

COMMENTARY

How Can Accounting Researchers Become More Innovative?

Sudipta Basu

SYNOPSIS: This essay is based on a presentation at the American Accounting Association Strategy Retreat in May 2011 on the assertion “Accounting research as of 2011 is stagnant and lacking in significant innovation that introduces fresh ideas and insights into our scholarly discipline.” It poses the question “How can accounting researchers become more innovative?” and discusses why accounting researchers may have become less innovative. It also outlines some changes in incentive structures and editorial processes needed to achieve greater innovation in accounting research.

Keywords: big questions; open contests; scientism; statistical significance; graphs; figures.

JEL Classifications: A11; A12; B23; C12; I23; J44; M4; O31.

We fervently hope that the research pendulum will soon swing back from the narrow lines of inquiry that dominate today’s leading journals to a rediscovery of the richness of what accounting research can be. For that to occur, deans and the current generation of academic accountants must give it a push.

—Michael H. Granof and Stephen A. Zeff (2008)

Rather than clinging to the projects of the past, it is time to explore questions and engage with ideas that transgress the current accounting research boundaries. Allow your values to guide the formation of your research agenda. The passion will inevitably follow.

—Joni J. Young (2009)

INTRODUCTION

Are most accounting academics and professionals excited when they receive the latest issue of *The Accounting Review* or an email of the Table of Contents? When I was a doctoral student and later an assistant professor, I looked forward to receiving new issues of top

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I thank Larry Brown, Chris Chapman, Stan Markov, Eric Press, Greg Waymire, and Joni Young for helpful comments and suggestions.

Submitted: April 2012

Accepted: April 2012

Published Online: December 2012

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accounting journals. But as my research horizons widened, I found myself less interested in reading a recent issue of an accounting journal than one in a nearby discipline (e.g., *Journal of Law and Economics*), or even a discipline further away (e.g., *Evolution and Human Behavior*). Many accountants find little insight into important accounting issues in the top U.S. academic journals, which critics allege focus on arcane issues that interest a narrowing readership (e.g., Sterling 1976; Garcha et al. 1983; Flesher 1991; Heck and Jensen 2007).¹

Several prominent scholars raise concerns about recent accounting research. Joel Demski's 2001 American Accounting Association (AAA) Presidential Address acknowledges the excitement of the mid-20th century advances in accounting research, but notes, "Of late, however, a malaise appears to have settled in. Our progress has turned flat, our tribal tendencies have taken hold, and our joy has diminished." The state of current U.S. accounting scholarship has been questioned repeatedly by recent AAA presidents, including Judy Rayburn (2006), Shyam Sunder (2006), Sue Haka (2008), and Greg Waymire (2012).²

Assuming that when there is smoke there is likely a fire, I adopt a "glass-half-empty" lens.³ I diagnose the problems in our discipline after briefly outlining a few long-term causes for the symptoms identified by critics. I seek remedies for the more urgent symptoms, drawing upon examples from other disciplines that are exploring ways to reinvigorate scholarship and restore academic relevance. While a few of these can be implemented by AAA, many others can be adopted by journal editors and authors. I hope that these personal views stimulate conversations that lead to better accounting scholarship.

My main suggestion is to re-orient accounting researchers toward addressing fundamental accounting questions, and to provide awards and incentives for innovative leadership, rather than for passively following accounting standard-setters. This will require educating young scholars in accounting history as well as the history of accounting thought. In addition, AAA annual meetings should feature a named lecture by an eminent non-accounting scholar to expose us to new ideas and methods. We should rely less on statistical significance for assessing importance and instead emphasize practical significance in judging the value of a research contribution. Accounting research should be made more accessible to practitioners, interested laymen, and academic colleagues in other disciplines by improving readability—for example by making articles shorter and less jargon laden, and replacing tables with more informative figures. Finally, we should more actively seek out and explore accounting domains beyond those captured in machine-readable databases.

WHAT ARE THE SYMPTOMS? WHAT IS THE DIAGNOSIS?

Demski (2007) and Fellingham (2007) contend that accounting is not an academic research discipline that contributes knowledge to the rest of the university. This assertion is supported by predominantly one-way citation flows between accounting journals and those of neighboring disciplines (Lee 1995; Pieters and Baumgartner 2002; Bricker et al. 2003; Rayburn 2006). Such sentiments imply low status of the accounting professoriate within the academy, and echo those of Demski et al. (1991), Zeff (1989), Sterling (1973), and, from longer ago, Hatfield (1924). Furthermore, and perhaps of greater concern, accounting research has little impact on accounting

¹ See, for example, the thread at: <http://www.trinity.edu/rjensen/AAAjournals.htm#Largay>, including a suggestion that in an AAA-commissioned survey, AAA members did not rank *The Accounting Review* as AAA's best journal. Rayburn (2006) points out that other disciplines like the History of Science face similar problems.

² Walsh (2011) lists a series of comments from past Academy of Management presidential addresses that could easily be substituted for AAA presidential addresses, with just "accounting" substituted for "management."

³ I do not wish to suggest that the glass was full previously (McKenzie and Nelson 2003), but rather that the rate of accounting-knowledge generation has likely slowed.

practice, and the divergence between accounting research and accounting practice has been growing over the last half century (e.g., Langenderfer 1987; Baxter 1988; Bricker and Previts 1990).

What other symptoms have critics identified? Demski (2008) highlights the lack of passion in many accounting researchers, while Ball (2008) bemoans the “absence of a solidly grounded worldview—a deep understanding of the functioning of financial reporting in the economy” among accounting professors and doctoral students alike. Kaplan (2011) suggests that accounting research is predominantly conducted in an ivory tower with little connection to problems faced by practitioners, whereas Sunder (2007) argues that mandatory uniform standards suppress thinking among accounting researchers, echoing Baxter (1953). Kinney (2001) submits that accounting researchers are not sure about which research domains are ours. Demski et al. (1991) raised all these concerns previously, implying that accounting research has been stagnant for decades. No wonder I (and others) find too many recent accounting papers to be tedious and uninteresting.

