INVITED EDITORIAL

Research Opportunities in Management Accounting

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Ten years ago, an article in The Accounting Review [Kaplan, 1983] identified some major changes in the way companies were organizing the production and delivery of their goods and services. The paper suggested that programs such as total quality management, just-in-time production, and computer-based manufacturing processes would provide exciting new opportunities for research in management accounting. The paper also encouraged new research to develop more representative and accurate costing systems that could replace the direct-labor-oriented cost accounting systems that many manufacturing companies were continuing to use even as direct labor costs had shrunk to less than 10 percent of total costs. The paper suggested, however, that the new research would likely have to be done in the field, with innovating organizations.

Without implying a causal connection, in the years since the publication of that paper, many articles have been published on performance measurements for organizations involved in total quality, time-based management, and automated manufacturing. Activity-based costing systems were soon identified in several companies and, during the past six years, large numbers of case studies, field research, and conceptual research has been published on activity-based cost systems. As an indicator of the demand and supply of research on management accounting systems, an entirely new journal (The Journal of Cost Management for the Manufacturing Industry) was established to serve as an outlet for academic and practitioner articles on cost management and performance measurement systems.

In fact, at a time when many business school disciplines, and even areas within accounting, are having the relevance of their research and teaching questioned by user groups, the management accounting discipline can take some pride in the active interaction among its research, education, and practice components (see [Sprouse, 1989; Dyckman, 1989; Kaplan, 1989; Cooper and Zeff, 1992]). Several management accounting faculty are actively involved with practitioners helping to document and develop new ideas that can be implemented; our undergraduate and graduate students are now being taught material, developed both from research and practice, that is highly relevant to the new and changing environment where management accounting is practiced; and practice innovations are

This is a personal essay that has benefited from comments provided by Anthony Atkinson, Chee Chow, Marc Epstein, Mike Shields, and, especially, Chris Argyris.
being studied by academic researchers and incorporated into our teaching programs. While management accounting academics can take some satisfaction from these developments, we certainly cannot be complacent. To maintain the momentum that has been established during the past ten years will require a much broader contribution from the academic research community. And as rapid changes continue to occur in our practice field, we must be constantly modifying and upgrading our curriculum.

In this essay, I will offer some personal views on what I consider to be the more and less promising opportunities for research in management accounting. I will illustrate my views by emphasizing the areas in which I have had direct involvement, such as activity-based costing and performance measurement, and spend less time, except by brief references, to areas in which I have had less direct experience, such as research in management control systems or incentive compatibility. Also, I will comment on research methods that I have personally used at some time during the past couple of decades, but will not discuss research methods, such as surveys and controlled experiments, that I have not used. Thus, this essay contains personal observations on the research I consider most promising for management accounting, but is not intended as a comprehensive or objective review of all contemporary research in management accounting.

**Limitations of Statistical Analysis to Test Emerging Theories**

Several management accounting faculty are currently applying traditional research methods, analytic modeling and statistical analysis, to contemporary management accounting issues. I believe, however, that these research methods are probably not the most promising for exploring these issues. I will illustrate my views with the particular research that has been done on activity-based cost (ABC) systems, though the comments could be extended to other topics as well. The statistical research on ABC has attempted to validate whether the assumptions underlying ABC systems are valid in practice; that is, the research attempts to test whether, after controlling for volume effects, indirect or overhead costs are correlated with complexity and variety factors [see, for example, (Foster and Gupta, 1990a)]. So far the results have been mixed. With recent advances in the theory of activity-based costing [Cooper and Kaplan, 1992], we now know that standard correlational or regression analysis of overhead costs against variety and complexity factors provide mis-specified tests of ABC theory. Activity-based cost systems measure the costs of resources used for organizational activities, while expenses recorded either in budgets or in actual costs represent the costs of resources supplied to perform activities. Especially for indirect expenses, large differences can exist each period between the costs of resources supplied and the costs of resources used; these differences represent the costs of unused capacity for the resources supplied. Therefore, the empirical research has really tested a much more complex hypothesis. By attempting to correlate period expenses with measures of the variety and complexity of operations within the period, the alternative to the null hypothesis in this research is that:

(a) the costs of resources used to handle variety and complexity, after controlling for volume effects, is greater than zero; and,
(b) managers adjust, each period, the supply of resources to the variety
and complexity demands actually experienced that period.

