Working Paper 450.06

A Scrapbook on What's Wrong with the Past, Present and Future of Accountics Science

by Robert E. Jensen
rjensen@trinity.edu
http://www.trinity.edu/rjensen/

Abstract

For operational convenience I define accountics science as research that features equations and/or statistical inference. Historically, there was a heated debate in the 1920s as to whether the main research journal of academic accounting, *The Accounting Review* (TAR) that commenced in 1926, should be an accountics journal with articles that mostly featured equations. Practitioners and teachers of college accounting won that debate.

TAR articles and accountancy doctoral dissertations prior to the 1970s seldom had equations. For reasons summarized below, doctoral programs and TAR evolved to where in the 1990s there where having equations became virtually a necessary condition for a doctoral dissertation and acceptance of a TAR article. Qualitative normative and case method methodologies disappeared from doctoral programs.

What's really meant by "featured equations" in doctoral programs is merely symbolic of the fact that North American accounting doctoral programs pushed out most of the accounting to make way for econometrics and statistics that are now keys to the kingdom for promotion and tenure in accounting schools ---
http://www.trinity.edu/rjensen/Theory01.htm#DoctoralPrograms

The purpose of this paper is to make a case that the accountics science monopoly of our doctoral programs and published research is seriously flawed, especially its lack of concern about replication and focus on simplified artificial worlds that differ too much from reality to creatively discover findings of greater relevance to teachers of accounting and practitioners of accounting. Accountics scientists themselves became a Cargo Cult.

Introduction

Accountics is the mathematical science of values.
Charles Sprague (1887) as quoted by McMillan (1998, p. 1)

The style of much of this paper is different in that it has frequent and lengthy quotations from my Web site... It's largely a set of scrapbook postings drawn from thousands of postings on the topic of accountics science. Whereas most research papers briefly summarize a point and then cite its references, this paper quotes the original writers of the papers. The reason is that these writers in many cases are well-known scholars. They word the controversies of accountics and economic science (in some ways a pseudo-science not much interested in replications of findings) better than I could summarize it myself. I want the readers of the paper to hear those voices in their own words.

Accountics science has done more than commandeer the top academic research journals in academic accountancy. It literally took over North American accountancy doctoral programs, the most prestigious academic accounting research journals, and promotion and tenure committees of top universities in North America.

Recommendation 2 of the American Accounting Association Pathways Commission (emphasis added)

Scrapbook1283--- http://www.trinity.edu/rjensen/TheoryTar.htm#Scrapbook1283

The Pathways Commission Implementing Recommendations for the Future of Accounting Education: The First Year Update
American Accounting Association
August 2013
http://commons.aaahq.org/files/3026eae0b3/Pathways_Update_FIN.pdf
Page 109 (emphasis added)

> *Promote accessibility of doctoral education by allowing for flexible content and structure in doctoral programs and developing multiple pathways for degrees. The current path to an accounting Ph.D. includes lengthy, full-time residential programs and research training that is for the most part confined to quantitative rather than qualitative methods. More flexible programs -- that might be part-time, focus on applied research and emphasize training in teaching methods and curriculum development -- would appeal to graduate students with professional experience and candidates with families,*

according to the report.
http://commons.aaahq.org/groups/2d690969a3/summary

From an operational standpoint I will define an accountics article as one that features equations and/or statistical inference testing tables. The article can be inferential or analytical or both. The article can be a case or even a field study although in most instances accountics articles are neither cases nor field studies. It would be a rare find over the past three decades to find an accounting doctoral dissertation that does not feature equations, including dissertations on accountancy history.

What started out as good intentions by accounting quants add rigor and respectability to academic accounting research became pushed to a fault by forcing out non-mathematical methodologies that are more often than not suited for complicated research problems that are just not conducive to accountics science.

I like a lot of accountics science journal articles and books and cite their findings frequently. It's not that accountics science is a bad thing in academic accounting research; however, it should not have become such a monopoly takeover of accountancy doctoral programs, top journals, and promotion and tenure requirements.

Accountics Science History

From an operational standpoint I will define an accountics science article as an accounting research article featuring equations and/or statistical inference testing tables. The article can be inferential or deductive (analytical) or both. The article can be a case or even a field study although in most instances accountics science articles are neither cases nor field studies. The distinction between normative and scientific research does not work well in accountics science articles that used analytical mathematics since these are often normative even though they differ from normative research articles that have no mathematics or statistics.

In 2007 Jean Heck and I wrote the following article. "An Analysis of the Evolution of Research Contributions by *The Accounting Review:* 1926-2005," *Accounting Historians Journal*, Volume 34, No. 2, December 2007, pp. 109-142.

Scrapbook1064 --- http://www.trinity.edu/rjensen/395wpTAR/Web/TAR395wp.htm#Scrapbook1064

"**An Analysis of the Evolution of Research Contributions by *The Accounting Review*: 1926-2005**," by Jean Heck and Robert E. Jensen, *Accounting Historians Journal*, Volume 34, No. 2, December 2007 Page 117

> Accounting professor Charles Sprague of Columbia University (then called Columbia College) coined the word "accountics" in 1887. The word is not used today in accounting and has some alternative meanings outside our discipline. However, in the early 20th century, accountics was the centerpiece of some unpublished lectures by Sprague. McMillan (1998, p. 11) stated the following:
>
> These claims were not a pragmatic strategy to legitimize the development of sophisticated bookkeeping theories. Rather, this development of a science was seen as revealing long-hidden realities within the economic environment and the double-entry bookkeeping system itself. The science of accounts, through systematic mathematical analysis, could discover hidden thrust of the reality of economic value. The term "accountics" captured the imagination of the members of the IA, connoting advances in bookkeeping that all these men were experiencing.

By 1900 a short-lived journal called *Accountics* emerged (Forrester, 2003). More importantly, the American Association of University Instructors of Accounting that became the American Accounting Association (AAA) in 1935, in the early 1920s had heated debates over whether the emerging journal called *The Accounting Review* (TAR) should be a journal of accountics versus being a journal devoted to a more widespread membership of accounting teachers and practitioners.

Accountics advocates lost when in 1926 TAR was <u>not</u> restricted to accountics articles with equations.)

Scrapbook1067 --- http://www.trinity.edu/rjensen/395wpTAR/Web/TAR395wp.htm#Scrapbook1067

"**An Analysis of the Evolution of Research Contributions by *The Accounting Review*: 1926-2005**," by Jean Heck and Robert E. Jensen, *Accounting Historians Journal*, Volume 34, No. 2, December 2007 Page 117

> Following World War II, practitioners outnumbered educators in the AAA (Chatfield 1975, p. 4). Leading partners from accounting firms took pride in publishing papers and books intended to inspire scholarship among professors and students. Over the years, some practitioners, particularly those with scholarly publications, were admitted into the Accounting Hall of Fame founded by The Ohio State University. Prior to the 1960s, accounting educators were generally long on practical experience and short on academic credentials such as doctoral degrees.
>
> A major catalyst for change in accounting research occurred when the Ford Foundation poured millions of dollars into the study of collegiate business schools and the funding of doctoral programs and students in business studies. Gordon and Howell (1959) reported that business faculty in colleges lacked research skills and academic esteem when compared to their colleagues in the sciences. The Ford Foundation thereafter provided funding for doctoral programs and for top quality graduate students to pursue doctoral degrees in business and accountancy. The Foundation even funded publication of selected doctoral dissertations to give doctoral studies in business more visibility. Great pressures were also brought to bear on academic associations like the AAA to increase the scientific standards for publications in journals like TAR.

In the above article we re-introduced the historical concept of accountics that for simplicity sake will simply be defined here as accounting research reported with mathematical tables and/or statistical inference tables.

Quotation 3 from Heck and Jensen, (2007, p. 119)

Scrapbook1069 --- http://www.trinity.edu/rjensen/395wpTAR/Web/TAR395wp.htm#Scrapbook1069

"**An Analysis of the Evolution of Research Contributions by *The Accounting Review*: 1926-2005**," by Jean Heck and Robert E. Jensen, *Accounting Historians Journal*, Volume 34, No. 2, December 2007 Page 117

> A perfect storm for change in accounting research arose in the late 1950s and early1960s. First came the critical Pierson Carnegie Report (1959) and the Gordon and Howell Ford Foundation Report

(1959). Shortly thereafter, the AACSB introduced a requirement requiring that a certain percentage of faculty possess doctoral degrees for business education programs seeking accreditation (Bricker and Previts, 1990). Soon afterwards, both a doctorate and publication in top accounting research journals became necessary for tenure (Langenderfer, 1987).

A second component of this perfect storm for change was the proliferation of mainframe computers, the development of analytical software (e.g., early SPSS for mainframes), and the dawning of management and decision "sciences." The third huge stimulus for changed research is rooted in portfolio theory discovered by Harry Markowitz in1952 that became the core of his dissertation at Princeton University, which was published in book form in 1959. This theory eventually gave birth to the Nobel Prize winning Capital Asset Pricing Model (CAPM) and a new era of capital market research. A fourth stimulus was when the CRSP stock price tapes became available from the University of Chicago. The availability of CRSP led to a high number of TAR articles on capital market event studies (e.g., earnings announcements on trading prices and volumes) covering a period of nearly 40 years.

This "perfect storm" roared into nearly all accounting and finance research and turned academic accounting research into an accountics-centered science of values and mathematical/statistical analysis. After 1960, there was a shift in TAR, albeit slow at first, toward preferences for quantitative model building --- econometric models in capital market studies, time series models in forecasting, advanced calculus information science, information economics, analytical models, and psychometric behavioral models. Chatfield (1975, p. 6) wrote the following:

> Beginning in the 1960s the Review published many more articles by non-accountants, whose contribution involved showing how ideas or methods from their own discipline could be used to solve particular accounting problems. The more successful adaptations included matrix theory, mathematical model building, organization theory, linear programming, and Bayesian analysis.

In the 21st Century the heroes of academic accounting research and those that dominate virtually all AACSB-accredited accounting school doctoral programs are virtually all accountics scientists.

Why Accountics Science Dominates Accounting Research

Researchers tend to look for answers where the looking is good,
rather than where the answers are likely to be hiding
David H. Freeman, *Discover Magazine*, December 10, 2013.
http://discovermagazine.com/2010/jul-aug/29-why-scientific-studies-often-wrong-streetlight-effect#.UnpmVRBjU3g

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."
Granof and Zeff --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Appendix01

My theory is that accountics science gained dominance in accounting research, especially in North American accounting Ph.D. programs, because it was easier to abrogate data collection responsibility and not have to engage in data collection drudgery:

1. .Most accountics scientists buy data, thereby avoiding the greater cost and drudgery of collecting data.

2. By relying so heavily on purchased data, accountics scientists abdicate responsibility for errors in the data.

3. Since adding missing variable data to the public database is generally not at all practical in purchased databases, accountics scientists have an excuse for not collecting missing variable data.

4. Software packages for modeling and testing data abound. Accountics researchers need only feed purchased data into the hopper of statistical and mathematical analysis programs. It still takes a lot of knowledge and creativity to formulate hypotheses and to invent and understand complex models. But the really hard work of collecting data and error checking is avoided by purchasing data.

I scanned all six issues of *The Accounting Review* (TAR) published in 2013 to detect what public databases were (usually at relatively heavy fees for a system of databases) in the 72 articles published in 2013 in *The Accounting Review* (TAR). The outcomes were as follows:

Databases

28	30.8%	Miscellaneous public databases used infrequently
27	29.7%	Compustat --- http://en.wikipedia.org/wiki/Compustat
18	19.8%	CRSP --- http://en.wikipedia.org/wiki/Center_for_Research_in_Security_Prices
13	14.3%	Datastream --- http://en.wikipedia.org/wiki/Thomson_Financial
05	5.5%	Audit Analytics --- http://www.auditanalytics.com/
91	100.0%	Total Purchased Public Databases
8		Non-public Databases (usually experiments) and mathematical analysis studies with no data
0		Non-accountics articles that do not feature equations

Many of these 72 articles by TAR in 2013 used more than one public database, and when the Compustat and CRSP joint database was used I counted one for the Compustat Database and one for the CRSP Database. Most of the non-public databases are behavioral experiments using students as surrogates for real-world decision makers.

My opinion is that 2013 is a typical year where over 90% of the articles published in TAR used public databases. The good news is that most of these public databases are enormous, thereby allowing for huge samples for which statistical inference is probably superfluous. For very large samples even miniscule differences are significant for hypothesis testing making statistical inference testing superfluous: The bad news is that the research findings are that findings are limited to only the variables that happen to be in the purchased databases that may overlook important causal factor variables not in the data. “Measurement effort abounds” while accountics scientists continue on paths of “comfortable path of sameness.”

Scrapbook5532---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5532

"**Some Thoughts About Accounting Scholarship**," by Joel Demski, AAA President's Message, *Accounting Education News*, Fall 2001
http://aaahq.org/pubs/AEN/2001/Fall2001.pdf (Emphasis added)

> . . .
> A second indicator is our journals. They have proliferated in number. But we struggle with an intertemporal sameness, with incremental as opposed to discontinuous attempts to move our thinking forward, and with referee intrusion and voyeurism. Value relevance is a currently fashionable approach to identifying statistical regularities in the financial market arena, just as a focus on readily observable components of compensation is a currently fashionable dependent variable in the compensation arena. Yet we know measurement error abounds, that other sources of information are both present and hardly unimportant, that compensation is broad-based and intertemporally managed, and that compensating wage differentials are part of the stew. Yet we continue on the comfortable path of sameness.
>
> Continued in article

Scrapbook1101 --- www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1101

"**Accounting Scholarship that Advances Professional Knowledge and Practice**," AAA Presidential Scholar Address by Robert S. Kaplan, *The Accounting Review*, March 2011, pp. 372-373 (emphasis added)

> *I am less pessimistic than Schön about whether rigorous research can inform professional practice (witness the important practical significance of the Ohlson accounting-based valuation model and the Black-Merton-Scholes options pricing model), but I concur with the general point that academic scholars spend too much time at the top of Roethlisberger's knowledge tree and too little time performing systematic observation, description, and classification, which are at the foundation of knowledge creation. Henderson 1970, 67–68 echoes the benefits from a more balanced approach based on the experience of medical professionals:*
>
> > . . . both theory and practice are necessary conditions of understanding, and the method of Hippocrates is the only method that has ever succeeded widely and generally. The first element of that method is hard, persistent, intelligent, responsible, unremitting labor in the sick room, not in the library … The second element of that method is accurate observation of things and events, selection, guided by judgment born of familiarity and experience, of the salient and the recurrent phenomena, and their classification and methodical exploitation. The third element of that method is the judicious construction of a theory … and the use thereof … [T]he physician must have, first, intimate, habitual, intuitive familiarity with things, secondly, systematic knowledge of things, and thirdly an effective way of thinking about things.

