



Conjectures regarding empirical managerial accounting research[☆]

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Received 23 October 2000; received in revised form 13 February 2001

Abstract

The empirical managerial accounting literature has failed to produce a substantive cumulative body of knowledge. This literature has not matured beyond describing practice to developing and testing theories explaining observed practice, like other areas of accounting research. While the lack of publicly available data is a popular reason for this literature's underdeveloped state, it is not the only one. Other conjectures include: its inductive approach, researchers' incentives, its use of non-economics-based frameworks, the lack of empirically testable theories, and its emphasis on decision making, not control. © 2001 Elsevier Science B.V. All rights reserved.

JEL classification: M41

Keywords: Empirical management accounting

1. Introduction

Ittner and Larcker (2001) review the empirical research in managerial accounting. They cast their net widely, beyond the mainstream accounting

[☆]The John M. Olin Foundation and the Bradley Policy Research Center at the University of Rochester provided financial support. Comments from Liz Demers, Joel Demski, Kathy Jones, Scott Keating, S.P. Kothari, Dave Larcker, Andrew Leone, Cheryl McWatters, Ross Watts, and Joanna Wu are gratefully acknowledged.

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journals and discuss papers in practitioner-oriented journals and non-North American journals.¹ In fact most of the papers they cite are in these non-mainstream journals. In addition to suggesting a variety of research opportunities and discussing important methodology issues, Ittner and Larcker (IL) offer several general observations regarding the empirical managerial accounting literature.

(T)he research is driven by changes in practice. ... (M)any papers are motivated purely by the fact that a certain topic has received considerable attention in the business press, with little effort to place the practice or study within some broader theoretical context.

(W)e are left with an underdeveloped body of research that fails to build on prior studies to increase our understanding of the topic, leaves many important research topics unexplored, and lacks the critical mass of related studies needed to reconcile conflicting results to reach consensus on the performance benefits from various manufacturing performance measurement practices.

I agree with these generalizations. After reading their review of this literature, I am left wondering what we have learnt. What generalizations can be drawn? What null hypotheses have been rejected? What burning, unanswered questions remain? Where are the intriguing anomalies? Or, in the parlance of an old fast food restaurant ad for hamburgers, “Where’s the beef?”

The failure to produce a substantive body of knowledge is not IL’s fault. The authors have faithfully discharged their responsibilities to survey the literature. The failure lies with the literature itself. My comments focus on trying to understand the current state of affairs in empirical managerial accounting research. Why do so few generalizable findings exist? Why are so many of the studies cited by IL published outside the mainstream, North American accounting journals?

The empirical managerial literature focuses on describing current accounting practice. Most other accounting research areas also started descriptively, but as empirical findings accumulated, theories were developed to explain what was observed and to predict phenomena yet to be observed. The empirical managerial literature has failed to take this next step. Why? Hopefully, by better understanding the reasons for this literature’s lack of progress, we will avoid making the same mistakes in the future.

The next section discusses the framework IL use to organize the empirical managerial literature. Section 3 compares the references cited in the IL survey to the other survey papers in this volume. The next two sections describe the

¹ They exclude most behavioral research, experimental and compensation studies, and qualitative case studies.

general function of research, and offer some conjectures as to why so little has been learnt from empirical managerial studies. The last section provides some conclusions.

2. Ittner–Larcker’s organizing framework

IL organize their empirical managerial accounting literature survey using the Value Based Management (VBM) framework. This framework, distilled from the consulting practices of several firms (notably McKinsey and KPMG), consists of the following steps:

- choose internal objectives that enhance shareholder value;
- select strategies and organizational designs to achieve the objectives;
- identify performance variables that create value;
- develop action plans;
- evaluate the success of action plans;
- assess and modify the internal objectives, strategies, plans, and control systems.

Copeland et al. (1996) of McKinsey basically present this framework as a normative approach. All firms should follow it. It does not make predictions about when particular compensation schemes will be used or what firms are most likely to adopt ABC. While it resembles various theories, it is not a positive theory in the sense that it neither explains nor predicts firm-related phenomena. It seems better suited for organizing a consulting engagement.

