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Combining Rigor and 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, while over the same time few new IS academic theories have taken hold. To the practitioners who innovate, journals often seem out of date, over-rigorous and increasingly irrelevant. As IS practice innovates, IS theory seems to be if anything becoming more risk averse. If IS journals assume research rigor is research excellence, they will fall behind the cutting edge of IS progress. This would be unfortunate, as advances need to be driven by theory as well as practice. The key is recognition that good research involves two dimensions not one, namely rigor and relevance. These correspond to avoiding errors of commission (lack of rigor), and avoiding errors of omission (lack of relevance). These two dimensions suggest a selective but open IS electronic archive could increase both relevance (by electronically publishing all submissions) and rigor (by electronically supporting expert and Wiki style reviews). Since the IS community designs and creates online systems, we should be able to improve the highly successful Los Alamos physics preprint archive, and lead the way in electronic knowledge exchange systems.

Keywords

Electronic publication, knowledge exchange system, electronic archive, IS future

Introduction

This discussion takes an academic journal as a knowledge exchange system with three core roles:

1. **Knowledge Creation**: To help innovate new research knowledge: Does the system foster tomorrow's important ideas today? Is it at the cutting edge of research? Does it create new knowledge?
2. **Knowledge Selection**: To select research knowledge of high academic quality: Is the knowledge presented likely true? Are the arguments logical, and are claims supported by valid data?
3. **Knowledge Growth**: To grow good research knowledge, by exposing it to review, and disseminating it to readers. Are readers educated by knowledge that is useful, clear, and timely?

We expect good journals to encourage new research, to discriminate good and bad research, and to widely disseminate knowledge (Paul, 2005). To illustrate this, a knowledge exchange system can be compared to an orchard whose fruit is research. In this analogy, the creation role is like planting new seeds. New ideas put into the academic world, like seeds, may be initially small and fragile, and may take time to grow to full size. Often one cannot tell the growth outcome until the seed forms a plant. Just as an orchard that receives no new plants will soon have only old trees, whose fruit bearing time is long gone, so a journal system that does not accept new ideas may soon have only old outdated theories. The selection role is like culling the weeds in a garden, or pruning a tree of diseased branches. Just as pruning reduces bad growth, so journals can reject research that is illogical or invalid. An orchard that is neither weeded nor pruned may be so overgrown that the trees will give little fruit, and likewise in a journal full of poor articles, a good one may be ignored. Finally the growth role can be compared to watering and fertilizing plants to increase their growth. Journals grow knowledge in two ways. Firstly author ideas can be "watered" by reviewer feedback, and secondly they are disseminated widely to readers who may interpret and use them. Publishing a concept lets it grow to maturity, as others analyze, interpret and develop it. An orchard that is neither watered nor fertilized will tend to have stunted trees that again produce little fruit. Likewise if reviews and reader responses are superficial and weak, ideas that might otherwise grow may not develop.

The above analogy suggests what every gardener knows, that for best results one should plant, prune and fertilize in a balance, i.e. good gardens need all three roles. It is now suggested that the rigor focus of IS journals today has upset this balance. The result is the knowledge equivalent of a garden that plants little but prunes a lot, and whose supplies of nourishment are going down. IS academic maintainers are becoming knowledge gatekeepers, rather than what they should be: knowledge gardeners. To use Eric Raymond's analogy, the cathedral of IS

academia, with its static exclusivity, is diminishing in influence, while the nearby bazaar of practice, with its dynamic openness, is growing (Raymond, 1997). If so, the IS/IT community must address the situation. This paper analyzes the current situation, identifies current trends, and suggests ways to reinvigorate our discipline.

Current Situation

This section considers whether IS academic journals are becoming over-rigorous, out-dated and irrelevant.