The enormous problem of relying on purchased public databases for so much accounting research is that it restrains diversity of research topics and target audiences. Academic accountancy researchers seem to speak above the accountancy profession rather than at it.

Quotation 4 from Heck and Jensen, (2007, P. 117)

Scrapbook1061 --- http://www.trinity.edu/rjensen/395wpTAR/Web/TAR395wp.htm#Scrapbook1061

"**An Analysis of the Evolution of Research Contributions by *The Accounting Review*: 1926-2005**," by Jean Heck and Robert E. Jensen, *Accounting Historians Journal*, Volume 34, No. 2, December 2007 Page 117

Jensen Comment
In her Presidential Message to the American Accounting Association (AAA) in August, 2005, Judy Rayburn discussed the issue of the relatively low citation rate of accounting research compared to citation rates for research in finance, management, and marketing. Rayburn concluded that the low citation rate for accounting research was due to a lack of diversity in topics and research methods. In this paper, we provide a review of the AAA's flagship journal, *The Accounting Review* (TAR), following its 80 years of publication and describe why some recent AAA leaders believe that significant changes should be made to the journal's publication and editorial policies. At issue is whether scholarly accounting research is overly focused on mathematical

Analysis and empirical research, or "accountics" as it has sometimes been called, at the expense of research that benefits the general practice of accountancy and discovery research on more interesting topics. We conclude from our review of TAR that after mostly publishing research about accounting practices for the first 40 years, a sweeping change in editorial policy occurred in the 1960s and 1970s that narrowly defined scholarly research in accounting as that which employs accountics.
Rayburn, J.D. (2006) "President's Message," *Accounting Education News* 33 (1): 1-4

Sophisticated Data Analysis Cannot Make Up for Missing Data

"You can disguise charlatanism under the weight of equations, and nobody can catch you since there is no such thing as a controlled experiment."
Nassim Nicholas Taleb, 2004

. . . The soft sciences usually cannot account for more than a quarter to a third of the factors that contribute to variation in their experiments on a good day. The rest of the variation is due to unknown factors that remain outside the purview of the investigator. That certainly makes these results "softer" than those of the hard sciences.
Pigliucci (p. 17)

David Johnstone posted the following message on the AECM Listserv on November 19, 2013:

Scrapbook1241 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1241

An interesting aspect of all this is that there is a widespread a priori or learned belief in empirical research that all and only what you have to do to get meaningful results is to get data and run statistics packages, and that the more advanced the stats the better. It's then just a matter of turning the handle. Admittedly it takes a lot of effort to get very proficient at this kind of work, but the presumption that it will naturally lead to reliable knowledge is an act of faith, like a religious tenet. What needs to be taken into account is that the human systems (markets, accounting reporting, asset pricing etc.) are madly complicated and likely changing structurally continuously. So even with the best intents and best methods, there is no guarantee of reliable or lasting findings a priori, no matter what "rigor" has gone in.

Part and parcel of the presumption that empirical research methods are automatically "it" is the even stronger position that no other type of work is research. I come across this a lot. I just had a 4th year Honor student do his thesis, he was particularly involved in the superannuation/pension fund industry, and he did a lot of good practical stuff, thinking about risks that different fund allocations present, actuarial life expectancies etc. The two young guys (late 20s) grading this thesis, both excellent thinkers and not zealots about anything, both commented to me that the thesis was weird and was not really a thesis like they would have assumed necessary (electronic data bases with regressions etc.). They were still generous in their grading, and the student did well, and it was only their obvious astonishment that there is any kind of worthy work other than the formulaic-empirical that astonished me. This represents a real narrowing of mind in academe, almost like a tendency to dark age, and cannot be good for us long term. In Australia the new push is for research "impact", which seems to include industry relevance, so that presents a hope for a cultural widening.

I have been doing some work with a lawyer-PhD student on valuation in law cases/principles, and this has caused similar raised eyebrows and genuine intrigue with young colleagues – they just have never heard of such stuff, and only read the journals/specific papers that do what they do. I can sense their interest, and almost envy of such freedom, as they are all worrying about how to compete and make a long term career as an academic in the new academic world.

Scrapbook1261 --- http://www.trinity.edu/rjensen/TheoryTar.htm#Scrapbook1261

Warnings from a Theoretical Physicist About Economics and Finance
"**Beware of Economists Peddling Elegant Models**," by Mark Buchanan, *Bloomberg*, April 7, 2013 --- http://www.bloomberg.com/news/2013-04-07/beware-of-economists-peddling-elegant-models.html
Emphasis added

. . .

In one very practical and consequential area, though, the allure of elegance has exercised a perverse and lasting influence. For several decades, economists have sought to express the way millions of people and companies interact in a handful of pretty equations.

The resulting mathematical structures, known as dynamic stochastic general equilibrium models, seek to reflect our messy reality without making too much actual contact with it. They assume that economic trends emerge from the decisions of only a few "representative" agents -- one for households, one for firms, and so on. The agents are supposed to plan and act in a rational way, considering the probabilities of all possible futures and responding in an optimal way to unexpected shocks.

Surreal Models

Surreal as such models might seem, they have played a significant role in informing policy at the world's largest central banks. Unfortunately, they don't work very well, and they proved spectacularly incapable of accommodating the way markets and the economy acted before, during and after the recent crisis.

Now, some economists are beginning to pursue a rather obvious, but uglier, alternative. Recognizing that an economy consists of the actions of millions of individuals and firms thinking, planning and perceiving things differently, they are trying to model all this messy behavior in considerable detail. Known as agent-based computational economics, the approach is showing promise.

Take, for example, a 2012 (and still somewhat preliminary) study by a group of economists, social scientists, mathematicians and physicists examining the causes of the housing boom and subsequent collapse from 2000 to 2006. Starting with data for the Washington D.C. area, the study's authors built up a computational model mimicking the behavior of more than two million potential homeowners over more than a decade. The model included detail on each individual at the level of race, income, wealth, age and marital status, and on how these characteristics correlate with home buying behavior.

Led by further empirical data, the model makes some simple, yet plausible, assumptions about the way people behave. For example, homebuyers try to spend about a third of their annual income on housing, and treat any expected house-price appreciation as income. Within those constraints, they borrow as much money as lenders' credit standards allow, and bid on the highest-value houses they can. Sellers put their houses on the market at about 10 percent above fair market value, and reduce the price gradually until they find a buyer.

The model captures things that dynamic stochastic general equilibrium models do not, such as how rising prices and the possibility of refinancing entice some people to speculate, buying more-expensive houses than they otherwise would. The model accurately fits data on the housing market over the period from 1997 to 2010 (not surprisingly, as it was designed to do so). More interesting, it can be used to probe the deeper causes of what happened.

Consider, for example, the assertion of some prominent economists, such as Stanford University's John Taylor, that the low-interest-rate policies of the Federal Reserve were to blame for the housing bubble. Some dynamic stochastic general equilibrium models can be used to support this view. The agent-based model, however, suggests that interest rates weren't the primary driver: If you keep rates at higher levels, the boom and bust do become smaller, but only marginally.

Leverage Boom

A much more important driver might have been leverage -- that is, the amount of money a homebuyer could borrow for a given down payment. In the heady days of the housing boom, people were able to borrow as much as 100 percent of the value of a house -- a form of easy credit that had a big effect on housing demand. In the model, freezing leverage at historically normal levels completely eliminates both the housing boom and the subsequent bust.

Does this mean leverage was the culprit behind the subprime debacle and the related global financial crisis? Not necessarily. The model is only a start and might turn out to be wrong in important ways. That said, it makes the most convincing case to date (see my blog for more detail), and it seems likely that any stronger case will have to be based on an even deeper plunge into the messy details of how people behaved. It will entail more data, more agents, more computation and less elegance.

If economists jettisoned elegance and got to work developing more realistic models, we might gain a better understanding of how crises happen, and learn how to anticipate similarly unstable episodes in the future. The theories won't be pretty, and probably won't show off any clever mathematics. But we ought to prefer ugly realism to beautiful fantasy.

(Mark Buchanan, a theoretical physicist and the author of "The Social Atom: Why the Rich Get Richer, Cheaters Get Caught and Your Neighbor Usually Looks Like You," is a Bloomberg View columnist. The opinions expressed are his own.)

Drawing Inferences from Very Large Data-Sets

Scrapbook5133---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5133

David Johnstone sent the following on the AECM Listserv:

> Indeed if you hold H_0 the same and keep changing the model, you will eventually (generally soon) get a significant result, allowing rejection of H_0 at 5%, not because H0 is necessarily false but because you have built upon a false model (of which there are zillions, obviously).

"**Drawing Inferences From Very Large Data-Sets**," by David Giles, E*conometrics Beat: Dave GilesBlog*, University of Victoria, April 26, 2013 ---
http://davegiles.blogspot.ca/2011/04/drawing-inferences-from-very-large-data.html
Emphasis Added

> . . .
>
> Granger (**1998**; **2003**) has reminded us that i**f the sample size is sufficiently large, then it's virtually impossible *not* to reject almost any hypothesis.** So, if the sample is very large and the *p*-values associated with the estimated coefficients in a regression model are of the order of, say, 0.10 or even 0.05, then this really **bad** news. Much, much, smaller *p*-values are needed before we get all excited about 'statistically significant' results when the sample size is in the thousands, or even bigger. So, the *p*-values reported above are mostly pretty marginal, as far as significance is concerned. When you work out the *p*-values for the other 6 models I mentioned, they range from to 0.005 to 0.460. I've been generous in the models I selected.
>
> Here's another set of results taken from a second, really nice, paper by **Ciecieriski *et al***. (2011) in the same issue of *Health Economics*:
>
> Continued in article

Jensen Comment
My research suggest that over 90% of the recent papers published in TAR use purchased databases that provide enormous sample sizes in those papers. **Their accountics science authors keep reporting those meaningless levels of statistical significance.**

What is even worse is when meaningless statistical significance tests are used to support decisions.

"**Statistical Significance - Again** " by David Giles, E*conometrics Beat: Dave Giles� Blog*, University of Victoria, December 28, 2013 ---
http://davegiles.blogspot.com/2013/12/statistical-significance-again.html

> **Statistical Significance - Again**
>
> With all of this emphasis on "Big Data", I was pleased to see **this post** on the *Big Data Econometrics* blog, today.

When you have a sample that runs to the thousands (billions?), the conventional significance levels of 10%, 5%, 1% are completely inappropriate. You need to be thinking in terms of *tiny* significance levels.

I discussed this in some detail back in April of 2011, in a post titled, "Drawing Inferences From Very Large Data-Sets". If you're of those (many) applied researchers who uses large cross-sections of data, and then sprinkles the results tables with asterisks to signal "significance" at the 5%, 10% levels, *etc.*, then I urge you read that earlier post.

It's sad to encounter so many papers and seminar presentations in which the results, in reality, are totally insignificant!

How Standard Error Costs Us Jobs, Justice, and Lives, by Stephen T. Ziliak and Deirdre N. McCloskey (Ann Arbor: University of Michigan Press, ISBN-13: 978-472-05007-9, 2007)
http://www.cs.trinity.edu/~rjensen/temp/DeirdreMcCloskey/StatisticalSignificance01.htm

Page 206
Like scientists today in medical and economic and other sizeless sciences, Pearson mistook a large sample size for the definite, substantive significance---evidence s Hayek put it, of "wholes." But it was as Hayek said "just an illusion." Pearson's columns of sparkling asterisks, though quantitative in appearance and as appealing a is the simple truth of the sky, signified nothing.

pp. 250-251
The textbooks are wrong. The teaching is wrong. The seminar you just attended is wrong. The most prestigious journal in your scientific field is wrong.