IL could have chosen a theory-based framework, such as the principal-agent paradigm incorporating the Milgrom and Roberts (1995) complementarities approach. It is well understood among academics that decision right assignments, performance measures, compensation plans, and other policies, are jointly determined, interdependent, and endogenous.² IL recognize these limitations of the VBM framework, and discuss the methodological issues raised (endogeneity and simultaneity). The primarily normative VBM framework portends the nature of the literature, its focus, and I believe its ultimate success.

Most of the studies reviewed by IL address practice (ABC, EVA, balanced scorecard). Many of IL’s recommended research topics focus on practice. “Researchers can make a significant contribution by providing evidence on the methods used to set financial and non-financial targets and the performance implications from these choices.” Empirical managerial accounting research has not evolved much beyond description of practice to developing and testing theories suggested from practice. In Section 4, I discuss the general nature of

²See Brickley et al. (2001) for a review.

how knowledge accumulates from theory-based research. I argue that little has been learnt from this literature, partly because researchers' incentives have shifted towards describing practice rather than developing and testing theories.

Describing practice, per se, is not unproductive. If the objective of research is producing empirically verifiable theories, a rich description of practice often leads to new theories. For example, finance researchers first documented the random walk of security prices, which lead to the efficient markets hypothesis (see Fama, 1965). However, the risk is that descriptive research can lead to—and even be motivated by—normative consulting engagements, and not to theory development and testing. Descriptive research alone will not build a coherent literature and understanding of managerial accounting practices.

3. Empirical analysis of the empirical managerial accounting literature

In this section I compare the references cited by IL to those cited by the other eight surveys in this volume. Each reference in nine survey papers is categorized into one of eight categories listed in Table 1.³ I present the following evidence cautiously. No attempt is made to eliminate references that did not summarize research but rather provided background. Drawing inferences from this data assumes that the citation frequencies in the survey papers are unbiased estimates of the citation frequencies in the literature. Clearly, the citation rates in the various survey papers depend on the scope and objective of each survey and the nature of the topic. Given these caveats, the evidence should be viewed as suggestive.

Table 2 lists the distribution of references cited by IL, the distribution of all the references in the other eight surveys, and the individual distributions of the other eight surveys. The IL survey cites far fewer mainstream North American accounting journals, but cites more non-mainstream accounting and practitioner journals than the other eight survey papers.⁴ Only 23% of the references in IL are to mainstream North American accounting journals, whereas in the other eight studies 51% of the citations are in these journals. This is due in part to IL's objective of casting their net widely. It also results from few published papers to review in the North American journals.

Equally as dramatic as the preceding citation rates is IL's low reference frequency to economics, finance, and statistics literatures compared to the other eight surveys (9% versus 24%). These citation rates are consistent with a literature that draws less on economics and finance than other areas of

³The references are counted in the version of each paper presented at the conference in April 2000.

⁴IL's citation distribution is statistically significantly different from the combined distribution and the eight individual distributions at the 0.05 level based on chi-square tests.

Table 1
Eight categories used to classify references cited in the nine surveys in this volume

Category	
Mainstream North American accounting journals	Accounting Review Contemporary Accounting Research Journal of Accounting and Economics Journal of Accounting Research Review of Accounting Studies
Other accounting journals ^a	Accounting Horizons Academy of Management Accounting Accounting, Organizations, and Society Journal of Accounting, Auditing and Finance Journal of Accounting and Public Policy Journal of Accounting Literature Journal of Business, Finance, and Accounting Journal of Management Accounting Research Management Accounting Research
Working papers	—
Books and monographs	—
Economics, finance, and statistics journals ^a	American Economic Review Bell/Rand Journal of Economics Econometrica Journal of the American Statistical Association Journal of Economic Literature Journal of Finance Journal of Financial and Economics Journal of Political Economy Journal of Public Economics Quarterly Journal of Economics
Practitioner-oriented journals ^a	Accounting and consulting firm publications AICPA and FASB publications Financial Analysts Journal Harvard Business Review Institute of Management Accountants Journal of Applied Corporate Finance
Management and strategy journals ^a	Academy of Management Journal Journal of Business Strategy Strategic Management Journal
Tax journals ^a	Journal of the American Taxation Association National Tax Journal

^a Representative journals, but not an exhaustive set.

accounting research. Fewer citations to economics and finance is consistent with a literature oriented to describing practice, not testing theories (which often are based on economics or finance). Only 3% of IL's citations are to