Bleeding Edge Theory

IS research seems to have specific theories on minor topics (like keystroke or mouse-click models), vague theories on important topics (like contingency theory) (Gutek, 1990), but few theories that make specific statements about important topics. Those that do, like the Technology Acceptance Model (TAM) (Davis, 1989), and Media Richness theory (MRT) (Daft, Lengel, & Trevino, 1987), have been influential. TAM suggests users assess technology by usefulness and ease of use, and MRT links "rich" media to rich interactions. Both not only say something useful, they also say it about an important subject. However such IS theories have two properties:

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

For example, MRT's "richness" dimension suggests that people cannot build relationships via "lean" email texts, but practice shows that this can happen. Either plain-text email is "multi-media" rich, or MRT over-simplifies human communication. Likewise TAM predicts that technology acceptance involves just ease of use and usefulness, which omits criteria like security, reliability and privacy critical to today's Internet (Mahinda & Whitworth, 2005). Many papers "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 the effect of so many minor "tweaks' to a major model was that they effectively canceled 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 this problem, the Unified Theory of Acceptance and Use of Technology (UTAUT) (Venkatesh & Morris, 2003) seemed to authoritatively upgrade TAM. Yet UTAUT merely tweaked TAM's core constructs, renaming usefulness as performance expectancy and ease of use as effort expectancy. It then combined this face-lifted TAM with eight equally old theories from psychology and sociology to create a "new" hybrid model.

The issue here is not that older theories are less relevant today, which is hardly surprising, but the lack of genuine theoretical innovation in the face of genuine practical innovation - IS authors seem to feel that the only way to get a new idea accepted is to graft it onto an old one (like TAM). Given the dramatic changes in IS practice over the last decade, that basic theory concepts have changed so little over so long a time is a concern. Even our new theories are old. Is this apparent inability to spawn genuinely new ideas for a new IS generation due to none being available? In the author's experience, attempts to publish alternatives to major IS theories are strongly resisted. It is hard to publish new theories in IS, if "new" means not an old theory tweak, and "theory" means a predictive framework not speculative conjecture. Reviewing the core IS theory literature, innovation is not a term that comes to mind, while reviewing 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 "obvious" inventions like the cell-phone and e-mail were not predicted by theory (Smith, Kulatilaka, & Venkatramen, 2002). Breakthroughs like chat rooms, blogs, text-messaging, wikis and reputation systems, are neither multi-media nor media rich, yet these simple products became "killer applications", while touted systems like decision rooms have faded away. While IS practice was advancing over the last decade, IS theory was 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 predicted a multi-media Internet needing bandwidth lost money. Those who invested in virtual reality games (where players donned multi-media virtual reality helmets) missed the development of online social gaming. The usability theories of the day, plus 25,000 hours of user testing, predicted that Mr Clippy, Office '97's friendly graphical help, 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 was socially annoying, i.e. impolite (Whitworth, 2005). Asked why plain text products have succeeded while multi-media or user-friendly products like Mr Clippy flopped, IS theory is strangely silent.

This 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 not only alive but thriving. 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 expect the 40+ hour week trend to continue (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 computer smart-help was supposed to replace people. While each case had a grain of truth, the overall prediction success rate of theory has been poor. Is the lesson of getting it theoretically wrong so often not to trust theory at all?

Practice Without Theory?

The above dilemma can resolve in two ways: either IS practice goes it alone or IS theory opens itself to innovation and change. Consider the two means of scientific progress:

1. **Pragmatic**: Find what works by intuitive or 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, and then develop in practice what theory suggests. Now theory, like a recipe used in baking a cake, is used before the progress is made.

In the first approach, practice innovates and theory retrospectively explains it, while in the second case theory predicts and practice follows to create progress, e.g. theory predicted space travel, then rockets were built. Neither approach is "better", as both should work together to create progress. In IS however the theory/practice relationship seems broken, as if rocket builders found that the less they knew of rocket theory, the better their rockets would fly. In this pragmatic approach, practitioners first build a new web site, interface or tool, then only "accessorize" a theory later (in order to publish). Cutting edge pragmatism, with its *all power to the IT artifact*, means that IS theory increasingly meets a "show me don't tell me" response. Physicists with the same approach would have required Einstein to 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, as IS practitioners feel IS theory is barren.

