Rigor Plus Relevance: The Open Electronic Archive Option

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Abstract
Currently IS practice abounds with application innovations, e.g. online auctions, blogs, wikis, chat, user spaces, multi-player games and reputation ratings. In contrast IS academic theories seem to change little over time. To the practitioners who create application advances, journals often seem out-dated, over-rigorous and irrelevant. Yet, as IS practice has innovated more, IS journals are, if anything, becoming more risk averse. The trend may reflect a mistaken belief that more rigor is better science. However in science, errors of commission (lack of rigor) trade off against errors of omission (lack of relevance). Is rigor at the expense of relevance causing IS theory to fall behind IS practice? One solution is an open IS publishing electronic archive, following the successful Los Alamos physics archive. If the IS community adds quality control to that model, it could lead the way in electronic knowledge exchange systems.

Keywords: electronic publication, citation, knowledge, innovation, research, electronic archive, IS future

Introduction
For the purposes of this discussion paper, an academic journal is defined as a knowledge exchange system (KES) with three core research roles:

1. **Creation**: To help create and develop new knowledge: Does the knowledge source foster tomorrow’s important ideas today? Is it at the cutting edge of research? Does it create new knowledge?

2. **Selection**: To present only valid knowledge of high academic quality: Is the knowledge of the highest scientific quality? Are its arguments logical and its claims supported by valid data?

3. **Growth**: To disseminate useful knowledge clearly and well, and in a timely fashion. Does the reader gain knowledge value? Does the journal educate its readers? Does it grow existing knowledge?

We expect good journals to encourage new research, to select scientifically valid from invalid knowledge, and to disseminate that knowledge well (Paul, 2005). To illustrate this, a knowledge exchange system can be compared to a garden, whose fruit is scientific progress. In this analogy, the creation role is like planting new seeds. New ideas put into the academic world, like seeds, are often initially small, fragile and take time to grow, and one may not know their outcome until they develop. It is a barren garden that is not receptive to new seeds. The selection role is like culling a garden’s weeds, or pruning a diseased tree. If knowledge is illogical or invalid journals can reject or deny it, which like pruning, reduces its growth in the field of academic knowledge. A garden without pruning will be overrun with weeds. The growth role is like watering or fertilizing plants so they grow. Disseminating ideas grows them in the minds of readers, and as people interpret and analyze them, their form develops. A garden without water and fertilizer will be stunted. This analogy suggests what every gardener knows, that for maximum results one must plant, prune and fertilize, i.e. good gardens need all three roles in a balance. Similarly, it is suggested that IS journals today need such a balance, as a garden that plants little but prunes a lot will soon have only old trees whose fruit bearing time is long gone. Have IS academic publications become protectors rather than growers of knowledge? This opinion paper addresses this question in three parts:

1. **Current situation** – where are we now?
2. **Current trends** – where are we going?
3. **Future options** – where could we go?
Current situation

This section asks if IS academic journals are becoming out-dated, over-rigorous and thus less relevant?

Bleeding Edge Theory

IS research seems to have strong theories on minor topics (like keystroke or mouse-click models), limited theories on major topics (like contingency theory) (Gutek, 1990), but only a few strong theories on major topics. Two such are the Technology Acceptance Model (TAM) (Davis, 1989) and Media Richness theory (MRT) (Daft, Lengel, & Trevino, 1987). TAM suggests users assess technology by usefulness and ease of use, and MRT links “rich” media to rich interactions. Both not only said something important, but said it about a subject that is also important. However such IS theories have two properties:

1. They are 10-20 years old, and
2. They seem less relevant today than they once were.

For example, that people build relationships via “lean” email suggests that MRT’s “richness” dimension oversimplifies human communication. Either plain-text email is “multi-media” rich, or MRT has omitted something. Likewise TAM’s idea that technology acceptance requires ease of use plus usefulness omits criteria like security, reliability and privacy, critical to today’s Internet (Mahinda & Whitworth, 2005). That such old theories are no longer as relevant as they once were is hardly surprising, but that they have changed so little over so long a time is. Given dramatic changes in IS practice, for IS academics to be essentially still using decades old theories is an issue. Certainly there are many papers that “extend” TAM in different directions (Brown, 2002; Heijden, 2003; Moon & Kim, 2001; Ong, Lai, & Wang, 2004; Shih, 2004; Taylor & Todd, 1995; Venkatesh & Morris, 2000; Yu, Ha, M., & Rho, 2005), but so many minor “tweaks” to a major model effectively cancel each other out, leading to: “... a state of theoretical chaos and confusion in which it is not clear which version of the many versions of TAM is the commonly accepted one.” (Benbasat & Barki, 2007, p2)