A simplistic diagnosis is that U.S. accounting research mimics the concerns and mores of the U.S. accounting profession. The accounting profession in the middle of the 20th century searched for principles underlying accounting practices, which provided a demand for normative academic theories. These demands were met by accounting classics such as Gilman (1939), Paton and Littleton (1940), and Edwards and Bell (1961). Although standards were originally meant to guide accounting practice, standard-setters soon slid down the slippery slope of enforceable rules (Baxter 1979). Consequently, ever more detailed rules were written to make reported numbers more reliable. Bureaucrats wanted to uniformly enforce explicit protocols, which lawyers creatively interpreted and financial engineers circumvented with new contracts. In parallel, accounting researchers abandoned normative debates and turned to measuring and evaluating the effects of alternative accounting rules and attempts to evade them (e.g., Zeff 1978). In sum, as U.S. GAAP moved from norm based to rule based, or from emphasizing relevance to increasing uniformity and reliability, accounting researchers began favoring formal quantitative methods over informal qualitative arguments. As U.S. GAAP and the Internal Revenue Code became ever more arcane, so did U.S. accounting research.

Another diagnosis is that our current state stems from accounting trying to become a more scientific discipline. During 1956–1964, the Ford Foundation gave Carnegie Mellon, Chicago, Columbia, Harvard, and Stanford \$14.4 million to try to make their business schools centers of excellence in research and teaching (Khurana et al. 2011). Contributions from other foundations raised the total to \$35 million (Jeuck 1986), which would be about \$268 million in 2012 dollars.⁴ The Ford Foundation espoused quantitative methods and economics with a goal of making business research more “scientific” and “professional” (Gordon and Howell 1959). Business schools responded by emphasizing statistical analyses and mathematical modeling, and mathematical training rather than accounting knowledge became increasingly required for publications in the top accounting journals (e.g., Chua 1996; Heck and Jensen 2007). While business researchers had some notable successes in the 1960s and 1970s soon after introducing these new techniques, the rate of innovation has allegedly since fallen.

Concurrently, U.S. business schools became credentialing machines guided by a “(student) customer is always right” ethos, so there was also less demand for accounting theory from accounting students and their employers (Demski 2007), and intermediate accounting textbooks replaced theory with rote memorization of rules (Zeff 1989).⁵ In 1967, the American Assembly of Collegiate Schools of Business (AACSB) increased the degree requirements for accredited

⁴ My calculation assumes a purchase of \$35 million in 1960 using <http://www.usinflationcalculator.com>

⁵ The heightened demand for academic credentials can be traced to a U.S. Supreme Court case, *Griggs v. Duke Power Co.*, 401 U.S. 424 (1971), which essentially outlawed intelligence and aptitude tests for employment screening purposes, and led to college degrees being used as a proxy for intelligence (e.g., O’Keefe and Vedder 2008).

accounting faculty from a master's-CPA combination to a Ph.D., effective in 1969. Many accounting doctoral programs were started in the 1960s to meet the new demand for accounting doctorates (Rodgers and Williams 1996), and these programs imitated the new elite accounting programs. Statistics, economics, and econometrics screening became requisite challenges (Zeff 1978), preceding accounting courses in many doctoral programs. Unsurprisingly then, doctoral students came to infer that accounting theory and institutional content are merely the icing on the cake of quantitative economics or psychology.

In summary, the forces that induced change in U.S. accounting academe in the aftermath of World War II still prevail. The goals and methods of accounting research have changed profoundly over the last half century (e.g., Zeff 1978), leading accounting researchers to more Type III error (e.g., Dyckman 1989): “giving the right answer to the wrong problem” (Kimball 1957) or “solving the wrong problem precisely” (Raiffa 1968). To the extent that accounting relevance has been sacrificed for tractability and academic rigor, these changes have slowed accounting-knowledge generation.

HOW CAN ACCOUNTING RESEARCH BECOME MORE INNOVATIVE?

Demski (2007) characterizes recent accounting research thus: “Innovation is close to nonexistent. This, in fact, is the basis for the current angst about the ‘diversity’ of our major publications. Deeper, though, is the mindset and factory-like mentality that is driving this visible clustering in the journals.” He laments further, “The vast bulk of our published work is insular, largely derivative, and lacking in the variety that is essential for innovation. Arguably, our published work is focusing increasingly on job placement and retention.” Demski et al. (1991) conjecture, “Accounting researchers apparently suffer from insecurity about their field of study, leading them to perturb fairly secure research paradigms (mostly those that have been accepted by economists) within an ever-narrowing circle of accounting academics isolated from the practice world. There is very little reward in the current academic system for experimentation and innovation that has the potential for impacting practice.” My sense is that many accounting researchers (especially those who have not practiced accounting) believe that the conceptual framework has resolved all fundamental accounting issues and that accounting researchers should help regulators fill in the technical details to implement their grand plan. As blinkers keep horses focused on the road ahead, the current conceptual framework blinds accounting academics to the important issues in accounting (especially the many flaws in the conceptual framework project).