Yet pragmatism working alone has serious limitations. If knowledge is like a tree, first pickings may come easily from the lower branches, but soon running around the tree gives only the odd windfall. One then needs the ladder of theory. A "try it and see" black box approach is least useful 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 to guide it! Trial and error does not work well here. As modern software becomes more social, it also becomes more complex. IS today is creating a system as complex as any space program, namely the architecture of an online global society. It is unlikely that such a system will succeed by trial and error alone – we need to know what we are doing. IS progress needs both theory and practice, as theory and practice are the two legs of progress. Why then is this not occurring?

The Rigor Problem

Rigor is here defined as the probability of avoiding scientific error, and is in contrast to relevance, the probability of discovering something new or useful. Academic quality is proposed to require both rigor and relevance, so as the physicist Gell-Mann (who invented the quark) notes, 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). In research lack of rigor is a Type I error (of commission), and lack of relevance is a Type II error (of omission). The latter are beneficial things one could have done but didn't, like buy a winning lottery ticket. Business calls type II errors "intangible" or opportunity costs, and they are a known cause of 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. The principle is that in the lottery of life you must buy a ticket (take a risk) to have a chance to win (opportunity). In research theory, reducing one error type to zero increases the other to 100%, e.g. by doing nothing one makes no mistakes (zero Type I error) but also misses all opportunities (Type II error is 100%). Pursuing a rigor focus reduces errors but also reduces usefulness, and if journal publications do this they will become commentators on paths already taken, rather than visionary innovators of what might be.

While more rigor is good, reducing type I errors (accepting faulty papers) *at the expense of* type II errors (rejecting useful papers) is not an overall gain, e.g. when authors write to be "bullet-proof" they often tend to say very little. Most journal submissions contain both knowledge value opportunities and knowledge errors. To reject a paper with nine good ideas and one bad one in the name of rigor is not a good trade, as nine benefits are missed to avoid one error, e.g. 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 potential. Rigor without relevance makes the error of not being useful, so journals that seek rigor alone will face a crisis of indifference.

If rigor increases with time, this creates a bias to the old, as new theories face a greater burden of proof than old ones. While established IS theories have evolve criticism resistances, a rising rigor standard means that new theories with similar faults to old ones fail to get published. If anything, the bias should be the other way, as new views usually begin imperfect, and develop only later, with help from others. That new theories respect old ones is reasonable, but that they answer critiques established theories don't answer either, is not. There must be parity, lest it seem that those who have climbed the tree of knowledge have pulled the ladder up behind them.

The drive to rigor may occur because top journal publishing is now the primary screening mechanism for tenure and promotion. Academic selectivity is now the basis of management staff decisions. Yet while promoting only a few is acceptable, when top IS journals accept in single digit percentages submission failure is the norm, which has a toxic effect on research creativity. A University course with a 90% failure rate would be morally unacceptable, yet our primary publication system is set up this way. IS academia needs innovators as well as perpetuators, but if the system norm is failure the expected lesson is conformity. Consider how academia grows its "staff": PhD students spend 3-6 years as apprentices under senior direction, then another 3-6 years trying to get tenure. At both stages, criticizing established theory is unwise if one wants a career. Why then, after nearly a decade of conformity training, would one expect innovation? In theoretical physics 25 year olds are expected to make break-throughs, but senior IS researchers explicitly advise young IS faculty not to innovate, e.g. a paper written after the ICIS 2005 rigor/relevance debate advises: "*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 that we, the IS community, are helpless but this is not so. By our actions each of us creates the publishing environment we work within, and so we can change it – given the understanding and will to do so. The understanding is that the low acceptance percentages of academic promotion need not drive our knowledge exchange system. The will seems to be arising as excess rigor reduces publication rates, which affects promotion, tenure, grants and students in the entire discipline (Valacich, Fuller, Schneider, & Dennis, 2006), i.e. excess rigor may threaten the viability of the IS discipline. As Lee Smolin (who developed quantum gravity theory) says: "*… 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).*