To tackle these problems, a major effort was made to upgrade TAM via the Unified Theory of Acceptance and Use of Technology (UTAUT) (Venkatesh & Morris, 2003). However UTAUT merely tweaks TAM’s core constructs, by renaming usefulness as performance expectancy, and renaming ease of use as effort expectancy. It then combines this face-lifted TAM with eight equally old theories from psychology and sociology to create the “new” model. How much of this model is really new is a matter of debate, but the problem suggested here is not with specific IS theories, but with the knowledge system itself, and its apparent inability to spawn new ideas for a new IS generation in a timely fashion. Is it hard to publish new theories in IS? Quite frankly, yes, if “new” means not an old theory tweak, and “theory” means a predictive framework not speculative conjecture. Authors seem to feel that the only way to get a new theory accepted is to graft it onto an old one (like TAM). Reviewing the core IS theory literature, innovation is not a term that comes to mind, while IS practice suggests the opposite. That progress is coming from practice and not theory suggests the latter has its priorities wrong.

Leading Edge Practice

It is easy to forget that inventions like the cell-phone were not predicted by theory (Smith, Kulatilaka, & Venkatraman, 2002). Breakthroughs like chat rooms, blogs, text-messaging, wikis and reputation systems, are neither multi-media nor media rich. Yet these simple text products became “killer applications”, while touted systems like decision rooms faded away. While IS practice advanced over the last decade, IS theory seemed to be essentially looking the other way. It was Google with its simple white screen and one entry box, not Yahoo with its media rich colors and pictures that scooped the search engine field. Investors who expected an Internet and multi-media bandwidth boom lost money. Those who invested in virtual reality games (where players donned multi-media virtual reality helmets) missed the real developments in online social gaming.

Usability theories plus 25,000 hours of user testing predicted that Mr Clippy, Office ‘97’s friendly graphical help assistant, would be a huge success (Horvitz, 2004). Yet Mr Clippy and same concept Microsoft Bob were voted the third and first (respectively) biggest software flops of 2001 (PCMagazine, 2001). Mr. Clippy’s removal was even a Windows XP sales pitch (Levitt, 2001). Microsoft is still only dimly aware of the problem (Pratley, 2004), that Mr. Clippy is impolite (Whitworth, 2005). Asked why plain text products succeed when multi-media, user-friendly ones do not, mainstream IS theory is strangely silent.
The pattern that practice leads while theory bleeds has a long history in computing. Twenty five years ago pundits proclaimed paper use dead, to be replaced by an electronic “paperless office” (Toffler, 1980). Yet today paper is more used than ever before. James Martin predicted program generators would make programmers obsolete, yet today programming is alive and well. A “leisure society” was supposed to arise as machines took over human work, but workers are now busier than ever before (Schor, 1991), and studies extend the 40+ hour week trend into the 1990s (Golden & Figart, 2000). Email was supposed to be only for routine tasks, the Internet was supposed to collapse without central control, video was supposed to become the Internet norm (given bandwidth), and people would tele-conference not travel. While each case had a grain of truth, overall IS predictions have been poor. Getting it wrong so often, what have we learned?

**Practice without theory?**

Consider the two valid means of scientific progress:

1. **Pragmatic**: Find what works by intuitive trial and error practice, and then explain it with theory later. Here theory, like the icing on a cake, is applied after the progress has been made,

2. **Theoretical**: Use theory to predict practice, and then move practically to that which theory suggests. Now theory, like a recipe used in baking a cake, is used before the progress is made.