Identifying the major unsolved questions in a field can provide new directions for research quests as well as a framework for teaching. For example, Hilbert (1900) posed 23 unsolved problems for mathematicians to test themselves against over the 20th century. His ideas were so successful in directing subsequent mathematics research that \$1 million Millennium Prizes have been established for seven unsolved mathematical questions for the current century.⁶ Many scientific disciplines compile lists of unsolved questions for their fields in an attempt to imitate the success of 20th century mathematics.⁷ There is even a new series of books titled, *The Big Questions: xxx*, where xxx is philosophy (Blackburn 2009), physics (Brooks 2010), the universe (Clark 2010), etc. The series “is designed to let renowned experts confront the 20 most fundamental and frequently asked questions in a major branch of science or philosophy.” There is, however, neither consensus nor much interest in addressing the big unanswered questions in accounting, let

⁶ The Clay Mathematics Institute announced on March 18, 2010, that Dr. Grigoriy Perelman had been awarded the first Millennium Prize for resolving the Poincaré conjecture. See: <http://www.claymath.org/millennium/>

⁷ See http://en.wikipedia.org/wiki/List_of_unsolved_problems. Morgenstern (1972) lists 13 questions for economics.

alone exploring and refining them, recent attempts notwithstanding (e.g., Ball 2008; Basu 2008; Robinson 2007).

Few accounting professors can identify even a dozen of the 88 members of the Accounting Hall of Fame, let alone why they were selected as “having made or are making significant contributions to the advancement of accounting.”⁸ Since many doctoral syllabi concentrate on recent publications to identify current research frontiers, most recent doctoral graduates have read just a handful of papers published before 2000. This leaves new professors with little clue to the “most fundamental and frequently asked questions” of our discipline. The American Economic Association recently celebrated the centenary of *The American Economic Review* by appointing a Top 20 Committee to select the “top 20” articles published in the journal over the previous 100 years (Arrow et al. 2011). Similarly, the *Financial Analysts Journal* picked the best articles over its first 50 years (Harlow 1995). Accounting academics could similarly identify the top 20 articles published in the first 100 years of *The Journal of Accountancy* (1905–2004), the top 25 articles published in *Accountancy* (1880–2005), or proportionately fewer papers for *The Accounting Review* (1926–2011).

If accounting researchers do not tackle the fundamental issues in accounting, we collectively face obsolescence, irrelevance, and oblivion.⁹ Demski et al. (1991) recommended identifying a “broad set of challenging, relevant research questions” to be distributed to seasoned researchers to develop detailed research proposals that would be presented at a “proposals conference,” with the proceedings distributed widely among accounting academics. Lev (1992) commissioned several veteran researchers, including Michael Brennan (Finance) and Daniel Kahneman (Psychology), to write detailed research proposals on “Why is there a conservatism bias in financial reporting?” Eight proposals were presented at a plenary session of the 1993 AAA Annual Meeting in San Francisco, and copies of the research proposals were included in the packets of all annual meeting attendees. This initiative provided the impetus for conservatism research over the last two decades (cf. Basu 2009).

I urge that we revive this successful initiative by instituting a Triennial AAA Research Challenge. At the start of each three-year cycle, AAA could solicit fundamental questions for an open research competition. A challenging research question would be selected, and initial research proposals would be submitted a year and a half later. A few of these would be selected for presentation at AAA section and regional midyear meetings for feedback. The research topic and initial research proposals could also be used for active learning sessions at AAA Doctoral and New Faculty Consortiums. A revised set of proposals would be submitted the following year, and the best of these would be invited for presentation in a plenary session at AAA annual meetings. The winners would be invited to write a monograph for the AAA Studies in Accounting Research series (or possibly a paper in *The Accounting Review*), as well as officially recognized with an award. AAA could also recognize significant research innovation by creating an award for Innovative Research Paper (for assistant and associate professors) and a career award for Innovative Researcher (for full and chair professors).

We could further motivate innovative accounting research by providing ambitious targets for specific research domains. President Kennedy’s (1962) speech setting the target of putting a man on the moon by the end of the decade inspired U.S. aeronautics researchers. Similarly, the £20,000

⁸ <http://fisher.osu.edu/departments/accounting-and-mis/the-accounting-hall-of-fame>

⁹ While Moehrle et al. (2009) list hundreds of papers whose authors claimed regulatory or practice relevance, they provide scant evidence that accounting research *has improved* accounting practice (versus affected via standard-setting or translated into mathematical jargon). Demski et al. (1991), Holthausen and Watts (2001), Granof and Zeff (2008), and Singleton-Green (2010) argue that accounting research has had little impact on actual standards beyond some perfunctory lip service.

prize offered in 1714 by the British Parliament to anyone who could accurately measure a ship's longitude led to significant advances in clock design and time measurement (Sobel 1995). A \$1.4 million X-Prize for better technology to clean up oil spills announced after the Deep Horizon oil spill in 2010 inspired 300 proposals, with the winning team taking less than six months to produce a technology six times as efficient as any pre-existing technology (Wadhwa 2011).¹⁰ Tullock (1966, 29) argues that open contests generate significant innovation because large prizes induce self-selection by researchers who are interested in and capable of addressing the questions, often from outside traditional disciplinary or organizational boundaries. AAA could sponsor prizes for specific research goals such as designing field-test protocols for assessing proposed accounting standards or assessing the desirability of sunset provisions for accounting standards and conceptual frameworks. The benefit of encouraging frontier exploration is that innovative young researchers are less likely to face gatekeepers (referees and editors) with fossilized viewpoints to enforce.

To facilitate research on fundamental accounting questions, AAA could sponsor or create open access archives and databases for unexplored or underserved accounting areas (e.g., partnership and proprietorship financial accounting, managerial accounting, historical accounting).¹¹ AAA should also solicit and commission more research studies and revive the monograph series to actively support fundamental research into accounting, as evidenced in the classic monographs by Ijiri (1975, 1989). AAA could also expand the recent initiative of intellectual biographies in *Accounting Horizons* to encompass biographies of ideas. Most junior accounting scholars, for example, cannot differentiate between the proprietary theory and the entity theory, and why the distinction matters.