You are searching, we know, for ways to avoid being wrong. Science, as Jeffreys said, is mainly a series of approximations to discovering the sources of error. Science is a systematic way of reducing wrongs or can be. Perhaps you feel frustrated by the random epistemology of the mainstream and don't know what to do. Perhaps you've been sedated by significance and lulled into silence. Perhaps you sense that the power of a Roghamsted test against a plausible Dublin alternative is statistically speaking low but you feel oppressed by the instrumental variable one should dare not to wield. Perhaps you feel frazzled by what Morris Altman (2004) called the "social psychology rhetoric of fear," the deeply embedded path dependency that keeps the abuse of significance in circulation. You want to come out of it. But perhaps you are cowed by the prestige of Fisherian dogma. Or, worse thought, perhaps you are cynically willing to be corrupted if it will keep a nice job

Bob Jensen's threads on the often way analysts, particularly accountics scientists, often cheer for statistical significance of large sample outcomes that praise statistical significance of insignificant results such as R^2

The Insignificance of Testing the Null

Scrapbook5143---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5143

"**Statistics: reasoning on uncertainty, and the insignificance of testing null**," by Esa Läärä
Ann. Zool. Fennici 46: 138–157
ISSN 0003-455X (print), ISSN 1797-2450 (online)
Helsinki 30 April 2009 © Finnish Zoological and Botanical Publishing Board 200
http://www.sekj.org/PDF/anz46-free/anz46-138.pdf

> The practice of statistical analysis and inference in ecology is critically reviewed. The dominant doctrine of null hypothesis significance testing (NHST) continues to be applied ritualistically and mindlessly. This dogma is based on superficial understanding of elementary notions of frequentist statistics in the 1930s, and is widely disseminated by influential textbooks targeted at biologists. It is characterized by silly null hypotheses and mechanical dichotomous division of results being “significant” ($P < 0.05$) or not. Simple examples are given to demonstrate how distant the prevalent NHST malpractice is from the current mainstream practice of professional statisticians. Masses of trivial and meaningless “results” are being reported, which are not providing adequate quantitative information of scientific interest. The NHST dogma also retards progress in the understanding of ecological systems and the effects of management programmes, which may at worst contribute to damaging decisions in conservation biology. In the beginning of this millennium, critical discussion and debate on the problems and shortcomings of NHST has intensified in ecological journals. Alternative approaches, like basic point and interval estimation of effect sizes, likelihood-based and information theoretic methods, and the Bayesian inferential paradigm, have started to receive attention. Much is still to be done in efforts to improve statistical thinking and reasoning of ecologists and in training them to utilize appropriately the expanded statistical toolbox. Ecologists should finally abandon the false doctrines and textbooks of their previous statistical gurus. Instead they should more carefully learn what leading statisticians write and say, collaborate with statisticians in teaching, research, and editorial work in journals.

Jensen Comment
And to think Alpha (Type 1) error is the easy part. Does anybody ever test for the more important Beta (Type 2) error? Some engineers test for Type 2 error with Operating Characteristic (OC) curves, but these are generally applied where controlled experiments are highly repeatable such as in quality control testing.

Can You Really Test for Multicollinearity?

Scrapbook5148---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5148

Multicollinearity --- http://en.wikipedia.org/wiki/Multicollinearity

"C**an You Actually TEST for Multicollinearity?**" by David Giles, E*conometrics Beat: Dave Giles' Blog*, University of Victoria, June 24, 2013 ---
http://davegiles.blogspot.com/2013/06/can-you-actually-test-for.html

. . .
Now, let's return to the "problem" of multicollinearity.

What do we mean by this term, anyway? This turns out to be the key question!

Multicollinearity is a phenomenon associated with our particular *sample of data* when we're trying to estimate a regression model. Essentially, it's a situation where there is insufficient information in the *sample of data* to enable us to enable us to draw "reliable" inferences about the individual parameters of the underlying (population) model.

I'll be elaborating more on the "informational content" aspect of this phenomenon in a follow-up post. Yes, there are various sample measures that we can compute and report, to help us gauge how severe this data "problem" may be. But they're not statistical *tests*, in any sense of the word

Because multicollinearity is a characteristic of the *sample*, and not a characteristic of the *population*, you should immediately be suspicious when someone starts talking about "testing for multicollinearity". Right?

Apparently not everyone gets it!

There's an old paper by Farrar and Glauber (1967) which, on the face of it might seem to take a different stance. In fact, if you were around when this paper was published (or if you've bothered to actually read it carefully), you'll know that this paper makes two contributions. First, it provides a very sensible discussion of what multicollinearity is all about. Second, the authors take some well known results from the statistics literature (notably, by Wishart, 1928; Wilks, 1932; and Bartlett, 1950) and use them to give "tests" of the hypothesis that the regressor matrix, X, is orthogonal.

How can this be? Well, there's a simple explanation if you read the Farrar and Glauber paper carefully, and note what assumptions are made when they "borrow" the old statistics results. Specifically, there's an explicit (and necessary) assumption that in the *population* the X matrix is *random*, and that it follows a *multivariate normal* distribution.

This assumption is, of course totally at odds with what is usually assumed in the linear regression model! The "tests" that Farrar and Glauber gave us aren't really tests of multicollinearity *in the sample*. Unfortunately, this point wasn't fully appreciated by everyone.

There are some sound suggestions in this paper, including looking at the sample *multiple* correlations between each regressor, and all of the other *regressors*. These, and other sample measures such as variance inflation factors, are useful from a diagnostic viewpoint, but they don't constitute tests of "zero multicollinearity".

So, why am I even mentioning the Farrar and Glauber paper now?

Well, I was intrigued to come across some STATA code (Shehata, 2012) that allows one to implement the Farrar and Glauber "tests". I'm not sure that this is really very helpful. Indeed, this seems to me to be a great example of applying someone's results without understanding (bothering to read?) the assumptions on which they're based!

Be careful out there - and be highly suspicious of strangers bearing gifts!

It's relatively uncommon for accountics scientists to criticize each others' published works. A notable exception is as follows:
"**Selection Models in Accounting Research**," by Clive S. Lennox, Jere R. Francis, and Zitian Wang, *The Accounting Review*, March 2012, Vol. 87, No. 2, pp. 589-616.
Emphasis Added

> This study explains the challenges associated with the Heckman (1979) procedure to control for selection bias, assesses the quality of its application in accounting research, and offers guidance for better implementation of selection models. A survey of 75 recent accounting articles in leading journals reveals that many researchers implement the technique in a mechanical way with relatively little appreciation of important econometric issues and problems surrounding its use. Using empirical examples motivated by prior research, we illustrate that selection models are fragile and can yield quite literally any possible outcome in response to fairly minor changes in model specification. We conclude with guidance on how researchers can better implement selection models that will provide more convincing evidence on potential selection bias, including the need to justify model specifications and careful sensitivity analyses with respect to **robustness and multicollinearity**.
>
> . . .
>
> CONCLUSIONS
>
> Our review of the accounting literature indicates that some studies have implemented the selection model in a questionable manner. Accounting researchers often impose ad hoc exclusion restrictions or no exclusion restrictions whatsoever. Using empirical examples and a replication of a published study, we demonstrate that such practices can yield results that are too fragile to be considered reliable. In our empirical examples, a researcher could obtain quite literally any outcome by making relatively minor and apparently innocuous changes to the set of exclusionary variables, including choosing a null set. One set of exclusion restrictions would lead the researcher to conclude that selection bias is a significant problem, while an alternative set involving rather minor changes would give the opposite conclusion. Thus, claims about the existence and direction of selection bias can be sensitive to the researcher's set of exclusion restrictions.
>
> Our examples also illustrate that the selection model is vulnerable to high levels of multicollinearity, which can exacerbate the bias that arises when a model is misspecified (Thursby 1988). Moreover, the potential for misspecification is high in the selection model because inferences about the existence and direction of selection bias depend entirely on the researcher's assumptions about the appropriate

functional form and exclusion restrictions. In addition, high multicollinearity means that the statistical insignificance of the inverse Mills' ratio is not a reliable guide as to the absence of selection bias. Even when the inverse Mills' ratio is statistically insignificant, inferences from the selection model can be different from those obtained without the inverse Mills' ratio. In this situation, the selection model indicates that it is legitimate to omit the inverse Mills' ratio, and yet, omitting the inverse Mills' ratio gives different inferences for the treatment variable because multicollinearity is then much lower.

In short, researchers are faced with the following trade-off. On the one hand, selection models can be fragile and suffer from multicollinearity problems, which hinder their reliability. On the other hand, the selection model potentially provides more reliable inferences by controlling for endogeneity bias if the researcher can find good exclusion restrictions, and if the models are found to be robust to minor specification changes. The importance of these advantages and disadvantages depends on the specific empirical setting, so it would be inappropriate for us to make a general statement about when the selection model should be used. Instead, researchers need to critically appraise the quality of their exclusion restrictions and assess whether there are problems of fragility and multicollinearity in their specific empirical setting that might limit the effectiveness of selection models relative to OLS.

Another way to control for unobservable factors that are correlated with the endogenous regressor (D) is to use panel data. Though it may be true that many unobservable factors impact the choice of D, as long as those unobservable characteristics remain constant during the period of study, they can be controlled for using a fixed effects research design. In this case, panel data tests that control for unobserved differences between the treatment group (D = 1) and the control group (D = 0) will eliminate the potential bias caused by endogeneity as long as the unobserved source of the endogeneity is time-invariant (e.g., Baltagi 1995; Meyer 1995; Bertrand et al. 2004). The advantages of such a difference-in-differences research design are well recognized by accounting researchers (e.g., Altamuro et al. 2005; Desai et al. 2006; Hail and Leuz 2009; Hanlon et al. 2008). As a caveat, however, we note that the time-invariance of unobservables is a strong assumption that cannot be empirically validated. Moreover, the standard errors in such panel data tests need to be corrected for serial correlation because otherwise there is a danger of over-rejecting the null hypothesis that D has no effect on Y (Bertrand et al. 2004).10

Finally, we note that there is a recent trend in the accounting literature to use samples that are matched based on their propensity scores (e.g., Armstrong et al. 2010; Lawrence et al. 2011). An advantage of propensity score matching (PSM) is that there is no MILLS variable and so the researcher is not required to find valid Z variables (Heckman et al. 1997; Heckman and Navarro-Lozano 2004). However, such matching has two important limitations. First, selection is assumed to occur only on observable characteristics. That is, the error term in the first stage model is correlated with the independent variables in the second stage (i.e., u is correlated with X and/or Z), but there is no selection on unobservables (i.e., u and υ are uncorrelated). In contrast, the purpose of the selection model is to control for endogeneity that arises from unobservables (i.e., the correlation between u and υ). Therefore, propensity score matching should not be viewed as a replacement for the selection model (Tucker 2010).

A second limitation arises if the treatment variable affects the company's matching attributes. For example, suppose that a company's choice of auditor affects its subsequent ability to raise external capital. This would mean that companies with higher quality auditors would grow faster. Suppose also that the company's characteristics at the time the auditor is first chosen cannot be observed. Instead, we match at some stacked calendar time where some companies have been using the same auditor for 20 years and others for not very long. Then, if we matched on company size, we would be throwing out the companies that have become large because they have benefited from high-quality audits. Such companies do not look like suitable “matches,” insofar as they are much larger than the companies in the control group that have low-quality auditors. In this situation, propensity matching could bias toward a non-result because the treatment variable (auditor choice) affects the company's matching attributes (e.g., its size). It is beyond the scope of this study to provide a more thorough assessment of the advantages and disadvantages of propensity score matching in accounting applications, so we leave this important issue to future research.

Models That Aren't Robust

Scrapbook5154---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5154

"**Allegory of the Cave**"
Those not familiar with Plato's Cave should take a look at
http://en.wikipedia.org/wiki/Plato%27s_Cave

The phrase is most often used to distinguish assumed (shadow) worlds that differ in usually important ways from the real world such as when economists assume steady-state conditions, equilibrium conditions, corporate utility functions, etc.

The Gaussian Copula function blamed for the collapse of the economy in 2007 is an example of a derivation in Plato's Cave that was made operational inappropriately by Wall Street Investment Banks:

"**In Plato's Cave**: Mathematical models are a powerful way of predicting financial markets. But they are fallible" *The Economist*, January 24, 2009, pp. 10-14 ---
http://www.trinity.edu/rjensen/2008Bailout.htm#Bailout

"**ECONOMICS AS ROBUSTNESS ANALYSIS**," by Jaakko Kuorikoski, Aki Lehtinen and Caterina Marchionn, he University of Pittsburgh, 2007 ---
http://philsci-archive.pitt.edu/3550/1/econrobu.pdf

ECONOMICS AS ROBUSTNESS ANALYSIS
Jaakko Kuorikoski, Aki Lehtinen and Caterina Marchionni
25.9. 2007

1.**Introduction**
Modern economic analysis consists largely in building abstract mathematical models and deriving

familiar results from ever sparser modeling assumptions is considered as a theoretical contribution. Why do economists spend so much time and effort in deriving same old results from slightly different assumptions rather than trying to come up with new and exciting hypotheses? We claim that this is because the process of refining economic models is essentially a form of robustness analysis. The robustness of modeling results with respect to particular modeling assumptions, parameter values or initial conditions plays a crucial role for modeling in economics for two reasons. First, economic models are difficult to subject to straightforward empirical tests for various reasons. Second, the very nature of economic phenomena provides little hope of ever making the modeling assumptions completely realistic. Robustness analysis is therefore a natural methodological strategy for economists because economic models are based on various idealizations and abstractions which make at least some of their assumptions unrealistic (Wimsatt 1987; 1994a; 1994b; Mäki 2000; Weisberg 2006b). The importance of robustness considerations in economics ultimately forces us to reconsider many commonly held views on the function and logical structure of economic theory.

Given that much of economic research praxis can be characterized as robustness analysis, it is somewhat surprising that philosophers of economics have only recently become interested in robustness. William Wimsatt has extensively discussed robustness analysis, which he considers in general terms as triangulation via independent ways of determination . According to Wimsatt, fairly varied processes or activities count as ways of determination: measurement, observation, experimentation, mathematical derivation etc. all qualify. Many ostensibly different epistemic activities are thus classified as robustness analysis. In a recent paper, James Woodward (2006) distinguishes four notions of robustness. The first three are all species of robustness as similarity of the result under different forms of determination. Inferential robustness refers to the idea that there are different degrees to which inference from some given data may depend on various auxiliary assumptions, and derivational robustness to whether a given theoretical result depends on the different modelling assumptions. The difference between the two is that the former concerns derivation from data, and the latter derivation from a set of theoretical assumptions. Measurement robustness means triangulation of a quantity or a value by (causally) different means of measurement. Inferential, derivational and measurement robustness differ with respect to the method of determination and the goals of the corresponding robustness analysis. Causal robustness, on the other hand, is a categorically different notion because it concerns causal dependencies in the world, and it should not be confused with the epistemic notion of robustness under different ways of determination.

