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COMMENTARY

Accounting Craftspeople versus Accounting Seers: Exploring the Relevance and Innovation Gaps in Academic Accounting Research

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SYNOPSIS: Is accounting research stuck in a rut of repetitiveness and irrelevancy? I would answer yes, and I would even predict that both its gap in relevancy and its gap in innovation are going to continue to get worse if the people and the attitudes that govern inquiry in the American academy remain the same. From my perspective in accounting information systems, mainstream accounting research topics have changed very little in 30 years, except for the fact that their scope now seems much more narrow and crowded. More and more people seem to be studying the same topics in financial reporting and managerial control in the same ways, over and over and over. My suggestions to get out of this rut are simple. First, the profession should allow itself to think a little bit normatively, so we can actually target practice improvement as a real goal. And second, we need to allow new scholars a wider berth in research topics and methods, so we can actually give the kind of creativity and innovation that occurs naturally with young people a chance to blossom.

INTRODUCTION

The reasonable man adapts himself to the world; the unreasonable one persists in trying to adapt the world to himself. Therefore, all progress depends on the unreasonable man.

—George Bernard Shaw (1903, Act IV)

Who provides you with the best feedback on your current set of teaching materials and research ideas? For me, at present, that ranked list would be: (1) knowledgeable and creative practitioners who are seeking to improve their field of practice, (2) young doctoral students and faculty from European or other non-American programs in business

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informatics, (3) a few of my own doctoral students from 15+ years ago, who teach and research in the same areas of accounting systems that I do, and (4) my own undergraduate and master's students. I do have systems, tax, and introductory colleagues who provide accounting context for me, but my feedback list has notable absences, like most of the mainstream Accounting and Information Systems faculty at Michigan State University (MSU) and, indeed, faculty throughout the U.S. accounting academy. Thirty years ago, those last two forums tolerated widespread diversity in both teaching and research ideas, but now those communities have coalesced into just a few approved "areas," none of which provide me with assistance on my methodological and topical problems. Academic accounting most recently has been developing more and more into an insular and myopic community with no methodological and practice-oriented outsiders tolerated. Why is this?

Becoming aware of how this narrowing of the accounting mind has hindered not just accounting systems, but also academic accounting innovation in general, American Accounting Association (AAA) president Gregory Waymire asked for some "unreasonable" (in the Shavian sense quoted above) accounting academics like me to address the low-innovation and low-relevance problem in academic accounting. I promptly reframed this charge as a question: "Is accounting research stuck in a rut of repetitiveness and irrelevancy?" In the pages that follow, I intend to explore that question from two perspectives: (1) methodological, and (2) sociological. My inspiration for the first perspective is derived from Buckminster Fuller plus Alan Newell and Herbert Simon. For the second, my role model is Lee Smolin.

PUTTING A (LIMITED) NORMATIVE MINDSET BACK INTO ACCOUNTING RESEARCH—THE CASE FOR DESIGN SCIENCE AND BEYOND[1]

We should help create the future, not just study the past.

—Paul Gray (Kock et al. 2002, 339)

In March of 2008, two very prominent and distinguished accounting academics—Michael H. Granof of The University of Texas and Stephen A. Zeff of Rice University—noted in *The Chronicle of Higher Education* that the research models that were being produced by accounting academics were indeed rigorous by the standards of statistical validity and logical positivism, but they were also of very little practical import:

> Starting in the 1960s, academic research on accounting became methodologically supercharged . . . The results however have been paradoxical . . . [as] those models have crowded out other forms of investigation. The result is that professors of accounting have contributed little to the establishment of new practices and standards, have failed to perform a needed role as watchdog of the profession, and have created a disconnect between their teaching and research. (Granof and Zeff 2008, A34)

Professors Granof and Zeff (2008, A34) went on further to note that "accounting researchers usually look backward rather than forward" and that they, unlike medical researchers, seldom play a significant role in the practicing profession. In general, the thrust of the Granof and Zeff (2008) criticism was that the normative/positive pendulum in accounting research had swung too far *toward* rear-view empiricism and *away from* creation of promising new accounting methods, models, and constructs. They appealed directly for expanding the set of acceptable research

[1] Portions of this section have been taken directly from an unpublished report written in 2009 by the author for the National Science Foundation (McCarthy 2009).

methods to include those accepted in other disciplines well respected for their scientific standing. Additionally, Granof and Zeff (2008, A34) noted that because accounting faculties "are associated with a well-defined and recognized profession . . . [they] have a special obligation to conduct research that is of interest and relevance to [that] profession," especially as the models of those practitioners evolve to fit new postindustrial environments.