Table 2
Distribution of references of papers published in this volume

	Ittner and Larcker	Other 8 papers	Bushman and Smith	Fields, Lys and Vincent	Healy and Palepu	Holthausen and Watts	Kothari	Lambert	Shackelford and Shevlin	Verrecchia
Mainstream North American accounting journals	23%	51%	33%	87%	67%	58%	45%	48%	44%	79%
Other accounting journals	30%	4%	3%	4%	7%	13%	4%	4%	1%	8%
Working papers	8%	11%	16%	2%	10%	17%	11%	9%	15%	0%
Books and monographs	13%	5%	5%	1%	1%	4%	7%	9%	4%	0%
Economics, finance and statistics journals	9%	24%	40%	3%	13%	4%	29%	31%	14%	13%
Practitioner-oriented journals	13%	2%	2%	2%	2%	4%	3%	0%	1%	0%
Management and strategy journals	3%	0%	1%	0%	0%	0%	0%	0%	0%	0%
Tax journals	0%	3%	0%	0%	0%	0%	0%	0%	20%	0%
Total references	194	1364	221	141	84	144	551	111	194	62

management and strategy journals, which suggests that the papers they review are not testing behavioral science theories either.

Similar inferences regarding the empirical managerial literature are obtained when IL's citation rates are compared to each of the individual eight surveys' citation rates. Looking at the eight individual surveys, we observe that each cites more mainstream accounting journals and finance, economics, and statistics journals and fewer professional/practitioner journals than the IL review.⁵

Upon closer examination of IL's references, 44 cited papers are in the mainstream North American accounting journals. After excluding 13 compensation studies and seven theory papers, 24 empirical managerial papers are published in these journals. Certainly, the shortage of empirical managerial papers published in mainstream North American accounting journals reflects the scarcity of data. I conjecture below that other reasons are contributing factors as well.

To summarize this section, IL cite more papers in practitioner-oriented journals and more papers outside the mainstream North American accounting journals, and rely less on economics, finance, and statistics than the other eight survey papers. These citation frequencies are consistent with the empirical managerial literature being long on describing practice (data description) and short on developing and testing hypotheses derived from economics and finance.

4. Role of theory in empirical studies

I assert that one reason that the empirical managerial literature has failed to produce a coherent body of knowledge is because the literature's objective is not to test theories. Moreover, in the few studies that do test theories, their hypotheses are often ad hoc or derived from a variety of different disciplines (contingency theory or expectancy theory). Unlike the incentive compensation studies (that began by explaining practice but now test agency-theoretic hypotheses) and capital market studies (that began by explaining practice but now test financial economics hypotheses), no unifying, economics-based theory has developed to guide empirical managerial accounting research.

Succinctly stated, a theory explains what has been observed, tests empirically the hypotheses derived from the theory, and then predicts what is yet to be

⁵Only 3% of the Fields et al. (2001) citations and 4% of the Holthausen and Watts (2001) citations are to economics, finance, and statistics. But 87% and 58% of the Fields et al. and Hothausen and Watts citations, respectively, are to mainstream North American accounting journals.

observed. As Hempel (1966) explains, knowledge accumulates through the systematic testing of hypotheses suggested by theories. Theory allows the systematic ordering of facts. Most survey papers in this volume offer an underlying theoretic framework to organize their literatures. Testing hypotheses derived from theory allows knowledge to accumulate in the sense that refuted hypotheses force revisions in the underlying theory. Theories seek to explain systematic empirical regularities and, generally, to afford a deeper and more accurate understanding of the phenomena in question. Theory broadens our knowledge and understanding by predicting and explaining phenomena that were not known when the theory was formulated. Theories suggest hypotheses that help guide scientific investigations regarding data to collect. The essential point is “without ... hypotheses, data analysis and classification is blind” (Hempel, 1966, p. 13). Early descriptive studies often start with preliminary (“strawmen”) hypotheses.⁶

Theory construction and empirical research interact. As described earlier, just as theories stimulate empirical work, rich empirical settings stimulate theory. Empirical facts and regularities cause theorists to construct explanations for what is observed. But in addition, theories make predictions about facts that have not yet been collected. Eventually, empirical anomalies cause theory revision (Kuhn, 1969).