A failure to reject the null hypothesis can occur either because the ABC
assumption, as stated in (a), is untrue, or (more likely, I believe) because
assumption (b) is violated since managers do not adjust the supply of re-
sources each period to actual demands. If the implications from ABC theory
are not already imbedded in practice, then it becomes quite difficult to test
the theory by examining existing practice. As a specific example, suppose
the managers in an organization do not understand how variety and com-
plexity affects the demands for support resources. Over the years, they
have developed rules of thumb for budgeting the supply of support re-
sources based on anticipated production and sales volumes. In this case,
researchers working from readily available data will not find strong corre-
lations between costs incurred and variety and complexity factors.

The empirical analysis is not useless; it's just difficult to do with any
credibility. Occasionally, however, when the research design is carefully
done (see, for example, [Banker and Johnston, 1993; Banker et al., 1992]),
the impact of the variety and complexity factors can shine through even
the estimation of a mis-specified model. Researchers should try to guide
their empirical investigations by the theory being tested so that they can
develop more powerful testing procedures. For example, they can choose
organizations where the costs of unused capacity are not likely to be high,
because of high growth situations. It's not clear to me, however, that the
academic research community should continue to devote significant re-
sources to using regression and correlational analysis to test whether ABC
effects occur in practice. Regression and correlational analysis, no matter
how much we like to use them in other contexts and despite the consider-
able skills we have accumulated in using and interpreting their results,
are likely not the appropriate research methods for tests of ABC theory, as
articulated in, say [Cooper, 1990; Cooper and Kaplan, 1992], because of
the difficulty of measuring unused capacity.

Others have claimed that ABC assumptions are not even testable. They
recoil when advocates claim that ABC systems are more accurate than
traditional cost systems. Having been indoctrinated with the view that true
costs are unobservable, the skeptics view the ABC efforts as a futile at-
tempt to get closer to "true costs." If "true costs" are indeed unobservable,
the critics believe that ABC claims to higher accuracy can never be vali-
dated or rejected. This skeptical view, however, is incorrect both about the
testability of ABC assumptions as well as how tests of scientific theories
are performed. ABC is a testable theory. We can establish "if-then" predic-
tive statements from ABC assumptions which if not confirmed will create
strong evidence against the theory. But regression or correlational studies
are unlikely to be powerful methods for attempting to disconfirm the pre-
dictions from the theory.

What would be an example of a more powerful research approach?
Instead of using readily available data, the researcher can look more closely
at internal company events and data so that the unused and even some
overused capacity for individual resources and activities can be identified
and measured. Take, for example, an organization, like a focused plant of
a pharmaceutical company, that historically has produced only a single
product in high volumes. Because of anticipated reductions in demand for the single product, or because of a desire to offer more variety to customers, managers of the plant may decide to shift the product mix to offer hundreds of specialized products. But suppose that the volume of the plant is identical before and after the product mix shift. Traditional (non-ABC) cost systems are of two types: (1) fully absorbed cost systems that allocate overhead based on direct labor or machine hour usage; and, (2) direct cost or contribution margin systems that assume that most overhead costs are "fixed" and independent of product mix. In the pharmaceutical setting described above, both fully absorbed and direct or contribution margin cost systems produce an identical prediction: that the demands for overhead resources will be unaffected by the product mix shift. In contrast, ABC theory predicts an increase in demands for overhead resources to handle the substantial increase in production run, purchasing, shipping, and product support activities. Examination of the supply of support resources before and after the product-mix shift should reveal whether the supply of support resources had to increase to handle the demands predicted from ABC theory.