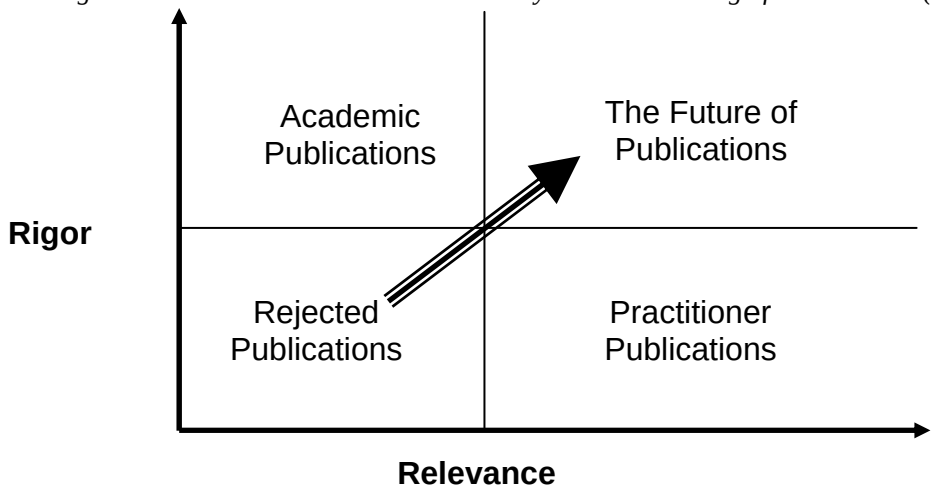

Figure 1: Combining Rigor and Relevance

Too much rigor can cause rigor mortis, so let us not kill our discipline in the name of rigor. Journals standards should be high enough to reject the trash but not so high that new ideas cannot get in. The future of knowledge publishing lies in combining rigor and relevance, not in either alone (Figure 1).

Current Trends

What publication trends does an increasing bias to rigor predict for editors, authors, reviewers and readers?

Editors

A rigor bias means that Type I errors 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 its later publication elsewhere can be attributed to lower standards. Since scarcity increases demand, a high journal rejection rate can be interpreted as a quality indicator. Exclusive journals will then attract more submissions, and so can reject more to become even more exclusive and attractive. However this trend, for journals to succeed by rigor, means fewer top IS journals, and indeed even though our field has expanded considerably, the distribution of IS journal rankings has 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 since many MISQ/ISR papers involve repeat authors, often senior professors who know the publication requirements of top journals, as they often review for them. Since rigor is easier to maintain for familiar and restricted content, journals will prefer older content. A historical review suggests that 1990's IS researchers focused on issues critical to practitioners a decade earlier (Szajna, 1994), and the situation is probably the same today. Rigor also means that multi-disciplinary research, often a point of knowledge expansion, will rarely reach top levels, as it can be criticized on multiple fronts. The rigor trend predicts successful IS journals will be exclusive in participation, restricted in scope, outdated in content and innovation averse. Exclusiveness does not seem a good end-point for a knowledge exchange system, as it harms promotion prospects in the field as a whole (Kozar, Larsen, & Straub, 2006).

Authors

Authors need publication "notches" on their curriculum vitae belt to survive. University tenure committee members, who often rate candidates outside their specialty, will tend to count paper numbers rather than assess paper quality, as this is more "objective". The effect for authors is that a high quality ground-breaking paper that takes years to develop, and a trivial spin-off of some previous work, one both count as "one". This means that individual innovations, which can have enormous spin-off benefits for the group, are not worth developing by the individual. Even successful innovations 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. publish overlapping variants, publish in least-publishable-units, and publish in groups. It doesn't pay to spend too much time working on a paper, i.e. really thinking about it. Authors are being pressured to manage, market and network, rather than to think. The trend is for authors to flood conferences and journals with more junk than goods, to recycle old ideas with catchy new labels but no new substance, or to make minor incremental "advances" to the gatekeeper's favorite theories. This does not seem a good end-point for a knowledge exchange system, as it reduces author quality and creativity.

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 naiveté, a scathing review within other praises can be seen as commendable rigor. The third alternative, to spend time growing the paper, is both more time consuming and 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. The expected trend is for reviewers to increasingly deny rather than grow value, which again does not seem a good knowledge exchange outcome, as the transfer of wisdom from reviewer to author is discouraged.