> In Woodward's typology, the kind of theoretical model-refinement that is so common in economics constitutes a form of derivational robustness analysis. However, if Woodward (2006) and Nancy Cartwright (1991) are right in claiming that derivational robustness does not provide any epistemic credence to the conclusions, much of theoretical model- building in economics should be regarded as epistemically worthless. We take issue with this position by developing Wimsatt's (1981) account of robustness analysis as triangulation via independent ways of determination. Obviously, derivational robustness in economic models cannot be a matter of entirely independent ways of derivation, because the different models used to assess robustness usually share many assumptions. Independence of a result with respect to modelling assumptions nonetheless carries epistemic weight by supplying evidence that the result is not an artefact of particular idealizing modelling assumptions. We will argue that although robustness analysis, understood as systematic examination of derivational robustness, is not an empirical confirmation procedure in any straightforward sense, demonstrating that a modelling result is robust does carry epistemic weight by guarding

against error and by helping to assess the relative importance of various parts of theoretical models (cf. Weisberg 2006b). While we agree with Woodward (2006) that arguments presented in favour of one kind of robustness do not automatically apply to other kinds of robustness, we think that the epistemic gain from robustness derives from similar considerations in many instances of different kinds of robustness.

In contrast to physics, economic theory itself does not tell which idealizations are truly fatal or crucial for the modeling result and which are not. Economists often proceed on a preliminary hypothesis or an intuitive hunch that there is some core causal mechanism that ought to be modeled realistically. Turning such intuitions into a tractable model requires making various unrealistic assumptions concerning other issues. Some of these assumptions are considered or hoped to be unimportant, again on intuitive grounds. Such assumptions have been examined in economic methodology using various closely related terms such as Musgrave's (1981) heuristic assumptions, Mäki's (2000) early step assumptions, Hindriks' (2006) tractability assumptions and Alexandrova's (2006) derivational facilitators. We will examine the relationship between such assumptions and robustness in economic model-building by way of discussing a case: geographical economics. We will show that an important way in which economists try to guard against errors in modeling is to see whether the model's conclusions remain the same if some auxiliary assumptions, which are hoped not to affect those conclusions, are changed. The case also demonstrates that although the epistemological functions of guarding against error and securing claims concerning the relative importance of various assumptions are somewhat different, they are often closely intertwined in the process of analyzing the robustness of some modeling result.

8. Conclusions

The practice of economic theorizing largely consists of building models with slightly different assumptions yielding familiar results. We have argued that this practice makes sense when seen as derivational robustness analysis. Robustness analysis is a sensible epistemic strategy in situations where we know that our assumptions and inferences are fallible, but not in what situations and in what way. Derivational robustness analysis guards against errors in theorizing when the problematic parts of the ways of determination, i.e. models, are independent of each other. In economics in particular, proving robust theorems from different models with diverse unrealistic assumptions helps us to evaluate what results correspond to important economic phenomena and what are merely artefacts of particular auxiliary assumptions. We have addressed Orzack and Sober's criticism against robustness as an epistemically relevant feature by showing that their formulation of the epistemic situation in which robustness analysis is useful is misleading. We have also shown that their argument actually shows how robustness considerations are necessary for evaluating what a given piece of data can support. We have also responded to Cartwright's criticism by showing that it relies on an untenable hope of a completely true economic model.

Viewing economic model building as robustness analysis also helps to make sense of the role of the rationality axioms that apparently provide the basis of the whole enterprise. Instead of the traditional Euclidian view of the structure of economic theory, we propose that economics should be approached as a Babylonian science, where the epistemically secure parts are the robust theorems and the axioms only form what Boyd and Richerson call a generalized sample theory, whose the role is to help organize further modelling work and facilitate communication between specialists.

Scientific Irreproducibility (Frequentists versus Bayesians)

Scrapbook5512---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsScienceStatisticalMistakes.htm#Scrapbook5512

"**Weak statistical standards implicated in scientific irreproducibility: One-quarter of studies that meet commonly used statistical cutoff may be false**." by Erika Check Hayden, *Nature*, November 11, 2013 ---
http://www.nature.com/news/weak-statistical-standards-implicated-in-scientific-irreproducibility-1.14131

The plague of non-reproducibility in science may be mostly due to scientists' use of weak statistical tests, as shown by an innovative method developed by statistician Valen Johnson, at Texas A&M University in College Station.

Johnson compared the strength of two types of tests: frequentist tests, which measure how unlikely a finding is to occur by chance, and Bayesian tests, which measure the likelihood that a particular hypothesis is correct given data collected in the study. The strength of the results given by these two types of tests had not been compared before, because they ask slightly different types of questions.

So Johnson developed a method that makes the results given by the tests — the *P* value in the frequentist paradigm, and the Bayes factor in the Bayesian paradigm — directly comparable. Unlike frequentist tests, which use objective calculations to reject a null hypothesis, Bayesian tests require the tester to define an alternative hypothesis to be tested — a subjective process. But Johnson developed a 'uniformly most powerful' Bayesian test that defines the alternative hypothesis in a standard way, so that it "maximizes the probability that the Bayes factor in favor of the alternate hypothesis exceeds a specified threshold," he writes in his paper. This threshold can be chosen so that Bayesian tests and frequentist tests will both reject the null hypothesis for the same test results.

Johnson then used these uniformly most powerful tests to compare *P* values to Bayes factors. When he did so, he found that a *P* value of 0.05 or less — commonly considered evidence in support of a hypothesis in fields such as social science, in which non-reproducibility has become a serious issue — corresponds to Bayes factors of between 3 and 5, which are considered weak evidence to support a finding.

False positives

Indeed, as many as 17–25% of such findings are probably false, Johnson calculates[1]. He advocates for scientists to use more stringent *P* values of 0.005 or less to support their findings, and thinks that the use of the 0.05 standard might account for most of the problem of non-reproducibility in science — even more than other issues, such as biases and scientific misconduct.

"Very few studies that fail to replicate are based on *P* values of 0.005 or smaller," Johnson says.

Some other mathematicians said that though there have been many calls for researchers to use more stringent tests[2], the new paper makes an important contribution by laying bare exactly how lax the 0.05 standard is.

"It shows once more that standards of evidence that are in common use throughout the empirical sciences are dangerously lenient," says mathematical psychologist Eric-Jan Wagenmakers of the University of Amsterdam. "Previous arguments centered on '*P*-hacking', that is, abusing standard statistical procedures to obtain the desired results. The Johnson paper shows that there is something wrong with the *P* value itself."

Other researchers, though, said it would be difficult to change the mindset of scientists who have become wedded to the 0.05 cutoff. One implication of the work, for instance, is that studies will have to include more subjects to reach these more stringent cutoffs, which will require more time and money.

"The family of Bayesian methods has been well developed over many decades now, but somehow we are stuck to using frequentist approaches," says physician John Ioannidis of Stanford University in California, who studies the causes of non-reproducibility. "I hope this paper has better luck in changing the world."

Association is not Causation: The Need for Granulation in Databases

We test for association, not causation."
Blackespoor, Linsmeier, Petroni, and Shakespeare (2013)

These questions call extrapolation and so cannot be answered using NLSy data alone.
Manski (1995, Page 20)

For me, she says, "this really showed the beauty of science, that you can have this personal experience that isn't reflected in big data."
Jennifer Jacquet as quoted by Robin Wilson, *Inside Higher Ed*, October 22, 2012 ---
http://chronicle.com/article/The-Hard-Numbers-Behind/135236/?cid=at&utm_source=at&utm_medium=en

However, one of the criteria for explanation is that it requires the least number of unwarranted assumptions, something philosophers call Occam's razor.
Pigliucci (2010, p. 74)

If accountics scientists took the trouble to seek out causes they would collect more granular data to supplement their purchased databases. See below for an illustration..

Scrapbook1556--- http://www.cs.trinity.edu/~rjensen/temp/AccounticsGranulationCurrentDraft.pdf

"**Audit Quality and Auditor Reputation: Evidence from Japan**," by Douglas J. Skinner and Suraj Srinivasan, *The Accounting Review,* September 2012, Vol. 87, No. 5, pp. 1737-1765. (Emphasis added)

> We study events surrounding ChuoAoyama's failed audit of Kanebo, a large Japanese cosmetics company whose management engaged in a massive accounting fraud. ChuoAoyama was PwC's Japanese affiliate and one of Japan's largest audit firms. In May 2006, the Japanese Financial Services Agency (FSA) suspended ChuoAoyama for two months for its role in the Kanebo fraud. This unprecedented action followed a series of events that seriously damaged ChuoAoyama's reputation. We use these events to provide evidence on the importance of auditors' reputation for quality in a setting where litigation plays essentially no role. Around one quarter of ChuoAoyama's clients defected from the firm after its suspension, consistent with the importance of reputation. Larger firms and those with greater growth options were more likely to leave, also consistent with the reputation argument.
>
> . . .
>
> To test whether the F2006 auditor switches away from ChuoAoyama are unusually frequent, we estimate a logit model of factors that explain auditor changes. The control variables are drawn from previous research on auditor switches and include firm size (log of total assets), growth (percentage change in total assets), leverage, change in leverage, profitability (ROA), a loss dummy, U.S. listing, keiretsu inclination, auditor industry expertise, earnings quality as measured by accruals, whether the firm completed an M&A transaction in the preceding two years, and industry fixed effects.22 We provide details of data sources and variable definitions in Appendix B. The keiretsu inclination variable

measures whether and to what extent these firms are part of the large corporate groups common in Japan (e.g., Aoki et al. 1994; Hoshi and Kashyap 2001).

We include dummy variables for whether the client is a ChuoAoyama client (CA), for fiscal year 2006 (F2006), and for the interaction of these two dummies (CA_F2006). The interaction variable is our primary interest because it measures the extent to which client firms switch away from ChuoAoyama in fiscal 2006, the period in which we argue that auditor reputation drives switching.

. . .

Our results are largely consistent with the importance of reputation effects. We find evidence that a relatively large number of ChuoAoyama's clients left the firm for other auditors as the seriousness of ChuoAoyama's quality problems became evident. The rate of client turnover at ChuoAoyama in fiscal year 2006, before it became apparent that the firm would be shut down but after audit-quality questions had been raised, was substantially higher than would otherwise be expected, consistent with clients leaving once the firm's reputation for quality was seriously diminished. Moreover, we find that the likelihood of switching is higher for larger clients and clients with higher market-to-book ratios, characteristics associated with a demand for higher-audit quality, and lower for firms with greater managerial ownership, indicating a lower demand for audit quality in such firms. Clients that moved to Aarata were also larger, with higher market-to-book ratios, a greater extent of cross-listing, and higher foreign ownership. These switches are not the result of clients following their audit teams to new auditors. Our event study results weakly support the auditor-quality argument, but are likely to lack power because questions about ChuoAoyama's audit quality were revealed over an extended period.

Our conclusions are subject to two caveats. First, we find that clients switched away from ChuoAoyama in large numbers in Spring 2006, just after Japanese regulators announced the two-month suspension and PwC formed Aarata. While we interpret these events as being a clear and undeniable signal of audit-quality problems at ChuoAoyama, **we cannot know for sure what drove these switches** (emphasis added). It is possible that the suspension caused firms to switch auditors for reasons unrelated to audit quality. Second, our analysis presumes that audit quality is important to Japanese companies. While we believe this to be the case, especially over the past two decades as Japanese capital markets have evolved to be more like their Western counterparts, **it is possible that audit quality is, in general, less important in Japan**

Jensen Comment
These are very honest admissions that extend to the entire history of most accountings science studies. The Skinner and Srinivasan inference that the audit firm's loss of reputation caused a third of the clients to switch is very tenuous and superficial since two thirds of the clients remained loyal and did not switch. This suggests at a minimum that reasons for switching are far more complicated than assumed by Skinner and Srinivasan.

In other words, like most accountics science papers causality that is inferred could be slightly off base or largely off base. There's no way of knowing because the accountics models cannot see the granules of causation. This is where non-science granular research might be of some help.

Non-science protocol analysis is not of much use as a follow up to the Skinner and Srinivasan study since changing auditor decisions in this study are one-time past historical events for the PwC-affiliated ChuoAoyama auditor client switching and are not frequently repeated observable decision events such as portfolio decisions of a trust investor or a bank's decision to set a credit limits of borrowers.

Non-science mail survey research where the clients of the ChuoAoyama audit firm at the time are surveyed are not likely to be of much use since there's no incentive for those clients to respond at all, and if some of them respond the results will be questionable since the respondents quite likely to provide answers they think the researchers and public want to hear.

Interview research holds out more promise but has problems as well unless carried out very carefully in a good design, probably using respected Japanese interviewers.

What granules of causation might be discovered in the interviews of clients who changed auditors after the ChuoAoyama audit firm scandal became revealed to the public?

Possible Answer 1
Skinner and Srinivasan suggest (but could not conclude) that nearly all the clients that changed audit firms did so because of the possible adverse effect keeping the scandalous audit firm would have on cost of capital increases for clients who used a scandal-ridden audit firm. But this suggestion is weak because it cannot explain why a majority of the ChuoAoyama audit firm's clients did not switch auditors.

Possible Answer 2
Skinner and Srinivasan did not consider the possibility that some clients switched auditors because the scandal gave them an excuse to dump an expensive and possibly over-priced auditor while at the same time appearing to be more noble when switching from a scandal-ridden auditor For example, the client may strongly suspect the audit firm is padding the work hours for no good reason. *If at least one interview found that the scandal was an excuse rather than the reason for switching auditors we have slightly more evidence of causality than we had with just the accountics science study that can say zero about causality*.

Possible Answer 3
Skinner and Srinivasan did not consider the possibility that some clients switched auditors because the scandal gave them an excuse to change to an auditor having a local office nearby that promised better service due to response times and at lower cost due to such things as lower travel expense billings. Auditors having nearby offices also improve relationship building at civic meetings, golf outings, etc. This may not be ideal from the standpoint of independence considerations, but clients are generally less concerned about independence than investors.