The first uses theory to retrospectively explain existing progress, while the latter uses theory to predict and thus create progress, e.g. theory predicted space travel, then rockets were built. Neither approach is “better”, as both link theory and practice, and progress usually involves a combination of styles. In IS however the theory/practice relationship seems broken, as if rocket builders found that the less they know of rocket theory, the better their rockets would fly. IS practitioners often pragmatically build a new web site, interface, tool, button or function, and then only “accessorize” a theory later (for publishing purposes). That the real IS advances are occurring pragmatically is a long term problem. Cutting edge pragmatism, with its *all power to the IT artifact*, means IS theory meets a “show me don’t tell me” response. Physicists with the same approach would have demanded that Einstein build a particle accelerator to get his voice heard in Physics. In the IS marriage of theory and practice, the partners are barely speaking to each other. The theory-practice disconnect in IS arises because practitioners rightly ask: “What use is IS theory?” and academics currently struggle answer this question.

Yet pragmatism alone has serious limitations. If knowledge is like a tree, first pickings come easily from the lower branches, but soon running around the tree gives only the odd windfall. One then needs the ladder of theory. The black box approach falters when the system under consideration becomes complex, i.e. has more ways to go wrong than right. Imagine a space shuttle or nuclear program without theory! Trial and error does not work well here. Yet IS today is creating a system as complex as any space program, namely the architecture of an online global society. Can such a system be created by pragmatic trial and error alone? IS practice needs theory, as theory without practice is like a person working blindly.

**The Rigor Problem**

It is now suggested that the above problems of IS theory arise primarily from an over-focus on rigor at the expense of relevance. As Murray Gell-Mann, who invented the quark, we need to: “… get away from the idea that serious work is restricted to beating to death a well defined problem in a narrow discipline…” (Gell-mann, 1994, p346). Rigor is here defined as the probability of avoiding scientific error, and relevance is the probability of making progress by discovering something new that is useful. Academic quality is proposed to require both rigor and relevance. The logic of experimental science gives lack of rigor the general name of Type I error (of commission), and lack of relevance the general name of Type II error (of omission). The latter are beneficial things one could have done, but didn’t, like buy a winning lottery ticket. Such “intangible” opportunity costs are a known cause of business failure (Bowman, 2005), e.g. VisiCalc and Word-Perfect no longer dominate spreadsheets and word processing respectively not from errors made, but from opportunities missed. In the lottery of life you must buy a ticket (risk error) to win. The logic of science notes that reducing one error type to zero inevitably increases the other error type to 100%, e.g. one way to make no mistakes is to do nothing (Type I error is zero), but this misses all opportunities (Type II error becomes 100%), i.e. reducing errors of commission to zero increases errors of omission to 100%.

Hence while more rigor is good, exchanging type I errors (accepting faulty papers) for type II errors (rejecting useful papers) is not a gain overall, e.g. if authors write to be “bullet-proof” they also tend to say very little. Most journal submissions offer value opportunities as well as error risks. To reject a paper with nine good ideas and one bad one is
to miss nine opportunities to avoid one error. For example, when Berners-Lee presented his World Wide Web idea to
the academic hypertext community they rejected it on its faults (Berners-Lee, 2000), but failed entirely to see its
enormous potential. Rigor without relevance means that good ideas are thrown out with bad. Eric Raymond, a
software practitioner, summed up the relevance/rigor contrast by comparing a open bazaar with a closed cathedral
(Raymond, 1997), the former impure but vibrantly growing by its openness, and the latter pure but static and fading
because it is closed.

Increasing journal rigor without an innovation counter-balance gives a bias to the old. More rigor means new
theories face a greater burden of proof to publish as time goes by. While the faults of established IS theories are
ignored, the rising rigor standard means similar faults in new theories prevent publication entirely. If anything, the
bias should be the other way, as new views usually begin imperfect, and develop only with help from others. That
new theories respect old ones is reasonable, but that they answer critiques existing theories don’t answer either, is
not. There must be a balance, lest it seem that those who have climbed the tree of knowledge have pulled the ladder
up behind them.

