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Collegial Mentorship

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ABSTRACT

Mentoring is typically thought of as a top-down process, but it can also be based on collegial mentorship. This article describes an example of peer-to-peer mentoring that helped advance research in the field of information systems.

Keywords: mentoring, peer-to-peer mentoring, collegial mentorship

I. THE ARIZONA – MINNESOTA COMPETITION: A CASE OF PEER-TO-PEER MENTORING

In the early 1980s, the departments of Management Information Systems at the University of Minnesota and the University of Arizona began research streams on collaboration technologies from different perspectives. At the University of Arizona, the engineering paradigm prevailed as the foundation for much of our research [Nunamaker et al. 1991]. Engineering is defined as "The application of scientific and mathematical principles to practical ends such as the design, creation and operation of efficient and economical structures, machines, processes, and systems" [Engineering: Dictionary.com 2007]. I therefore analyzed problems in the field and built computer systems to solve those problems. Along the way I derived insights about both the problems and the technologies. As such, our work was both exploratory and applied science. It was several layers removed from theory.

At the University of Minnesota, an experimental paradigm prevailed. Minnesota researchers derived hypotheses and designed systems to test those hypotheses. Along the way, they developed insights about theory and about the technologies, and they published the now classic Minnesota Experiments [Dickson et al. 1977]. I perceived their work as several layers removed from real workplace problems, and so, of course, less meaningful than our own. However, Minnesota received high praise in respected circles. They received kudos that we did not. More importantly, from our perspective, Minnesota received a National Science Foundation grant, and we did not. A friendly spirit of competition[1] emerged between the programs. We noticed that the Minnesota perspective on experimental research was better received by our colleagues in the discipline than our engineering perspective. It was clear that the Minnesota perspective had value and that we could learn from them.

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[1] This sense of competition was expressed in humorous ways. I once visited Gary in Minnesota in the dead of winter. At lunch time, I bundled well against the cold for a three-block walk to a restaurant -- a heavy coat, a sweater, a hat, gloves, warm socks, and a scarf. However, by the time I arrived at the restaurant, I was so cold my eyeballs hurt. Gary, however, made the trip in just a sport coat, to demonstrate, perhaps, that Minnesotans were tougher than Arizonans.

I had met Gary on a number of occasions over the years at our meetings and at conferences we often discussed producing *MISQ*, various research projects, and Gary's passion for golf. Fortunately, I ran into Gary Dickson at a conference when I was struggling with the issue of development versus experiment. I asked him "What value is there in conducting experiments? How do you conduct them? What can you learn from them that you can not learn from solving problems for organizations in the field?" At that conference and others to follow, I quizzed and probed Gary to learn more about his approach. However, because we came from such different paradigms, my questions did not always make sense to him, and his answers didn't always make sense to me. We filtered one another's inputs through our own mental models. It took a great deal of interaction and discussion over a period of about three years for me to gradually come to grips with the experimental perspective. Even though I had five theoretical statistics courses in graduate school, the courses did not prepare me for designing and running experiments. Along the way, I bounced the things I heard from Gary off other colleagues in further attempts to understand his approach. It was worth the effort. Experimental science became an important component of the Arizona research program [e.g., Nunamaker, et al. 1990].

From Gary, however, I learned that one could also gain proof-of-concept before a technology is available. Gary and his students wanted to conduct a series of experiments to test the effects of color in computer user interfaces, but color monitors and the computers that would drive them were hard to come by. My approach would have been to wait until I could find the funds to purchase color monitors. The Minnesota group mocked up color monitors with slides on an overhead projector and ran them past the experimental subjects. Their findings were robust and readily generalized to the computer technology when it became generally available. In later years, I applied that concept successfully in a number of cases where we mocked up complex, sophisticated systems and tested the concepts well before it was possible to build them. In one case it was more than 10 years between the time we obtained proof of concept and the time it was possible to build the system and test it in the field.

One of the most important lessons I learned from Gary was that an academic paper should be built around a single key thought (a lesson that is sometimes hard to remember). He frequently argued that when you narrow your focus to a single key message, its contribution becomes clear both to the writer and the reader. The paper becomes much easier to write, because you have a mechanism for deciding what should and what should not be included in the paper. Anything that does not advance the key thought can be thrown out or perhaps saved for a different paper. It is a discipline that raises the quality of our work.

From this peer-to-peer mentorship, a program of experimental research emerged at Arizona to complement the engineering work. With the engineering work, we could demonstrate proof-of-concept, that a particular class of technologies could be developed and used to good effect. With the experimental work we could tease out the effects of technology and work practice from those of organizational domain, culture, and environment, and we could begin to separate fact from anecdote. Thus, collegial mentorship gave rise to a more sophisticated research approach. Out of this probing and inquiry we formed the basis of what is now called design science [Nunamaker, et al., 1990-1991]. This research methodology involves system building, experimentation, and field studies to produce an information system or IT artifact that contributes to solving real problems in the field [Hevner, et al., 2004].