Alternatively, consider a facility that, while holding total production and sales volume constant, made a substantial reduction (according to ABC theory) in the demand for support resources by:

(i) reducing the variety of products or customers to be supported;
(ii) improving the processes that perform batch and product-sustaining activities; and/or,
(iii) improving product designs so that products require fewer and more common parts.

ABC theory predicts that unused capacity will be created in the resources supplied to handle the batch and product-sustaining activities after these changes. In contrast, traditional cost theories predict no change in the demand for these resources (since total production volume remains constant). This situation is more difficult to test, and certainly regression and correlational analysis provide weak (non-powerful) tests in such situations. If managers have acted to eliminate the unused capacities (by reducing the supply of resources), then the effects can show up through lower costs of indirect resources supplied, after the reduction in variety and complexity demands has occurred. If, however, managers have not eliminated the unused capacity that has been created (according to ABC theory), then the researcher will need to develop a research design that can detect unused capacities in particular resources when the unused capacities exist.

These examples are intended to show that ABC is a testable theory; we can construct "if-then" statements that permit disconfirmability. The challenge for the empirical researcher is to devise powerful tests of the theory, tests that are not contaminated by omitted variables. Ideally, researchers would ask companies to "run the experiment" to test the theory. In practice, such cooperation is unlikely. Still, researchers must be very careful to

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1Volume can be measured either as the number of units produced, or, if unit volumes are not a good aggregation of total volume, some combination of input factor such as total labor and machine hours.
develop research designs and research methods that are driven from the theory being tested. They should not use research methods just because they have been trained in those methods or because the methods have become "accepted" for testing other theories.

The Role for Analytic Research

A second research stream has developed to apply analytic methods to study ABC systems. Several papers I have seen presented recently (for example, [Datar and Gupta, 1992]) have the following implicit logic. The researchers stipulate that ABC systems, in principle, may provide a better correspondence between the demands (by products) for support resources and the costs of resources supplied. But, they argue that since ABC systems are both expensive and complicated to implement, cost system designers must make approximations in the number and selection of activities and activity cost drivers. The researchers then construct scenarios in which the information produced by approximate ABC systems that they have designed leads to product costs that are further, than a simple traditional system, from the "true costs" the researchers have assumed. Or, the analytic researchers construct closed-models of a managerial decision based on the revealed cost information and show that decisions based on their approximate ABC system produce lower profits than if the hypothetical decision-maker had acted based on the information produced by a simple traditional cost system.

This stream of research has been beneficial in cautioning practitioners that not every ABC system they design will benefit them. But, again, will much more of this type of analytic research add to our useful stock of knowledge? For a metaphor, suppose the Wright Brothers were describing to an academic audience of transportation specialists that they had discovered how to make a heavier-than-air vehicle fly. Some academic researchers could then construct a sufficiently poorly designed aircraft to demonstrate clearly (and graphically) that attempting to use the technological innovation will make practitioners distinctly worse off, especially compared to the traditional approach of traveling by horseback or train.

Role for Design Research versus Analysis Research

Initial attempts to implement a new technical theory, such as building a heavier-than-air vehicle that flies or an activity-based cost system that provides more accurate cost information, can easily be unsuccessful. The failures of a new vehicle or system can be even more dramatic when compared to a traditional approach with which the practitioner or implementer has a great deal more experience. We should not conclude, however, that because early attempts, especially by inexperienced practitioners, to implement an innovation are unsuccessful that the technical innovation itself is flawed or that further research to improve it is unwarranted.

