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Empirical Research in Information Systems: On the Relevance of Practice in Thinking of IS Research

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A Note on Benbasat and Zmud

I found the article by Benbasat and Zmud both interesting and provocative. Because it is written by two leading North American IS scholars—both former or current editor-in-chiefs of MISQ—the paper's call for a greater concern for relevance in our research should not be taken lightly. I think that the propositions Benbasat and Zmud suggest are welcome and help set up directions for future research procedures in IS. As a European scholar who has taught and done research on both sides of the Atlantic, I do not see all issues raised, however, in the same light. In this commentary I highlight some of these differences.

I was surprised that Benbasat and Zmud do not look at broader institutional issues that affect how relevance is defined in different research contexts. Variation in these issues also explains some differences between the European and the North American IS research. These broader issues define how the "practice" is expected to shape IS research and theory and also how this practice is understood. Thus, the relevance of practice is not only about how a researcher learns to pay attention to the areas of interest to practitioners and to communicate their findings lucidly, but also about what the researcher sees as practice and what elements are relevant in understanding and changing that practice. Thus, putting the question of the relevance in the foreground demands us to probe a larger texture of relations between the IS researchers and the "practice" and to take the concept of practice more seriously. This rethinking should cover topics like institutional policies and incentives in IS research, the organization of IS groups, the professional image of IS researchers, and the manner of studying IS practice.

In the area of institutional policies a clear problem is the lack of critical mass of IS researchers and the associated lack of a long-term research perspective. Without strong and well-organized IS institutions, it is difficult to establish good relations with industry in order to really understand and shape industrial practices. My experience shows that it takes at least three to five years to do anything, which can make a difference in practice. Thus, anything that really addresses relevant concerns is beyond the scope of a single Ph.D. study.

The professional image of IS researchers—because of its dependency on rigor, scientific expertise, and tenure policies—also leads to opportunistic research behaviors that tend to ignore practice. Too often North American IS researchers want to *work with specific research solutions* that are looking for problems (like doing a survey-based study or an experimental laboratory investigation in which they are experts). Fewer engage in systematic attempts to solicit problems (in a long-term perspective), which would then lead to theoretical and rigorous "solutions" in practice. Many times this working style is due to the highly specialized focus of empirical researchers and their individual working style. This leads to researchers' low understanding of technical matters and associated practical problems, and consequently many IS professionals do not esteem them highly. Accordingly, empirical researchers seldom work on topics that are close to the leading-edge practice such as installing new systems or visioning new modes of technology use. Instead, they follow suit when the topic is already passé by reporting about leading-edge practice. Because of the high specialization and small size, there are also too few large heterogeneous IS research groups. Consequently, research practices that truly engage researchers deeply—like *development research,* which combines both constructive and empirical elements—are largely missing. My own experiences from highly relevant research are mostly from *constructive and innovative technology development and transfer projects,* which necessitated a thorough dialogue between the technical and social theoretical frames as well as between everyday experiences and employed theoretical frames.

I was also surprised at the lack of reflectivity on what determines what is relevant for the practice. Is relevance only something that suggests immediate solutions for CIOs and that they can digest in one afternoon by reading a *MISQ* article, or is relevance also something that can elevate and reshape professionals' thinking and actions in a longer perspective? Much of the discussion in the paper implies only the former concept of relevance.

And I was surprised at the way in which relevance and rigor were related like a set of axioms. I could call it the "Benbasat-Zmud rigor law": rigor will replace relevance if there are no powers to stop it! Because of the institutional differences, my experience in Europe is many times the contrary: relevance will replace rigor if there are no powers to stop it! The paper also makes a claim that the "IS field does not possess the evidence with which to illustrate the impact of its research." Although this is a rhetorical trick, I do not think it is a justified one. There are several innovations that can be traced to IS academics: megapackages (SAP), component-based architectures, and approaches to IS strategy and design. With empirical research the situation is bleaker, but a look at the related fields like computer-supported cooperative work research shows that many of the practices—like a reliance on ethnomethodology—are driven by theory-based research.

The paper ignores the most important way in which our research findings are appropriated in practice—through our teaching. Textbooks have an immensely important role in shaping the minds of future generations. The reliance on textbooks has, however, a side effect: much of what we teach on technology and practice is outdated by five years at least. Moreover, much of what is said in these textbooks does not relate to our research findings. These both may explain the low esteem of empirical research by practitioners. Unfortunately, the sole exposure to this knowledge delivery mode by many North American graduate students makes them also incapable (and clearly not motivated) of reading anything other than well-packaged "teaching hamburgers." No wonder, then, that managers and other professionals (our former students) don't read our research papers! This situation is different in many parts of Europe where students at the master level are expected to read original scientific texts. They do not always like them and have hard times understanding them. But later on they often appreciate these requirements when they have entered the professional life. Consequently, many European practitioners are more inclined to read research texts (there are no empirical studies, but I base it on my exposure to both cultures). This is a cultural difference and difficult to change unless educational policies and expectations are changed. Even so, I would not trade off academic writing style and demand that we write in a manner that is simple, concise, and clear, as Benbasat and Zmud suggest. Instead I would expect that we educate our practitioners to appreciate brilliant intellectual efforts! Several IS phenomena are hard to understand and may demand difficult and esoteric language because they cannot be couched in the "common language." Still, the message of a text written in an esoteric language can be relevant. For example, the fashionable use of Heidegger in understanding design or use of IT is neither possible nor useful unless the reader can work through Heidegger's thick concepts and ideas. My nightmare would be to emasculate Heidegger and dress him into the *HBR* format!