AAA should also record interviews and lectures by major innovators in accounting research similar to the "History of Finance" section of the American Finance Association website (<http://www.afajof.org/association/historyfinance.asp>), and post the streaming videos on the AAA website. We could also take advantage of the upcoming centennial of AAA to issue a calendar listing, "Important Dates in Accounting History," similar to that issued by the American Economic Association on its 125th anniversary (<http://www.aeaweb.org/calendar/>). AAA could also publicize accounting history resources that accounting faculty and students can use to round out their incomplete educations.¹²

HOW CAN ACCOUNTING RESEARCH BECOME MORE RELEVANT?

As alluded to earlier, business schools in the mid-1960s embarked on a program of scientific research, with significant funding from the Ford Foundation and allied foundations.¹³ The Defense Department had been impressed by new operations research tools such as linear programming that had helped the World War II effort (e.g., Whitley 1986), and sought to graft such improved management techniques into the private sector by improving business school training. The Carnegie Foundation sought to reprise its successful reform of medical education via scientific research

¹⁰ The X-Prize Foundation offers large prizes up to \$30 million for significant innovations such as to rapidly, accurately, and economically sequence 100 whole human genomes. For further details, see <http://www.xprize.org>

¹¹ The Global Accounting Data Archive Network (GADAN), a joint project of AAA and the European Accounting Association at <http://raw.rutgers.edu/digitalibrary/home.htm>, is a great start but needs better publicity as well as expansion.

¹² See, for example, Waymire and Basu (2008) and the online compilation by Stephen Zeff at: http://www.byuaccounting.net/mediawiki/index.php?title=Historical_Resources_for_U.S._Accounting_Academics_and_Doctoral_Students

¹³ Bottom (2009) challenges this conventional account and shows that foundation support for social science and business education dates back to the period immediately after World War I, when many successful think tanks were founded (e.g., Hoover Institution, 1919; Brookings Institution, 1927) that actively engage in public policy debates.

(Flexner 1910) by reforming business education similarly (Pierson 1959). Gordon and Howell (1959) recommended quantitative techniques and economics to help business schools become more scientific. Merrill Lynch, Pierce, Fenner & Smith provided data and seed funding for the Center for Research into Security Prices (CRSP) at The University of Chicago, which transformed finance and accounting research by providing a large machine-readable database for statistical analyses. The normative verbal arguments of the 1950s and 1960s were replaced by arguments expressed in mathematical language (Zeff 1978; Dyckman 1988), which made accounting articles look more like those in economics journals. Thus, business school researchers followed economics researchers into aggressive quantification.

The initial successes of the 1960s finance and accounting researchers at Chicago led to imitation by researchers at other schools. The quantitative takeover was facilitated by new journals such as the *Journal of Accounting Research*, which was started in 1963 to support and publicize the new “scientific” research. Accounting doctoral programs began emphasizing a foundation in economics methods, including training in microeconomics and econometrics, which replaced requirements in foreign languages (Zeff 1978), accounting theory, and accounting history. The increasing emphasis on academic rigor infected the publication process, as quantitative methods trumped accounting institutional knowledge and practical relevance (e.g., Ellison 2002; Swanson 2004).

Unfortunately, business schools ignored the earlier warnings of prominent economists that copying the physical sciences would create not an economic science, but rather “scientism,” a dogmatic imitation of scientific methodology (e.g., Hayek 1942, 1943, 1944; Knight 1947).¹⁴ Indeed, Crick (1988, 5) warns, “The basic laws of physics can be expressed in exact mathematical form, and they are probably the same throughout the universe. The ‘laws’ of biology, by contrast, are often only broad generalizations, since they describe rather elaborate chemical mechanisms that natural selection has evolved over billions of years.” Since human behavior reflects both biological and cultural selection (cf. Darwin 1871; Boyd and Richerson 1985), social scientists study phenomena that are considerably more complex than those studied by physicists, and consequently they find it much harder to predict accurately (Hayek 1955). Unfortunately, the “scientific method” and the linear scientific paper do not accurately describe how real scientists operate (e.g., Medawar 1963, 1965), so accounting academics imitate a false model. I briefly note two errors that modern U.S. accounting research seems especially prone to before moving on to recommendations for producing more relevant research.

Is Academic Accounting a “Cargo Cult Science”?

In a commencement address at Caltech titled “Cargo Cult Science,” Richard Feynman (1974) discussed “science, pseudoscience, and learning how not to fool yourself.” He argued that despite great efforts at scientific research, little progress was apparent in school education. Reading and mathematics scores kept declining, despite schools adopting the recommendations of experts. Feynman (1974, 11) dubbed fields like these “Cargo Cult Sciences,” explaining the term as follows:

In the South Seas there is a Cargo Cult of people. During the war they saw airplanes land with lots of good materials, and they want the same things to happen now. So they’ve arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas—he’s the controller—and they wait for the airplanes to land. They’re doing everything right. The form is perfect. It looks exactly the

¹⁴ “Scientism” has been used to denote several other related concepts, see: <http://en.wikipedia.org/wiki/Scientism>

way it looked before. But it doesn't work. No airplanes land. So I call these things Cargo Cult Science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land.

Feynman (1974) argued that the key distinction between a science and a Cargo Cult Science is scientific integrity: “[T]he idea is to give all of the information to help others judge the value of your contribution; not just the information that leads to judgment in one particular direction or another.” In other words, papers should not be written to provide evidence for one's hypothesis, but rather to “report everything that you think might make it invalid.” Furthermore, “you should not fool the layman when you're talking as a scientist.”

