

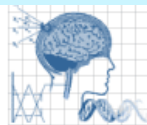
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Contents

0. The Think-Piece: Scientific Publishing in the Global Electronic Village - Some Thoughts. <i>Dan Trietsch and Rajendra Akerkar</i>	1 - 4
1. The impact of Proxy caches on Browser Latency. <i>Andrzej Sieminski</i>	5 - 21
2. Computational Efficiency of Optimized Shortest Path Algorithms. <i>P. Biswas, P. K. Mishra and N. C. Mahanti</i>	22 - 37
3. Essential spectrum of the operators generated by PDE systems of stratified fluids and L_p -estimates for the solutions. <i>A. Giniatoulline</i>	38 - 56
4. Iterative Discovering of User's Preferences Using Web Mining. <i>Maciej Kiewra</i>	57 - 66
5. Oblivious Search and Updates for Outsourced Tree-Structured Data on Untrusted Servers. <i>Dang Tran Khanh</i>	67 - 84
6. Relationship between Product Based Loyalty and Clustering based on Supermarket Visit and Spending Patterns. <i>Chad West, Stephanie MacDonald, Pawan Lingras, and Greg Adams</i>	85 - 100
7. Towards Mobile Z Schemas. <i>M. Bettaz, and M. Maouche</i>	101 - 117
8. DNA Computing Approach to Semantic Model. <i>Yusei Tsuboi, Zuwairie Ibrahim, and Osamu Ono</i>	118 - 130
9. Preprocessing and Image Enhancement Algorithms for a Form-based Intelligent Character Recognition System. <i>Dipti Deodhare, NNR Ranga Suri, and R. Amit</i>	131 - 144

Editorial

I am happy to release the new issue of IJCSA. This issue contains more papers than the previous issues of IJCSA. It is an indication that IJCSA is gaining attention of many computer science researchers. Naturally, the editorial process is becoming dense, thus we have decided to appoint a new Managing Editor for the journal. I am pleased to announce that Dr. David Camacho has been appointed as Managing Editor of IJCSA, effective May 1, 2005. I welcome Dr. Camacho to IJCSA Council and am confident that he will bring IJCSA to a new level of professionalism. He will help Editor-in-Chief in organizing editorial activities.

This issue contains nine papers from various areas of computer science, namely web mining, security, neural networks, internet, database etc.

As a supplement to this Editorial, I would like to share our views on innovative scientific research publications that are essential to our future economic and professional achievement in all areas of endeavor. The idea is to launch an electronic Open forum - *ongoing electronic conference* on Computer Science and related areas.

We are proposing a solution akin to an *ongoing electronic conference*. A solution whereby authors publish any article they wish to publish; reviewers, voluntarily, comment on whatever articles they wish to review; and editors (perhaps self-appointed, perhaps representing an established journal)—with authors' permission—archive any quality-assured paper they wish to archive.

At present, we don't have all the answers how to organize this. We don't even have all the questions. But here are some. What the best way to start this seed growing? Should the forum organizer have editorial rights of refusal with respect to papers posted in the site? Or is any editor welcome to collect papers in any such forum? How should people be accredited (for the role of reviewer or the role of editor) so that they may indeed be called "peers"? Should people be accredited for the role of author? Or should we allow any interested human the right to fully participate? I take this opportunity to invite you to e-brainstorming on this matter (The think-piece is given on next page). We will greatly value your comments, views and suggestions on creating such forum. Furthermore, I sincerely hope that you will be interested to serve on this important forum creation.

- Rajendra Akerkar

(The Think–Piece)

Scientific Publishing in the Global Electronic Village—Some Thoughts

Dan Trietsch¹ and Rajendra Akerkar

Abstract

Ideally, researchers (authors) should produce innovative and meaningful research without trying to fit it into the “comfort zone” of editors and reviewers. Editors should accept work that they appreciate, not reject work that they don’t. Reviewers should provide constructive criticism and positive suggestions—perhaps even corrections and enhancements—to research work that interests them. Not sit in judgment on work that editors selected for them to review, which they may or may not be really qualified to judge and which they may or may not wish to see published. Furthermore, like authors and editors, reviewers should receive academic credit for their important role. Our current academic publication system fails to achieve these desired effects. We posit that the World Wide Web technology makes possible a chaordic solution to this problem.