Confounding the rigor issue is that publishing in top journals is now the primary screening mechanism for tenure,
promotions and appointments. For this purpose, selectivity is good, as not all can advance. Yet when top IS journals
accept in single digit percentages, submission failure is the norm. A University course with a 90% failure rate would
be unacceptable, yet our own publication system is now set up this way. IS academia needs innovators as well as
perpetuators, but the expected lesson of frequent failure is conformity. Consider how academia grows its “staff”:
PhD students spend 3-6 years as apprentices under senior direction, then 3-6 years trying to get tenure. At both
stages, criticizing established theory is unwise. Why expect innovation after nearly a decade of conformity training?
Contrast this with theoretical physics, where 25 year olds are expected to make breakthroughs. Senior IS researchers
now explicitly advise young IS faculty not to innovate, e.g. a paper written after an ICIS rigor/relevance debate
notes: “So for now, unfortunately, I would not recommend PhD students or junior faculty to aim for ‘IS research that
really matters.’ My recommendation ... would be to stick to their career paths. ... not too much research that really
matters seems publishable.” (Desouza, El-Sawy, Galliers, Loebbecke, & Watson, 2006). To say “unfortunately”
suggests the IS community can do nothing about this, but that is incorrect. We, the academic community are creating
this environment, and so we the academic community can change it – if we want to.

Let us not kill our discipline in the name of rigor, as too much rigor causes rigor mortis. Academic journals should
set high standards, but not so high that new ideas can’t get in. Rigor is only one part of academic quality, as rigor
without relevance makes the ultimate mistake – of doing nothing useful. The future of knowledge publishing lies in
combining rigor and relevance, not either alone (Figure 1). IS journals today face a challenge of relevancy, and a
crisis of indifference.

**Current Trends**

Given an increasing bias to rigor, what publication trends are expected for editors, authors, reviewers and readers?
Editors

Given a rigor bias, Type I errors of publishing will affect journal reputation more than Type II errors. If a journal publishes wrongly readers will know, but a useful paper falsely rejected does not see the publication light of day, and later publication elsewhere can be attributed to a standards difference. Since scarcity increases demand, a high journal rejection rate can be seen as an indicator of quality. Exclusive journals that attract more submissions can reject more to become even more exclusive.

A trend for journals to succeed by rigor means fewer top IS journals, and indeed while our field has expanded, overall IS journal rankings have remained remarkably consistent over time (Rainer & Miller, 2005). Already we seem at the point where only MISQ and ISR count and it is impossible to create more “A” journals. Yet two journals generating say 50 papers a year poorly represents a field with 5,000 in its professional society and perhaps 10,000 in all, especially given repeat authors, often senior professors, who have figured out how to publish in top journals. Such exclusiveness might seem good, but could have harmful effects on promotion prospects in the field as a whole (Kozar, Larsen, & Straub, 2006).

Rigor is easier to maintain for familiar and restricted content. In the 1990’s IS researchers focused on issues critical to IS practitioners a decade earlier (Szajna, 1994). The situation is probably the same today. If rigor makes journals traditional, expanding multi-disciplinary fields will be poorly represented at the top levels (as they can be criticized on multiple fronts). Journals could make their content more relevant by adopting an “affirmative action” innovation policy, aiming to publish a first time author each issue (or state none was found). However the rigor trend predicts successful IS journals will be exclusive in participation, restricted in scope, outdated in content and innovation averse. This does not seem a good end-point for knowledge exchange system.

Authors

Authors need publication “notches” on their curriculum vitae belt to survive. University tenure committee members who rate candidates outside their specialty tend to count paper numbers not paper quality. This is more “objective” only in being more reliable, not in being more valid. The effect for authors is that a ground-breaking paper and a trivial one both count as “one”. Despite the huge spin-off benefits of innovation, even valid new papers are risky, as they may not flourish until after the tenure decision. Publish or perish means it pays authors to increase paper numbers rather than paper content, e.g. to publish overlapping variants, to publish in least-publishable-units, and to publish in groups. Authors are being pressured to manage, market and network, rather than to think. In sum, the trend is for authors to flood conferences and journals with more junk than goods, to recycle old ideas with catchy new labels but little new substance, and to make minor incremental “advances” to the gatekeeper’s favorite theories. This does not seem a good end-point for knowledge exchange system.

