

Reverse the Curse of the Top-5

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Abstract

The past 40 years has seen a large increase in the number of articles submitted to journals ranked in the top-5 of their discipline. This increase is the rational response, by faculty, to the overweighting of publications in these journals by university promotions and tenure committees. The ranking factors for academic journals, however, arose for a completely different purpose, to guide the journal acquisition decisions by budget-constrained university librarians. Using journal impact factors to infer the quality of an faculty members' publications incurs a high incidence of both Type 1 errors, when we conclude incorrectly that a paper published in a top-5 journal is a high-impact paper, and Type 2 errors, when we conclude that papers (and books) not published in top-5 journals have low impact. In addition, a third type of error gets introduced as faculty pursue the research they perceive is favored by editors of top-5 journals, at the potential expense of more innovative and relevant research, perceived to be unpublishable in a top-tier journal. Accounting scholarship, in particular, has underinvested in research about innovative practices or the emerging accounting issues faced by contemporary organizations (Kaplan, 2011), likely because such research is viewed as unpublishable in top-5 journals. This gap persists despite recent scholarship that has documented how important, fundamental ideas can emerge from "use-inspired" research (see Pasteur's Quadrant (Stokes, 1997)). The paper concludes by suggesting reforms to overcome the dysfunctional fixation on publication in top-5 journals.

¹ Text of speech delivered at American Accounting Association Annual Meeting (August 2017). I am indebted to the co-speakers on the panel, Catherine Schrand and Robert Swieringa, for valuable references that provided the framework for the talk and this paper.

The American Economic Association, in its January 2017 annual meeting, convened a panel of a half-dozen Nobel Prize winners in economics to discuss what it called "[The Curse of the Top Five](#)." Figure 1 shows the nearly 100% increase in submissions to top-5 ranked economics journals between 2000 and 2010, a growth that has continued up through the current time. Clearly, more and more economists are trying to publish their work in a rarefied tier of journals, a trend that is also occurring for the natural, life, and physical science journals, and, I suspect, in top-tier academic accounting journals. The demand for paper placement in top-ranked academic journals is the rational response of faculty to the over-weighting of publications in top-tier journals by university promotions and tenure committees. Heckman and Moktan (2018) provides extensive empirical evidence of this effect among economics faculty and departments.

Journal ranking began in the 1970s when the irresistible force of rising journal subscriptions costs ran into the almost immovable object of library budgets. Garfield (2006) describes how he helped to develop a journal impact factor (JIF) to guide library selection decisions. The JIF, anticipating the Google search mechanism, was based on the number of references made to articles published in a journal.

This metric, however, soon got adopted by promotions and tenure committees as a primary tool for assessing the academic quality of faculty, based on where they published their papers. This is a completely different purpose for the JIF from the one for which it had been designed. Of course, by accident, this might be an excellent metric for assessing the quality and impact of a faculty member's research. But a quite extensive literature, particularly in the life sciences, describes many errors and problems introduced when JIF publication scores are used to assess the quality of the articles published within them (see, for example, Callay, 2016; Larivière, Kiermer et al, 2016; EASE, 2007; and Seglen, 1997). While all the problems are worth learning more about, I will focus on only one, the distribution of citation counts among papers published in journals with the highest JIFs.

Type 1 and Type 2 Errors with the JIF

The distribution of citations to articles in top-tier journals follows a highly-skewed distribution, not a uniform one (see Figure 2). A small fraction of influential papers accounts for most of the citations, with the great majority (at least 70%) getting few or none. This is not a problem for the original intent of creating the JIF, helping libraries decide to which journals they should subscribe. But it is a huge problem when using JIFs as a surrogate for the quality of every paper published in a journal. Because the distribution of citations for articles is highly skewed, the total of number of citations for the journal is not strongly correlated with the number of citations for any individual article. Second, even the number of citations for an individual article is not necessarily a good measure of the quality of the article. The single most prevalent conclusion from the many articles commenting on JIFs is: "the journal cannot in any way be taken as representative of the quality of the article." Scholars concur that the best approach for evaluating the scientific quality of articles is to have qualified experts read and evaluate

them, a point I will return to later in this paper.

The prevalent use of JIFs to infer the quality and impact of a faculty member's publications leads to a high incidence of both Type 1 and Type 2 errors. The Type 1 errors, the false positives, are the large numbers of low impact papers published in journals with high JIFs. In these cases, promotions committees incorrectly reject the null hypothesis that the author's paper is unimportant. The high incidence of such false positives, while disconcerting is actually less consequential than the Type 2 (false negative) errors, when promotions committees incorrectly treat research as unimportant and low quality if it has not been published in top-tier journals or, worse, in books, conference proceedings, and other publication outlets ignored by the JIF metric. Many non-top-tier publications can actually have impact factors far above the median of articles published in top-tier journals.

Consider the following Type 2 errors in economics. Books by authors such as Adam Smith, John Maynard Keynes, and Gary Becker have had huge influence and impact. References to the books of these three authors are likely orders of magnitude higher than all but a very few economics papers published in top-5 journals. Imagine the absurdity of a university committees complimenting these authors on their excellent books, but deferring their promotion until they could condense them into 4,000-word papers and have them accepted and published in a top-5 journal.

