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ABSTRACT

This article highlights important issues about IS/IT relevance and its impact on academic publication impact. The author presents his strong opinion about the lack of an identity within IS and harps on the requirement to focus more on core strengths that IS can leverage. Throughout the article, the author also stresses on the fact that today there is even more need to work closely with industry to better understand and solve critical commercial problems

I. INTRODUCTION

The debate in ISWorld seems to be addressing three critical issues.

- The relevance and impact of IS/IT;
- Whether large corporations (e.g., Microsoft, SUN, Intel) care about what we academics do
- Much of academic publication is trash!

In this position paper, I discuss each of these issues.

II. ISSUE ONE: RELEVANCE

To address the particular relevance issue, I looked up "rel-e-vant" in the New Merriam-Webster dictionary. It says "Bearing on the matter at hand: germane, material, and applicable". If you think about this definition you realize that there is a perception that if something does not apply to the matter at hand, or is not immediately applicable, then it is irrelevant.

A good amount of academic research may not be applicable to the matter on hand. Many important ideas were researched within academic laboratories for years before they became germane for society and hence caught the attention of vendors and commercial companies. I know of esteemed colleagues in academia and research laboratories working on interfacing chips inside a humans' cells. There are interesting notions about a Body Area Network (BAN) that will use silicon chips inserted inside the human body to interact (or network) with information servers that reside on the Internet. BAN is estimated to happen around the year 2030. Thus after 30 years, this research will bear on the matter at hand, hence let it not be relevant now. It will become relevant later.

The issue of conducting relevant work from a business or corporate standpoint, is quite different. Sales, revenues and financial growth charts affect much of the viability of a business. Every single decision must reflect on the bottom-line. Profit, revenue and the stock market decide

relevance. Thus it becomes very difficult for most industries to spend employee time on things that are not germane to the matter at hand, e.g., its customers' immediate needs. But in spite of such pressures, the pharmaceutical industry conducts basic research for at least a decade before the first commercial development happens.

The last five years of the dot com boom created another relevance consideration: venture capital financing. To raise money, a startup must please its financiers. Proposing to do something that will be relevant 30 years from today will give a heart attack to most VCs. The hype of e-commerce created an illusion that most money invested in Internet companies will give many-fold returns in less than 18 months. Finally the bubble has burst and reality is settling in. But we should all remember that the Internet in 1969 (a.k.a. Arpanet) was not funded by VCs but by DARPA/NSF. It took nearly 25 years before the Internet became used for commercial activities. There is a lot of truth to the fact that graduate students and faculty actually worked on developing most of the protocols that make up the Internet. We as academicians should feel proud of this achievement. Some argue that if a firm the size of Microsoft, Intel, or AT&T were asked to build an Internet with today's scope and reach, they would have failed severely since it was the openness of TCP/IP that led to fast diffusion.

III. DO LARGE CORPORATIONS CARE ABOUT ACADEMIA?

The second issue deals with the question: "Do Microsoft, Sun, Intel care what goes on inside academia?" Sure, they have to. Mosaic, the first X-windows browser, was created at the University of Illinois, Urbana-Champaign. Microsoft tried to copy Internet Explorer from Mosaic/Netscape but, in my opinion, did a lousy job. SUN bears its academic roots in the name itself (borrowed from Stanford University Network). Who doesn't care what is going on down at the basement of the William Gates building of Computer Science at Stanford or at the laboratory of computer science at MIT? But there is some truth to this general feeling that much cutting-edge research is happening within corporations today. Being a Telecom person, I see two interesting trends:

1. Most active researchers who are pushing the state of the art and care about impact are very active writing what are called Internet-Drafts, which later become standard documents called RFCs. These researchers are either Chairs or members of working groups with IETF, ITU-T or various Technical Forums. The speed at which technical research is being conducted cannot be matched by the long journal publication time. Journals today act more as archival records of time-tested concepts. But in the technical research world, things may have a shorter life and yet be a strong concept. New ideas of capturing such stages of research should be considered by a newer breed of journals.