Similar concerns were raised in the 1990s by the senior accounting scholar Richard Mattessich (1995, 183) in his treatise *Critique of Accounting*:

> Academic accounting—like engineering, medicine, law, and so on—is obliged to provide a range of tools for practitioners to choose from, depending on preconceived and actual needs . . . The present gap between practice and academia is bound to grow as an increasing number of academics are being absorbed in either the modeling of highly simplified (and thus unrealistic) situations or the testing of empirical hypotheses (most of which are not even of instrumental nature). Both of these tasks are legitimate academic concerns, and this book must not be misinterpreted as opposing these efforts. What must be opposed is the one-sidedness of this academic concern and, even more so, the intolerance of the positive accounting theorists toward attempts of incorporating norms (objectives) into the theoretical accounting framework.

Mattessich, Zeff, and Granof were followed most recently in the same vein by Robert Kaplan (2011), who noted in the AAA 2010 Presidential Scholar Lecture that:

- most accounting research for the past 40 years has been *reactive* in the sense that it concentrates on studying existing practice, but does not advance that practice; and
- accounting scholars have missed opportunities to apply innovations from other disciplines to important accounting issues—an especially noticeable difference when compared with researchers from other professional schools who understand gaps in practice and try to address them by applying contemporary engineering and science.

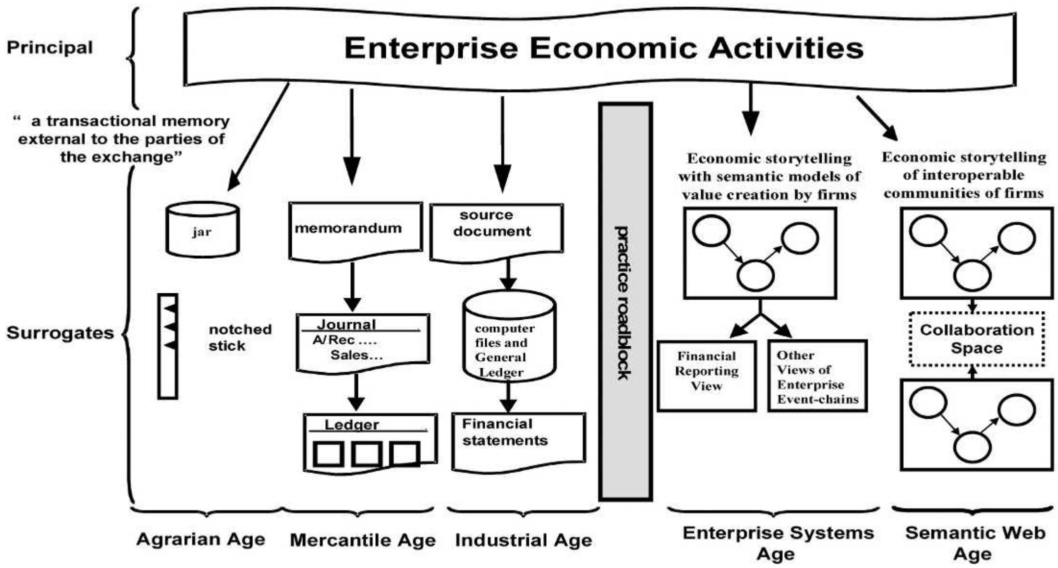
In my opinion, these weaknesses noted by Granof, Zeff, Mattessich, and Kaplan are attributable primarily to the insularity and myopia of the American-led accounting academy. Our research excludes practice and stifles innovation because of the way our journals, doctoral programs, and academic presentations are structured.