Economics Nobel Laureate, Robert Lucas (who gave a plenary talk at the 2007 AAA meetings), published perhaps his two most important papers (Lucas, 1972 on rational expectations; Lucas, 1988 on economic development) in journals not ranked among the top-10 in economics. The "Lucas critique" of traditional economic policy (Lucas, 1976), published in a proceedings volume of the Carnegie-Rochester Conference, currently has more than 8,000 citations, placing it in a very high percentile among all articles published in top-5 economics journals.

Another economics Nobel Laureate, Angus Deaton, with co-author Ann Case, has published two enormously influential publications in proceedings volumes (Case and Deaton, 2015, 2017). These articles document that white, non-college educated Americans experienced increasing mortality rates during 2000-2015, primarily due to deaths of despair: opioid abuse, alcoholism, suicides, and traffic accidents (see Figure 3). To my knowledge, this is the first time in US history that a major subpopulation of any type has experienced an increasing rather than a decreasing mortality rate over such an extensive time period. Their data have extraordinary implications for public policy and future research, and, incidentally, also help us understand the 2016 Republican primaries and the subsequent national election. Deaton, at the 2017 AEA panel, stated that he could not have published these two papers in a top five or even top-10 economics journal. But Deaton works on problems he thinks are important, not on papers he can publish in a top-5 journal. A promotions committee that classifies his papers, as well as Lucas's seminal works, as inconsequential because of where they

were published makes a very profound type-2 error. A top-10 list of highly cited, influential articles not published in top-5 economics journals appears in Table 7 of Heckman and Moktan (2018).

Amazingly, we teach our doctoral students all the time about Type 1 and Type 2 errors, yet fail to apply these concepts to the most important decisions we academics make, who to attract, retain and promote as our colleagues. Many institutions, around the world, use a performance metric with an unacceptably high incidence of both type 1 and type 2 errors. Promotion committees have apparently fallen into a trap familiar to accountants: “if you can’t measure what you want [quality of published papers], want what you can measure [JIF of the journal in which a paper is published]”. The problem was also described by (Kerr, 1975), “On the folly of hoping for A [e.g., papers with high quality and impact], while rewarding B [publishing in journals with high JIF]. This is a stunning dysfunction.

Academic performance evaluation committees, when they rely on this imperfect metric, also commit, what I will call, a Type 3 error. They systematically discourage faculty and doctoral students from working on potentially innovative and important problems when they counsel them to work only on topics, sometimes minor incremental additions to existing academic literature, that can be published in a top-five journal. The Type 3 error systematically discourages accounting academics from learning about problems and innovations occurring in their professional practice of accounting (Kaplan, 2011) since papers about such work are unlikely be published in a top-5 journal.

Basic Research, Applied Research, and Pasteur’s Quadrant

What are the consequences of systematically discouraging research based on contemporary phenomena and innovations in practice? Vannevar Bush (1945) introduced the dichotomy of basic and applied research, with basic research attempting to understand, explain, and predict natural and other phenomena, but with no regard for eventual practical implications. Scientists who performed outstanding basic research include Niels Bohr, Albert Einstein, Michael Faraday, James Maxwell, Francis Crick, and James Watson. Applied research, as typified by the work of Thomas Edison, seeks to answer a question or solve a problem in the real world. Applied research uses known science to create quite marvelous new things. In addition to Edison’s multiple inventions, other examples of excellent applied research include the development of the transistor and the laser, both of which are based on known quantum mechanical principles that their inventors applied and packaged in innovative and enormously useful ways. Think of Steve Jobs’ invention of the iPhone and the iPad, neither of which produced any fundamental new knowledge, but both of which, by integrating and applying existing technologies from multiple disciplines, have greatly enriched our lives. In Finance, the invention, at JP Morgan Chase in the 1990s, of credit default swaps produced a highly useful and widely-adopted new financial instrument based on existing knowledge in financial economics.

Recent scholarship, however, has shown that Bush's simple linear model of basic research and applied research is too limited. Stokes (1997) argued that some of the best science is carried out in what he called "Pasteur's Quadrant" (see Figure 4) where researchers are motivated simultaneously to expand our understanding of basic phenomena while also increasing our capabilities to improve society and the world. Pasteur, a giant of modern science, founded the field of microbiology and his discoveries led directly to solving practical problems. He invented a process, named after him (pasteurization), to kill bacteria that caused wine, beer and milk to go sour. He also helped to develop germ theory and then applied the theory to develop vaccines for anthrax and rabies. How did Pasteur get started on this pioneering research? Pasteur was a professor of chemistry at the University of Strasbourg, which is adjacent to some of the best wine regions of the world. Owners of local vineyards came to Pasteur to seek his advice about why their wine were becoming contaminated and tasted badly. Pasteur took on the challenge to identify the source of the contamination, which led to his fundamental discoveries on the bacterial origin of contamination. Subsequently, he saved the silk industry by discovering how certain microbes destroyed silkworm eggs, and then developed the process to prevent contamination from occurring. Research in Pasteur's Quadrant, motivated by problems and anomalies in the world, create breakthrough scientific discoveries that not only help us *understand* fundamental phenomena, but also can be applied to *influence* and *change* the phenomena.