My relationship with my advisor Dan Teichroew was for 37 years until the time of his passing. He is considered by many to be one of the founders of the information systems field and was an exemplary top-down mentor. It was a relationship that would never change. Consider the following example: One day (fifteen years after receiving my Ph.D.) I walked into Dan's office at the University of Michigan, as usual he was on the phone, I waited patiently for him to complete the call and then he wrote out a message that said "Go to the library and get me a copy of X." I would always be a student and would never be a peer in his eyes, but our relationship as an advisor to a student is what most people think of as typical "mentoring."

From Dan Teichroew, I learned to define a whole domain, a larger context into which smaller efforts fit. For example, when we worked together on automatic code generation, we positioned the work in the larger context of systems development – requirements definition, systems design, database generation, code testing, deployment, and maintenance. Had we focused our attention solely on automatic code generation, when other related problems came up that were interesting, we might have ignored them because they were outside our focus. When I began working in the area of requirements teams, I therefore situated it in the larger context of collaboration. It turned out that the advances we made for requirements teams could be applied to teams in many other domains. These insights led us to interesting collaboration challenges in fields as diverse as military operations [Briggs et al. 1999], inner-city classrooms, and corporate board rooms [Vogel et al. 1990]. The insights we gained from those experiences gave us a better grasp on how to approach requirements negotiation. Among other things, this led to a collaboration track at the Hawaii International Conference on System Sciences, which has now run for more than 20 years. Eventually the collaboration work led to research questions with Homeland Security, and that in turn led to our current research stream on deception detection.

Over the years I turned to a diverse group of scholars for mentoring. The following are a few examples of the peer-to-peer mentoring from which I have benefited over the years:

From Gordon Davis, I learned a great deal about the MIS environment – how and where an MIS department fits into an organization. Gordon was an invaluable resource on issues concerning the running of an academic department. I also learned a great deal from Gordon about curriculum questions. For example, we wrestled for 30 years about how to teach systems analysis and design. I learned there are many more ways to approach such teaching than I would have imagined possible. We still may not have the right answer, but I am a far more sophisticated teacher of systems analysis and design than I might have otherwise been.

From Andy Whinston, I learned to stay current on the cutting edge by finding applications for the latest technical breakthroughs into ongoing research streams. Andy taught me to constantly scan the academic environment looking for new approaches, new insights and interpretations. For example when pen-based interfaces and voice-recognition technologies became available in the early 1990s, we built them into collaboration technology prototypes, not because we saw an immediate need to do so, but to be sure we understood their potential. When a need arose, we knew whether and how the latest technologies could meet it.

From Paul Gray, I learned key lessons about how to survive and thrive as an academic. Paul has a collection of thought provoking maxims for surviving in the academy. He was the first one I heard say that the number of papers required for tenure is $n+2$, where n is the number of papers you have already published. He often told his students that the only good dissertation is a done dissertation. He was also the ultimate editor. Anything that was really critical I would send it to Paul for his advice.

From Judee Burgoon, I learned a great deal about theory development and the logic of causal scientific enquiry [Cao et al. 2006]. I also learned important concepts from people outside of academia, e.g., Lou Schutte, vice chairman of Sunstrand, taught me how to approach business people, how to explain our research in terms they could appreciate and understand. My work is firmly founded on an amalgam of these and many other such insights from those who would not be regarded as mentors in the traditional sense. Most of the mentoring I have received has been of the peer-to-peer nature and has been invaluable in shaping careers. Thank you to Gary, Dan, Gordon, Andy, Paul, Judee and Lou and also to many unmentioned mentors including many of my former students.

II. CONCLUSIONS

I derived the concepts that drive our research programs from my interactions with my mentors – both those who took me under their wings, and those who tolerated my persistent probing questions. Mentorship need not be only a top-down process, it can also be a peer-to-peer process. Like so many other things in academia, mentorship can be a knowledge discovery process, and its consequences advance our entire field.

I encourage faculty to actively engage in collegial mentorship and otherwise seek diverse perspectives on their research questions. Researchers practicing different methodologies, practitioners grappling with real problems, and faculty in other disciplines are all potential sources of valuable insight. All too often there is a tendency for faculty to view their own work as important and the work of others as trivial. However, collegial mentoring involves just the opposite; it requires faculty to actively seek the value in other approaches and recognize the limitations of their own work. I believe faculty will find greater success in their career if they follow this approach and I encourage them do so.

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Jay F. Nunamaker, Jr. is a leading researcher and entrepreneur on collaboration technology and group support systems, founder of the MIS Department at the University of Arizona, a major professor to over 80 doctoral graduates, and past chairman of the ACM IS curriculum committee. Jay was selected as recipient of the AIS Leo and Fellow Awards, as well as the Computer Educator of the Year Award from the International Association of CIS. He received the Educator of the Year Award by the DPMA Education Foundation in 1996 and the Andersen Consulting Professor of the Year in 1992. He serves on the editorial board of six journals. He has been a track chair for a large segment of the IS program at the Hawaii International Conference on Systems Sciences. He is the author of over 220 papers and author/editor of 10 books and 7 videos.

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