Even though more and more detailed rules are constantly being written by the SEC, FASB, IASB, PCAOB, AICPA, and other accounting experts (e.g., Benston et al. 2006), the number and severity of accounting scandals are not declining, which is Feynman's (1969) hallmark of a pseudoscience. Because accounting standards often reflect standard-setters' ideology more than research into the effectiveness of different alternatives, it is hardly surprising that accounting quality has not improved. Even preliminary research findings can be transformed journalistically into irrefutable scientific results by the political process of accounting standard-setting. For example, the working paper results of Frankel et al. (2002) were used to justify the SEC's longstanding desire to ban non-audit services in the Sarbanes-Oxley Act of 2002, even though the majority of contemporary and subsequent studies found different results (Romano 2005). Unfortunately, the ability to bestow status by invitation to select conferences and citation in official documents (e.g., White 2005) may let standard-setters set our research and teaching agendas (Zeff 1989).

Academic Accounting and the “Cult of Statistical Significance”

Ziliak and McCloskey (2008) argue that, in trying to mimic physicists, many biologists and social scientists have become devotees of statistical significance, even though most articles in physics journals do not report statistical significance. They argue that statistical tests are typically used to infer whether a particular effect exists, rather than to measure the magnitude of the effect, which usually has more practical import. While early empirical accounting researchers such as Ball and Brown (1968) and Beaver (1968) went to great lengths to estimate *how much* extra information reached the stock market in the earnings announcement month or week, subsequent researchers limited themselves to answering *whether* other factors moderated these effects. Because accounting theories rarely provide quantitative predictions (e.g., Kinney 1986), accounting researchers perform *nil* hypothesis significance testing rituals, i.e., test unrealistic and atheoretical null hypotheses that a particular coefficient is exactly zero.¹⁵ While physicists devise experiments to measure the mass of an electron to the accuracy of tens of decimal places, accounting researchers are still testing the equivalent of whether electrons have mass. Indeed, McCloskey (2002) argues that the “secret sins of economics” are that economics researchers use quantitative methods to produce qualitative research outcomes such as (non-)existence theorems and statistically significant signs, rather than to predict and measure quantitative (how much) outcomes.

Practitioners are more interested in magnitudes than existence proofs, because the former are more relevant in decision making. Paradoxically, accounting research became less useful in the real world by trying to become more scientific (Granof and Zeff 2008). Although every empirical article in accounting journals touts the statistical significance of the results, practical significance is rarely considered or discussed (e.g., Lev 1989). Empirical articles do not often discuss the meaning of a

¹⁵ Furthermore, econometrics-based researchers routinely report incorrect p-values by ignoring the impact of multiple comparisons, so their inferences are usually unreliable (e.g., Basu et al. 2012).

regression coefficient with respect to real-world decision variables and their outcomes. Thus, accounting research results rarely have practical implications, and this tendency is likely worst in fields with the strongest reliance on statistical significance such as financial reporting research.

Ziliak and McCloskey (2008) highlight a deeper concern about over-reliance on statistical significance—that it does not even provide evidence about whether a hypothesis is true or false. Carver (1978) provides a memorable example of drawing the wrong inference from statistical significance:

What is the probability of obtaining a dead person (label this part D) given that the person was hanged (label this part H); this is, in symbol form, what is $P(D|H)$? Obviously, it will be very high, perhaps 0.97 or higher. Now, let us reverse the question. What is the probability that a person has been hanged (H), given that the person is dead (D); that is, what is $P(H|D)$? This time the probability will undoubtedly be very low, perhaps 0.01 or lower. No one would be likely to make the mistake of substituting the first estimate (0.97) for the second (0.01); that is, to accept 0.97 as the probability that a person has been hanged given that the person is dead. Even though this seems to be an unlikely mistake, it is exactly the kind of mistake that is made with interpretations of statistical significance testing—by analogy, calculated estimates of $P(D|H)$ are interpreted as if they were estimates of $P(H|D)$, when they clearly are not the same.

As Cohen (1994) succinctly explains, statistical tests assess the probability of observing a sample moment as extreme as observed conditional on the null hypothesis being true, or $P(D|H_0)$, where D represents data and H_0 represents the null hypothesis. However, researchers want to know whether the null hypothesis is true, conditional on the sample, or $P(H_0|D)$. We can calculate $P(H_0|D)$ from $P(D|H_0)$ by applying Bayes' theorem, but that requires knowledge of $P(H_0)$, which is what researchers want to discover in the first place. Although Ziliak and McCloskey (2008) quote many eminent statisticians who have repeatedly pointed out this basic logic, the essential point has not entered the published accounting literature.

In my view, restoring relevance to mathematically guided accounting research requires changing our role model from applied science to engineering (Colander 2011).¹⁶ While science aims at finding truth through application of institutionalized best practices with little regard for time or cost, engineering seeks to solve a specific problem using available resources, and the engineering method is “the strategy for causing the best change in a poorly understood or uncertain situation within the available resources” (Koen 2003). We should move to an experimental approach that simulates real-world applications or field tests new accounting methods in particular countries or industries, as would likely happen by default if accounting were not monopolized by the IASB (Dye and Sunder 2001). The inductive approach to standard-setting advocated by Littleton (1953) is likely to provide workable solutions to existing problems and be more useful than an axiomatic approach that starts from overly simplistic first principles.

To reduce the gap between academe and practice and stimulate new inquiry, AAA should partner with the FEI or Business Roundtable to create summer, semester, or annual research internships for accounting professors and Ph.D. students at corporations and audit firms.¹⁷ Accounting professors who have served as visiting scholars at the SEC and FASB have reported

¹⁶ I do not endorse social engineering, state economic planning, etc., which stem from an engineering attitude that all relevant forces and goals are well understood and what is lacking is merely a plan of action (Hayek 1944).