Learning about Pasteur's Quadrant, I began to think about the fundamental discoveries in management theory that have been inspired by practice. Consider the work done in the 1920s and 1930s by Harvard Business School faculty Elton Mayo and Fritz Roethlisberger. They became interested in employee motivation at Western Electric's Hawthorne plant (Mayo, 1933; Roethlisberger and Dickson, 1939), and their research helped to launch the field of organizational behavior. In Finance, the Black-Scholes-Merton options pricing model was clearly inspired to solve a practical pricing problem. Financial options already existed and were widely traded, but people didn't understand their pricing, an anomaly that inspired the discovery of the fundamental options pricing model. The work of Tversky, Kahneman, and Thaler changed the way we think about individual decision making by studying how people actually made decisions under uncertainty, rather than assuming that they made them in ways that were analytically convenient for economists' mathematical models. These are marvelous examples of how fundamental management knowledge arose from scholars studying phenomena, problems, and anomalies in actual practice.

Accounting Research and Pasteur's Quadrant

I could not think of any pure, fundamental accounting research, a discovery unrelated to practice, to place in the upper left-hand quadrant of Stokes' diagram. But accounting is an applied, practical discipline; we build from fundamental advances made in related social science disciplines, such as economics, psychology, sociology, and political science. So I was not overly depressed by this gap in our research production

function.

For the lower right-hand quadrant, applied useful research, we have been conducting research during the past 50 years that tested and explored the practical implications of Finance's efficient market hypothesis. The research exploited our knowledge of accounting information and the institutional structures of capital markets and governance. We have also applied principal-agent models, from economics, to understand important concepts in the design and implementation of management and employee incentives. I am sure we could easily find additional examples to populate this quadrant.

Accounting scholarship also has several good examples of use-inspired research that we can place within Pasteur's Quadrant. Pacioli's description of double-entry bookkeeping was inspired by the system of accounts used by 15th century Venetian merchants. Somewhat more recently, Ohlson (1995) introduced fundamental concepts for Accounting Based Valuation (ABV). This discovery was followed by Palepu and Healy (2013) who made the concept implementable by financial analysts, providing them with an alternative and more practical valuation approach than Finance's discounted cash flow model. After extensive field studies and case studies of how companies use various mechanisms to simultaneously inspire, control, and motivate employees, Simons (1995) introduced the levers of management control framework. I can, immodestly, suggest that both activity-based costing (Kaplan-Cooper, 1996) and the balanced scorecard (Kaplan-Norton, 1996) introduced new measurement concepts based on identifying and then filling gaps in contemporary measurement practices.

Unfortunately, much of recent academic accounting research exists in the death valley of the lower left-hand quadrant of Stokes' 2 by 2 diagram. Such research, including many articles in top-5 journals, is neither fundamental nor useful. As a discipline, we really should do much less of this research. We need more research in Edison's quadrant; i.e., research that is relevant and useful. Even better would be to have more research that introduces new, fundamental ideas into our literature and our teaching, based on in-depth study of practice innovations, gaps, and anomalies.

What makes it hard for academics to conduct research that is useful? Consider the knowledge enterprise diagram from Roethlisberger (1977, 393). The Roethlisberger diagram places the study and influence of phenomena as the foundation for research, with normal academic social science research as its pinnacle. The problem, as described by Roethlisberger, is that academic scholars have become trained to be excellent and prolific at the top of the knowledge enterprise. Virtually all accounting research, especially the research published in top-5 journals, exists at the diagram's peak. But the work required to produce research in Pasteur's Quadrant must originate at its foundation. Scholars need to immerse themselves in practice to identify new issues, new anomalies, and, even better, new discoveries that will advance both theory and practice. But such research, as noted by Schrand (2018) and others, does not typically

get published in top-5 journals.

We can now see how JIF ratings contribute to type-3 errors. They systematically discourage scholars from discovering and solving important problems in their professional fields. The JIF ratings lead to most accounting scholars striving to write papers that can be accepted in a top-5 journal but ending up producing research that is neither fundamental nor useful.

Recommendations to Change Academic Evaluation Processes

Having established the nature of the problem, I can now finally address my assigned topic: how do we modify academic performance evaluation to encourage research that is more relevant and useful? What approaches can we introduce and what practices should we actively discourage to (i) reduce the quantity of accounting research in the death-valley quadrant of being neither basic nor useful, and (ii) increase the quantity of scholarship that falls in the Pasteur and Edison quadrants? I will illustrate with three examples that are suggestive of potential reforms.

The Ross School at the University of Michigan, in its annual survey of each faculty's activity and accomplishment, has added a "practice" category to the three traditional categories of research, teaching and service. We, as accounting academics, know that just the act of measurement influences behavior, even without explicit reward or punishment. Faculty, knowing that every year they will have to fill out this fourth section of the annual report to the dean, may get into a frame of mind to think about what they might do during the year that would enable them to avoid having nothing to report in the practice category. It also allows faculty who may have a blank "research" section to be able to say they are engaged in practice, such as by active participation in a professional association (e.g., AICPA, IMA, FEI), either nationally or regionally, publishing in a practitioner journal, or presenting or teaching at a practitioner conference. This enables faculty who are not active in research to excel on the practice dimension.

