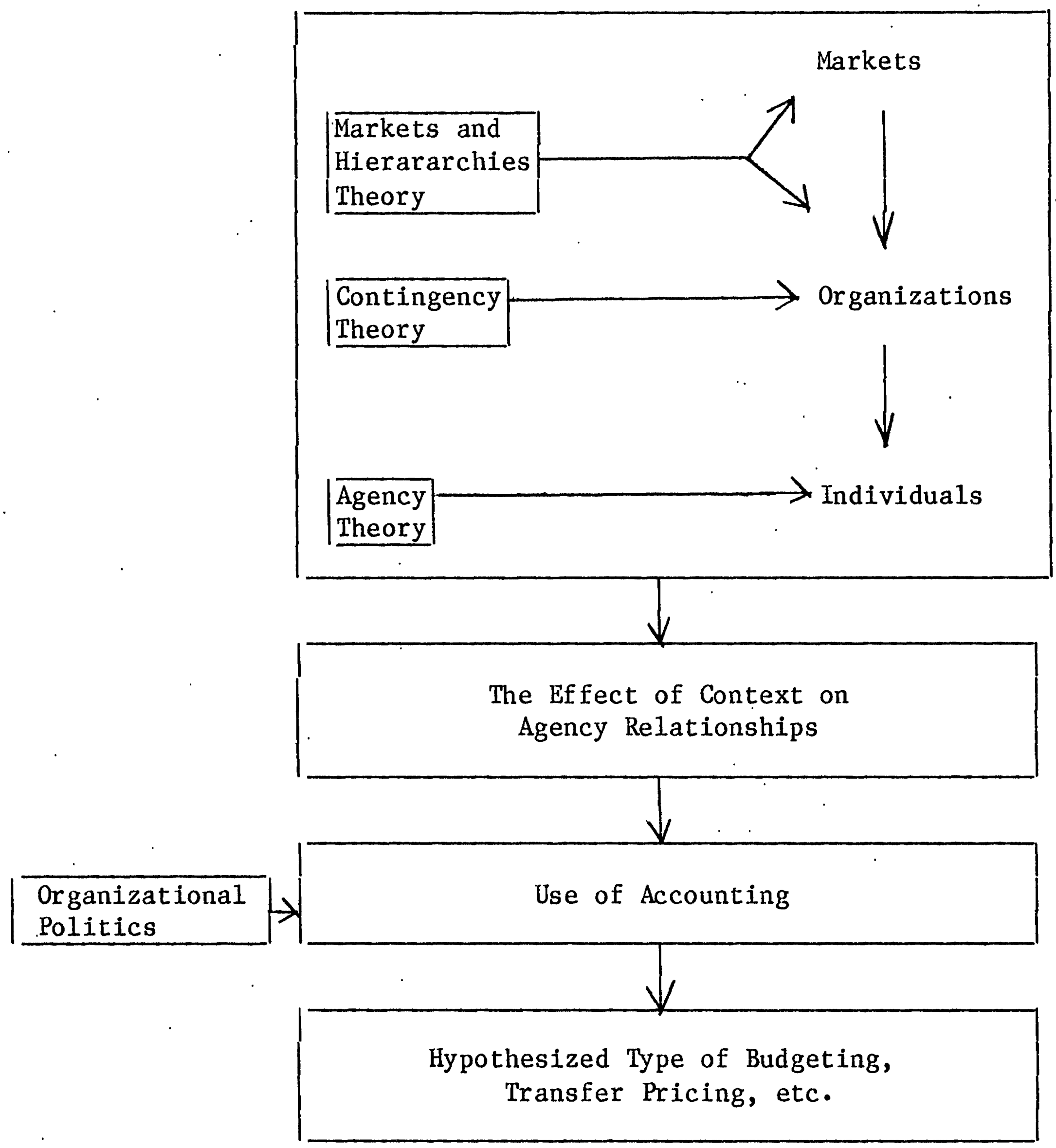
[13]For example, see Spence [1975] and Leibenstein [1979]. The seminal work is generally regarded to be Coase [1937].

[14]The economics of internal organization has been too "micro" for mainstream economic theory.

or horizontally integrate, while others do not? Why do some firms rely on external labor markets while others "promote only from within"?

An Editorial

My view of this world is something like the following.



While agency theory is generally context-free managerial accounting is highly dependent on its context (e.g., nature of the firm's production function, markets, and employees). Markets and hierarchies, contingency theory, and other work in economics may provide a context for agency theory that will be useful for predicting agency relationships in particular contexts and the use of management accounting in agency relationships. (Of course, there are other uses of managerial accounting -- this is only one example.)

In my view, a positive theory of managerial accounting must rely on basic (and perhaps simple) assumptions about rational, self-interest-oriented economic behavior. Presumably, managers demand and supply accounting methods on the basis of cost/benefit considerations and their own self-interest.[15] Our task is to predict self-interest and cost/benefit considerations in particular economic (and political) contexts, and to predict their influence on the supply and demand of managerial accounting.