Readers

Readers want 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 adds 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. An obvious way to do this is to read more superficially, e.g. skim keywords, just read the conclusions or abstract or headings, to rebalance reader effort vs. reading result. The predicted end point is academic readers grazing but not digesting, again not a good outcome for a knowledge exchange system, as less knowledge is transferred.

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. The reader can decide for him or herself whether this is happening or not. 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 will count, and they will become increasingly exclusive, conservative and irrelevant to research progress. The final vision, of journals as exclusive castles of knowledge, manned by editor-sovereigns and reviewer-barons, raising a barricade of rigor against the mass of aspiring peasant-authors, is not inspiring. An alternative vision is now suggested based on the spirit of the Internet itself, where knowledge flows freely in unexpected ways.

Future Options

The general solution to the problems and trends above is to increase knowledge creation, selectivity and growth together, i.e. satisfy both rigor and relevance criteria. This can be achieved by altering the current system, by creating a new one, or both. General calls for more relevance in IS research (Lee, 1999) or higher quality (Paul, 2005) fall into the first category. Actual changes to the structure, policy or evaluation of the journal system itself count for more, e.g. journals could adopt an "affirmative action" policy to innovation by aiming to publish a first time author each issue (or state none was found), or a separate "letters" publication could handle preliminary reports in a more timely fashion. Timeliness affects relevance, as publication delay is a Type II error. 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's publishing delay (now normal) to 1-2+ years of review, after 1-2+ years of data gathering and paper development means published academic papers are already 3-5 years old when "born". This is a long gestation period, and IS changes a lot in 3-5 years. How relevant is high quality that arrives too late to 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). Reducing review times increases journal 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). 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). However rather than tinker with aspects like journal rating criteria, some suggest that computer-mediated interaction (CMI) could change the architecture of academic knowledge exchange entirely.

An electronic archive

This section suggests that an open electronic IS archive could increase new idea creation, improve knowledge selection, and increase knowledge dissemination. The latter inspired physicist Paul Ginsparg to create 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. Despite some journals trying to prevent preprints being posted, author pressure has favored the no copyright public archive, and even journals like Nature have relented. 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, we need to fix this.

At first electronic publishing seemed simple – build it cheaply and they will come. Cost economics force journal editors to be the Scrooges of academic knowledge (as every print journal page costs), but electronic publishing is cheap. Journals cannot publish pages they cannot pay for with subscriber economics, though some ask authors to help with long article page costs. Electronic publishing changes this situation, by reducing printing, binding, shipping and storage costs. If memory is cheap, and it is, one can electronically publish 100% of submissions for much same price as 5%. Such dissemination power has several advantages. While the literature seems huge, a particular topic may still have only a handful of relevant print-published papers, so a researcher might find even rejected and first submissions useful. Also, not all rejected papers are all bad, e.g. they may have brilliant parts but a weak main idea. Researchers may gain value even from rejected papers. As nature can grow a lily on a

compost heap, so a bad paper can spark a good idea. Much that is currently rejected for print publishing could, if published electronically, add value to IS research.

Yet while free electronic publishing improves creation and dissemination, it loses the selectivity that supports academic promotions. That electronic publishing maximizes dissemination at the cost of selection could explain the limited success of e-journals so far - easy publishing devalues the academic currency of "being published". Yet if dissemination and selectivity inevitably trade-off, is the solution simply to find a "sweet" percentage that matches a desirable promotion rate, say 30%? This paper suggests that the tradition of print journals has confounded dissemination and selection, which are really separate goals, i.e. *one can have both*. If one separates conceptually dissemination and selection, then online publishing can not only coexist with promotional print publishing (as the Los Alamos archive illustrates), but in fact allow more selectivity, e.g. while print journals are limited to an accept/reject dichotomy, electronic journals can rank on a multi-point scale – see Figure 2, where current publishing represents only the top white triangle. If a top print journal accepts only 10%, the remaining 90% are not discriminated at all. In contrast an e-journal could accept everything (except for "spam") and also grade everything. Promotion could then depend not only on rare successes but on a more representative average. E-journals may be currently seen as lower status than p-journals not because they publish more but because they select less. By comparison, IS conferences were once seen as lower status than journals, but some IS conferences today are so selective they are considered of higher quality than some journals (Grudin, 2005). The challenge of online publishing is to increase *both* dissemination and selectivity, not just the former.