Harvard Business School, my current affiliation, attempts to motivate scholarship that has an impact on practice by defining three audiences for our work: researchers, educators, and practitioners. The HBS criteria for promotion to tenure require that a candidate's work demonstrate leadership, a major substantial impact, on one of the three audiences, and a significant impact on a second. A candidate cannot be promoted to tenure purely on the research dimension. A research candidate must also demonstrate some impact either for educators, through innovation in case writing and course development, or for practitioners, such as by interpreting and publishing his or her research in books and practitioner journals. The two-audience promotion standard nudges faculty to consider audiences, other those that read top-5 academic journals, for their work.

At my previous school, Tepper School at Carnegie Mellon University, we did not

simply count publications in top-tier journals. I can recall several instances where faculty were promoted to tenure with significantly fewer top-tier publications than many others of their vintage and national cohort group. At each promotion decision point, from assistant to non-tenured associate professor, from non-tenured associate to tenured associate, and from tenured associate to tenured full professor, the candidate submitted three or four papers for all members of the appointments committee, the full professors, to read. The dean assigned two faculty members to present the case, pro and con, for the promotion decision. The promotions decision was not an exercise in counting. Quite the contrary, it was highly interactive, characterized by extensive and active dialogue and debate among all the attendees. In Simons' levers-of-control framework, CMU operated its evaluation and promotions process as an *interactive* system, not a *diagnostic* one. Counting publications in top-5 journals, or calculating a weighted-average metric of published work is a diagnostic system. You do not want to use a diagnostic system for your most important, strategic decisions. These decisions, while informed by data, require active discussions, interactions and debates.

Recommendations for Business Schools and Accounting Departments

Each business school and accounting department can take several small steps to engage faculty members more intimately with practice. Some of my suggestions will reinforce the recommendations of Swieringa (2018) and Schrand (2018) but may be worth the repetition. First, encourage faculty to teach in executive education programs, but not just courses on "accounting for nonfinancial managers." Faculty should be encouraged to teach their research in executive classrooms and pay attention to the feedback and evaluations. Executives are much more demanding, than undergraduate or MBA students, in demanding that the content taught to them be relevant to their actual experience and practice (Garvin, 2007). Personally, I sought out executive program assignments as an opportunity to teach and test my emerging thinking about activity-based costing, balanced scorecard, and enterprise risk management. It's not a randomly controlled experiment, but I gained confidence in the validity and impact of my ideas by presenting and testing them in executive classrooms.

Second, provide opportunities for faculty to visit with and learn from managers at companies and nonprofits. Most schools have Advisory Boards populated with alumni and local business and nonprofit leaders. The organizations represented on a business school or accounting department's Advisory Board are sites that faculty members can visit, conduct research, and write teaching cases.

Third, nudge faculty into having at least one relevant research stream by asking them to occasionally speak about their research and its impact at meetings of the school's alumni. Faculty may then have more motivation to develop content that will be relevant for that audience.

Recommendations for the American Accounting Association

AAA Doctoral and New Faculty Consortia invite editors of the top journals to

appear and talk to the doctoral students and new faculty. This is good. Doctoral students and new faculty should meet editors from top-5 journals to learn and understand the process for getting articles accepted in those journals. But who is not invited to these meetings? The participants less frequently see someone who succeeded in academia while being heavily engaged in practice. The AAA therefore, implicitly, and perhaps subconsciously, elevates the role model to become an editor of a top-5 journal, but does not elevate, as a role model, someone who has actively engaged with practice. Imagine if at comparable meetings of medical students, residents, and new medical faculty, only editors of top life sciences journals were on the program but not innovative and highly successful surgeons and oncologists. The AAA can certainly draw upon dozens of faculty who are distinguished scholars and also have actual experience engaging with practice. We should encourage them to talk to doctoral students and new faculty members about what they've learned from and contributed to practice.

For AAA journals, we should rethink the primary mission of *Accounting Horizons*. Many believe that this is the journal to communicate our research do practitioners. I disagree. Practitioners don't read *Accounting Horizons*. They have their own journals to read. Academics that want to reach practitioners with their ideas should write articles for journals such as *Financial Analyst Journal*, *Journal of Applied Corporate Finance*, *Journal of Accountancy*, *CPA Journal*, *Harvard Business Review*, *Sloan Management Review* and *California Management Review*. As another outlet, Schrand (2018) encouraged faculty to write chapters for practitioners' handbooks. Acceptance by editors of practitioner journals and handbooks helps to validate that your ideas are relevant and impactful for their target audience.