Once we get beyond demonstrating with simple analytic examples that any innovation can be implemented badly, a more interesting question is how to design and implement the new technical theory without failures. Academics could engage in research that will inform system designers about the circumstances in which poorly designed ABC systems are likely to oc-
cur, the symptoms they can look for (before the crash occurs) to detect when a proposed design might be inadequate, and, most constructively, how to design cost-effective ABC systems that out-perform, by the researchers' criteria, traditional cost systems. Even the casual reader can notice that the word "design" appeared four times in the previous sentence. This is not an accident. The new research agenda for management accountants should encompass more design and less analysis. Our research should be more like engineering and less like science. We should take basic principles and apply them to the new environment in which management accounting is being practiced. We have to learn how to perform and evaluate research whose output is something new: a prototype, a management accounting system that seems to work, according to criteria we develop, in an actual setting. In this regard, our research can resemble that done in computer science and bio-technology, where creating new algorithms, programs, enzymes and drugs that meet pre-specified goals are considered significant research accomplishments.

It remains an open question about the role for analytic research to facilitate and evaluate these design activities. But it does seem clear that if analytic research is used solely to demonstrate all the ways that a plane can crash, it will play only a limited role when 747 or supersonic jets are designed.

Role for Field Research

Having discussed the apparently limited role for traditional research methods, what will be the most productive methods for providing important insights into management accounting issues? My crystal ball on high-payoff topics and research methods is not very clear today but it does suggest that a very different set of research methods than we have used in the past will likely be required.

Several articles in the mid-1980s [Kaplan, 1983; 1986; Bruns and Kaplan, 1987] advocated that management accounting researchers make much more extensive use of field research to study management accounting phenomena. With the typical academic bias to emphasize research methods over research topics and research substance, we can now observe "Field Research" sessions at the annual AAA Meeting, and "field research" representatives at doctoral consortiums. But almost at the same time as the call for more field research was being advocated, observations of contemporary practice revealed that even traditional field research methods would not produce the intended benefits. Standard field research methods can be mobilized to study the match between the demand for new management accounting procedures (a demand created by the new environment for manufacturing and service organizations) and the supply of innovative practices. Unfortunately, an initial attempt at learning about innovative management practices in companies undergoing major changes in their organization of manufacturing operations proved highly disappointing [Kaplan, 1985], a finding that was later replicated in other studies (see, for example, [Karmarkar, Lederer and Zimmerman, 1990]).

Management accounting scholars, unlike their counterparts in operations management (OM), could not easily find widespread adopters of in-
novative practices. Our OM colleagues could look to leading Japanese manufacturers in the automobile, machine tool, and electronics industries for observable examples of total quality management, just-in-time production, computer-integrated manufacturing, and design for manufacturability. The new phenomena could be studied in organizations that already had considerable experience with the new practices. But no "Japan" existed for learning about or studying innovative management accounting practices. Therefore, standard cross-sectional field research studies would largely capture traditional management accounting systems operating in environments radically different from the ones for which the systems were designed. This situation is not conducive for developing and testing theories using cross-sectional studies. Practice innovations in management accounting typically occur in only a few organizations, and typically only at a pilot site at those organizations. Thus, while management accounting innovation is occurring, the innovations are not widespread or systematic. As a consequence, research on innovative practice has to be done opportunistically, with small sample sizes, and, most likely, in longitudinal studies.

Traditional or normal social science research methods, such as empirical analysis of large data sets, analytic models of accepted and understood phenomena, and cross-sectional field research, can be effective for studying the universe as it now exists, for understanding "what-is." But these normal science methods are less helpful for management accounting research when major structural changes are occurring in organizations and in the roles performed by management accounting systems within these organizations.

This situation leaves academic management accounting researchers who are interested in actual practice with three choices for their research programs: (1) find areas of study where widespread adoption has already occurred so that normal science methods can be productively employed to study "what-is"; (2) engage in case study and longitudinal research methods to study "what's new," areas where adoption is underway; and (3) study areas where no adoption exists. In this third alternative, successful change or adoption is either so slow or unlikely that academics engage in action and intervention research to create something that did not exist before. The researcher can become part of the change process while studying what's "to-be." I will discuss each possibility in turn.