Possible Answer 4
Skinner and Srinivasan did not consider the possibility that some clients switched auditors because the scandal gave them an excuse to change from an audit firm that communicated poorly with some clients. Reasons in general that companies give for changing auditors are that their auditors communicated poorly with management and audit committees.

Possible Answer 5
Skinner and Srinivasan did not consider the possibility that some clients switched auditors because the

scandal gave them an excuse to change from an audit firm that was inefficient and superficial in the audit. For example, the audit teams might be comprised of novice auditors having little or no experience with the industry and/or the types of accounts being audited. For example, auditors being assigned to audit interest rate swaps might keep asking naïve questions about derivative instruments contracts and hedging.

Possible Answer 6
Skinner and Srinivasan did not consider the possibility that some clients switched auditors because the scandal gave them an excuse to change from a newly assigned partner in charge that the client really disliked relative to previous partners in charge. Audit firms change partners in charge of audits for various reasons, and client experiences with a new partner and charge may greatly sweeten or sour the audit experience.

Accountics Scientists More Interested in Their Tractors than Their Harvests

If accountics scientists took more interest in their research harvests I would expect to see the following:

1. Excitement about validating the harvests, especially replications, by independent researchers.
   Criticisms: http://www.trinity.edu/rjensen/TheoryTAR.htm#Replication
   There is some criticism that scientists sometimes become lax in independently reproducing the findings of other scientists, but the most important findings are replicated whenever possible.

2. Efforts by journal editors to encourage commentaries and other debates regarding the importance of harvests published in their journals.
   Criticisms --- http://www.trinity.edu/rjensen/TheoryTAR.htm#TARversusAMR

3. Efforts by authors to communicate their harvests to the worlds of accounting teachers and practitioners. There are various ways to do this, most notably the AAA Commons, blogs, and listservs such as the AAA AECM listserv.
   Criticisms--- http://www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm

Accountics scientists fail in all the above domains.

Perhaps the biggest single thing that separates accountics scientists from real scientists is the virtual lack of interest in replicating the outcomes (harvests) reported in accountics science articles.

Selected articles that are replicated are replicated years later and only in collection with extensions that are not themselves independently replicated.

Top accountics science journals do not publish replication articles or even commentaries on those articles. It's not so much that accountics science journals like TAR have policies for refusing commentaries. Infrequently a commentary is published, but it's never focused on a previous article in the journal and hence is not a commentary on an article.

Scrapbook6013 --- www.trinity.edu/rjensen/Theory01.htm#Scrapbook6013

"David Ginsberg, chief data scientist at SAP, said communication skills are critically important in the field, and that a key player on his big-data team is a "guy who can translate Ph.D. to English. Those are the hardest people to find."
James Willhite (see below)

Might we also say the same thing about accountics scientists slaving over their enormous purchased "big data" databases?

"Getting Started in "Big Data"," by James Willhite, *The Wall Street Journal*, February 4, 2013 ---
http://blogs.wsj.com/cfo/2014/02/04/getting-started-in-big-data/?mod=djemCFO_h

> Wanted: Ph.D.-level statistician with the technical skill to use data-visualization software and a deep understanding of the _____ industry.
>
> Fill in the blank with almost any business: consumer products, entertainment, health care, semiconductors or fast food. The list reflects the growing range of companies trying to mine mountains of data in hopes of improving product design, supply chains, customer service or other operations.
>
> . . .
>
> At the most basic level, big data is the art and science of collecting and combing through vast amounts of information for insights that aren't apparent on a smaller scale. Financial executives who want to harness big data face a critical hurdle: Finding people who can glean it, understand it, and translate it into plain English.
>
> The field is so new that the U.S. Bureau of Labor Statistics doesn't yet have a classification for data scientists, according to BLS economist Sara Royster. That makes it tough to estimate the unemployment rate or salaries for job seekers in the field.
>
> But executives and recruiters, who compete for talent in the nascent specialty, point to hiring strategies that can get a big-data operation off the ground. They say they look for specific industry experience, poach from data-rich rivals, rely on interview questions that screen out weaker candidates and recommend starting with small projects.
>
> David Ginsberg, chief data scientist at business-software maker SAP AG , said communication skills are critically important in the field, and that a key player on his big-data team is a "guy who can translate Ph.D. to English. Those are the hardest people to find."
>
> Along with the ability to explain their findings, data scientists need to have a proven record of being able to pluck useful information from data that often lack an obvious structure and may even come from a dubious source. This expertise doesn't always cut across industry lines. A scientist with a keen knowledge of the entertainment industry, for example, won't necessarily be able to transfer his skills to the fast-food market.
>
> Some candidates can make the leap. Wolters Kluwers NV, a Netherlands-based information-services provider, has had some success in filling big-data jobs by recruiting from other, data-rich industries, such as financial services. "We have found tremendous success with going to alternative sources and

looking at different businesses and saying, ‘What can you bring into our business?’ ” said Kevin Entricken, the company’s chief financial officer.

The trick, some experts say, is finding a candidate steeped in higher mathematics with hands-on familiarity with a particular business. “When you have all those Ph.D.s in a room, magic doesn’t necessarily happen because they may not have the business capability,” said Andy Rusnak, a senior executive for the Americas in Ernst & Young’s advisory practice.

Companies can hamstring themselves in big-data projects by thinking too long term, Mr. Rusnak said. They should focus instead on what they can discover in an eight- to 10-week period, he said, and think less about business transformation.

Dunkin’ Brands Group Inc. aims to wring all the value it can out of its data, by using it to entice customers to visit its stores more often and try new doughnuts and drinks. Last week, it went national with a loyalty program that will allow it to harvest data on customer habits.

The program allows the company to target individuals who opt into the program with specific offers aimed at making them more frequent customers. “If you’ve only been coming in the morning, perhaps we’d give you an offer for the afternoon,” said Dunkin’ Chief Information Officer Jack Clare.

Netflix’s Mr. Amatriain said, “I like to face candidates with real practical problems.” He said he will say to an applicant, “You have this data that comes from our users. How can you use it to solve this particular problem? How would you turn it into an algorithm that would recommend movies?” He said that the question is deliberately open-ended, forcing candidates to prove that they can understand not only the math, but what he calls “the big picture approach to using big data to gain insights.”

Jensen Comment
If accountics scientists are to accomplish the above they will have to abandoned their comfortable Cargo Cust isolation form the real world ---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm

"**Academic Research With Mass Appeal**," by Erin Zlome, *Bloomberg Business Week*, January 28, 2013 ---
http://www.businessweek.com/articles/2013-01-28/academic-research-with-mass-appeal

Business professors are great at writing jargon-filled, hard-to-digest research papers. But every once and a while, they knock it out of the park with the general public. A small pool of research achieved such blockbuster status in 2012 by becoming the most read, most downloaded, or most written-about pieces authored by professors at top business schools. Tax evasion, finding a job, and the benefits of teaching employees Spanish are some of the topics that got non-students reading.

At Harvard Business School, an excerpt from Clayton Christensen’s book *How Will You Measure Your Life?* was the year’s most read preview of forthcoming research. The passage uses the downfall of Blockbuster and the rise of Netflix (NFLX) as an analogy for how we may end up paying a high cost for small decisions.

Continued in article

MIT, like Harvard, places enormous value on having both feet planted in the real world

The professions of architecture, engineering, law, and medicine are heavily dependent upon the researchers in universities who focus on needs for research on the problems of practitioners working in the real world.

If accountics scientists want to change their ways and focus more on problems of the accounting practitioners working in the real world, one small step that can be taken is to study the presentations scheduled for a forthcoming MIT Sloan School Conference.

Financial Education Daily, May 2012 ---
http://paper.li/businessschools?utm_source=subscription&utm_medium=email&utm_campaign=paper_sub
Emphasis Added

> Learning best practice from the best practitioners
>
> MIT Sloan invites more than 400 of the world's finest leaders to campus every year. The most anticipated of these visits are the talks given as part of the Dean's Innovative Leader Series, which features the most dynamic movers and shakers of our day.
>
> At a school that places enormous value on having both feet planted in the real world, the Dean's Innovative Leader Series is a powerful learning tool. Students have the rare privilege of engaging in frank and meaningful discussions with the leaders who are shaping the present and future marketplace.

Bob Jensen's threads on other steps that should be taken by accountics scientists to become more focused on the needs of the profession ---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm

Presumably a successful replication "reproduces" exactly the same outcomes and authenticates/verifies the original research. In scientific research, such authentication is considered extremely important. The *IAPUC Gold Book* makes a distinction between reproducibility and repeatability at http://goldbook.iupac.org/

For purposes of this article, replication, reproducibility, and repeatability will be viewed as synonyms.

Allowance should be made for "conceptual replications" apart from "exact replications ---
http://www.jasnh.com/pdf/Vol6-No2.pdf

I still stand by my opinion that if scientific research findings are not replicated then they are:

1. Not important enough to replicate (nobody much cares)
2. Impossible to replicate (e.g., only enough DNA for one test)
3. Unable to meet the cost-benefit test for replication

Replication is still the gold standard where it counts such as in FDA requirements for reproducing drug impact findings. The FDA rarely relaxes its standards except in the case where delays may cause significant loss of life.

I think accountics science findings are rarely replicated because nobody much cares. Having said this, however, I find some accountics science studies that should be replicated because I, for one, find them to be important. Why they are not replicated is a mystery --- other than the fact that most accountics science research journals won't publish replications or even abstracts of replications.

What's the point of conducting replication research that cannot get out of the closet?

Very infrequently a commentary forthcoming accountics science presentation announcement appears in the American Accounting Association Commons comprised heavily of accounting teachers and some practitioners. The Commons is a perfect forum for explaining a explaining accountics science articles in plain English. Accountics scientists just do not take the time to do so and contribute virtually no messages whatsoever to the Commons to explain their research ---
http://www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm#Commons

Accountics scientists tend not to reach out to practitioners and accounting teachers to explain their findings in Web forums such as the American Accounting Association's AECM Listserv.--- http://listserv.aaahq.org/cgi-bin/wa.exe?A0=AECM&X=66E0D76A3AA0219246&Y=rjensen%40trinity.edu

Accountics scientists contribute virtually nothing to the Commons or otherwise write commentaries on their own works or the works of other accountics scientists.

Appeal for a "Daisy Chain of Replication":

Scrapbook1086 --- http://www.trinity.edu/rjensen/TheoryTar.htm#Scrapbook1086

"**Nobel laureate challenges psychologists to clean up their act: Social-priming research needs "daisy chain" of replication**," by Ed Yong, *Nature*, October 3, 2012 --- http://www.nature.com/news/nobel-laureate-challenges-psychologists-to-clean-up-their-act-1.11535

> Nobel prize-winner Daniel Kahneman has issued a strongly worded call to one group of psychologists to restore the credibility of their field by creating a replication ring to check each others' results.
>
> Kahneman, a psychologist at Princeton University in New Jersey, addressed his open e-mail to researchers who work on social priming, the study of how subtle cues can unconsciously influence our thoughts or behaviour. For example, volunteers might walk more slowly down a corridor after seeing words related to old age[1], or fare better in general-knowledge tests after writing down the attributes of a typical professor[2].
>
> Such tests are widely used in psychology, and Kahneman counts himself as a "general believer" in priming effects. But in his e-mail, seen by *Nature*, he writes that there is a "train wreck looming" for the field, due to a "storm of doubt" about the robustness of priming results.
>
> Under fire
>
> This skepticism has been fed by failed attempts to replicate classic priming studies, increasing concerns about replicability in psychology more broadly (see 'Bad Copy'), and the exposure of fraudulent social psychologists such as Diederik Stapel, Dirk Smeesters and Lawrence Sanna, who used priming techniques in their work.
>
> "For all these reasons, right or wrong, your field is now the poster child for doubts about the integrity of psychological research," Kahneman writes. "I believe that you should collectively do something about this mess."
>
> Kahneman's chief concern is that graduate students who have conducted priming research may find it difficult to get jobs after being associated with a field that is being visibly questioned.
>
> "Kahneman is a hard man to ignore. I suspect that everybody who got a message from him read it immediately," says Brian Nosek, a social psychologist at the University of Virginia in

Charlottesville. David Funder, at the University of California, Riverside, and president-elect of the Society for Personality and Social Psychology, worries that the debate about priming has descended into angry defensiveness rather than a scientific discussion about data. “I think the e-mail hits exactly the right tone,” he says. “If this doesn’t work, I don’t know what will.”

Hal Pashler, a cognitive psychologist at the University of California, San Diego, says that several groups, including his own, have already tried to replicate well-known social-priming findings, but have not been able to reproduce any of the effects. “These are quite simple experiments and the replication attempts are well powered, so it is all very puzzling. The field needs to get to the bottom of this, and the quicker the better.”

Chain of replication

To address this problem, Kahneman recommends that established social psychologists set up a “daisy chain” of replications. Each lab would try to repeat a priming effect demonstrated by its neighbour, supervised by someone from the replicated lab. Both parties would record every detail of the methods, commit beforehand to publish the results, and make all data openly available.

Kahneman thinks that such collaborations are necessary because priming effects are subtle, and could be undermined by small experimental changes.

Norbert Schwarz, a social psychologist at the University of Michigan in Ann Arbor who received the e-mail, says that priming studies attract skeptical attention because their results are often surprising, not necessarily because they are scientifically flawed.. “There is no empirical evidence that work in this area is more or less replicable than work in other areas,” he says, although the “iconic status” of individual findings has distracted from a larger body of supportive evidence.

“You can think of this as psychology’s version of the climate-change debate,” says Schwarz. “The consensus of the vast majority of psychologists closely familiar with work in this area gets drowned out by claims of a few persistent priming sceptics.”

Still, Schwarz broadly supports Kahneman’s suggestion. “I will participate in such a daisy-chain if the field decides that it is something that should be implemented,” says Schwarz, but not if it is “merely directed at one single area of research”.