Since we don't lack for outlets for our ideas to reach practitioners, *Accounting Horizons* can fill a distinctive niche by providing an outlet to migrate ideas in the reverse direction: from practice to academics. We should use *Horizons* to publish research that describes practice innovations, practice anomalies and gaps in practice. I have used *Horizons* in this way for my own work. During the past 25 years, I've published a half-dozen articles in *Horizons* that describe practice-based and practice-inspired research. These papers seem to have had a good impact, at least by the conventional metric of citation counts, with three of them having citation counts that are an order of magnitude higher than the median citation count in a top-5 accounting journal of the same vintage. This is another example of the Type-2 error of under-valuing articles that appear in journals with lower JIFs even when those articles can have far higher impact - being read, being cited, and being used for further research - than most articles in top-5 journals.

Beyond *Horizons*, the AAA should further exploit its Section journals as outlets for practice-based articles. The Section journals can encourage faculty to submit in-depth studies of current problems, innovations, anomalies, gaps, and opportunities within the Section's practice domain, such as auditing, tax, IT, and public policy. Such

papers, positioned at the base of Roethlisberger's knowledge enterprise diagram, can provide the foundation for academic research that ends up in Pasteur's Quadrant.

The AAA can also convene meetings where accounting faculty interact closely and extensively with innovative practitioners, such as recent Accounting IS Big Data conferences. The practitioners chosen to participate in such meetings should not be selected because of their lofty position in a corporation, public accounting firm, regulatory agency, or standard-setting entity. The practitioners should be chosen carefully, for their vision, innovation, and quality of ideas. For an example from my vintage, Robert Elliott, a senior technical partner of Peat Marwick (now KPMG), was a far superior communicator of practitioner insight and knowledge than whoever happened to be the CEO of a major accounting firm at that time

The AAA can also work with the AACSB to provide opportunities for "Scholarly Academics" (defined in AACSB accreditation standards) to become qualified as "Practice Academics," those who "sustain currency and relevance through professional engagement, interaction, and relevant activities."

Recommendations for University Promotion Committees

Finally, I have suggestions for those of you in a position to influence evaluation criteria and processes in business schools and universities. First, it's really not a good idea to outsource your most important decision to a handful of editors who are external to your school, and who don't know or care at all about your school's mission and strategy. You should work out for yourself, and be clear about, what you want your faculty members to accomplish. I would be surprised, given the high incidence of Type 1, 2, and 3 errors, that many schools have a mission for its faculty to publish as many papers as they can in top-5 journals.

Second, as I mentioned earlier, to evaluate the quality of your faculty's research papers, there is no good substitute than to actually read the papers. Now, perhaps the senior faculty at some schools no longer have the expertise to read the papers of junior faculty and evaluate their quality and potential impact. In this situation, rather than default to a highly imperfect metric, I recommend that you consider someone or several people, to work with you to read, interpret, and evaluate the papers, and make a recommendation on the promotion decision, according to the criteria you have established for your school's mission and strategy. A tenure decision represents a 30 to 40-year commitment by the school. Given current salaries for tenured professors of accounting, say \$250K and often much higher, and today's low long-term interest rates, promoting a faculty member to a tenured full professor position is equivalent to making a \$3 to \$7 million investment decision. Should you feel a little uncomfortable or unqualified to make this risky multi-million investment, it's worth spending a little money to get coaching and guidance to lower your high risk of making a Type-1 or Type-2 error when you default to a performance metric based on publications in journals with high JIFs.