The Innovation Roadblock in Accounting Systems

The rear-view empiricism research malaise that all four of these scholars attribute to accounting as a whole is especially present in its technical subfield of accounting information systems (AIS). In fact, it is even more exaggerated, because as time goes on, an increasingly high percentage of AIS researchers aspire to develop reputations not in the field they teach (i.e., accounting systems), but in the accounting mainstream (i.e., financial reporting). Thus, they follow many of the misdirected paths described above, and their results are similarly disappointing. With some notable exceptions—primarily in work that involves semantic modeling of accounting phenomena or computerized monitoring and auditing—university-driven modernization in accounting systems has been virtually nonexistent since the 1970s, and what limited improvements that have occurred can be primarily attributed to the independent practice marketplace.

Graphically, this limited innovation can be seen in Figure 1, which is a combined adaptation from Dunn and McCarthy (1992) and David et al. (2003). According to Waymire and Basu (2008), an accounting system is a transactional memory external to the parties of the exchange, and throughout history, these systems have evolved to different stages based on available information technology in different ages. For accounting since the 1970s, though, that evolution has been

FIGURE 1
Accounting Systems Evolution



largely stymied by a practice mindset that clings to the storage and information processing technology ideas of paper-based double-entry.

Letting accounting and information technology practitioners drive innovation has led to some significant positive changes in accounting systems during the last 40 years—like their limited embodiment within the larger software framework of enterprise-wide business systems, and their linkage to automated methods for enforcing and tracking control methods within companies. However, some practitioner-driven innovation—unexamined theoretically from a computer science perspective—has also facilitated the introduction of unmanageable and unscientific practices, such as (1) the undisciplined spreadsheet materialization of accounting entries and summary financial reporting figures (a programming and control nightmare), (2) rampant and unfettered use of faceted account codes (a knowledge representation and integration nightmare), and (3) inconsistent and incompatible use of XML interoperability schemes among different countries and different fields. Quite simply, present accounting representation structures seem unable to deal with the complexity of modern enterprise organization structures and sets of offered services, or with many of the highly complex new sets of financial instruments offered in modern capital markets.

There is a small cadre of computer-science-literate (as opposed to empirical-research-literate) AIS researchers within American universities, but they have little foundational support, either from the accounting research mainstream or from their (in theory, at least) closest topical associates on business school faculties—the professors from management information systems (MIS). As described above, non-support from the accounting mainstream is driven by an overly empirical mindset and, indeed, it is this same problem that causes the MIS field to be of little foundational help, because it has similarly evolved (somewhat later) on the paths that Granof and Zeff (2008) describe. Social science methods—as adapted from fields like finance, economics, and behavioral science—dominate the MIS research paradigms to the exclusion of technology-oriented computer science methods from software engineering, database design, and knowledge representation.

However, the MIS research linkage to accounting systems technology is not as vacuous as the accounting mainstream research linkage because of two mitigating factors. The first of these is the *design science* resurgence in American-based MIS—a movement that seeks to restore, at least partially, the legitimacy of pursuing prescriptive research aimed at developing new artifacts for building better business and accounting systems using the methodologies of computer science. Design science in MIS is still a nascent and underappreciated field, but its specifically normative approach seems to have gathered the attention of a critical mass of researchers (unlike the accounting design scientists, whose numbers are decreasing each passing year). The second factor affecting the possible accelerated use of computer science methods in accounting systems development research is the prevailing research norms of technically oriented business systems research groups outside of the U.S.A.; most prominently, those in European Union (EU) universities governed by EU-wide and EU-nation research contracts. The EU seems to explicitly recognize that funding research projects that combine engineering expertise with business expertise is a way of incubating new business methods and software services in Europe. Thus, this nexus of countries now serves as the worldwide hub in the development of technologies like (1) the development of ordered business process workflows, (2) the specification, bundling, and integrated use of services as an offered product to consumers, (3) the development of model-driven business enterprise architectures and semantic databases, and (4) the development of business ontologies within both enterprise systems environments and e-commerce interoperability environments.