Continued in article

But Steven Levitt and John List, two economists at the University of Chicago, discovered that the data had survived the decades in two archives in Milwaukee and Boston, and decided to subject them to econometric analysis. The Hawthorne experiments had another surprise in store for them. Contrary to the descriptions in the literature, they found no systematic evidence that levels of productivity in the factory rose whenever changes in lighting were implemented.

"Light work," *The Economist*, June 4, 2009, Page 74 ---
http://www.economist.com/finance/displaystory.cfm?story_id=13788427

Accountics Science Journals Are Obsessed With Quantification to a Fault

There continue to be, and perhaps will always be, areas of science where mathematical approaches are not terribly useful and where in fact an obsession with quantification may be positively deteriorous.
Piglio (2010, Page 203)

Scrapbook1247 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1248

"**Is mathematics an effective way to describe the world?**" by Lisa Zyga, *Physorg*, September 3, 2013 --- http://phys.org/news/2013-09-mathematics-effective-world.html

Mathematics has been called the language of the universe. Scientists and engineers often speak of the elegance of mathematics when describing physical reality, citing examples such as π, E=mc2, and even something as simple as using abstract integers to count real-world objects. Yet while these examples demonstrate how useful math can be for us, does it mean that the physical world naturally follows the rules of mathematics as its "mother tongue," and that this mathematics has its own existence that is out there waiting to be discovered? This point of view on the nature of the relationship between mathematics and the physical world is called Platonism, but not everyone agrees with it.

Derek Abbott, Professor of Electrical and Electronics Engineering at The University of Adelaide in Australia, has written a perspective piece to be published in the Proceedings of the IEEE in which he argues that mathematical Platonism is an inaccurate view of reality. Instead, he argues for the opposing viewpoint, the non-Platonist notion that mathematics is a product of the human imagination that we tailor to describe reality.

This argument is not new. In fact, Abbott estimates (through his own experiences, in an admittedly non-scientific survey) that while 80% of mathematicians lean toward a Platonist view, engineers by and large are non-Platonist. Physicists tend to be "closeted non-Platonists," he says, meaning they often appear Platonist in public. But when pressed in private, he says he can "often extract a non-Platonist confession."

So if mathematicians, engineers, and physicists can all manage to perform their work despite differences in opinion on this philosophical subject, why does the true nature of mathematics in its relation to the physical world really matter?

The reason, Abbott says, is that because when you recognize that math is just a mental construct—just an approximation of reality that has its frailties and limitations and that will break down at some point because perfect mathematical forms do not exist in the physical universe—then you can see how ineffective math is.

And that is Abbott's main point (and most controversial one): that mathematics is not exceptionally good at describing reality, and definitely not the "miracle" that some scientists have marveled at. Einstein, a mathematical non-Platonist, was one scientist who marveled at the power of mathematics. He asked, "How can it be that mathematics, being after all a product of human thought which is independent of experience, is so admirably appropriate to the objects of reality?"

In 1959, the physicist and mathematician Eugene Wigner described this problem as "the unreasonable effectiveness of mathematics." In response, Abbott's paper is called "The Reasonable Ineffectiveness of

Mathematics." Both viewpoints are based on the non-Platonist idea that math is a human invention. But whereas Wigner and Einstein might be considered mathematical optimists who noticed all the ways that mathematics closely describes reality, Abbott pessimistically points out that these mathematical models almost always fall short.

What exactly does "effective mathematics" look like? Abbott explains that effective mathematics provides compact, idealized representations of the inherently noisy physical world.

"Analytical mathematical expressions are a way making compact descriptions of our observations," he told Phys.org. "As humans, we search for this 'compression' that math gives us because we have limited brain power. Maths is effective when it delivers simple, compact expressions that we can apply with regularity to many situations. It is ineffective when it fails to deliver that elegant compactness. It is that compactness that makes it useful/practical ... if we can get that compression without sacrificing too much precision.

"I argue that there are many more cases where math is ineffective (non-compact) than when it is effective (compact). Math only has the illusion of being effective when we focus on the successful examples. But our successful examples perhaps only apply to a tiny portion of all the possible questions we could ask about the universe."

Some of the arguments in Abbott's paper are based on the ideas of the mathematician Richard W. Hamming, who in 1980 identified four reasons why mathematics should not be as effective as it seems. Although Hamming resigned himself to the idea that mathematics is unreasonably effective, Abbott shows that Hamming's reasons actually support non-Platonism given a reduced level of mathematical effectiveness.

Here are a few of Abbott's reasons for why mathematics is reasonably ineffective, which are largely based on the non-Platonist viewpoint that math is a human invention:

• Mathematics appears to be successful because we cherry-pick the problems for which we have found a way to apply mathematics. There have likely been millions of failed mathematical models, but nobody pays attention to them. ("A genius," Abbott writes, "is merely one who has a great idea, but has the common sense to keep quiet about his other thousand insane thoughts.")

• Our application of mathematics changes at different scales. For example, in the 1970s when transistor lengths were on the order of micrometers, engineers could describe transistor behavior using elegant equations. Today's submicrometer transistors involve complicated effects that the earlier models neglected, so engineers have turned to computer simulation software to model smaller transistors. A more effective formula would describe transistors at all scales, but such a compact formula does not exist.

• Although our models appear to apply to all timescales, we perhaps create descriptions biased by the length of our human lifespans. For example, we see the Sun as an energy source for our planet, but if the human lifespan were as long as the universe, perhaps the Sun would appear to be a short-lived fluctuation that rapidly brings our planet into thermal equilibrium with itself as it "blasts" into a red giant. From this perspective, the Earth is not extracting useful net energy from the Sun.

• Even counting has its limits. When counting bananas, for example, at some point the number of bananas will be so large that the gravitational pull of all the bananas draws them into a black hole. At some point, we can no longer rely on numbers to count.

• And what about the concept of integers in the first place? That is, where does one banana end and the next begin? While we think we know visually, we do not have a formal mathematical definition. To take this to its logical extreme, if humans were not solid but gaseous and lived in the clouds, counting discrete objects would not be so obvious. Thus axioms based on the notion of simple counting are not innate to our universe, but are a human construct. There is then no guarantee that the mathematical descriptions we create will be universally applicable.

For Abbott, these points and many others that he makes in his paper show that mathematics is not a miraculous discovery that fits reality with incomprehensible regularity. In the end, mathematics is a human invention that is useful, limited, and works about as well as expected.

Continued in article

Scrapbook1250 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1250

"**Economics has met the enemy, and it is economics**," by Ira Basen, *Globe and Mail*, October 15, 2011 --- http://www.theglobeandmail.com/news/politics/economics-has-met-the-enemy-and-it-is-economics/article2202027/page1/

> After Thomas Sargent learned on Monday morning that he and colleague Christopher Sims had been awarded the Nobel Prize in Economics for 2011, the 68-year-old New York University professor struck an aw-shucks tone with an interviewer from the official Nobel website: “We're just bookish types that look at numbers and try to figure out what's going on.”
>
> But no one who'd followed Prof. Sargent's long, distinguished career would have been fooled by his attempt at modesty. He'd won for his part in developing one of economists' main models of cause and effect: How can we expect people to respond to changes in prices, for example, or interest rates? According to the laureates' theories, they'll do whatever's most beneficial to them, and they'll do it every time. They don't need governments to instruct them; they figure it out for themselves. Economists call this the “rational expectations” model. And it's not just an abstraction: Bankers and policy-makers apply these formulae in the real world, so bad models lead to bad policy.
>
> Which is perhaps why, by the end of that interview on Monday, Prof. Sargent was adopting a more realistic tone: “We experiment with our models,” he explained, “before we wreck the world.”
>
> Rational-expectations theory and its corollary, the efficient-market hypothesis, have been central to mainstream economics for more than 40 years. And while they may not have “wrecked the world,” some critics argue these models have blinded economists to reality: Certain the universe was unfolding as it should, they failed both to anticipate the financial crisis of 2008 and to chart an effective path to recovery.
>
> The economic crisis has produced a crisis in the study of economics – a growing realization that if the field is going to offer meaningful solutions, greater attention must be paid to what is happening in university lecture halls and seminar rooms.
>
> While the protesters occupying Wall Street are not carrying signs denouncing rational-expectations and efficient-market modelling, perhaps they should be.
>
> They wouldn't be the first young dissenters to call economics to account. In June of 2000, a small group of elite graduate students at some of France's most prestigious universities declared war on the economic establishment. This was an unlikely group of student radicals, whose degrees could be expected to lead them to lucrative careers in finance, business or government if they didn't rock the boat. Instead, they protested – not about tuition or workloads, but that too much of what they studied bore no relation to what was happening outside the classroom walls.
>
> They launched an online petition demanding greater realism in economics teaching, less reliance on mathematics “as an end in itself” and more space for approaches beyond the dominant neoclassical model, including input from other disciplines, such as psychology, history and sociology. Their

conclusion was that economics had become an “autistic science,” lost in “imaginary worlds.” They called their movement Autisme-economie.

The students' timing is notable: It was the spring of 2000, when the world was still basking in the glow of “the Great Moderation,” when for most of a decade Western economies had been enjoying a prolonged period of moderate but fairly steady growth.

Some economists were daring to think the unthinkable – that their understanding of how advanced capitalist economies worked had become so sophisticated that they might finally have succeeded in smoothing out the destructive gyrations of capitalism's boom-and-bust cycle. (“The central problem of depression prevention has been solved,” declared another Nobel laureate, Robert Lucas of the University of Chicago, in 2003 – five years before the greatest economic collapse in more than half a century.)

The students' petition sparked a lively debate. The French minister of education established a committee on economic education. Economics students across Europe and North America began meeting and circulating petitions of their own, even as defenders of the status quo denounced the movement as a Trotskyite conspiracy. By September, the first issue of the Post-Autistic Economic Newsletter was published in Britain.

As The Independent summarized the students' message: “If there is a daily prayer for the global economy, it should be, ‘Deliver us from abstraction.'”

It seems that entreaty went unheard through most of the discipline before the economic crisis, not to mention in the offices of hedge funds and the Stockholm Nobel selection committee. But is it ringing louder now? And how did economics become so abstract in the first place?

The great classical economists of the late 18th and early 19th centuries had no problem connecting to the real world – the Industrial Revolution had unleashed profound social and economic changes, and they were trying to make sense of what they were seeing. Yet Adam Smith, who is considered the founding father of modern economics, would have had trouble understanding the meaning of the word “economist.”

What is today known as economics arose out of two larger intellectual traditions that have since been largely abandoned. One is political economy, which is based on the simple idea that economic outcomes are often determined largely by political factors (as well as vice versa). But when political-economy courses first started appearing in Canadian universities in the 1870s, it was still viewed as a small offshoot of a far more important topic: moral philosophy.

In The Wealth of Nations (1776), Adam Smith famously argued that the pursuit of enlightened self-interest by individuals and companies could benefit society as a whole. His notion of the market's “invisible hand” laid the groundwork for much of modern neoclassical and neo-liberal, laissez-faire economics. But unlike today's free marketers, Smith didn't believe that the morality of the market was appropriate for society at large. Honesty, discipline, thrift and co-operation, not consumption and unbridled self-interest, were the keys to happiness and social cohesion. Smith's vision was a capitalist economy in a society governed by non-capitalist morality.

But by the end of the 19th century, the new field of economics no longer concerned itself with moral philosophy, and less and less with political economy. What was coming to dominate was a conviction that markets could be trusted to produce the most efficient allocation of scarce resources, that individuals would always seek to maximize their utility in an economically rational way, and that all of this would ultimately lead to some kind of overall equilibrium of prices, wages, supply and demand.

Political economy was less vital because government intervention disrupted the path to equilibrium and should therefore be avoided except in exceptional circumstances. And as for morality, economics would concern itself with the behaviour of rational, self-interested, utility-maximizing Homo economicus. What he did outside the confines of the marketplace would be someone else's field of study.

As those notions took hold, a new idea emerged that would have surprised and probably horrified Adam Smith – that economics, divorced from the study of morality and politics, could be considered a science. By the beginning of the 20th century, economists were looking for theorems and models that could help to explain the universe. One historian described them as suffering from “physics envy.” Although they were dealing with the behaviour of humans, not atoms and particles, they came to believe they could accurately predict the trajectory of human decision-making in the marketplace.

In their desire to have their field be recognized as a science, economists increasingly decided to speak the language of science. From Smith's innovations through John Maynard Keynes's work in the 1930s, economics was argued in words. Now, it would go by the numbers.

Continued in a long article

I think biologists tend to avoid quant biology because quant biology does not sufficiently capture real world complexities and therefore have findings that are too superficial. These are the same reasons why accounting teachers and practitioners most often avoid accountics research studies (that are quant by definition). Quants do their research in Plato's Cave with "convenient" assumptions that are too removed from the real and much more complicated world. For example, the real world is seldom in a state of equilibrium or a "steady state" needed to greatly simplify the mathematical derivations.

Scrapbook1258 --- http://www.trinity.edu/rjensen/TheoryTar.htm#Scrapbook1258

Volker Laux and D. Paul Newman, "Auditor Liability and Client Acceptance Decisions," *The Accounting Review,* Vol. 85, No. 1, 2010 pp. 261–285

Jensen Comment
One of the dubious assumptions of the entire Laux and Newman analysis is equilibrium of an audit firm's litigation payout for a particular client that has a higher likelihood to fail. If a client has a higher than average likelihood to fail then it most likely is not in an equilibrium state.

Another leap of faith is continuity in the payout and risk functions to a point where second derivatives can be calculated of such firms. In reality such functions are likely to be highly non-continuous and subject to serious break points. It is not clear how such a model could ever be applied to a real world audit client.