Some might argue that without data, generating hypotheses is a useless activity. However, clever empiricists will discover interesting data sets to test important hypotheses. This is especially true today given the wide variety of machine-readable data sets available and access to internet-based information.

It is easy to overlook, but important to emphasize, how economic principles generate testable hypotheses and allow the accumulation of knowledge about accounting. Consider the LIFO/FIFO method choice. This literature is summarized in Kothari (2001), Fields et al. (2001), and Shackelford and Shevlin (2001). Given the descriptive studies of stock prices (their random walk behavior) and using principles from economics, financial economists deduced the efficient markets hypothesis. One implication of this hypothesis (and the maintained hypothesis about capital markets valuing cash flows) predicted that firms shifting to LIFO for both tax and financial reporting (because of the tax conformity rule) should have positive abnormal returns to the extent the market did not anticipate the LIFO adoption. The alternative, mechanistic hypothesis predicted negative abnormal returns—the market is functionally fixated on accounting earnings, which are now lower. The early tests were consistent with market efficiency (Sunder, 1975). Later studies refined the earlier tests using more sophisticated hypotheses as inconsistencies in earlier

⁶For example, the early executive compensation studies (e.g., Coughlan and Schmidt, 1985; Murphy 1985) sought to reject the hypothesis that pay and performance were unrelated, as was often claimed in the popular press at the time.

tests were discovered. As the research proceeded, knowledge about the efficiency of the capital markets with respect to accounting information accumulated and our theories of market efficiency evolved.

Accounting empiricists often underestimate the importance of rigorous theory in designing their studies. Weak theory development is probably the most recurring reason for the accounting journals to reject empirical papers. A paper's motivation and contribution critically depend on theory. Theory structures the study and suggests alternative hypotheses.

Theories need not be stated in terms of mathematics. The essential element is the logic of the analysis. Mathematics makes the logic more rigorous and transparent. For example, consider agency theory that examines the trade-off between incentives and risk. Mathematics has proven very useful in developing a series of rigorous principal-agent models (Lambert, 2001). However, important theories exist that were not stated in mathematical terms. Consider Fama's (1965, 1970) statement of the efficient markets hypothesis, Williamson's (1975, 1985) transaction cost economics, and the Jensen and Meckling (1976) agency theory. These are important non-mathematical theories. But more importantly, they spawned a wide variety of empirical work seeking to test the theories' hypotheses. Other examples of non-mathematical theories abound: Coase's (1937) theory of firms versus markets, Stigler's (1971) theory of regulation, the Watts and Zimmerman (1978, 1986) positive theory of accounting choice, the Scholes and Wolfson (1992) tax framework, and the Smith and Watts (1992) predictions regarding incentives and firms' investment opportunity sets. Bushman and Smith (2001) describe a non-mathematical theory of accounting and corporate governance in this volume.

5. Conjectures regarding the empirical managerial accounting literature

In this section I offer six conjectures regarding why the empirical managerial literature has failed to accumulate a systematic set of findings. They include: the lack of reliable, consistent data; the literature's atheoretical approach; changing incentives of researchers; the literature's failure to embrace economics as its underlying discipline; few empirically testable theories; and the literature's almost exclusive focus on decision making, not control.

5.1. Lack of data

The paucity of "good" data is a longstanding and popular refrain for the empirical managerial accounting literature's lack of progress. Compared to financial accounting research with its Compustat, EDGAR, CRSP, IBES, and NAARS files, empirical managerial research is definitely wanting. Probably the single biggest factor hampering empirical managerial research is the lack of

consistent data about what firms do internally. No cross-sectional data set exists about firms' budgeting systems, transfer pricing methods, standard cost systems, cost allocation schemes, and so forth. This has a number of implications:

Doctoral students gravitate away from this research area towards data-rich environments, such as capital markets, executive compensation, and tax.

Data collected from surveys suffer from well-known problems such as response and surveyor biases. These limitations require researchers to be more careful in drawing inferences from studies employing survey methods.

Data collected from companies to which researchers happen to have access are likely to be a non-random sample of firms. For example, firms having problems may be more willing to allow researchers access than successful firms concerned about potential competitors gaining access to their proprietary data.

To the extent researchers gain access to proprietary data sets their studies are not replicable. However, useful insights can be gleaned from such data sets.