2. Many problems in the telecommunications world are first encountered by practitioners since they face them every day. An example, is a technology called Multi-Protocol Label Switching (MPLS). When IP was being moved over ATM switches, clear inefficiencies were seen. People considered the overhead of maintaining SVCs/PVCs and the tiny payload of ATM cells. Hence a company called Ipsilon proposed IP switching, cutting out several complex ATM signaling tasks and making IP over ATM smoother. Others from Toshiba, IBM and finally Cisco (Tag Switching) further enhanced these concepts to create MPLS, a fundamental technology now being used by all carriers in their backbones. Such problems are extremely difficult for academics to encounter since the environment isn't there. Hence it is even more important than before to consult and work closely with industry.

IV. IS ACADEMIC PUBLICATION "TRASH"?

The third issue is the assertion that academic publications are trash. Being a trained computer scientist and working in IS in a Business School, I encounter this problem daily. Interestingly I never hear from my CS colleagues about issues like relevance or impact. But this topic comes up

on ISWorld every now and then. My feeling is that it has something to do with *creation*. When you create, you obtain an identity. One of the biggest problems facing IS as a discipline today is that it lacks an identity of its own. When you read IS literature and talk to researchers, you will often find that they are doing behavioral studies, econometrics, management, social theory and hard-core technical. But what is their core area? All the above methods are dominant cores to other disciplines. With all due respect, I find several publications in pure IS journals simply trying to survey, analyze, and then propose some bizarre hypothesis about a new technology, an organizational impact or simply conjecture about what the phenomenon is.

IS, to me, has become kind of a "parasitic" discipline. Now, there may be some really core areas where IS excels and if we can market that properly, then we will not have these arguments or debates about relevance. I can think of several new areas where IS can contribute. New technologies within organizations have far reaching consequences. The impact of I/T and new technology in organizations is critical to understand. Systems analysis and design should become a pivotal core area for IS. It needs to be reinvented as systems are changing rapidly. They are becoming smaller (they now fit on a Palm), lighter, and pervasive.

If academic IS publications are trash, then I think the editors of those journals are equally to be blamed. Why blame poor tenure-track faculty who are desperately trying to play the numbers game? Sitting on P&T meetings, I am often amazed how the discussions start by "that guy has 8 pubs and he has 5". Gees, Einstein had 3!

V. CONCLUSIONS

Richard Hamming, the inventor of the Hamming code, framed it right. He said that in technology and adoption, there are three relevant questions: First, what can happen? This is typically shown by inventors, scientists and entrepreneurs. Second, what will happen? This is decided by engineering and economics. Engineering takes a prototype and mass produces it while the right market price decides adoption by users. Third, what should happen? This is often decided by what society as a whole is willing to accept. For example, cell phones are well accepted and yet society is concerned about whether radiation and brain cancer can result from their use. IS needs to find out where in the three questions it fits well and contributes the best.

IS is a relatively new field. It needs to create and market its own identity. It should not try to do everything because then it is destined to fail. In this globally connected world, IS researchers must look beyond one's own discipline. Borders are transcending, technology and wealth creation is well and alive and it is a wonderful chance to live through this exciting time and do something worthwhile. *Let the people who contribute their knowledge to advance our community be well rewarded.*

ABOUT THE AUTHOR

Dr. Samir Chatterjee is a computer scientist and Associate Professor at the J. Mack Robinson College of Business, Georgia State University. He is fascinated with the Internet and its impact on our daily lives. In the past, his research involved ATM scheduling systems, efficient routing protocols, TCP/IP performance over HFC cable networks and all-optical networking. Currently he is exploring fundamental challenges in QOS, Voice over IP, real-time protocols and secured PKI infrastructures. A member of ACM, IEEE, and IEEE Communications Society, he authored numerous journal and conference papers. He is active in the SIP working group within IETF. He served as an expert on the Computer Science and Technology Board panel of the National Research Council.

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