Another assumption is that the audit firm's e*x ante* utility function and a client firm's utility function are respectively as follows:

$$U^A = ep[W - (1 - a)(1 - \theta)L - 0.5ka^2] - 0.5ce^2. \tag{20}$$

$$U^E = ep(\theta(X - I) - W - (1 - a)(1 - \theta)(I - sD)). \tag{22}$$

Yeah right. Have these utility functions ever been validated for any real world client and auditor? As a matter of fact, what is the utility function of any corporation that according to agency theory is a nexus of contracts? My feeble mind cannot even imagine what a realistic utility function looks like for a nexus of contracts.

I would instead contend that there is no audit firm utility function apart from the interactions of the utilities of the major players in client acceptance/retention decision and audit pricing decisions. For example, before David Duncan was fired by Andersen, the decision to keep Enron as a client was depended upon the interactive utility functions of David Duncan versus Carl Bass versus Joseph Berardino. None of them worked from a simplistic Andersen utility function such as the one shown in Equation 20 above. Each worked interactively with each other in a very complicated way that had Bass being released from the Enron audit and Berardino burying his head in the sands of Lake Michigan.

The audit firm utility function, if it exists, is based on the nexus of people rather than the nexus of contracts that we call a "corporation."

The Laux and Newman paper also fails to include the role of outside players in some decisions regarding risky players. A huge outside player is the SEC that is often brought into the arena. Currently the SEC is playing a role in the "merry-go-round of auditors" for a corporation called Overstock.com that is currently working with the SEC to find an auditor. See "Auditor Merry Go Round at Overstock.com," *Big Four Blog*, January 8, 2010 --- http://www.bigfouralumni.blogspot.com/

Another leap of faith in the Laux and Newman paper is that auditor "liability environment" can be decomposed into *"three components: (1) the strictness of the legal regime, defined as the probability that the auditor is sued and found liable in case of an audit failure, (2) potential damage payments from the auditor to investors and (3) other litigation costs incurred by the auditor, labeled litigation frictions, such as attorneys' fees or loss of reputation."* It would seem that these three components cannot be decomposed in real life without also accounting for the nonlinear and possibly huge covariances.

A possible test of this study might be reference to one case illustration demonstrating that in at least one real world instance "an increase in the potential damage payment actually leads to a reduction in the client rejection rate." In the absence of such real world partial validation of the analytical results, we are asked to accept a huge amount on unsupported faith in untested assumptions inside Plato's Cave.

Scrapbook1252 --- http://www.trinity.edu/rjensen/TheoryTar.htm#Scrapbook1252

A Plenary Session Speech at a Chartered Financial Analysts Conference
Video: James Montier's 2012 Chicago CFA Speech **The Flaws of Finance** ---
http://cfapodcast.smartpros.com/web/live_events/Annual/Montier/index.html
Note that it takes over 15 minutes before James Montier begins

Major Themes (emphasis added)

1. The difference between physics versus finance models is that physicists know the limitations of their models.
2. Another difference is that components (e.g., atoms) of a physics model are not trying to game the system.
3. The more complicated the model in finance the more the analyst is trying to substitute theory for experience.
4. There's a lot wrong with Value at Risk (VaR) models that regulators ignored.
5. The assumption of market efficiency among regulators (such as Alan Greenspan) was a huge mistake that led to excessively low interest rates and bad behavior by banks and credit rating agencies.
6. Auditors succumbed to self-serving biases of favoring their clients over public investors.
7. Banks were making huge gambles on other peoples' money.
8. Investors themselves ignored risk such as poisoned CDO risks when they should've known better. I love his analogy of black swans on a turkey farm.
9. Why don't we see surprises coming (five excellent reasons given here)?
10. The only group of people who view the world realistically are the clinically depressed.
11. Model builders should stop substituting elegance for reality.
12. All financial theorists should be forced to interact with practitioners.
13. Practitioners need to abandon the myth of optimality before the fact.
    Jensen Note
    This also applies to abandoning the myth that we can set optimal accounting standards.
14. In the long term fundamentals matter.
15. Don't get too bogged down in details at the expense of the big picture.

16. Max Plank said science advances one funeral at a time.

17. The speaker then entertains questions from the audience (some are very good).

Failure to Search for Research Problems of Interest to Practitioners

Although empirical scientific method has made many positive contributions to accounting research, it is not the method that is likely to generate new theories, though it will be useful in testing them. For example, Einstein's theories were not developed empirically, but they relied on understanding the empirical evidence and they were tested empirically. Both the development and testing of theories should be recognized as acceptable accounting research.
"1993 American Accounting Association President's Message," by Gary Sundem, *Accounting Education News* 21 (3). Page 3.

Scrapbook1101 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1101

"**Accounting Scholarship that Advances Professional Knowledge and Practice**," AAA Presidential Scholar Address by Robert S. Kaplan, *The Accounting Review*, March 2011, pp. 372-373 (emphasis added)

> More recently, other observers of business school research have expressed concerns about the gap that has opened up in the past four decades between academic scholarship and professional practice.
>
> > Examples include: Historical role of business schools and **their faculty is as evaluators of, but not creators or originators of, business practice**. (Pfeffer 2007, 1335) Our journals are replete with an examination of issues that no manager would or should ever care about, while concerns that are important to practitioners are being ignored. (Miller et al. 2009, 273)
>
> In summary, while much has been accomplished during the past four decades through the application of rigorous social science research methods to accounting issues, much has also been overlooked. As I will illustrate later in these remarks, we have missed big opportunities to both learn from innovative practice and to apply innovations from other disciplines to important accounting issues. By focusing on these opportunities, you will have the biggest potential for a highly successful and rewarding career.
>
> **Integrating Practice and Theory: The Experience of Other Professional Schools**
> Other professional schools, particularly medicine, do not disconnect scholarly activity from practice. Many scholars in medical and public health schools do perform large-scale statistical studies similar to those done by accounting scholars. They estimate reduced-form statistical models on cross-sectional and longitudinal data sets to discover correlations between behavior, nutrition, and health or sickness. Consider, for example, statistical research on the effects of smoking or obesity on health, and of the correlations between automobile accidents and drivers who have consumed significant quantities of alcoholic beverages. Such large-scale statistical studies are at the heart of the discipline of epidemiology.
>
> Some scholars in public health schools also intervene in practice by conducting large-scale field experiments on real people in their natural habitats to assess the efficacy of new health and safety practices, such as the use of designated drivers to reduce alcohol-influenced accidents. Few academic accounting scholars, in contrast, conduct field experiments on real professionals working in their actual jobs (Hunton and Gold [2010] is an exception). The large-scale statistical studies and field experiments about health and sickness are invaluable, but, unlike in accounting scholarship, they represent only one component in the research repertoire of faculty employed in professional schools of medicine and health sciences.
>
> Many faculty in medical schools (and also in schools of engineering and science) continually innovate. They develop new treatments, new surgeries, new drugs, new instruments, and new radiological procedures. Consider, for example, the angiogenesis innovation, now commercially represented by Genentech's Avastin drug, done by Professor Judah Folkman at his laboratories in Boston Children's Hospital (West et al. 2005). Consider also the dozens of commercial innovations and new companies that flowed from the laboratories of Robert Langer at MIT (Bowen et al. 2005) and George Whiteside at Harvard University (Bowen and Gino 2006). These academic scientists were intimately aware of gaps in practice that they could address and solve by applying contemporary engineering and science. They produced innovations that delivered better solutions in actual clinical practices. Beyond contributing through innovation, medical school faculty often become practice thought-leaders in their field of expertise. If you suffer from a serious, complex illness or injury, you will likely be referred to a physician with an appointment at a leading academic medical school. How often, other than for expert testimony, do leading accounting professors get

asked for advice on difficult measurement and valuation issues arising in practice?

One study (Zucker and Darby 1996) found that life-science academics who partner with industry have higher academic productivity than scientists who work only in their laboratories in medical schools and universities. Those engaged in practice innovations work on more important problems and get more rapid feedback on where their ideas work or do not work.

These examples illustrate that some of the best academic faculty in schools of medicine, engineering, and science, attempt to improve practice, enabling their professionals to be more effective and valuable to society. Implications for Accounting Scholarship To my letter writer, just embarking on a career as an academic accounting professor, I hope you can contribute by attempting to become the accounting equivalent of an innovative, world class accounting surgeon, inventor, and thought-leader; someone capable of advancing professional practice, not just evaluating it. I do not want you to become a "JAE" Just Another Epidemiologist . My vision for the potential in your 40 year academic career at a professional school is to develop the knowledge, skills, and capabilities to be at the leading edge of practice. You, as an academic, can be more innovative than a consultant or a skilled practitioner. Unlike them, you can draw upon fundamental advances in your own and related disciplines and can integrate theory and generalizable conceptual frameworks with skilled practice. You can become the accounting practice leader, the "go-to" person, to whom others make referrals for answering a difficult accounting or measurement question arising in practice.

But enough preaching! My teaching is most effective when I illustrate ideas with actual cases, so let us explore several opportunities for academic scholarship that have the potential to make important and innovative contributions to professional practice.

Continued in article

Scrapbook6013 -- http://www.trinity.edu/rjensen/Theory01.htm#Scrapbook6013

"**The Impact of Academic Accounting Research on Professional Practice: An Analysis by the AAA Research Impact Task Force**," by Stephen R. Moehrle, Kirsten L. Anderson, Frances L. Ayres, Cynthia E. Bolt-Lee, Roger S. Debreceny, Michael T. Dugan, Chris E. Hogan, Michael W. Maher, and Elizabeth Plummer*, Accounting Horizons*, December 2009, pp. 411- 456.

> SYNOPSIS:
> The accounting academy has been long recognized as the premier developer of entry-level talent for the accounting profession and the major provider of executive education via master's-level curricula and customized executive education courses. However, the impact that the academy's collective ideas have had on the efficiency and effectiveness of practice has been less recognized. In this paper, we summarize key contributions of academic accounting research to practice in financial accounting, auditing, tax, regulation, managerial accounting, and information systems. Our goal is to increase awareness of the effects of academic accounting research. We believe that if this impact is more fully recognized, the practitioner community will be even more willing to invest in academe and help universities address the escalating costs of training and retaining doctoral-trained research faculty. Furthermore, we believe that this knowledge will attract talented scholars into the profession. To this end, we encourage our colleagues to refer liberally to research successes such as those cited in this paper in their classes, in their textbooks, and in their presentations to nonacademic audiences.

Jensen Comment
This paper received the AAA's 2010 *Accounting Horizon's* best paper award. However, I don't find a whole lot of recognition of work in practitioner journals. My general impression is one of disappointment. Some of my comments are as follows:

Unsubstantiated Claims About the Importance of Accountics Event Studies on Practitioners
The many citations of accounting event studies are more like a listing of "should-have-been important to practitioners" rather than demonstrations that these citations were "actually of great importance to practitioners." For example, most practitioners for over 100 years have known that earnings numbers and derived ratios like P/E ratios impact investment portfolio decisions and acquisition-merger decisions. The findings of accountics researchers in these areas simply proved the obvious to practitioners if they took the time and trouble to understand the complicated mathematics of these event studies. My guess is that most practitioners did not delve deeply into these academic studies and perhaps do not pay any attention to complicated studies that prove the obvious in their eyes. In any case, the authors of the above studies did not contact practitioners to test out assumed importance of accountics research in these events studies. In other words, this AAA Task Force did not really show, at least to me, that these events studies had a great impact on practice beyond what might've been used by standard setters to justify positions that they probably would've taken with or without the accountics research findings.

Mention is made about how the FASB and government agencies included accounting professors in some deliberations. This is well and good but the study does not do a whole lot to document if and how these collaborations found accountics research of great practical value.

Practitioner Journal Citations of Accountics Research
The AAA Task Force study above did not examine practitioner journal citations of accountics research journals

like TAR, JAR, and JAE. The mentions of practitioner journals refer mostly to accounting professors who published in practitioner journals such as when Kenney and Felix published a descriptive piece in the 1980 *Journal of Accountancy* or Altman/McGough and Hicks published 1974 pieces in the *Journal of Accountancy*. Some mentions of practitioner journal citations have to go way back in time such as the mention of the Mautz and Sharaf. piece in the 1961 *Journal of Accountancy*.

Accountics professors did have some impact of auditing practice, especially in the areas of statistical sampling. The types of sampling used such as stratified sampling were not invented by accounting academics, but auditing professors did make some very practical suggestions on how to use these models in both audit sampling and bad debt estimation.

Communication with Users
There is a very brief and disappointing section in the AAA Task Force report. This section does not report any Task Force direct communications with practitioners. Rather it cites two behavioral studies using real-world subjects (rather than students) and vague mention studies related to SAS No. 58.

Unsubstantiated Claims About the Importance of Mathematical Models on Management Accounting Practice
To the extent that mathematical models may or may not have had a significant impact on managerial accounting is not traced back to accounting literature per se. For example, accounting researchers did not make noteworthy advances of linear programming shadow pricing or inventory decision models originating in the literature of operations research and management science. Accounting researcher advances in these applications are hardly noteworthy in the literature of operations research and management science or in accounting practitioner journal citations.

No mention is made by the AAA Task Force of how the AICPA funded the mathematical information economics study *Cost Determination: A Conceptual Approach,* and then the AICPA refused to publish and distanced itself from this study that was eventually picked up by the Iowa State University Press in1976. I've seen no evidence that this research had an impact on practice even though it is widely cited in the accountics literature. The AICPA apparently did not think it would be of interest to practitioners.

The same can be said of regression models used in forecasting. Business firms do make extensive applications of regression and time series models in forecasting, but this usage can be traced back to the economics, finance, and statistics professors who developed these forecasting models. Impacts of accounting professors on forecasting are not very noteworthy in terms of accounting practice.

Non-Accountics Research
The most valid claims of impact of accounting academic research on practice were not accountics research studies. For example, the balanced score card research of Kaplan and colleagues is probably the best cited example of accounting professor research impacting practice, but Bob Kaplan himself is a long-time critic of resistance to publishing his research in TAR, JAR, and JAE.