Clearly, "better" data is always preferred to "poorer" data. But it is hard to lay all the blame for the empirical managerial accounting literature's lack of progress on this one reason. Economics has tackled very interesting, non-traditional questions and made considerable progress on them lacking machine-readable, standardized data. Lazear (2000) describes numerous examples where economists have successfully attacked non-traditional problems (discrimination, the family, theory of the firm, and education). Many of these studies rely on the creative use of ad hoc data sets. For example, Wolfson's (1985) oil and gas paper illustrates the insightful combination of interesting theory and unique data. Masten and Crocker (1985) and Allen and Lueck (1992) test incentive contracting hypotheses using natural gas contracts collected by a US government agency, and landowner–farmer contracts from a 1986 Nebraska and South Dakota leasing survey, respectively. Given the accomplishments of economists, I find it difficult to attribute our slow progress in empirical managerial accounting to the scarcity of machine-readable data sets.

Unfortunately, the "poor data" mantra has led to various dysfunctional outcomes. "Poor data" is often used to justify weak (or no) theory and/or badly designed and implemented research methods. Some researchers mistakenly believe that all journal editors and referees impose the same empirical standards on managerial studies as they do on large-scale financial accounting studies. This belief is used to avoid these journals and the high academic standards they impose. However, 24 empirical managerial

accounting studies have been published in mainstream North American journals, thus refuting the claim that these journals reject all such papers.

5.2. *Atheoretical approach*

In 1986, the Harvard Business School held a colloquium on field studies in managerial accounting to encourage “authors to understand and document the management accounting practices of actual organizations. ... A second, and even more important, objective of the colloquium was to begin the process by which field research methods in management accounting could be established as a legitimate method of inquiry” (Bruns and Kaplan, 1987, p. 2–3). Hopwood (1983), Kaplan (1983, 1984, 1986), and others encouraged researchers to conduct more field-based studies documenting contemporary practices. Kaplan (1986) describes a research process that first focuses on case studies and field studies, and then eventually develops models and theories. While Kaplan (1986) points out that theory is useful in guiding empirical research, his prescription for managerial accounting research called for observation and description. Accounting researchers should be “in the field attempting to understand how accounting information is developed or used in actual organizations” (p. 429).

Researchers will need to leave their offices and study the practices of innovating organizations. ... The challenge for academic researchers is to discover the Pierre du Ponts, Donaldson Browns, Alfred Sloans, and Frederick Taylors of the 1980s; to describe and document the innovative practices that seem to work for successful companies. The research will be more inductive than deductive, but likely productive both for the individual researcher and for the management accounting discipline (Kaplan, 1984, p. 415).⁷

Notice that Kaplan is not calling for researchers to go into the field and test hypotheses from theories. He asserts that, unlike other social sciences, managerial accounting has not “accumulated a reliable and systematic body of factual knowledge” (p. 432) and therefore, it is premature to develop theories or test propositions.

⁷Peters and Waterman (1982) adopted the same approach in their popular book, *In Search of Excellence*. They studied the management practices of 62 large, successful US firms. Many of these firms have failed to continue their previous performance trends (e.g., Atari, Eastman Kodak, Wang Labs, Proctor & Gamble, Levi-Strauss, and Xerox). Few of the eight basic principles of Peters and Waterman (bias for action, staying close to the customer, autonomy and entrepreneurship, productivity through people, hands-on, value driven executives, stick to the knitting, simple form, lean staff, dedication to central values) have provided the hoped for panacea predicted by the authors. The problem is a lack of theory. The eight prescriptions of Peters and Waterman suggest a “one-size-fits-all” approach. All eight must be used.

Over 15 years have elapsed since the first call for more descriptive field-based research. Such a body of studies now exists. But it has not led to the theory building and testing that was envisioned. Perhaps it is too early, not enough field studies have accumulated, or the ones conducted are of low quality. These are certainly plausible justifications. However, other accounting research areas did not require 15 years between the initial descriptive research and eventual theory building and testing. Alternatively, perhaps the appeal for primarily inductive, descriptive research has not proven as productive a path as originally claimed.