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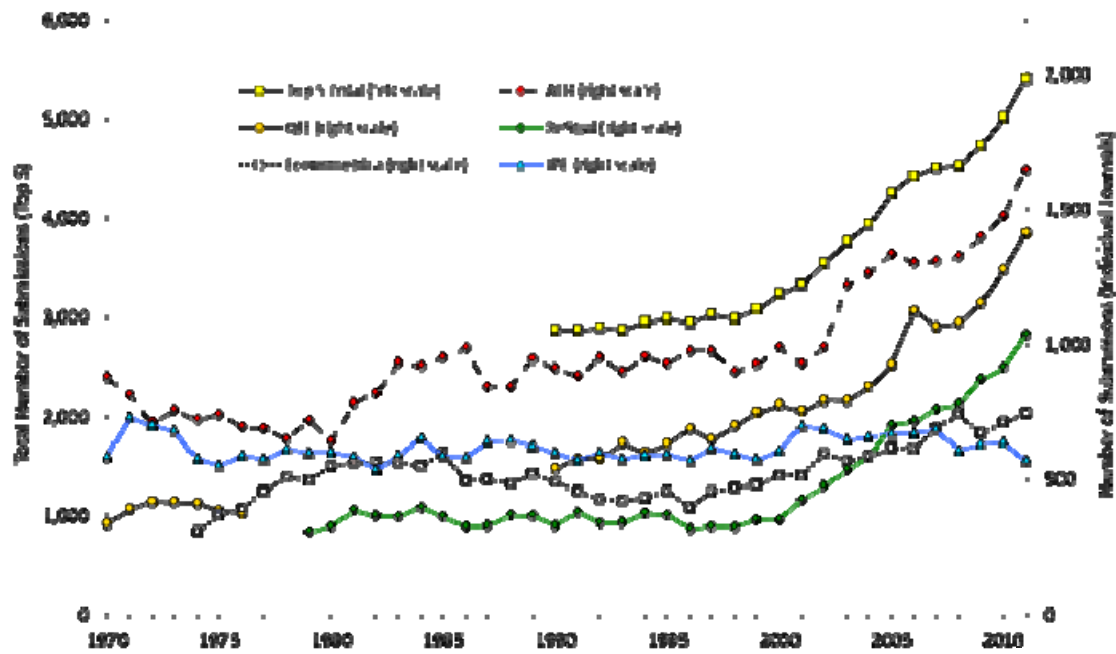
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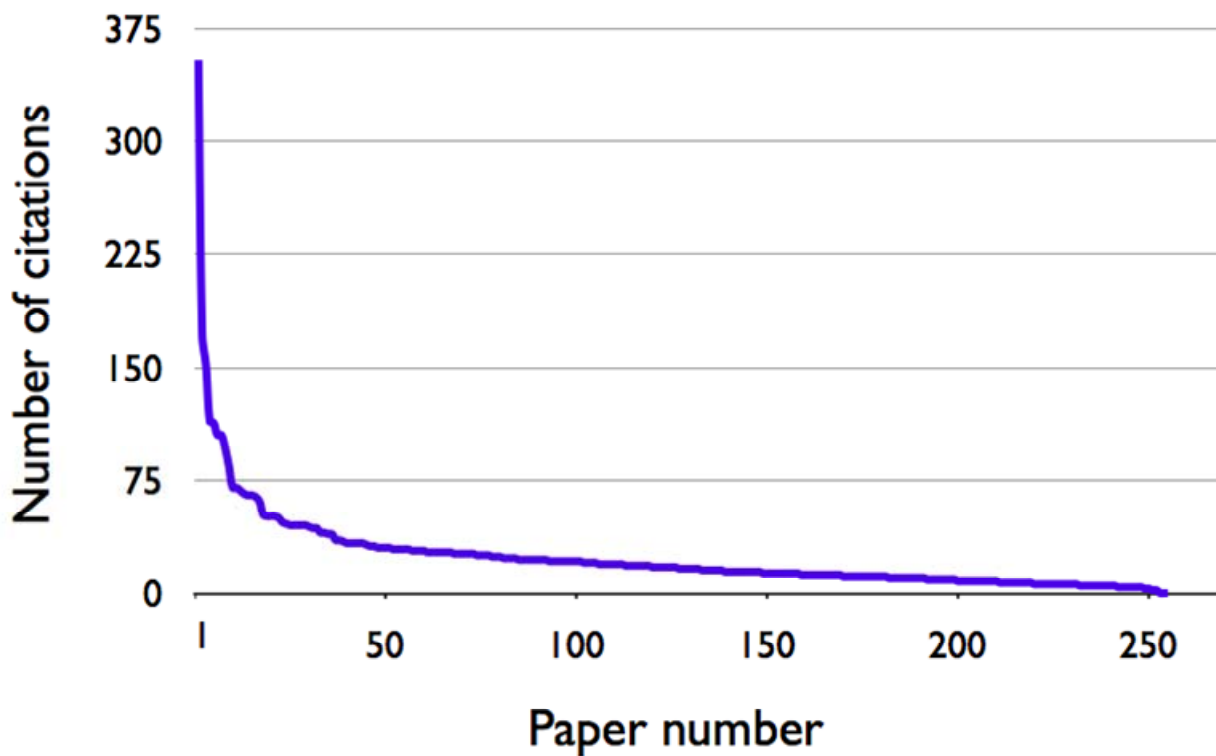
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Figure 1 Submissions to top-5 ranked economics journals: 1970-2010



Source: David Card and Stefano DellaVigna, "Nine facts about top journals in economics," 21 January 2013 VOS CEPR's Policy Portal, <http://voxeu.org/article/nine-facts-about-top-journals-economics> (accessed 13 Oct 2017).

Figure 2 Citations in 2014 to *Nature Chemistry* articles published in 2012-2013.