¹⁷ Demski et al. (1991) recommended “engaging in periodic roundtable discussions with members of the accounting profession and industry to identify research-worthy issues of relevance for the accounting profession and industry.” While the Ross Institute at NYU has organized roundtables for several years, I doubt that they have had much impact on accounting research.

positively about their experience (e.g., [Jorgensen et al. 2007](#)), and I believe that such practice internships would provide opportunities for valuable fieldwork that supplements our experimental and archival analyses. Practice internships could be an especially fruitful way for accounting researchers to spend their sabbaticals.

Another useful initiative would be to revive the tradition of *The Accounting Review* publishing papers that do not rely on statistical significance or mathematical notation, such as case studies, field studies, and historical studies, similar to the *Journal of Financial Economics* ([Jensen et al. 1989](#)).¹⁸ A separate editor, similar to the book reviews editor, could ensure that appropriate criteria are used to evaluate qualitative research submissions ([Chapman 2012](#)). A co-editor from practice could help ensure that the topics covered are current and relevant, and help reverse the steep decline in AAA professional membership. Encouraging diversity in research methods and topics is more likely to attract new scholars who are passionate and intrinsically care about their research, rather than attracting only those who imitate current research fads for purely instrumental career reasons.¹⁹

The relevance of accounting journals can be enhanced by inviting accomplished guest authors from outside accounting. The excellent April 1983 issue of *The Accounting Review* contains a section entitled “Research Perspectives from Related Disciplines,” which includes essays by Robert Wilson (Decision Sciences), Michael Jensen and Stephen Ross (Finance and Economics), and Karl Weick (Organizational Behavior) that were based on invited presentations at the 1982 AAA Annual Meeting. The thought-provoking essays were discussed by prominent accounting academics (Robert Kaplan, Joel Demski, Robert Libby, and Nils Hakansson); I still use [Jensen \(1983\)](#) to start each of my Ph.D. courses. Academic outsiders bring new perspectives to familiar problems and can often reframe them in ways that enable solutions ([Tullock 1966](#)).

I still lament that no accounting journal editor invited the plenary speakers—Joe Henrich, Denise Schmandt-Besserat, Michael Hechter, Eric Posner, Robert Lucas, and Vernon Smith—at the 2007 AAA Annual Meeting to write up their presentations for publication in accounting journals. It is rare that Nobel Laureates and U.S. Presidential Early Career Award winners address AAA annual meetings.²⁰ I strongly urge that AAA annual meetings institute a named lecture given by a distinguished researcher from a different discipline, with the address published in *The Accounting Review*. This would enable cross-fertilization of ideas between accounting and other disciplines. Several highly cited papers published in the *Journal of Accounting and Economics* were written by economists ([Watts 1998](#)), so this initiative could increase citation flows from accounting journals to other disciplines.

HOW CAN WE MAKE U.S. ACCOUNTING JOURNALS MORE READABLE AND INTERESTING?

Even the greatest discovery will have little impact if other people cannot understand it or are unwilling to make the effort. [Zeff \(1978\)](#) says, “Scholarly writing need not be abstruse. It can and should be vital and relevant. Research can succeed in illuminating the dark areas of knowledge and facilitating the resolution of vexing problems—but only if the report of research findings is communicated to those who can carry the findings further and, in the end, initiate change.” If our

¹⁸ Sound research does not need statistical tests to be convincing. For example, the [Tversky and Kahneman \(1974\)](#) and [Kahneman and Tversky \(1979\)](#) papers are highly influential even though neither contains a single p-value.

¹⁹ Tighter U.S. immigration restrictions for working professionals over the last two decades may have played a role. Since academics receive priority in the green card process, ambitious immigrants from developing countries may enter accounting academe simply to gain entry into the U.S.

²⁰ The 2007 AAA Annual Meeting plenary addresses were videotaped, but unfortunately they are no longer available for posting.

journals put off readers, then our research will not stimulate our students or induce change in practice (Dyckman 1989).

Michael Jensen (1983, 333–334) addressed the 1982 AAA Annual Meeting saying:

Unfortunately, there exists in the profession an unwarranted bias toward the use of mathematics even in situations where it is unproductive or useless. One manifestation of this is the common use of the terms “rigorous” or “analytical” or even “theoretical” as identical with “mathematical.” None of these links is, of course, correct. Mathematical is not the same as rigorous, nor is it the same as analytical or theoretical. Propositions can be logically rigorous without being mathematical, and analysis does not have to take the form of symbols and equations. The English sentence and paragraph will do quite well for many analytical purposes. In addition, the use of mathematics does not prevent the commission of errors—even egregious ones.

Unfortunately, the top accounting journals demonstrate an increased “tyranny of formalism” that “develops when mathematically inclined scholars take the attitude that if the analytical language is not mathematics, it is not rigorous, and if a problem cannot be solved with the use of mathematics, the effort should be abandoned” (Jensen 1983, 335). Sorter (1979) acidly described the transition from normative to quantitative research: “the golden age of empty blindness gave way in the sixties to bloated blindness calculated to cause indigestion. In the sixties, the wonders of methodology burst upon the minds of accounting researchers. We entered what Maslow described as a mean-oriented age. Accountants felt it was their absolute duty to regress, regress and regress.” Accounting research increasingly relies on mathematical and statistical models with highly stylized and unrealistic assumptions. As Young (2006) demonstrates, the financial statement “user” in accounting research and regulation bears little resemblance to flesh-and-blood individuals, and hence our research outputs often have little relevance to the real world.

Figure 1 compares how frequently accountants and members of ten other professions are cited in *The New York Times* in the late 1990s (Ellenberg 2000). These data are juxtaposed with the numbers employed in each profession during 1996 using U.S. census data. Accountants are cited less frequently relative to their numbers than any profession except computer programmers. One possibility is that journalists cannot detect anything interesting in accounting journals. Another possibility is that university public relations staffs are consistently unable to find an interesting angle in published accounting papers that they can pitch to reporters. I have little doubt that the obscurantist tendencies in accounting papers make it harder for most outsiders to understand what accounting researchers are saying or find interesting.