Not every empirical paper must test hypotheses. Purely descriptive studies that inform us about heretofore-unknown facts are useful. However, it appears not to have been fruitful for researchers to wander the hallways of corporations and manufacturing plants searching for facts unguided by tentative hypotheses. As Hempel (1966, p. 13) states,

(T)he maxim that data should be gathered without guidance by antecedent hypotheses about the connections among the facts under study is self-defeating, and it is certainly not followed in scientific inquiry. On the contrary, tentative hypotheses are needed to give direction to a scientific investigation.

5.3. *Changing research incentives*

Perhaps the empirical managerial accounting literature has failed to evolve from describing practice to developing and testing theories because researchers no longer have these incentives. Maybe researchers face stronger incentives to describe practice than to develop and test theories. If business schools are encouraging faculty to conduct more “practical” and less “theoretical” research, then faculty incentives have changed. Descriptive research usually generates more citations in the popular press and thereby improves the school’s reputation in the business community than more theoretical research. The 2000 *Business Week* business school rankings now include a measure of each school’s “intellectual capital”. Faculty citations in *The Wall Street Journal* and *Business Week*, along with citations in scholarly journals are used to assess intellectual capital. Faculty consulting also enhances the school’s presence in the business community. All too often business students tend to value faculty consulting activities over research, especially “theoretical” research to the extent that a school’s ranking in the popular press depends on student and the business community’s perceptions, schools have incentives to reward faculty for descriptive research.

The audience of our research papers is no longer just others in the academy as it was 30 years ago. Now, we seem to be conducting our research because it informs practitioners (Demski and Zimmerman, 2000). For example, Maher (2000, p. 341) states, “The motivation for some empirical research in

management accounting has been to test the claims of consultants who propose ‘new’ management methods”. If this conjecture is true, then other accounting research areas should also be witnessing a similar movement from developing and testing theories to practitioner-oriented studies.⁸

5.4. *Non-economics-based frameworks*

Lazear (2000) argues that “economics is the premier social science”, citing its expanding scope of inquiry beyond consumers, firms, and markets into explaining other social interactions and its adoption by other disciplines (finance, accounting, law, political science, and sociology). “Economics has been successful because, above all, economics is a science.” The other accounting areas surveyed in this volume (accounting choice, agency theory, capital markets, corporate governance, disclosure, tax, and value relevance) almost exclusively rely on economics-based theory.

Other social sciences, such as cognitive psychology, could provide the necessary basic framework to develop accounting theories. However, the empirical evidence from the last 40 years indicates that with few exceptions, most accounting research innovations have their conceptual roots in economics.⁹ Either economics is more powerful or alternatively, the critical mass of accountants well trained in other social sciences is too small to produce a cumulative literature in accounting. (Creating knowledge requires large economies of scale involving skilled researchers who employ an underlying framework that uses a common language.)

To the extent, empirical managerial studies test hypotheses, they often employ non-economics-based theories (expectancy theory and contingency theory). If economics-based hypotheses are more productive in furthering knowledge than other social sciences (as suggested by Lazear), then another factor retarding empirical managerial accounting’s lack of progress is its reliance on non-economics-based theories.

⁸Casual observations of the value relevance and valuation literatures are consistent with this conjecture. See Holthausen and Watts (2001) and Kothari (2001).

⁹Consider the following partial list: the role of accounting disclosures in capital markets relies on the efficient markets hypothesis and capital asset pricing model; many of the topics taught in managerial accounting such as fixed versus variable costs, transfer pricing, and cost allocations are based on microeconomics; information economics spawned theories of accounting disclosures, the audit risk model, and revealed flaws in the controllability principle; the early normative debates regarding the theory of income measurement often relied on economic income; agency theory has generated models of contracting and stimulated compensation research; experimental markets research has generated similar studies in accounting; economic theories of the firm and corporate governance have stimulated accounting choice and earnings management studies; and much of the accounting-based tax research follows from the economics paradigm of Scholes and Wolfson (1992).

5.5. *Few empirically testable theories*

While managerial accounting empiricists have been lax in developing and testing hypotheses derived from rigorous theories, managerial accounting theorists share some of the blame. Rigorous managerial accounting theory papers too infrequently take the next step and tease out the theory's empirical implications (testable hypotheses). Rarely do we observe sub-sections in theory papers titled, "Empirical Predictions". To some, mathematical elegance (and certainly tractability) seems preferred over relevance. An implicit assumption apparently exists that theorists should develop models and empiricists should take these models and deduce the empirical implications from the theory. More high quality empirical managerial research would be forthcoming if theorists made a greater effort to generate models with testable predictions and to discuss their models' empirical implications.