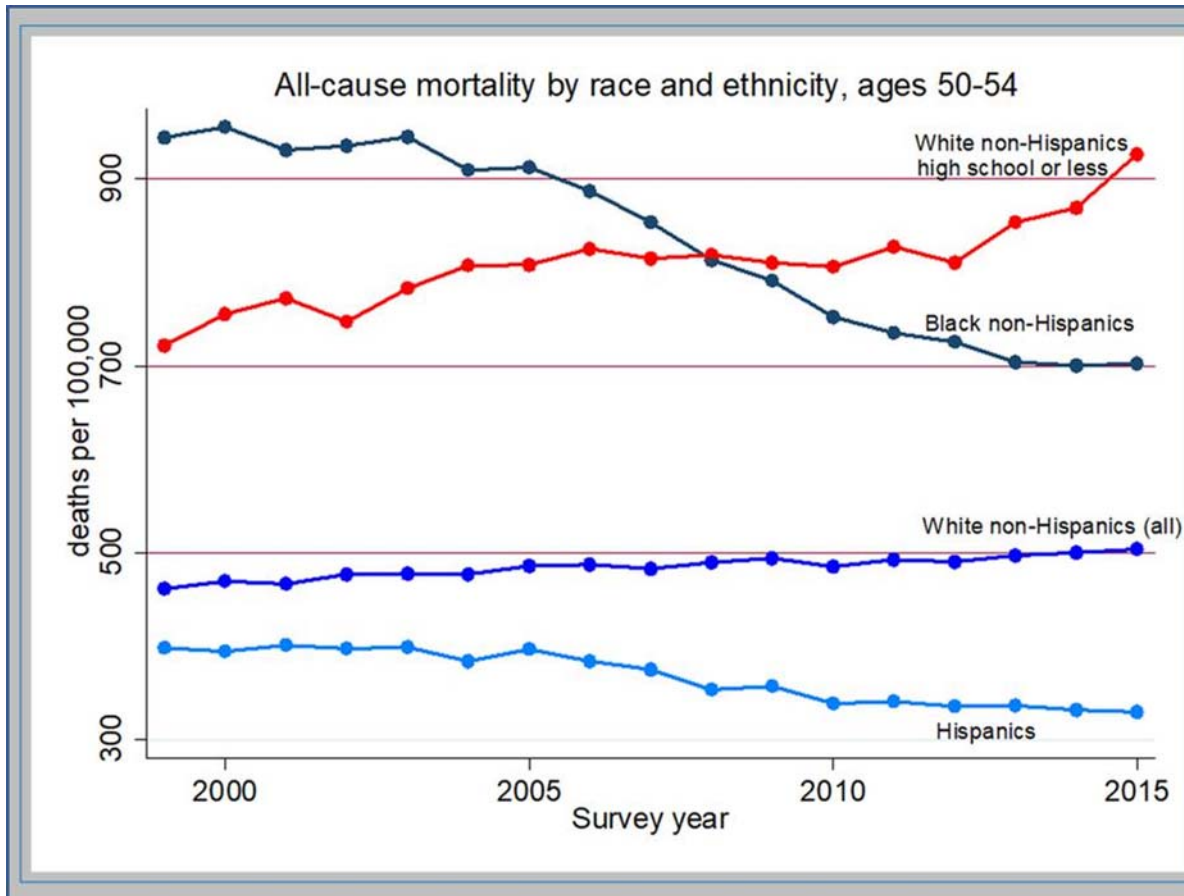


Source: <http://blogs.nature.com/thescepticalchymist/2015/12/nature-chemistrys-2014-impact-factor-citation-distribution.html>
(accessed 12 Oct 2017)

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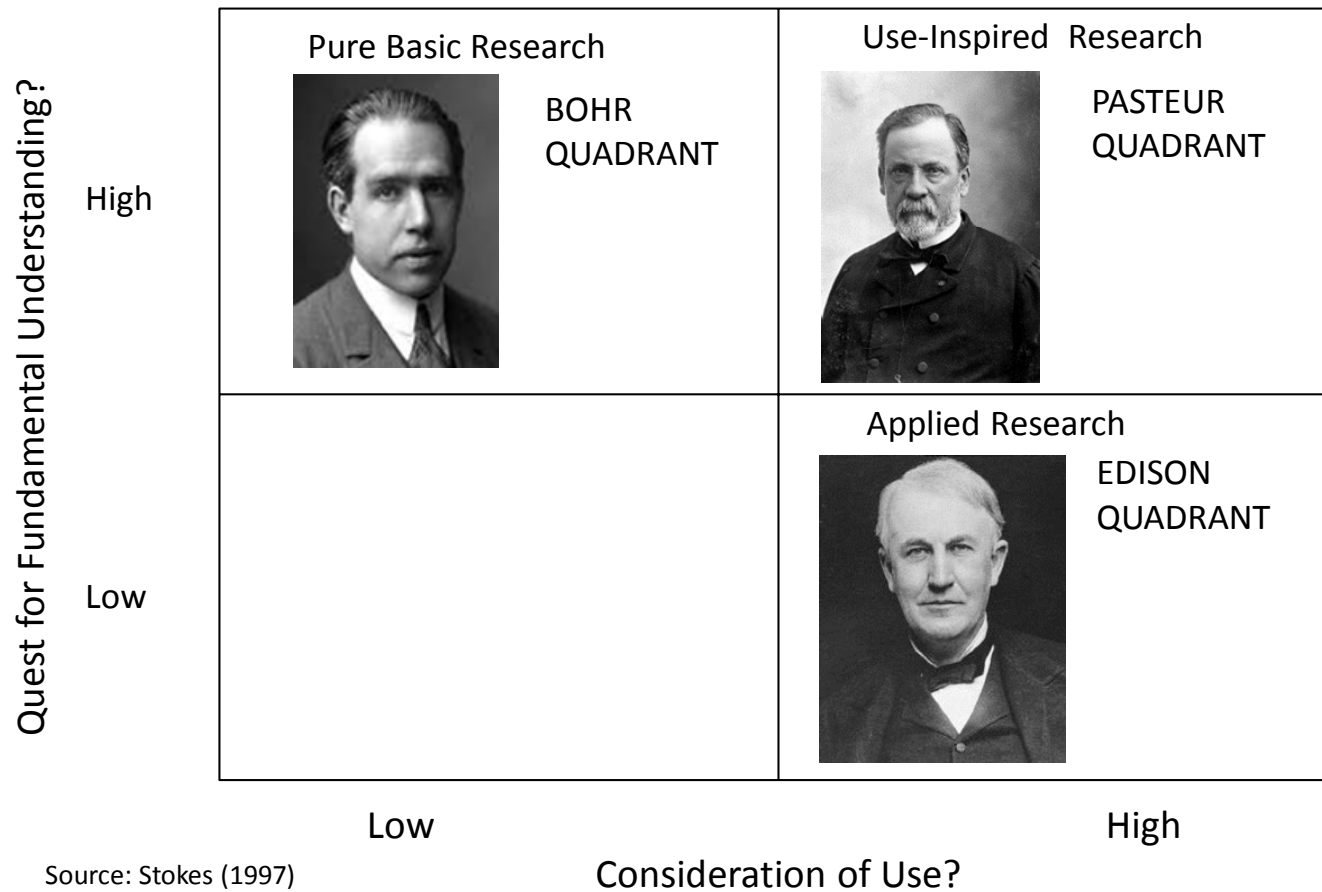
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Figure 3: All-cause US mortality by race and ethnicity, ages 50-54