Accounting articles have also become much longer over time, and I am regularly asked to review articles with introductions that are six to eight pages long, with many of the paragraphs cut-and-pasted from later sections. In contrast, it took Watson and Crick (1953) just *one* journal page to report the double-helix structure of DNA. Einstein (1905) took only *three* journal pages to derive his iconic equation $E = mc^2$. Since even the best accounting papers are far less important than these classics of 20th century science, readers waste time wading through academic bloat (Sorter 1979). Because the top general science journals like *Science* and *Nature* place strict word limits on articles that differ by the expected incremental contribution, longer scientific papers signal better quality.²¹ Unfortunately, accounting journals do not restrict length, which encourages bloated papers. Another driver of length is the aforementioned trend toward greater rigor in the review process (Ellison 2002).

²¹ <http://www.nature.com/nature/authors/gta/index.html#a1> and <http://www.sciencemag.org/site/feature/contribinfo/index.xhtml>

FIGURE 1

Relative Frequency of References to Accountants in *The New York Times* from [Ellenberg \(2000\)](#)

Table 2. References to Selected Professions in the New York Times Feature Sections Produced by the New York Times FirstSearch Data Base

Profession	Frequency of recent citations ^a	1996 total employed (1,000) ^b	Relative frequency
Lawyers	8,101	880	9.2
Economists	1,201	148	8.1
Architects	1,097	160	6.9
Physicians	3,989	667	6.0
Statisticians	34	14	2.4
Psychologists	479	245	2.0
Dentists	165	137	1.2
Teachers (not university)	3,938	4,724	0.8
Engineers	934	1,960	0.5
Accountants	628	1,538	0.4
Computer programmers	91	561	0.2
Total	20,657	11,034	1.9

^a The New York Times FirstSearch Data Base coverage includes "international, national, business and New York regional news as well as sciences, medicine, arts, sports, and lifestyle news." The database is not a complete compendium of all articles published within any time period searched, and different search strategies could provide different results. The time period covered is not specifically defined but should reflect primarily recent publications.

^b Employment numbers in each profession compiled from the U.S. Bureau of Labor Statistics 1996 figures (U.S. Bureau of the Census 1997) and the 1998-99 Edition of the *Occupational Outlook Handbook* (U.S. Bureau of Labor Statistics 1998).

My first suggestion for making published accounting articles less tedious and boring is to impose strict word limits and to revise the "Notes" sections for shorter contributions. Word limits force authors to think much harder about how to communicate their essential ideas succinctly and greatly improve writing. Similarly, I would encourage accounting journals to follow *Nature* and provide guidelines for informative abstracts.²² A related suggestion is to follow the science journals, and more recently, *The American Economic Review*, by introducing online-only appendices to report the lengthy robustness sections that are demanded by persnickety reviewers.²³ In addition, I strongly encourage AAA journals to *require* authors to post online with each journal article the data sets and working computer code used to produce all tables as a condition for publication, so that other independent researchers can validate and replicate their studies ([Bernanke 2004](#); [McCullough and McKittrick 2009](#)).²⁴ This is important because recent surveys of science and

²² http://www.nature.com/nature/authors/gta/Letter_bold_para.doc

²³ <http://www.aeaweb.org/issue.php?doi=10.1257/aer.101.7>

²⁴ Bob Jensen tirelessly advocates for more replication studies, commentaries, data visualization, and other reforms in "accountings" and accounting research. See: <http://www.trinity.edu/rjensen/>

management researchers reveal that data fabrication, data falsification, and other violations in published studies is far from rare (Martinson et al. 2005; Bedeian et al. 2010).

I also urge that authors report results graphically rather than in tables, as recommended by numerous statistical experts (e.g., Tukey 1977; Chambers et al. 1983; Wainer 2009). For example, Figure 2 shows how the data in Figure 1 can be displayed more effectively without taking up more page space (Gelman et al. 2002). Scientific papers routinely display results in figures with confidence intervals rather than tables with standard errors and p-values, and accounting journals should adopt these practices to improve understandability. Soyer and Hogarth (2012) show experimentally that even well-trained econometricians forecast more slowly and inaccurately when given tables of statistical results than when given equivalent scatter plots. Most accounting researchers cannot recognize the main tables of Ball and Brown (1968) or Beaver (1968) on sight, but their iconic figures are etched in our memories. The figures in Burgstahler and Dichev (1997) convey their results far more effectively than tables would. Indeed, the finance professoriate was convinced that financial markets are efficient by the graphs in Fama et al. (1969), a highly influential paper that does not contain a single statistical test! Easton (1999) argues that the 1990s non-linear earnings-return relation literature would likely have been developed much earlier if accounting researchers routinely plotted their data. Since it is not always straightforward to convert tables into graphs (Gelman et al. 2002), I recommend that AAA pay for new editors of AAA journals to take courses in graphical presentation.

I would also recommend that AAA award an annual prize for the best figure or graphic in an accounting journal each year. In addition to making research articles easier to follow, figures ease the introduction of new ideas into accounting textbooks. Economics is routinely taught with diagrams and figures to aid intuition—demand and supply curves, IS-LM analysis, Edgeworth boxes, etc. (Blaug and Lloyd 2010). Accounting teachers would benefit if accounting researchers produced similar education tools. Good figures could also be used to adorn the cover pages of our journals similar to the best science journals; in many disciplines, authors of lead articles are invited to provide an illustration for the cover page. *JAMA (Journal of the American Medical Association)* reproduces paintings depicting doctors on its cover (Southgate 1996); AAA could print paintings of accountants and accounting on the cover of *The Accounting Review*, perhaps starting with those collected in Yamey (1989). If color printing costs are prohibitive, we could imitate the *Journal of Political Economy* back cover and print passages from literature where accounting and accountants play an important role, or even start a new format by reproducing cartoons illustrating accounting issues. The key point is to induce accountants to pick up each issue of the journal, irrespective of the research content.