There are many areas where AIS professors interact closely with practitioners who make use of their AIS professor software and systems contributions, especially in the areas of internal control and systems security. But most of this research is of the non-accountics and even non-mathematical sort.

One disappointment for me in the AIS area is the academic research on XBRL. It seems that most of the noteworthy creative advances in XBRL theory and practice have come from practitioners rather than academics.

Impact of Academic Accountants on Tax Practice
Probably the best section of the AAA Task Force report cites links between academic tax research and tax practice. Much of this was not accountics research, but credit must be given its due when the studies having an impact were accountics studies.

Although many sections of the AAA Task force report disappointed me, the tax sections were not at all disappointing. I only wish the other sections were of the same quality.

For me the AAA Task Force report is a disappointment except where noted above. If we had conducted field research over the past three years that focused on the A,B,C,D, or F grades practitioners would've given to academic accounting research, my guess is that most practitioners would not even know enough about most of this research to even assign a grade. Some of them may have learned about some of this research when they were still taking courses in college, but their interest in this research, in my opinion, headed south immediately after they received their diplomas (unless they returned to college for further academic studies).

One exception might be limited exposure to academic accounting research given by professors who also teach CEP courses such as CEP courses in audit sampling, tax, audit scorecard, ABC costing, and AIS. I did extensive CEP teaching on the complicated topics of FAS 133 on accounting for derivative financial instruments and hedging activities. However, most of my academic research citations were in the areas of finance and economics since there never has been much noteworthy research on FAS 133 in the accountics literature.

Is there much demand for CEP courses on econometric modeling and capital markets research?

Most practitioners who are really into valuation of business firms are critical of the lack of relevance of Residual Income models and Free Cash Flow models worshipped *ad nauseum* in the academic accounting research literature.

A huge difference between engineering professors and accountics science professors is that engineering professors are highly interested in unsolved applied research problems of practicing engineers such as power generation, power transmission, fuel efficiency, weapons development, medical technology, computing efficiency, and on and on. Practicing engineers track this academic research and many inventions can be traced back to academic engineering discoveries.

In contrast, accounting practitioners have little if any interest in accountics science findings. I could only find three inventions by accounting professors worth mentioning, and none of them are attributed to accountics scientists.

	Practitioner Clinical Application	Invented by Accounting Professor
1	**Balanced Scorecard ---** http://en.wikipedia.org/wiki/Balanced_scorecard	Bob Kaplan (shared invention)
2	**REA ---** https://www.msu.edu/~mccarth4/McCarthy.pdf	Bill McCarthy
3	**Business Budgeting in 1922** http://en.wikipedia.org/wiki/Budgeting	James O. McKinsey

This seems to be a pretty dismal record of inventions compared to what other disciplines have invented for their professions. I take it as a sign that academic accountants tend not to venture off campus to study problems whereas practitioners can be studied and addressed in accounting research on campus.

The Pathways Commission strongly recommends much more focus applied problems of the accounting profession:

Scrapbook1137 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1137

The Pathways Commission Implementing Recommendations for the Future of Accounting Education: The First Year Update
American Accounting Association
August 2013
http://commons.aaahq.org/files/3026eae0b3/Pathways_Update_FIN.pdf
Page 109 (Emphasis Added)

Accounting Profession

1. The need to enhance the bilateral relationship between the practice community and academe.

From the perspective of the profession, one impediment to change has been the lack of a consistent relationship between a broadly defined profession (i.e., public, private, government) and a broadly defined academy—large and small public and private institutions. This impediment can be broken down into three subparts. First, the Commission recommends the organizations and individuals in the practice community work with accounting educators to provide access to their internal training seminars, so faculty can remain current with the workings of the profession. These organizations also need to develop internship-type opportunities for interested faculty.

Second, the practice community and regulators need to reduce the barriers academics have in obtaining research data. All stakeholders must work together to determine how to overcome the privacy, confidentiality, and regulatory issues that impede a greater number of researchers from obtaining robust data needed for many of these research projects. Having access to this data could be instrumental in helping the academy provide timely answers to the profession on the impact of policy decisions on business practice.

Third, the profession and the academy need to share pedagogy best practices and resources, especially with respect to rapidly changing educational delivery models as both are essential segments of the lifelong educational pathway of accounting professionals.

Conversely, academia is not without fault in the development of this relationship. The Commission recommends that more institutions, possibly through new accreditation standards, engage more practitioners as executives in residence in the classroom. These individuals can provide a different perspective on various topics and thus might better explain what they do, how they do it, and why they do it. Additionally, the Commission recommends institutions utilize accounting professionals through department advisory boards that can assist the department in the development of its curriculum.

In her Presidential Message at the AAA annual meeting in San Francisco in August, 2005, Judy Rayburn addressed the low citation rate of accounting research when compared to citation rates for research in other fields. Rayburn concluded that the low citation rate for accounting research was due to a lack of diversity in topics and research methods:

> *Accounting research is different from other business disciplines in the area of citations: Top-tier accounting journals in total have fewer citations than top-tier journals in finance, management, and marketing. Our journals are not widely cited outside our discipline. Our top-tier journals as a group project too narrow a view of the breadth and diversity of (what should count as) accounting research.*
> "President's Message," *Accounting Education News* 33 (1): Page 4.

Conclusion About the Future of Accountics Science:

Accountics Scientists Became a Cargo Cult

> *George Ellis, for instance, reviewing Leonard Susskind's book the Cosmic Landscape, concluded that "heavyweight physicists" are claiming to prove the existence of parallel universes "even though there is no chance of observing them." Along similar lines, Michael Atiyah, commenting on another book about theoretical physics, Lawrence Krauss's Hiding in the Mirror, observed that there is no danger of a ""mathematical take-over" of physics, leading o speculations that, while mathematically elegant, are "far removed, or even alien to, physical reality."*
> Pigliucci (2010, P. 25)

Accountics science will, in my opinion, keep its monopoly hold on North American doctoral programs. The reason is simply that research using purchased databases for over 90% of the accountics science research relieves researchers of having to go through the drudgery of having to collect data and being responsible for errors in the data. Unrealistic assumptions avoids having to study dynamic real world systems that seldom, if ever, reach steady states.

Accountics science has become a Cargo Cult detached from the realities of practicing profession. For more on the Cargo Cult of accountics science see Appendix 1687

Is accounting research stuck in a rut of repetitiveness and irrelevancy?

Scrapbook1201 --- www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm#Scrapbook1201

"**Accounting Craftspeople versus Accounting Seers: Exploring the Relevance and Innovation Gaps in Academic Accounting Research**," by William E. McCarthy, *Accounting Horizons*, December 2012, Vol. 26, No. 4, pp. 833-843 --- http://aaajournals.org/doi/full/10.2308/acch-10313

> Is accounting research stuck in a rut of repetitiveness and irrelevancy?
> I (Professor McCarthy) 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.

Scrapbook1211 --- www.cs.trinity.edu/~rjensen/temp/AccounticsDamn.htm#Scrapbook1211

Is Academic Accounting a Cargo Cult Science?

Is accountics "**How Can Accounting Researchers Become More Innovative?** by Sudipta Basu, *Accounting Horizons*, December 2012, Vol. 26, No. 4, pp. 851-87 --- http://aaajournals.org/doi/full/10.2308/acch-10311

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 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 a theoretical 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 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|H0), where D represents data and H0 represents the null hypothesis. However, researchers want to know whether the null hypothesis is true, conditional on the sample, or P(H0|D). We can calculate P(H0|D) from P(D|H0) by applying Bayes' theorem, but that requires knowledge of P(H0), 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 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.

Continued in article

I have little hope for the first two recommendations of the AAA Pathways Commission.

Scrapbook1283 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1283

The Pathways Commission Implementing Recommendations for the Future of Accounting Education: The First Year Update
American Accounting Association
August 2013
http://commons.aaahq.org/files/3026eae0b3/Pathways_Update_FIN.pdf
Page 109 (Emphasis Added)

In the report, the commission acknowledges that some sporadic changes have been adopted, but it seeks to put in place a structure for much more regular and ambitious changes.

The report includes seven recommendations:

- Integrate accounting research, education and practice for students, practitioners and educators by bringing professionally oriented faculty more fully into education programs.
- Promote accessibility of doctoral education by allowing for flexible content and structure in doctoral programs and developing multiple pathways for degrees. The current path to an accounting Ph.D. includes lengthy, full-time residential programs and research training that is for the most part confined to quantitative rather than qualitative methods. More flexible programs -- that might be part-time, focus on applied research and emphasize training in teaching methods and curriculum development -- would appeal to graduate students with professional experience and candidates with families, according to the report.
- Increase recognition and support for high-quality teaching and connect faculty review, promotion and tenure processes with teaching quality so that teaching is respected as a critical component in achieving each institution's mission. According to the report, accounting programs must balance recognition for work and accomplishments -- fed by increasing competition among institutions and programs -- along with recognition for teaching excellence.
- Develop curriculum models, engaging learning resources and mechanisms to easily share them, as well as enhancing faculty development opportunities to sustain a robust curriculum that addresses a new generation of students who

are more at home with technology and less patient with traditional teaching methods.

- Improve the ability to attract high-potential, diverse entrants into the profession.

- Create mechanisms for collecting, analyzing and disseminating information about the market needs by establishing a national committee on information needs, projecting future supply and demand for accounting professionals and faculty, and enhancing the benefits of a high school accounting education.

- Establish an implementation process to address these and future recommendations by creating structures and mechanisms to support a continuous, sustainable change process.

I hope I’m wrong, and that one day some leading accountics scientists, not all, will commence to invent things of great value to accounting practitioners like engineering professors invent things of great value to practicing engineers. The first step will be to immerse more accountics scientists and their students into the practitioner world.

I hope I’m wrong and that accounting researchers and their doctoral students will once again develop “multiple pathways” to doctoral degrees with more varied and non-quantitative research methodologies. But it took decades to narrow those doctoral programs down to accountics science programs, and it will probably take decades to expand those doctoral programs toward doctoral dissertations without equations.

Scrapbook1263 --- http://www.trinity.edu/rjensen/TheoryTAR.htm#Scrapbook1263

"**Research on Accounting Should Learn From the Past**" by Michael H. Granof and Stephen A. Zeff, *Chronicle of Higher Education*, March 21, 2008 (Emphasis Added)

To be sure, some accounting research, particularly that pertaining to the efficiency of capital markets, has found its way into both the classroom and textbooks — but mainly in select M.B.A. programs and the textbooks used in those courses. There is little evidence that the research has had more than a marginal influence on what is taught in mainstream accounting courses.

What needs to be done? First, and most significantly, journal editors, department chairs, business-school deans, and promotion-and-tenure committees need to rethink the criteria for what constitutes appropriate accounting research. That is not to suggest that they should diminish the importance of the currently accepted modes or that they should lower their standards. But they need to expand the set of research methods to encompass those that, in other disciplines, are respected for their scientific standing. The methods include historical and field studies, policy analysis, surveys, and international comparisons when, as with empirical and analytical research, they otherwise meet the tests of sound scholarship.

Second, chairmen, deans, and promotion and merit-review committees must expand the criteria they use in assessing the research component of faculty performance. They must have the courage to establish criteria for what constitutes meritorious research that are consistent with their own institutions' unique characters and comparative advantages, rather than imitating the norms believed to be used in schools ranked higher in magazine and newspaper polls. In this regard, they must acknowledge that accounting departments, unlike other business disciplines such as finance and marketing, are associated with a well-defined and recognized profession. Accounting faculties, therefore, have a special obligation to conduct research that is of interest and relevance to the profession. The current accounting model was designed mainly for the industrial era, when property, plant, and equipment were companies' major assets. Today, intangibles such as brand values and intellectual capital are of overwhelming importance as assets, yet they are largely absent from company balance sheets. Academics must play a role in reforming the accounting model to fit the new postindustrial environment.

Third, Ph.D. programs must ensure that young accounting researchers are conversant with the fundamental issues that have arisen in the accounting discipline and with a broad range of research methodologies. The accounting literature did not begin in the second half of the 1960s. The books and articles written by accounting scholars from the 1920s through the 1960s can help to frame and put into perspective the questions that researchers are now studying.

For example, W.A. Paton and A.C. Littleton's 1940 monograph, An Introduction to Corporate Accounting Standards, profoundly shaped the debates of the day and greatly influenced how accounting was taught at universities. Today, however, many, if not most, accounting academics are ignorant of that literature. What they know of it is mainly from textbooks, which themselves evince little knowledge of the path-breaking work of earlier years. All of that leads to superficiality in teaching and to research without a connection to the past.

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 is a professor of accounting at the McCombs School of Business at the University of Texas at Austin. Stephen A. Zeff is a professor of accounting at the Jesse H. Jones Graduate School of Management at Rice University.

March 18, 2008 reply from Paul Williams [Paul_Williams@NCSU.EDU]
Steve Zeff has been saying this since his stint as editor of *The Accounting Review* (TAR); nobody has listened. Zeff famously wrote at least two editorials published in TAR over 30 years ago that lamented the colonization of the accounting academy by the intellectually unwashed. He and Bill Cooper wrote a comment on Kinney's tutorial on how to do accounting research and it was rudely rejected by TAR. It gained a new life only when Tony Tinker published it as part of an issue of *Critical Perspectives in Accounting* devoted to the problem of dogma in accounting research.

It has only been since less subdued voices have been raised (outright rudeness has been the hallmark of those who transformed accounting into the empirical sub-discipline of a sub-discipline for which empirical work is irrelevant) that any movement has occurred. Judy Rayburn's diversity initiative and her invitation for Anthony Hopwood to give the Presidential address at the D.C. AAA meeting came only after many years of persistent unsubdued pointing out of things that were uncomfortable for the comfortable to confront.

Paul Williams