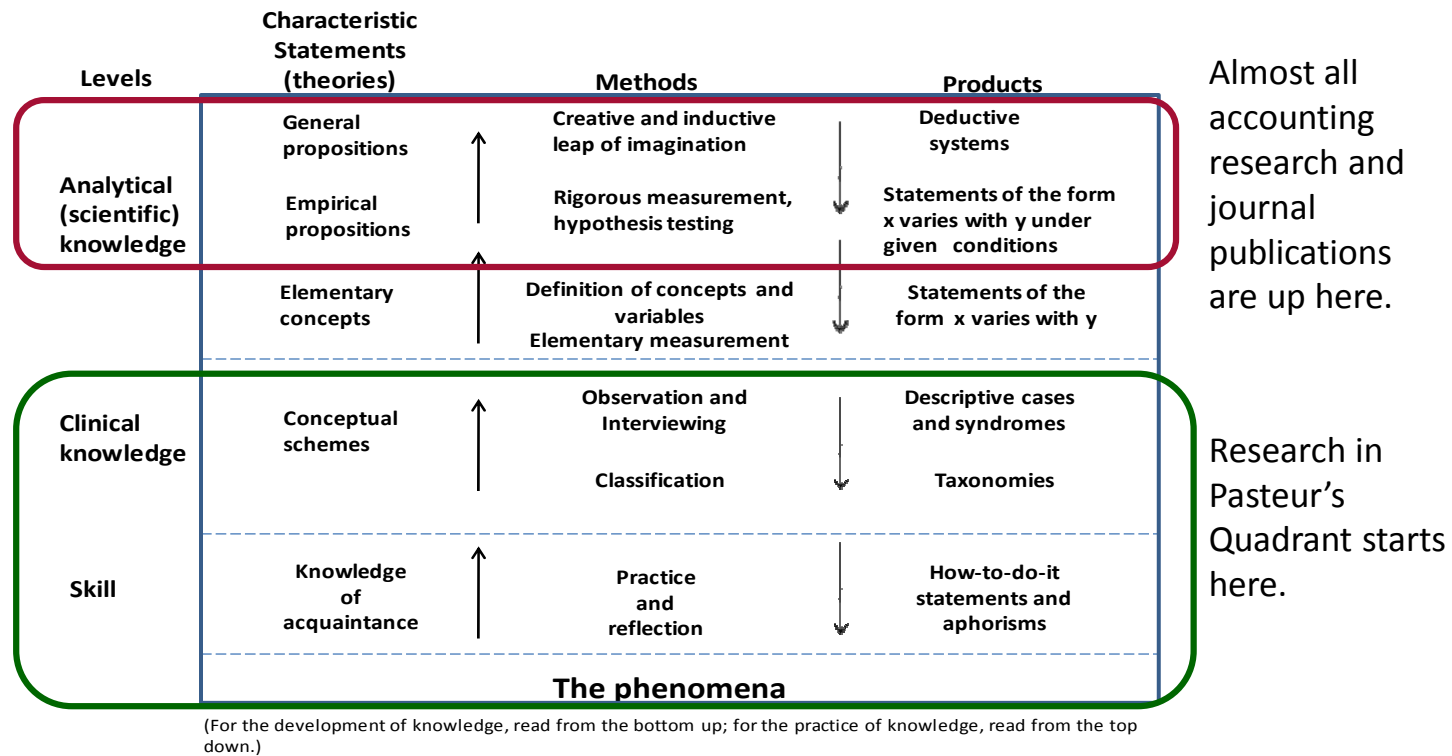
Source: A. Case and A. Deaton, "Mortality and Morbidity in the 21st Century," *Brookings Papers on Economic Activity* (2017)

Figure 4: Pasteur's Quadrant



Source: Stokes (1997)

Figure 5: The Roethlisberger diagram of The Knowledge Enterprise



Source: Roethlisberger (1977, 393).