"What-is" Research

Two recent dissertations at Harvard Business School, by Ittner (1992) and Anderson (1993), provide good examples of applying standard field research methods to study "what-is."² Both of these dissertations tested theories that were articulated twenty or more years ago in a related management discipline, operations management. Ittner's work focused on the economics of quality. He investigated two widely-held, extensively-promoted, but little-documented, tenets in the cost of quality literature. The first ten-
et held that a favorable trade-off was available to companies. They could, by spending more on prevention and appraisal activities, enjoy a more than one-to-one reduction in spending on internal and external failure activities. The second tenet was that "quality was free;" the costs of achieving higher conformance quality levels was more than repaid by overall reductions in manufacturing costs after the higher quality levels were achieved. Ittner was able to test both these ideas. He collected data from a reasonably large sample of firms that had implemented total quality programs and therefore had already accumulated substantial quality cost data for internal use. He also gained access to several years of monthly data from a company that had sustained a Total Quality program.

Anderson tested theories on the costs of product and process variety, and the benefits from focused factories (see [Skinner, 1974; Wheelwright and Hayes, 1979]). She collected and analyzed data from a company that had consciously organized its production facilities according to the precepts of these theories. She was able to demonstrate empirically the productivity and cost penalties associated with increased variety.

Both Ittner and Anderson could collect internal data from organizations with enough variation among the independent variables to analyze and test theories about the dependent variables they were interested in (the cost of quality and the cost of variety, respectively). Both of these studies could be executed and evaluated against accepted standards developed for field study research. As I attempt to generalize from the success of these two efforts, I tentatively have concluded that the enabling factor was that both researchers tested theories that had been influential and in existence long enough for company practice to have changed based on the theories. Ittner and Anderson could also fill a void since the discipline (operations management) in which these theories had been developed and practiced did not itself have a strong measurement focus. This combination of circumstances produced an empirical validation and testing gap that could be filled by management accounting researchers interested in the cost implications of innovations in operations management.

Similar opportunities undoubtedly exist for management accounting researchers to identify new theories that have been adopted in practice in management disciplines such as marketing, corporate strategy, and technology management. Each of these disciplines has theories with cost, profit, and performance measurement implications that have been adopted and implemented in many organizations. These theories, however, have yet to be subject to careful empirical testing. Especially with management accounting's current capabilities to capture more accurately the cost economics of new management practices, management accounting researchers have several fertile fields of inquiry open to them. This research could be collaborative: management accounting researchers can team with colleagues from other management disciplines (e.g., operations management,

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3The research indicated that the "cost of quality" trade-off was not observable. Companies could lower failure costs even while maintaining or even reducing their expenditures on prevention and appraisal. Also, Ittner found that reductions in directly identifiable "quality costs" significantly underestimated all the benefits to manufacturing performance from improved quality.
marketing, and strategy) to subject the theories from these disciplines to empirical testing.

"What's New" Research

A second approach would have academic researchers observing and documenting the changes and innovations now underway in organizations. At present, many companies are experimenting with new performance measurement systems, activity-based cost systems, and management control systems. Researchers can associate themselves with such organizations to become intimately familiar with the circumstances of such experiments and the process of implementation and change. For example, much of the recent literature in activity-based costing has documented the implementation of this new costing system. A second set of examples are the studies done by several Stanford accounting faculty that described the early costing innovations in companies implementing just-in-time and flexible manufacturing systems during the 1980s [Patell, 1987; Foster and Horngren, 1988a, 1988b]. Additional examples of "what's new" research can be found in several studies of operational control and performance measurement systems [Cross and Lynch, 1989; Banker et al, 1993; Nanni et al, 1990; Young and Selto, 1993]. In these studies, accounting researchers, occasionally teaming with their colleagues in operations management, investigated new measurement systems that incorporated both financial and non-financial elements. Such systems could be observed in organizations that had already made extensive commitments to total quality management and business process improvement. Simons [1992] is an excellent example of research on new uses for management control systems by incoming CEOs.

