EDITOR'S COMMENTS

Irreducibly Sociological Dimensions in Research and Publishing

Editors can behave in unexpected ways. An editor can accept for publication a research paper that falls in a school of thought with which he actually disagrees. An editor can manage the review process for a research paper that lies outside of her expertise. I have done these and other things that have caused some people to express surprise. There are reasons, of course, for my behavior. The reasons have to do with my understanding of our overall research enterprise.

As an editor, I have been mindful of the larger research structure in which I merely occupy a role. Research unfolds in this overall structure. A single person—whether an editor-in-chief or a person with a freshly minted doctorate—hardly exercises complete free will in the sorts of research questions, theories, and methods that he or she may choose to entertain. A research structure, consisting of these and other elements, is already in existence and its use is sanctioned by established researchers.

Another dimension to our research enterprise is related to what has been called the "natural science model" of research. According to this model, researchers concern themselves with a theory to explain and predict a phenomenon, where their research activity generally consists of articulating or refining the theory so that it may more accurately explain and predict the phenomenon. The needs of the current theory (refining, articulating, and extending it) provide the focal point for further research activity, hence specifying future research directions. Kuhn (1962) calls this activity "puzzle solving" and "normal science." It is a research process that runs on its own momentum, where the normal activity of identifying where the current theory needs more work has the effect of setting the research agenda for the scientific community. Only when normal science fails to improve a theory, such that it encounters more and more situations it actually fails to explain or predict, would the theory be rejected and a new one erected in its place (Kuhn calls this very occasional activity "revolutionary science"). In my understanding, most of our information systems (IS) research community subscribes to the natural science model. Good examples of puzzle solving and normal science can be found in the stream of research on the technology acceptance model (TAM), the stream of research in GSS investigating the impact of technology on decision quality, the stream of research on information technology and media richness theory, and the stream of research on the productivity-paradox/business-value of information technology.

A third dimension is that research involves not only logic and procedures, but also the norms, values, and culture of the particular scientific community. I am particularly fond of the following passage by Kuhn (1970, pp. 237-238), where he reveals the importance he places on the extra-rational elements in how scientific research proceeds:

> Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time. In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a *group* of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the

> ideology, current in their discipline (and to a great extent in other scientific fields as well); and finally, *let them make the choice*. If that technique does not account for scientific development as we know it, then no other will.

The shared values of a scientific group provide the context in which logic and procedures unfold. A stronger way to express this, based on my reading of the history of science, is that logic and procedures cannot operate outside of the context of the shared values of a scientific group. A shared value system is necessary for different individuals to act as contributing members of a research enterprise.

Understanding our research enterprise in this way, I take different stances in my different roles as a researcher and as an editor.

As a researcher (not as an editor), I disagree with much of the research that I see in many of the manuscripts submitted for possible publication (and actually published) in *MIS Quarterly*. For instance, suppose there is a school of thought that advocates theory A, whereas I do research in a different school of thought that advocates theory B, which happens to be contrary to theory A. In my role as a researcher, I would prefer to dismiss theory A.

On the other hand, as an editor (not as a researcher), I believe that any school of thought must find out for itself, through its own efforts at puzzle solving and normal science, whether its theory is worthy or not. Upon receiving a manuscript from any school of thought, I would not impose my own research preferences. Instead, my desire as an editor is actually to support this school of thought in working through its own puzzle solving and normal science, *according to its own standards, methods, and assumptions*. If indeed its theory is deficient, members of this school of thought would have to arrive at this conclusion by themselves. This approach would also allow for the happier possibility that the theory holds good potential and, through puzzle solving and normal science, could be rigorously confirmed and widely accepted. One result of such an editorial attitude is that I could end up accepting this manuscript for publication, even if its research perspective is contrary to my own.

Another result is that there is no inherent problem in my serving as an editor for a manuscript that falls outside of my own area of expertise. Authors of such manuscripts even nominate me to serve as their editor. In this situation, I see my function not so much as applying my own research expertise (indeed, the reason for involving reviewers is to tap them as sources of the needed expertise), but, instead, as structuring the review process so that the manuscript's research can be judged according to the standards, methods, and assumptions of its own school of thought. Editors are sometimes stereotyped as super experts, able to make substantive judgments on the fine details of the research submitted to them. In some cases, this image holds. In other cases, I have found that, as an editor, I do not so much apply my own research skills as I apply my skills in structuring and guiding the review process. There is also the consideration that, if a manuscript could only go to an editor who is already an expert in all aspects of the manuscript's topic, then our overall knowledge would not grow.

This philosophy has also motivated three of my other actions as an editor. First, I only occasionally go outside of our IS research community for reviewers because, in neither being imbued with nor sharing the same value system, researchers who have not done IS research are less likely to appreciate the significance and nuances of the puzzles to which we IS researchers turn our attention. Second, with regard to making IS research relevant to practitioners, I have acted with some colleagues to establish a new journal, whose review system would explicitly recognize and reward a practitioner-oriented direction in academic research. The momentum of normal science, in which the needs of the current theory (not the needs of the practitioner) establish the puzzles to which researchers turn their attention, can be so strong

as to prevent the research direction from being diverted to a more practitioner-relevant path; hence, in my view, the drastic action of launching a new journal is warranted. Third, I have encouraged some authors to pursue what March and Smith (1995) call "design science" research, what my Georgia State University colleagues call "improvement research," and what some IS researchers call "technology" research. In this sort of research, it is still important for a theory to explain and predict a phenomenon, but it is less important than the instrumental use of a theory to build a system (not just the technological system, but also the social system) that is efficient and effective in the eyes of managers, executives, and other members of the "real world." It is a form of research that, in my view, is no less valuable than research following the natural science model. The logic, procedures, and value system associated with this type of research are not the same as those associated with the natural science model that now predominates in IS research. Again, the momentum of normal science can be so strong as to prevent the research direction from being diverted to a different, desirable path. A social intervention into our research structure could be required to give design-science/improvement/technology research the full recognition that it deserves.

Being an editor has required me to turn my attention to the irreducibly sociological dimensions of our research enterprise, not just its logical and procedural dimensions. In general, this should provide some of the rationale for why editors sometimes behave in ways that initially appear unexpected and surprising.

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References

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