I think that we need an accounting journal to “fill a gap between the general-interest press and most other academic journals,” similar to the *Journal of Economic Perspectives (JEP)*.²⁵ Unlike other economics journals, *JEP* editors and associate editors solicit articles from experts with the goal of conveying state-of-the-art economic thinking to non-specialists, including students, the lay public, and economists from other specialties.²⁶ The journal explicitly eschews mathematical notation or regression results and requires that results be presented either graphically or as a table of means. In response to the question “List the three economics journals (broadly defined) that you read most avidly when a new issue appears,” a recent survey of U.S. economics professors found that *Journal of Economic Perspectives* was their second favorite economics journal (Davis et al. 2011), which suggests that an unclaimed niche exists in accounting. Although *Accounting Horizons*

²⁵ <http://www.aeaweb.org/jep/submissions.php>

²⁶ *Annual Reviews of Financial Economics* is a new journal that invites experienced researchers in finance to critically review their sub-fields of current research. See: <http://www.annualreviews.org/journal/financial>

FIGURE 2

Relative Frequency of References to Accountants in *The New York Times* from [Ellenberg \(2000\)](#) as Displayed Graphically by [Gelman et al. \(2002\)](#)

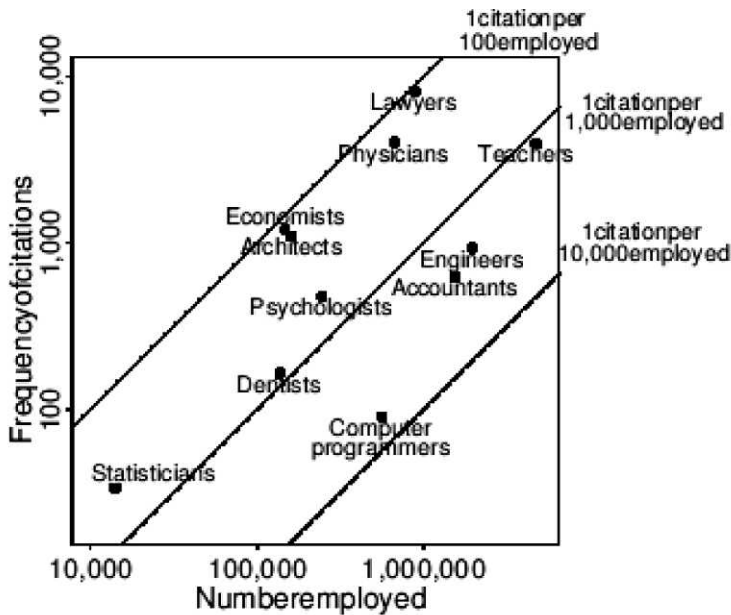


Figure 2. Top panel: Table from [Ellenberg \(2000\)](#) displays counts and rates of citations of various professions from the New York Times database. Bottom panel: Display as a figure shows the relative positions of the different professions much more clearly. The log-log display allows comparison across several orders of magnitude. The x and y axes are on the same scale, so that any 45° line indicates a constant relative frequency.

The top panel of the original figure is the table displayed in Figure 1.

could be restructured along these lines to better reach practitioners, it might make sense to start a new association-wide journal under the AAA aegis.

CONCLUSION

I believe that accounting is one of the most important human innovations. The invention of accounting records was likely indispensable to the emergence of agriculture, and ultimately, civilization (e.g., [Basu and Waymire 2006](#)). Many eminent historians view double-entry bookkeeping as indispensable for the Renaissance and the emergence of capitalism (e.g., [Sombart 1919](#); [Mises 1949](#); [Weber 1927](#)), possibly via stimulating the development of algebra ([Heffer 2011](#)). Sadly, accounting textbooks and the top U.S. accounting journals seem uninterested in whether and how accounting innovations changed history, or indeed in understanding the history of our current practices ([Zeff 1989](#)).

In short, the accounting academy embodies a “tragedy of the commons” ([Hardin 1968](#)) where strong extrinsic incentives to publish in “top” journals have led to misdirected research efforts. As

Zeff (1983) explains, “When modeling problems, researchers seem to be more affected by technical developments in the literature than by their potential to explain phenomena. So often it seems that manuscripts are the result of methods in search of questions rather than questions in search of methods.” Solving common problems requires strong collective action by the social network of accounting researchers using self-governing mechanisms (e.g., Ostrom 1990, 2005). Such initiatives should occur at multiple levels (e.g., school, association, section, region, and individual) to have any chance of success.

While accounting research has made advances in recent decades, our collective progress seems slow, relative to the hard work put in by so many talented researchers. Instead of letting financial economics and psychology researchers and accounting standard-setters choose our research methods and questions, we should return our focus to addressing fundamental issues in accounting. As important, junior researchers should be encouraged to take risks and question conventional academic wisdom, rather than blindly conform to the party line. For example, the current FASB–IASB conceptual framework “remains irreparably flawed” (Demski 2007), and accounting researchers should take the lead in developing alternative conceptual frameworks that better fit what accounting does (e.g., Ijiri 1983; Ball 1989; Dickhaut et al. 2010). This will entail deep historical and cross-cultural analyses rather than regression analyses on machine-readable data. Deliberately attacking the “fundamental and frequently asked questions” in accounting will require innovations in research outlooks and methods, as well as training in the history of accounting thought. It is shameful that we still cannot answer basic questions like “Why did anyone invent recordkeeping?” or “Why is double-entry bookkeeping beautiful?”

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