5.6. *Emphasis on decision making, not control*

It is well understood that accounting systems serve both decision making and control roles (Zimmerman, 2000). However, much of the empirical managerial research and the practice literature on which it is based emphasize the decision making/planning function to the near exclusion of control. For example, total quality management, re-engineering, activity-based costing, the theory of constraints, value chain management, just-in-time, and the balanced scorecard all assume that agents will enthusiastically adopt the new approach because it promises to maximize firm value. The maintained assumption of ABC is that if you provide managers with supposedly more accurate product costs they will embrace them. This "Field of Dreams" (if you build it, they will come) approach ignores employee self-interest. In particular, adopting ABC creates windfall gains and losses among internal managers because product costs are part of most firms' internal control systems.

Except for the recent interest in economic value-added metrics, most management fads have shunned new techniques that better align shareholder and employee interests. It has become popular among practicing management accountants to assert that their role includes both planning (improve decision making) and control (reduce agency conflicts). They wish to become an equal member of the decision-making team (Siegel and Sorensen, 1999, p. 5).

Note the difficulty in empirically assessing the relative importance of decision making or control for a given firm's accounting system. In equilibrium, firms' control systems should not be binding, and hence it would appear that accounting systems are not being used for control.

I am unsure as to why some academic and practicing accountants seem to favor accounting's decision-making role in favor of its control role. Maher (2000) and Siegel and Sorensen (1999) argue that the term "accountant"

appears to have an increasingly negative connotation among students and practitioners. In 1999, the Institute of Management Accountants changed the name of its monthly magazine from *Management Accounting* to *Strategic Finance*. Accountants are viewed as passive bystanders or scorekeepers while others “play the game”.¹⁰ Perhaps decision-making-type research is more popular to practitioners, and hence accounting researchers have more incentives now to conduct such studies. As much as practitioners and academics would like to believe, firms’ internal accounting systems are used primarily for decision making, wishing it so does not make it happen. If researchers enter field sites thinking the accounting system is being used for decision making when in fact it is being used for control, then an incorrect implicit theory is guiding their data collection and analysis. Little wonder that the empirical findings from a misguided theory produces scant results. Holthausen and Watts (2001) draw a similar conclusion about value relevance research in financial accounting.

6. Conclusions

IL observe that the empirical managerial literature has failed to develop a body of knowledge that builds on prior studies and has left many important questions unanswered. The literature has failed to move from describing practice to developing and testing theories, as have other accounting areas. Certainly the lack of progress is partially attributable to the difficulties in securing “good” data. However, other fields (notably economics) have overcome data limitations. A Compustat-like data set for management accounting is unlikely to be produced. Nonetheless, individual researchers can become more innovative in discovering interesting data sets.

Progress requires better collaboration between managerial accounting empiricists and theorists. Theorists should seek to develop models that yield refutable implications. And empiricists must stop using the “bad data” apology to excuse papers that either do not test hypotheses or test poorly formulated hypotheses. Managerial accounting researchers likely are best served by relying on economics-based hypotheses. Finally, accounting researchers should not ignore why accounting is what it is. Management accountants used to be called “controllers”. While some may find the control

¹⁰ Recognizing that accounting’s primary role is control, might be unpopular to some. “Control” raises the specter of agency problems, self-interest, and hence “greed”. Some people find it unseemly to view society as avaricious. Viewing management accounting as part of a firm’s control system makes accountants into cops and places them outside of the decision-making team. A similar tendency to ignore accounting’s control function exists in capital markets research that focuses on descriptive research with little theory building (value relevance and valuation). See Holthausen and Watts (2001) and Kothari (2001) for surveys of these literatures.

function of accounting pejorative, ignoring this likely important function leads to incorrect theories being applied and ultimately to studies that do not enhance our stock of knowledge.

To the extent that incentives within business schools have shifted towards more consulting-like, practice-oriented research, then less theory development and testing papers will be written. In the long run, our stock of knowledge, not only in empirical managerial accounting research, but also in all areas of accounting inquiry, will suffer.

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