Reviewers

Suppose reviewers, who often labor unpaid and unknown, and are usually over-worked, wish to finish reviews quickly. The easiest option is to simply accept, but if another reviewer finds serious faults, this could be professionally embarrassing. The next easiest response is to find enough faults to reject the paper. When enough faults are found, the review is over. While to praise when others condemn implies naïveté, a scathing review within a set of praises can be seen as commendable rigor. The third alternative, to spend time growing the paper, is more risky. If the authors ignore good advice, the reviewer has wasted their time, while if they take it, the authors get credit for the reviewer’s ideas. In summary, the expected trend is for reviewers to increasingly deny rather than grow value, which again does not seem a good knowledge exchange outcome.

Readers

Readers read to get meaning for their effort, to get cognitive value without breaking their cognitive bank. More rigor means more complex papers that take more effort to read, but contain fewer risky innovative ideas. If journals feel obliged to publish the n+1th rigorous paper on a topic, whether it adds anything or not, authors will tend to repeat the same ideas in sophisticated ways. For the reader, the chance of reading an apparently new paper only to find that it really says little new, goes up. If the number of new ideas per paper is going down, then readers are getting less value for their reading effort, and will likely take steps to redress the imbalance. One obvious way to do this is to read more superficially, e.g. skim for keywords rather than read for meaning, just read the conclusions, or just the abstract and section headings. This better balances reader effort vs. usual reading result. The predicted end point is academic readers grazing but not digesting, again not a good outcome for a knowledge exchange system.
Summary

The expected result of a rigor bias is journals more rigorous than relevant, authors more prolific than productive, reviewers denying not inspiring, and readers grazing but not digesting. These trends may self-reinforce, e.g. few high rigor journals means authors over-submit which increases reviewer denying which reduces reader value. The general trend is that only the A grade journals count, and they become increasingly exclusive and conservative. The final vision, of journals as exclusive castles of knowledge, manned by editor-sovereigns and reviewer-barons, raising a barricade of rigor against a mass of peasant-authors, is not inspiring. An alternative vision is now suggested, which it is proposed is more akin to the spirit of the Internet itself, where knowledge flows freely in unexpected ways.

Future Options

The problem that an over emphasis on rigor creates has been well stated by Lee Smolin (who developed quantum gravity theory): “… there is now the problem of making sure that young people have the freedom to wander across boundaries established by their elders without fear of jeopardizing their careers. It would be naïve to say this is not a significant issue.” (Smolin, 2001, p183). This section proposes two ways among many to improve journal relevance. One is to rate existing journals by timeliness, and the other is to open up the publication field entirely with an open electronic IS archive. Both solutions increase dissemination rate, and the latter includes the former, as an open electronic archive would mean instant availability of all publications, which would then still be available for future publication if no copyright was held. Creativity seems to occur at the intersection of different fields of knowledge, so letting knowledge flow across specialty boundaries is a good idea. In was in this spirit that physicist Paul Ginsparg created the Los Alamos bulletin board, the first truly successful electronic journal of science. Every morning theoretical physicists download new papers in their field and discuss them over morning coffee. Laughlin asked Ginsparg why this academic advance had not spread to other fields: “Paul suggested that physicists are self-selected to value eccentricity and novelty of ideas above all else, even at considerable professional risk to themselves.” (Laughlin, 2005, p179). If the IS academic culture is driving our anarchist innovators out into IS practice to make breakthroughs there, that is a problem which must be addressed.

Increase timeliness

Timeliness is part of relevance, and publication delay represents a Type II error. Currently while IS practice changes in months, journal cycle times are typically in years. MISQ recently noted it had about a year’s backlog of accepted papers that could not be published for print cost reasons (Saunders, 2005). Adding a year backlog to 1-2+ years review and 1-2+ years paper development makes academic papers 3-5 years old when first published. IS changes a lot in 3-5 years. How relevant is quality that is too late to have impact? In today’s IS climate, timeliness is not an option, but a requirement.

Journal rating systems could add timeliness criteria to measures of perceived rank (Rainer & Miller, 2005) or citation rate analysis (Katerattanakul, Han, & Hong, 2003). Journals could report turn-around times (from submission to editor decision), end-to-end times (from submission to publication) (Snodgrass, 2003), and other value measures like readership size and composition, reader rated usefulness and knowledge source influence (Nerur, Sikora, Magalaraj, & Balijepally, 2005). This would help focus journals on relevance as well as rigor.