Introduction

The effect of hi-tech innovations—especially digital computers and computer networks—on the peer-reviewed academic publication process had been impressive. Word-processing programs streamlined manuscript typing. Xerography greatly facilitated duplication. Not much later, the need for producing hard copies for submission and using the postal services to deliver them (first to the editor and then to the referees and, eventually, the publisher) essentially disappeared, or is quickly disappearing as we write this. Furthermore, the very idea that publication means bound paper journals has been questioned and seems to be on the way out; e.g., using a hybrid model, many traditional print journals now provide electronic copies of accepted papers on the web before their formal publication on paper. Nonetheless, so far, the changes have been limited to innovative technical solutions for old process steps; “paving the cow path,” so to speak. E.g., word-processing replaced typing, which, in turn, had replaced hand written documents (hence the term “*manuscript*”) just a few decades earlier; email and web-based sites replaced surface mail; electronic publication is replacing bound-paper publication. But no new process step was added and no old process step was deleted. The main purpose of this think-piece is to suggest much more profound changes that the new technology—specifically the World Wide Web—makes possible. We believe that these changes may be even more important than the dramatic time- and paper-saving achieved by the previous steps. Indeed, it is possible and desirable to change the very nature of the academic debate process that academic publications are designed to bring about.

Flaws in the current system

Our first task is to identify the main problems with the current process. Arguably, if the present system satisfies the stakeholders and produces quality results it should not be

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tampered with. But does it? Let's examine the system from the point of view of researchers (authors), editors, and reviewers.

Researchers—at least many of them—would like to publish their best possible results without regard as to whether they are in the [anticipated] comfort zone of editors and reviewers. Instead, they often learn to avoid controversy, to limit themselves to intra-disciplinary subjects (while praising the splendor of inter-disciplinary work), to fill in little neglected areas in the present theories instead of building new theories and pointing to new directions. Many experienced researchers know that their best (actual or potential) work is not publishable. If it looks too simple (never mind how difficult it was to see originally), it is not likely to be published: reviewers don't like to recommend "trivial" results. If it looks too complex, it is not likely to be published: reviewers don't like to recommend results that overwhelm them. It is safer to say: "I am not convinced that the results are correct" than to say "I could not judge whether the results are correct." If the research is inter-disciplinary, the same problem applies: reviewers who are experts in one discipline don't feel comfortable vetting results that (necessarily) involve at least one additional discipline in which they are (usually) not experts. If it is in a field not central to the researcher's expertise, the jargon does not fit and rejection follows. If it says that some established scientific leaders have erred, it is not likely to be published: reviewers tend to take the safer bet that the existing thought leaders are correct; after all, their results have been accepted by former reviewers who must have vetted them carefully. (In some extreme cases, editors even ask the existing leaders to act as referees and thus, effectively, provide the negative report necessary to reject criticism against them.) All these considerations leave a very tight range within which research is likely to be conducted and published. We all sing the virtues of "out of the box" thinking, but what we find ourselves doing is typically different.

Editors—we believe—want to accept innovative and interesting research, and also with some degree of technical quality (without blatant errors). Editorial mission statements also reveal a desire to make the collective research accepted coherent in some sense; i.e., the pieces should form a meaningful whole. Special issues and edited books reflect this desire even more explicitly. What editors don't relish, one hopes, is the chance to reject the work of many colleagues. And yet a lot of the work an editor does is exactly this: rejecting the work of many colleagues. Some of this rejection is simply due to the fact that prestigious journals attract more submissions than they have room for. Part is for the reasons mentioned before. Another unpleasant part of editors' work is chasing after unwilling reviewers. Some editors even find it necessary to explicitly say that they will no longer consider submissions by researchers who consistently refuse to provide the necessary refereeing service for others' work or fail to do so in a timely manner. It is definitely a problem.

What about the reviewers' point of view? Our discussion so far may have created the impression that we have some beef against reviewers. But this is not the case. After all, researchers and reviewers are the same people. As Pogo had said, "we met the enemy and it is us." Perhaps the problem has something to do with the fact that reviewers are not volunteers but rather they are appointed by editors. Furthermore, the job seems

thankless: paradoxically, the anonymity that is supposed to protect the referee's ability to comment honestly also alleviates the need to comment honestly and prohibits meaningful recognition for the task. In addition, we tend to forget that on average we must recommend acceptance of more than one paper written by others for every paper we publish. (This assumes that an average paper has two authors and requires two positive reports to be accepted. Considering that one positive report is rarely sufficient for acceptance, and that many papers are submitted to several journals until they find an open slot, the true figure is probably much higher.) If, in some sense, the reviewers are the ones who debate the virtue of a paper, then such debate should not be limited to appointed referees. When an unwilling referee who only provides the service as a favor or a duty and not because s/he is necessarily even interested in debating a paper blocks its publication, it will never be debated by anybody else who might be interested. In effect, the process forces some of us (who are appointed as referees) to debate a paper we may have no interest in even reading, while often preventing authors from reaching others who may be more interested.