In an interesting side note to the normative-positive battles that sometimes occur in the MIS field, it is informative to note that recently, Europeans (or, more specifically, members of the German informatics community) have organized specifically to prevent colonization of their research endeavors by North American research values. Such resistance by "unreasonable men" (again, in the Shavian sense) might be exactly what is needed by members of the AAA research community to get themselves back on the track to innovative and practice-oriented research methods. Indeed, the possible need to organize such takeover resistance in European accounting was noted by Hopwood (2008) prior to the German memorandum.

What Exactly Is Design Science?

Design science is a new name for an old idea in accounting. It is *normative* engineering research, where a computer-science-trained investigator tries to design new models, methods, and practices with the aim of improving practice. According to Buckminster Fuller (1992), the goal of *design science* is "to solve problems by introducing into the environment new artifacts, the availability of which will induce their spontaneous employment by humans and thus, coincidently, cause humans to abandon their previous problem-producing behaviors and devices." Much of accounting research prior to 1970 was intrinsically normative, arguing for new artifacts to make accounting work better in practice.

The MIS practice of design science has been codified by March and Smith (1995), Hevner et al. (2004), and Peffers et al. (2008). All of these codifications have two overall phases: (1) a design/build stage that is explicitly prescriptive (normative), and (2) an evaluate stage that is explicitly descriptive (positive). Design adaptations to accounting research have been described by David et al. (2002) and Geerts (2011), but their detailed exposition is not needed here. It is sufficient to note that the major products of design projects are new models, new constructs, new methods, and new instantiations (March and Smith 1995). As examples of design science research output, I illustrate in Figure 2 two sets of examples: (1) artifacts taken from computer science and information systems research, and (2) examples taken from work by me and my AIS colleagues in semantic modeling. Readers interested in more details should consult the original sources given.

FIGURE 2
Outputs of Design Science Research

	Computer Science & Information Systems	Semantic Modeling of Accounting Phenomena
Models	relational database model (Codd 1970)	Resource-Event-Agent ontology (McCarthy 1982; Geerts and McCarthy 1999, 2006; ISO 2007)
Constructs	Boyce-Codd normal form (Codd 1974)	epistemological adequacy (McCarthy and Hayes 1969; Geerts and McCarthy 2000)
Methods	structured analysis (DeMarco 1979)	REA-patterned implementation compromise (Rockwell and McCarthy 1999)
Instantiations	Watson (Ferucci et al. 2010)	relational accounting systems (Gal and McCarthy 1986)

Is Building an Accounting System an Empirical Activity?

When I used to present research work of the type shown in the last column of Figure 2 to accounting workshops (this is something I have stopped doing in recent years because I rarely received any useful feedback for all the energy I expended), I was often confronted with two preemptive challenges: (1) What has this got to do with accounting? and (2) Where are the data? I view the first of these questions as an artifact of the way accounting workshop traditions have evolved to produce extended vacuous debate at the cost of careful listening to ideas outside the mainstream, so I try to dismiss it quickly. For the second question, however, I appeal to the reasoning of two famous computer/social scientists—Allen Newell and Herbert Simon—presented in their 1975 Turing Award acceptance speech, where they persuasively argued that much of the computer type of design science I outline above is actually empirical:

> Computer science is an empirical discipline. We would have called it an experimental science, but like astronomy, economics, and geology, some of its unique forms of observation and experience do not fit a narrow stereotype of the experimental method. None the less, they are experiments. Each new machine that is built is an experiment. Actually constructing the machine poses a question to nature; and we listen for the answer by observing the machine in operation and analyzing it by all analytical and measurement means available. Each new program that is built is an experiment. It poses a question to nature, and its behavior offers clues to an answer. Neither machines nor programs are black boxes; they are artifacts that have been designed, both hardware and software, and we can open them up and look inside. We can relate their structure to their behavior and draw many lessons from a single experiment . . . We build computers and programs for many reasons. We build them to serve society and as tools for carrying out the economic tasks of society. But as basic scientists we build machines and programs as a way of discovering new phenomena and analyzing phenomena we already know about. Society often becomes confused about this, believing that computers and programs are to be constructed only for the economic use that can be made of them (or as intermediate items in a developmental sequence leading to such use). It needs to understand that the phenomena surrounding computers are deep and obscure, requiring much experimentation

to assess their nature. It needs to understand that, as in any science, the gains that accrue from such experimentation and understanding pay off in the permanent acquisition of new techniques; and that it is these techniques that will create the instruments to help society in achieving its goals. (Newell and Simon 1976, 114)