Opportunities clearly exist to study the extension of newly developed measurement procedures, whether activity-based costing or performance measurements—on quality, cycle time, on-time delivery, time-to-market, and cost of non-conformance—outside the manufacturing setting in which these procedures were initially developed. The new settings could encompass service organizations such as health care, telecommunications, financial services, and transportation organizations, government agencies, and professional service firms—advertising agencies, and engineering, auditing, and management consulting firms. The new settings could also be in non-production areas of the organization, for example, corporate departments of engineering, finance, legal, environmental, human resources and research and development. The research could also explore innovative management accounting systems in different countries. Robin Cooper's current research on target costing, value engineering, and management control systems in Japan is an excellent example of the benefits from investigating measurement and control topics in different cultures.

Careful research into innovative practice could reveal what is in practitioners' heads when they act; it would describe what they believe in and the design principles that guided their action [Schön, 1983]. The research

4See the case studies and associated teaching notes in Chapters 5-7 [Cooper and Kaplan, 1991; Foster and Gupta, 1990] and the experiences described in Cooper et al [1992].
could also document the historical circumstances that led to the innovation, and the principles of learning the practitioner used [Argyris, 1991]. With this knowledge, the researcher could make a priori predictions about the types of resistances the design innovation will encounter and its likelihood of success.

To perform "what's new" research, the researcher must identify opportunistically innovating companies and then conduct in-depth observation and documentation to describe the management accounting innovation. The best of this research does not just describe practice. The researcher, using the observations as a data base, formulates theories that provide a conceptual framework to explain the successful innovations. These theories can then be tested using normal science investigative methods when widespread adoption of the innovation begins to pervade practice.

"To-be" Research

In a substantial departure from the passive, observational role for researchers that is traditional in normal social science, academic researchers in management can actually be active participants in the change process [Argyris et al., 1985; Argyris, 1993]. Such action research is required when adoption of new methods is slow or unlikely, for example when implementation requires both comprehensive, conceptual understanding of a technical innovation as well as experience with overcoming organizational resistances to change. In this circumstance, the researcher becomes like the practitioner, a part of the design and implementation process, and hence comes closer to developing not only a more complete theory of management accounting, but contributing to a more general theory of management.

The longitudinal action-oriented research can take several forms: (1) New settings; (2) Implementation; and (3) Integration. If a management accounting innovation has yet to be tried in a particular setting, the researcher can play an active role, working with managers, to extend and customize the innovation to that setting. The research into the applicability of management accounting approaches to the new settings will likely require design research: developing and evaluating new systems, attempting to identify some of the different or unique features that arose in the new settings, and being sensitive to implementation concerns.

A second stream of "to-be" research would explore the wide set of issues that arise when attempting to implement new management accounting concepts. The difficulty of creating change in organizations is a phenomenon quite familiar to both academic researchers and practitioners. A new measurement procedure or control system may fail to be accepted or used in an organization not because of technical deficiencies with the procedure or system but because of individual and organizational barriers that resist change [Argyris and Kaplan, 1993; Shields and Young, 1989].

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5Banker et al. [1990] and Datar et al. [1993] are good examples of rigorous research that extended ABC analysis of manufacturing operations to product design and engineering applications. The researchers helped to create new insights that did not exist before their intervention.
The current period of organizational upheavals and change provides a marvelous opportunity for accounting researchers to study the processes used and the barriers that must be overcome if new measurement systems are to be introduced successfully into organizations. More than most innovations, changing the way performance and activities are measured turns out to be one of the most controversial and difficult changes to introduce into organizations. Accounting researchers could find highly productive opportunities to collaborate with their organizational behavior colleagues who have expertise in management change processes to study the effective and ineffective processes used by companies to implement new cost and performance measurement systems. Such research would allow behavioral and organizational accounting researchers to study actual phenomena. The field-based, action research would be a valuable supplement to existing research methods, such as survey administration and analysis, and experiments done on students and practitioners in artificial settings, that are the modal methods in use by many behavioral/organizational accounting scholars.