A potential solution approach

In a nutshell, we propose a solution whereby authors publish any article they wish to publish; reviewers, voluntarily, comment on whatever articles they wish to review; and editors (perhaps self-appointed, perhaps representing an established journal)—with authors' permission—archive any quality-assured paper they wish to archive (using contributed reviews or invited ones, as per their preference or policy).

Our solution is akin to an ongoing electronic conference. As in conferences, not all articles are archived in the refereed proceedings, but they are all open for public debate. In more detail, any author who wishes to publish an article in a given field or in an inter-disciplinary area would be able to post it in an appropriate forum, perhaps with links to other forums (with one forum designated as main and the rest as auxiliary—to ameliorate the negative effects of multiple postings). This act by itself should suffice to make the article public, but it does not yet mean that the article merits attention, let alone archival by any editor. The posting may be anonymous, should the author so choose: it is easy to create protected electronic pseudonyms so that eventually the author will be able to claim credit for the work. At this stage, any academic who so wishes may choose to review the article publicly (again, optionally anonymously, but this question is debatable), and post the review on the same board. (There may also be an anonymous relay function that makes possible discussing issues with authors, and reviewers may choose to use this feature before publishing their review.) Needless to say, nobody will choose to review a paper in which they have no interest. Reviews will probably be motivated either by discovering errors in the posting or by finding the paper interesting and wishing to help it along, and perhaps even suggest enhancements. We anticipate that many papers will never be reviewed—nobody can force people to comment on work they are not interested to comment on. Eventually, such papers will most probably be forgotten, although there is always a chance that they will be rediscovered eventually. A fraction of papers, however, will be reviewed at least once, and then automatically move to an area where papers and revisions with reviews are posted. These areas may be sorted further by the number of reviews attached—one

assumes that a paper with many reviews is considered more interesting than one with few, so such sorting may help readers who don't have time to read everything to focus. Of course, another way for readers to select papers to read will be a search function that will guide them towards papers in areas that interest them, or by some other search criteria. Note that because they are attached to papers, reviews are essentially mini-papers in their own right. So reviewers have a public voice and gain academic recognition, thus providing them with an incentive to read many uninteresting papers in search of the one on which they feel that they should publicly comment.

The system we propose is a chaord (Hock, <http://www.newhorizons.org/future/hock.htm>). A chaord is a system on the borderline between chaos and order that essentially manages itself through the efforts of all participants without a formal manager or ownership. The prototypical chaord is the Visa credit card system developed by Hock and his colleagues. A famous chaord is the internet itself. Who are the voluntary partners who may create and support this chaord? The obvious candidate is the community of academics and universities (including research institutes)—the same ones who have been so instrumental in the creation of the internet. Why do we need the universities? The monetary resources required are miniscule, but we need them to recognize publication in this forum, as well as reviewing articles there and archiving them, as worthy academic pursuits that are relevant for promotion and tenure decisions. Universities, however, will not do this unless individual faculty members, emphatically including senior ones, will choose to endorse the system and help it grow first. One way that occurred to us to attract excellent papers into the system is to invite seasoned researchers (e.g., more than x years in the profession with more than y papers published) to submit their ***“best unpublished paper”*** to a prototypical forum of this sort. Needless to say, any researcher who ever wrote a paper that he or she considers one of his best but editors and reviewers reject will be interested in a chance to expose this paper to the widest possible potential peer review—which is exactly what our system promises.

Finally, we don't have all the answers how to organize this. We don't even have all the questions. But here are some. What the best way to start this seed growing? We can argue that the idea should apply to all scientific subjects. For example, this will facilitate the publication of inter-disciplinary papers. But it can also be argued that it is best to start with a particular area (e.g., computer science). Should the forum organizer have editorial rights of refusal with respect to papers posted in the site? Or is any editor welcome to collect papers in any such forum? How should people be accredited (for the role of reviewer or the role of editor) so that they may indeed be called “peers”? Should people be accredited for the role of author? Or should we allow any interested human the right to fully participate? Perhaps we should have both open and closed forums. Likewise, the question of anonymity is open, and perhaps we should have outlets that allow it and other outlets that don't. Another question: is there any type of publication that we should quarantine or remove from the site? If so, who judges and how? All these questions, and ones we omitted, will have to be answered, perhaps by trial and error. But it's time to consider a change in the existing system.