Although it seems that mainstream accounting researchers do not regard design science or the construction of normative accounting artifacts as empirical, Newell and Simon (1976) offer a different perspective that should at least be considered in the accounting academy. For that to actually happen, though, we must assess matters beyond actual research methodologies, and try to answer the following "people" question: "How do you allow for at least some innovation in research thought in an academy that sharply discourages any originality at all outside of its present main areas?" For that sociological perspective, we need to understand some (admittedly general) categorization schemes from physicist Lee Smolin.

RESEARCH CRAFTSPEOPLE VERSUS RESEARCH SEERS

Every society admires its live conformists and its dead troublemakers.

—Mignon McLaughlin (1981)

At MSU, the National Superconducting Cyclotron Laboratory is right across the street from the business school and, like many college faculty members, I am sure, I am fascinated by the physics research community, especially by its work in particle physics. I try to understand progress there by reading the latest books written for non-specialists by eminent physics researchers like Brian Greene (2000) and Lisa Randall (2011).

The dominant paradigm in that particle physics community at present is string theory, a model that captures my attention and imagination because of its *elegance*—a characteristic of models that I am particularly attracted to. So, naturally, when I discovered *The Trouble with Physics*—a book by Lee Smolin (2006) that argues against string theory—I tried as best I could (given my limited background and knowledge) to understand its arguments. It was a good intellectual exercise that put me totally outside of my field of expertise.

Toward the end of Smolin's (2006) book, however, I found material—wherein he talks about the sociology of research—that propelled me back to my own community in accounting. Over the course of five chapters in that last section, Smolin (2006) laments repeatedly the singular mindset of string theory, especially with its dogmatic disregard for other possible avenues of work. He also bemoans the dominance of scholars in senior leadership positions who are seemingly able to direct the best open positions and the premium journal space toward the dominant thought processes of current work, to the exclusion of a more diverse set of research approaches. However, the chapter that really caught my eye was called "Seers and Craftspeople," because it seemed to narrate well why the standard model of physics (or of accounting) seemed impervious to challenge. It was the very nature of the people participating: the *Craftspeople*, who were currently in ascendance in particle physics (as they are in accounting), versus the *Seers*, who are currently relegated to the edges of academic respectability in both fields. In McLaughlin terms (quote above), seers are the troublemakers who try to change a field's direction in innovative ways, while craftspeople are the conformists who try to keep a field locked in to its current thought processes.

It is difficult for me to condense the descriptions of these two Smolin (2006) groups, but I hope readers can gain an appreciation of his typification by visualizing some of his differentiating attributes.

Craftspeople are researchers who:

- are methodologically clever and committed to incremental progress;
- are pragmatic, flexible, and aggressive;
- are problem solvers, not long-range thinkers;
- are usually the best students (GPA) in high school, college, and graduate school;
- follow the crowd; and
- favor virtuosity in calculating over reflection on hard conceptual problems.

Seers are researchers who:

- are dreamers (may have become artists or writers under different circumstances);
- are independent and self-motivated;
- are foundational thinkers (account for both history and context of a problem);
- are often passionate and charismatic teachers;
- have the ability to be alone;
- are oft-times technically unimpressive as compared to craftspeople;
- need time and the freedom to think; and
- are often impatient and hard to get along with (mad at being marginalized).

To Smolin's (2006) list, I would add (for accounting):

- craftspeople are often aggressively myopic, with little tolerance for ideas outside their mainstream mindset, while seers are usually more benignly focused; and
- craftspeople are committed to "good" careers, as measured by count-and-weigh publication schemes, journal rankings, citation analysis, and workshop performances, while seers are committed to ideas, not careers.