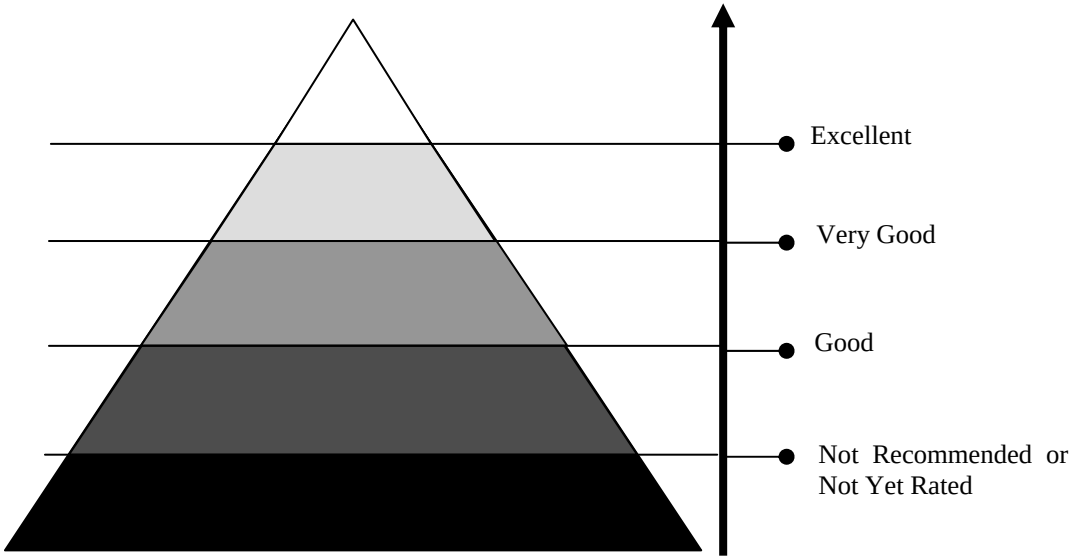

Figure 2: A Selective Open Electronic Archive

In this proposed concept, new submissions would enter the system's quality rating system at the bottom, graded as "Not Yet Rated". They would then find their position in a quality hierarchy based on their assessment, perhaps at the same time progressing through various versions, as a wiki does. What today is simply "rejected" for print may in the future be the "Best second tier paper of the year". A further advantage is *the democratization of selection*, since as well as traditional editor and reviewer assessments, online publishing also allows reader assessment, such as by number of reader views or downloads. Since readers and reviewers rate from different perspectives, a paper that reviewers disliked could still rise by popular acclaim. Conversely ratings by respected reviewers could direct readers to a hard to read paper of high value. This approach resolves the timeliness problem which all print journals today face. Yet while excellent papers would still rise quickly, others might rise only after years of work, as in gardening, not everything "grows" at the same rate.

Some reviews of electronic academic publications ask: "… to what extent introducing advanced technologies supports the ultimate objective of research – creating knowledge." (Hovav & Gray, 2004), but much electronic publishing simply "computerizes" traditional structures, e.g. submissions by email. It is better to design a new type of socio-technical system (Whitworth, 2006) to support the original principles of knowledge creation, selection and growth. Consider some of the knowledge exchange options technology could offer:

1. *Reader to Reader*: An online reader-to-reader recommendation system (cf. Amazon's system).