Reducing review times could help performance. In 1999 the Association for Information Systems introduced two online journals, the traditional double-blind peer review Journal of the AIS, and the Communications of the AIS. The latter offered authors the choice of a light (one person) or a full three person review. In 2001 CAIS was rated significantly higher (18th) than JAIS (30th) in journal impact rankings (Barnes, 2005; Mylonopoulos & Theoharakis, 2001), and in 2003 while JAIS published 16 articles, CAIS published 95, as about 80% of authors chose a light review. This “experiment” in publishing suggests journals pursuing rigor without timeliness will be increasingly “under siege” (Grudin, 2004, p20).

Increase dissemination

Rather than merely tinker with journal rating criteria, computer-mediated interaction (CMI) could change the architecture of academic knowledge exchange entirely. Cost economics seem behind current journal acceptance rates that force journal editors to be the Scrooges of academic knowledge. For a print journal, the pages disseminated depend on page cost and subscriber fee economics. Journals cannot publish pages they cannot pay for, though some journals ask authors to contribute to page costs of longer articles.
Electronic publishing changes this situation, by reducing printing, binding, shipping and storage costs, so it is possible to publish not 5% but 100% of submissions. If memory is cheap, and it is, one could publish electronically everything submitted. This would make available the papers that print publishing rejects. For a knowledge exchange system, this has several advantages. While the literature seems huge, a particular topic may still have only a handful of relevant print-published papers. A researcher on that topic might find even rejected and first submissions useful. Further, not all rejected papers are all bad, as they may have brilliant parts but a weak main idea, so researchers may gain value from rejected papers in their field if they browse them correctly. As nature can grow a lily on a compost heap, so a bad paper can spark a good idea. Much that is currently discarded by the print accept/reject dichotomy could, if published, add value to IS research. Given search engine power, why not let readers, not gatekeepers, decide what to read?

**Increase selectivity**

However would not publishing all devalue the academic currency of “being published”? Is this not why e-journals have apparently failed, being already seen as lower status than p-journals? The answer is that the goal of discriminating quality remains, even if one can publish everything. Simply computerizing the current system is not the answer. E-journals are lower status than p-journals because they select less, not because they publish more. IS conferences were once seen as lower status than journal, but today some highly selective IS conferences are rated higher than journals (Grudin, 2005). Print journals confound dissemination and selection, when in fact they are separate issues. Once dissemination and selection are separated conceptually, we see that online publishing can allow more discrimination not less. While print journals are limited to an accept/reject dichotomy, electronic journals can rank on a multi-point scale (Figure 2). If everything were published, guides to selecting would appear, a phenomenon like Zagat’s restaurant listings. Electronic journals need not select less because they publish more, as more restaurants need not imply fewer quality restaurants.

New submissions could enter the system at the bottom, as not yet rated, and work up the hierarchy based on their assessment, progressing through various versions as a wiki does. What today is simply “rejected” may in the future be the “Best second tier paper of the year”. As well as traditional assessment (by editors and reviewers), online publishing allows reader assessment via electronic voting, with one vote per registered reader. Readers and reviewers would rate from different perspectives, so a paper reviewers disliked could rise by popular acclaim, or reviewers could direct readers to a hard to read paper of high value. Finally some papers could rise quickly, while others might rise only after years of work. In gardening terms, not everything “grows” at the same rate. The challenge of online publishing is to increase both dissemination and selectivity, not just the former.

![Figure 2. A Selective Electronic Journal](image-url)
Increase knowledge interactions

If advancing research is the primary goal of academic knowledge exchange, some who have reviewed electronic academic publications wonder: “…to what extent introducing advanced technologies supports the ultimate objective of research – creating knowledge.” (Hovav & Gray, 2004). Yet an open electronic archive could create more new knowledge than ever before, if only some of the options were tried, for example:

1. **Reader to Reader**: An online reader recommendation system.
2. **Reader to Author**: Authors could open their article to reader comments.
3. **Author to Reader**: Authors could comment to readers outside what they have published.
4. **Author to Author**: A community of journal authors could help each other.
5. **Author to Reviewer**: Authors could talk directly to reviewers.
6. **Reviewer to Reviewer**: Many authors wish the reviewer who loves their paper would talk to the one who hates it, and resolve their differences.
7. **Editor to Editor**: Editors could network to place a paper. Sending good work to the wrong place means authors wait months to find it rejected not by quality but type. If related journals with different audiences formed a collective to exchange misplaced papers, if an author’s paper missed one journal it could hit another.
8. **Reader to Editor**: Some academic journals accept “Letters to the Editor”, as reader comments are often informative, and also allow clarification replies by the author(s), e.g. CACM and CAIS.
9. **Author to Editor**: Authors could tell Editors their view of their reviews, as a journal will decline if bad reviewers drive away good authors.

The details of the above are beyond the scope of this paper, suffice to say, as the discipline that espouses IS, we should lead the way in its use. One might ask, if electronic knowledge systems have such potential, why don’t they already dominate academic publishing? The problem is the current focus on cost instead of value, and a failure to see the power of community synergy. For example, the same question could have been asked of the Internet before it reached critical mass. The issue “Who will pay?” was a bogey that held back Internet development for decades, because the answer “Everyone” was not then evident. Today the question “Who will pay for the Internet?” is a non-question. Everyone pays because everyone gets value - enormous value. Likewise to argue that e-journals will not make money as p-journals do is to fail to see their synergy potential. An open electronic IS archive will work if it connects all IS, not some IS exclusive IS subset. Community synergies work better the bigger they are, and successful online businesses like Google and E-bay illustrate this principle. They follow the business model that when everyone uses something and gains real value from it, the community will support it.

Conclusions

Scholarly journals originally aimed to actively develop, select and diffuse knowledge. However over time, judging publications was found to be also a useful way to judge their authors. Hence publication data is now used in hiring, promotion, tenure and merit pay decisions. Journals have become not only the cultivators of academic knowledge, but also the gatekeepers of academic power, affecting individual advancement, academic department rankings, research fund targeting and library fund allocation (Rainer & Miller, 2005). This dual role, it is argued, has diverted academia from its original goals. Journals have become promotion arenas, with theories battle weapons, rather than knowledge fields with theories as plows to cultivate scientific truth. This problem strikes at the core of our discipline, as to survive a profession must look outward beyond itself, not inward to itself. Has IS publishing become more about promotion than knowledge? A discipline that forgets its original business may fall by the wayside. If IS academia does not reinvent itself it may become, if it is not already, a byway on the highway of IS progress.

We must refocus IS academia on the business of creating, selecting and disseminating knowledge. This may make the job of promotion and tenure committees harder, but let those chips fall where they may. As academics, our job is to grow, select and disseminate knowledge, not serve administrative needs like selection and promotion. Let us do our job, and let others worry about theirs. Over the last decade, atheoretical IS practice has innovated systems like E-bay, wikipedia and MySpace, while IS theory, hobbled by rigor, seems to have struggled behind as best it could. This is not acceptable, as IS progress needs both theory and practice. Academia should be a melting pot of new ideas not a static pool of old ones. In this spirit, let us upgrade our academic knowledge exchange systems. It is time to open the
doors of our ivory towers to more people, more ideas and more exchanges. Alternatively, an electronic knowledge archive could exist alongside our closed castles of print, as a marketplace for the instant exchange of new ideas. All that is required is to combine rigor and relevance, rather offer one or the other. Let readers themselves decide what they will and will not read, but also let reviewers and editors give them guidance.

Let us return to our academic roots, of publishing knowledge freely to all without fear or favor. We should not think small, as we have the technology to do this. IS, straddling as it does other disciplines, is well positioned to build a universal electronic “knowledge port” for cross-disciplinary knowledge travelers. Let the “specialty” of IS be not this or that knowledge content, but the interaction of that knowledge content at the crossroads of technology. Let us be the Singapore of technology, a place where ideas are traded for mutual benefit. An IS online journal/archive would more than a journal, it would be a vibrant online academic community, attractive far beyond the boundaries of any IS content. In my view, the first to successfully create an open electronic IS knowledge archive, with information exchange options like commenting, voting and version control, will sweep the field, and open up a new generation of academic publishing.

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