The third research opportunity would explore the integration of new cost and performance measurement systems with the many other initiatives occurring today in organizations. Recent innovations in management accounting are not occurring in a vacuum. As mentioned earlier, organizations are simultaneously exploring programs such as total quality management, time-based management, business process re-engineering, employee empowerment, customer-focused service-driven organizations, design-for-manufacturability, computer-integrated manufacturing, theory of constraints, strategic alliances with suppliers and customers, core competencies, and shareholder value analysis. What is the role for new performance measures, for activity-based cost and profitability measurement, for new incentive and compensation schemes, and for newly-designed management control systems as organizations implement some or all of these initiatives? Are new measurement systems necessary? Are they enabling factors? What new barriers arise if new measurement systems do or do not change? Management accounting researchers could team with colleagues in related management disciplines to begin to develop the answers to these questions.

**Who Does What?**

At the present time, the nature and evaluation of research on "what's new" or what's "to-be" is neither as well-understood nor as well-accepted as normal social science research. This new research, therefore, is best not done by doctoral students or newly-minted assistant professors for several reasons.

First, situations still arise when normal science methods can and should be productively employed. As described earlier, normal science methods can be applied to interesting "what-is" practice, practice that already exists and about which extensive data can be obtained. Doctoral students and new assistant professors should gain expertise in use of normal science methods. If they do not gain significant skills in normal science research early in their careers, they are not likely to learn them later. Such
research also establishes their credentials should they perform more risky and unstructured research later in their careers. As one of my colleagues has stated, "We feel much better about Picasso knowing that he could draw real people when he wanted to." That is, Picasso painted people with one eye and a triangular head because he wanted them to appear that way, not because that was his best attempt at a realistic representation.

Second, the longitudinal and action research methods may require a greater maturity and knowledge of individual and organizational behavior. This knowledge, for many people, accumulates over time and is not always present during doctoral studies or early non-tenured faculty years. Also, the "what's new" and "to-be" research may take longer to execute, a relevant consideration for doctoral students and non-tenured faculty who have relatively short evaluation horizons to confront.

Younger faculty, who have obtained some mastery and experience with normal science methods, can contemplate the second type of research, studying "what's new." This research will help to move the management accounting field forward by identifying, documenting, and beginning to theorize about innovations now underway. Faculty who have established their research credentials and capabilities in "what-is" and "what's new" research, and who, ideally, are protected by academic tenure, may want to engage in the more risky and controversial research that engages them in helping to create practice that did not exist before. Research on what's "to-be" may eventually contribute to a more comprehensive theory of management accounting, in particular, and management, in general.

Summary

Respected and well-established research methods that have been employed by many management accounting researchers during the past quarter century may still have a role to play. Their role, however, must be to study those issues where the methods are appropriate and relevant. When organizations are undergoing great changes in their operations and in their measurement and management accounting systems, methods other than those used to investigate normal science problems will likely be required if the research is to produce significant insights that will inform both education and practice.

In this article, I have suggested that normal science methods are best applied to theories and practices that are already in widespread use. This may constrain such studies to ideas that have already been introduced and put into practice from other management disciplines. Research on management accounting issues themselves should involve methods appropriate for design processes. They should shed light on the creative processes required to design new cost and performance measurement systems, and new management information and control systems. Perhaps we can learn from engineering and other applied science fields about methods and evaluation criteria for such design research.

In addition, many new management accounting systems will be installed in settings quite different from the classic manufacturing factory where most of our cost accounting ideas initially were spawned. Researchers may concentrate so that they can become familiar with the particular institu-
tional contexts of designing new measurement systems appropriate for government agencies, professional service firms, or health care delivery organizations.

Other research opportunities can arise from doing collaborative research with faculty experts in other management disciplines. This research will explore implementation and integration issues associated with installing innovative measurement systems in organizations undergoing significant changes. Management accounting researchers following this route will be moving beyond their traditional technical, analytic skills to develop knowledge and expertise in organizational change as well as in contemporary developments in related management disciplines such as operations and technology management, marketing, human resource management, and strategy.

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