2. *Reader to Author*: Authors could open their article to direct reader comments.
3. *Author to Reader*: Authors add discretionary comments to readers outside what they have published.
4. *Author to Author*: A community of journal authors who help each other.
5. *Author to Reviewer*: Opening up authors responses to reviewers.
6. *Reviewer to Reviewer*: Many authors wish the reviewer who loves their paper would talk to the one who hates it, and that they would resolve their differences or take a more moderate approach.
7. *Editor to Editor*: Editors could network to place a paper appropriately. Sending good work to the wrong place means authors wait months to find it rejected not by quality but type. Related journals with different audiences could form a collective to exchange misplaced papers.
8. *Reader to Editor*: Academic disciplines like Physics publish "Letters" with shorter comments that also allow author clarification replies, cf. CACM letters to the editor.
9. *Author to Editor*: Authors could advise Editors of their view of the value of their reviews, as a journal will decline if bad reviewers drive away good authors.

The above interaction options expand not only traditional publishing but also most current electronic publishing. Yet as e-mail, and the spam it allows, illustrate, such systems entail dangers as well as opportunities. A selective open electronic archive is a socio-technical system, and so must "perform" on both social and technical levels, e.g. one social requirement is that it benefits the IS community fairly, i.e. is legitimate (Whitworth & deMoor, 2003). The Internet itself shows the benefits of focusing on value as well as cost, and the power of community synergy, e.g. if the question "Who will pay?" was asked of the Internet before it reached critical mass few would have realized that the answer is "Everyone". Today the question "Who will pay for the Internet?" is a non-question, as its knowledge value is evident. Just as the enormous value of the Internet would never have arisen based solely on a cost-benefit analysis, so e-journals will not make money as p-journals do. Critical mass is here critical because community synergies work better the bigger they are, as successful online businesses like E-bay show. In this new business model, when everyone uses and gains value from something, it is a success, as the community supports it, e.g. if the Internet were today destroyed by a virus, we would all simply build it again tomorrow, because it is so useful.

Conclusions

Scholarly journals originally aimed to actively develop, select and diffuse knowledge, but over time, judging publications was also found to be a useful way to judge 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 and theories battle weapons, rather than knowledge fields with theory plows. This problem strikes at the core of our discipline, as to prosper a profession must look outside itself, not inward to its own promotions. A discipline that forgets its original business may fall by the wayside, and IS academia could become a byway on the highway of progress.

One way to refocus on our original knowledge business is to develop a selective open electronic archive that accepts, rates and publishes everything. Transparent internal ratings could provide checks and balances. 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 selection and promotion needs. Let us do our job and let others worry about theirs. Over the last decade, non-theoretical IS practice has innovated systems like E-bay, wikipedia and MySpace, while IS theory, hobbled by rigor, has struggled behind as best it could. This is unacceptable, as 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 use our technology to upgrade our knowledge exchange systems, to turn ivory towers in multi-storey markets of knowledge with goods of all qualities. Let us return to our academic roots, of publishing knowledge freely for mutual critique, and use technology to put this vision online. An electronic knowledge archive could exist alongside print publications, as a marketplace of ideas, or supplant them. It doesn't matter, as long as the knowledge grows.

The goal of perfect rigor must be abandoned for something better: the combination of rigor and relevance. Let readers themselves decide what they will and will not read, but let reviewers and editors still give guidance. Readers may no longer need knowledge gatekeepers, but they still need knowledge guides, to unmask frauds and point to excellence. IS, straddling as it does other disciplines, could build a universal electronic "knowledge portal" for cross-disciplinary knowledge travelers. If creativity occurs at the intersection of knowledge fields, let

us support cross-disciplinary knowledge flows. Rather than navel-gaze that the IS "specialty" is this or that knowledge, perhaps our role is to encourage knowledge flow at the crossroads of technology, i.e. to be the Singapore of technology, the point where knowledge from psychology, engineering, computer science, information science, health science, business and mathematics (to mention some) crosses and interacts. An online IS archive that is both respected and open could attract people from beyond current IS boundaries to invigorate and expand the discipline. We should not think small, as with options like commenting, voting and version control, the result could be not just an e-journal but a vibrant online academic community. The first to successfully create an open electronic IS knowledge archive that works on both social and technical levels will open up a new generation of academic publishing.

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