In conceptual modeling terms, these class-level attributes constitute the archetypal essence of what it means to exist either within the boundaries of mainstream accounting thought or to be left outside. And although I will admit that the stereotypes do not apply in many, many cases, they are informative (and probably outrageous) enough to start a discussion of why innovation lags in our field.

For the accounting research community at present, I think the summary question concerning these two groups is the same organizational issue that Smolin (2006, 309) starts his chapter with:

> Do we have a system that allows someone capable of ferreting out that wrong assumption or asking that right question into the community of people we support and (equally important) listen to? Do we embrace creative rebels with this rare talent, or do we exclude them?

Accounting research norms today adhere to the McLaughlin quote given above, encouraging conformity and excluding innovation. I hope it is obvious to the reader that we should bring the troublemakers and rebels back from the fringes of acceptability and into the core of the academy, so we can start to have more of an effect on real accounting practice.

SUMMARY

At the outset of this paper, I proposed to explore the question "Is accounting research stuck in a rut of repetitiveness and irrelevancy?" from two perspectives. The first was methodological, where we looked at the explicitly normative practice of design science and speculated, at least briefly, how its application in accounting might help us to close the practice/innovation gap. The second was sociological, where we looked at the Smolin (2006) typology and speculated how explicit nurturing of research "seers" might also lead to more innovation in accounting research. Which of these approaches to enhanced innovation would I recommend be encouraged first in the accounting academy?

It is tempting to choose the very explicit path of design science because that approach already has a number of methodologies associated with it in the MIS literature, each emphasizing a design-build-evaluate framework that combines both prescriptive and descriptive types of research. In fact, the way design science was actually envisioned by pioneers like Buckminster Fuller was as a framework for creating new artifacts of all types, not just new computer-based artifacts. With these frameworks in mind, we could bring some accounting academics back into the explicit practice improvement process that Kaplan (2011) recommended be adopted from the biomedical field.

However, as important as allowing an explicit design mindset back into accounting would be, I strongly believe that we need to solve the sociological problems of the Smolin (2006) type first. For there to be a revolution in innovation in the accounting academy, there has to be a revolution in the hearts and minds of established accounting scholars. We need to welcome nonconforming and innovative young researchers with open minds, not with myopic guidance aimed at funneling them into established areas already mined heavily and dominated by senior researchers. Actually, doing this does not necessitate a completely new attitude; it simply involves going back 30–40 years when academic accounting was more open and benignly expansive. For example, these were two of my own early experiences:

- When I sent my first academic paper to *The Accounting Review* in June 1977, I was a doctoral student at the University of Massachusetts who was spending a lot more time cross-campus at the computer science department than I was spending at the business school. I was convinced I did a good job on the paper, but I was to realize later that I was a clear outlier in the graphical-illustration-driven format I used and in the advanced degree of data modeling concepts that I wrote with. This was the way people thought in engineering, but not in accounting. Nonetheless, Steve Zeff, the journal editor, took great care to treat the manuscript with respect and to find reviewers who could understand my ideas. Eventually, the paper was published, and it kept me on track for other, more advanced work in the same vein.
- Just a few years later, I was a new professor at Michigan State who had been hired explicitly to invigorate the accounting systems area. In a curriculum coordination meeting with Professor Al Arens where we were trying to establish systems as a prerequisite for auditing, he suggested that I start using a large, paper-based, bookkeeping practice set that he had written for the AIS class. This was an outrageous power mismatch: I was an Assistant Professor with less than two years on faculty, and he was a Chaired Full Professor whose teaching reputation, both locally and nationally, was as high as it could get. Nonetheless, to the surprise of no one who understands my research mindset, I told him that I was philosophically opposed to doing that in my course. When I said that, he looked me right in the eye and said "Bill, you need to teach what you believe," and that was the end of that. The practice set went into the auditing course, where it remained for many years.

I am convinced that there is a high probability that those two incidents would have unfolded differently today if I were just starting a career at a research-oriented school. Accounting has become more dogmatic and dominated by the conventional wisdom of just a few groups (Williams 2009). To increase innovation in accounting research today, we need more *junior* people who are unreasonable, rebellious, troublemaking seers, and more *senior* people (like Arens and Zeff) who will not try to redirect and constrain them.

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