It is not hard to understand why there has been so little positive research in managerial accounting. Obtaining data is costly and, given the economics of research, will probably limit empirical research in managerial accounting. The lack of theory and testable hypotheses limits our opportunities for systematic empirical work. Of course, much can be learned from purely descriptive studies -- inductive reasoning can play an important role in developing theory and testable hypotheses. But I believe the next major breakthroughs will come from empirical work that follows theory.

Where will this theory come from? One option is to apply the rigor of the incentive contractng literature to the markets and hierarchies work. "Incentive contracting meets markets and hierarchies" could both expand the boundaries of the incentive contracting literature and provide insights into the deductive validity of markets and hierarchies.

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[15]For example, see Zimmerman [1979] and Maher [1981].

V. MANAGEMENT ACCOUNTING IN 1992

If in 1962 we[16] had predicted the development of management accounting over the next twenty years, how accurate would we have been? To answer that question, I surveyed the literature from 1926 through 1961, and realized that the past may not be a good predictor of the future (e.g., the hot topic in 1961 was the direct/absorption costing controversy). I also read the predictions of some previous prognosticators; hence, I am quite modest about my ability to predict the state of the art in the years 1992 or 2002.

Despite my concern about the lack of theory and the difficulty of obtaining data, my prognosis about the future of managerial accounting research is optimistic. There is no shortage of problems to explore. Some potential avenues for future research include the following categories--which are neither mutually exclusive nor exhaustive.

1. Application of management science models. Unexplored areas include (1) use of simulation and (2) problems with implementing management science models.

2. Systems. We know very little about the impact of computer-based systems on accounting. I am thinking here primarily of indirect effects; namely, the impact of computer-based systems on firms' technologies, structure, personnel, etc., which in turn will affect managerial accounting.

3. Modeling: Agency theory and the informatin economics literature. The effects of simultaneously expanding to multiperiods and multiple agents remain largely unknown, as does the effect of application to richer institutional contexts. Thus far, there has been little exploitation of our comparative advantage in developing richer institutional contexts for these problems.

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[16]The "we" is editorial.

4. Human information processing. While there has been very little work on management accounting topics to date,[17] there is no shortage of problems to explore. Virtually all planning, control, and decision-making topics could benefit from additional work.
5. Economics of internal organization. This area is almost totally undeveloped; yet, coupled with work in agency theory, it holds promise for the development of positive theory and testable hypotheses about the relationship between the properties of firms and the properties of accounting methods.
6. Organizational and political considerations obviously have an important impact on the development of management accounting. Yet we know only a little about organizational impacts, and I have seen little imported from political theorists to help us understand the impact of organizational politics on accounting. My bias is to rely first on economic theory to develop a comprehensive theory of managerial accounting, then to use organizational and political theory to enrich our understanding of how accounting develops in particular firms.
7. Empirical work in managerial accounting. The paucity of empirical work over the past twenty years should make me skeptical about its prospects over the next twenty years. While the cost of analyzing data may have decreased, the cost of collecting data has not; hence, the economics of research casts doubt on its future. Further, in the absence of theory and testable

[17]Exceptions include Harrell [1977] and Magee and Dickhaut [1978].

hypotheses, the research has a descriptive character that some find unappealing. I am moderately optimistic about imminent breakthroughs in theory, however. Empirical work -- even case studies of problems implementing normative models -- has the potential for considerable social payoff. It remains to be seen whether the private payoff is sufficient to generate the research.

8. Textbooks. As noted earlier, one criterion for evaluating the impact of research is to evaluate its impact on textbooks. An optimistic forecast is that textbooks will (1) more thoroughly integrate recent advances in modeling and (2) emphasize the relationships between the properties of firms and the nature of managerial accounting systems. If positive theory is developed, management accounting will become more institutional in characterizing management demand for accounting data. The effect on textbooks could be as dramatic as the change in focus in recent decades from True Costs to Costly Truth.

CONCLUSIONS

We can observe a number of points of inflection in the managerial accounting literature since 1926. Some important ones include the search for principles of cost and managerial accounting in the early 1950s; the application of management science models to managerial accounting in the 1960s; and the development of information economics and agency theory in the accounting literature in the late 1960s and early 1970s. Perhaps the most important change was to a user orientation in the 1960s.

Another inflection point seems apparent today. One direction is toward more empirical research in managerial accounting which could have numerous payoffs:

(1) A better understanding of data problems involved in applying management science models;

(2) Improvement in normative management accounting "theory";

(3) Development of a positive theory of managerial accounting;

(4) A better understanding of the real problems faced by managers.

In conclusion, I hope the reader does not consider this another "call for research" similar to those we all have heard from various quarters. Such "calls," of course, usually include the strong recommendation that we researchers deal with a particular problem in the "interest of society" or "the professions." Calls for research are more credible when they influence the economics of research. Unfortunately, my influence on the economics of research is not sufficient to increase theory development and empirical research in managerial accounting. However, I hope these remarks convey both a sense of the many potential avenues of research in managerial accounting and a spirit of optimism that some of these avenues will be pursued.

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