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Rigor and Relevance in MIS Research: Beyond the Approach of Positivism Alone

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Benbasat and Zmud offer a diagnosis of "why one tends today to observe a lack of relevance to practice in IS research" and a prescription of guidelines that "the IS academic community might follow to introduce relevance into their research efforts and articles." I will comment, first, on the ramifications of their self-avowed positivist orientation; second, on their model-in-use of what relevant research is (i.e., the instrumental model); and third, on the need for the IS research community to take a broad approach to the matter of relevance. I will also refer to the respective commentaries offered by Applegate, by Davenport and Markus, and by Lyytinen.

Ramifications of Positivism for Relevance

Davenport and Markus comment that Benbasat and Zmud have *not gone far enough* in their argument on relevance. I agree with Davenport and Markus, but offer an additional rationale.

Benbasat and Zmud did not go far enough *in that they restricted themselves to the perspective of positivism.*[1] In the social sciences, positivism refers to the belief that social-science research should emulate how research is done in the natural sciences. Interestingly, Davenport and Markus happen to assert that IS research ought to emulate research in medicine and law. A point that Davenport and Markus do not state explicitly, but that would further strengthen their position, is that medicine and law are not natural sciences, but professions. Inquiry in the professions, such as medicine, law, engineering, and architecture, does not quite proceed in the same manner, if it does at all, as inquiry in the natural sciences. Inquiry in the natural sciences pursues the goal of truth in formal propositions; inquiry in the professions pursues the goal of effectiveness in actions. Inquiry in the natural sciences produces knowledge about what the world is; inquiry in the professions produces knowledge about how to intervene in the world and change it in order to satisfy real-world needs.[2] Clearly, if we wish our research to be relevant to practitioners, then we ought to consider doing our research in a way that emulates inquiry in the professions, whether in addition to or instead of doing research in a way that emulates inquiry in the natural sciences.

[1] In their paper, Benbasat and Zmud say the following: "It is important to mention at the start that the views expressed in this paper are those of two North American IS academics who have mainly espoused a positivist research tradition."

[2] Of course, science and technology can be related, but they are not the same (just as physics and engineering are related, but are not the same; as biology and medicine are related, but are not the same; as economics and finance are related, but are not the same; as sociology and social work are related, but are not the same; and so forth).

As long as Benbasat and Zmud's recommendations contain the presumption that IS research should emulate how research is done in the natural sciences, we ought to consider what ramifications this positivist perspective would have on efforts to make IS research relevant. The history of science, as rendered by Thomas Kuhn, is instructive. He writes (1977, p. 146, emphasis added):

> No science, however highly developed, need have applications which will significantly alter existing [real world] practice. The classical sciences like mechanics, astronomy, and mathematics had few such effects even after they were recast during the Scientific Revolution. The sciences which did were those born of the Baconian movement of the seventeenth century, particularly chemistry and electricity. But even they did not reach the levels of development required to generate significant applications until the middle third of the nineteenth century. *Before the maturation of these fields at mid-century, there was little of much socio-economic importance that scientific knowledge in any field could produce.*

Of course, the world did not stand still until natural scientists were ready with knowledge to help solve real-world problems. Real-world practice used knowledge, but it was not knowledge produced from inquiry in the manner of the natural sciences. Rather, it was knowledge produced independently of and prior to any natural-science inquiry. The following observations by Kuhn are provocative (p. 144, emphasis added):

> When Kepler studied the optimum dimensions of wine casks, the proportions which would yield maximum content for the least consumption of wood, he helped to invent the calculus of variations, but existing wine casks were, he found, already built to the dimensions he derived. When Sardi Carnot undertook to produce the theory of the steam engine, a prime mover to which, as he emphasized, science had contributed little or nothing, the result was an important step toward thermodynamics; his prescription for engine improvement, however, had been embodied in engineering practice before his study began. *With few exceptions, none of much significance, the scientists who turned to [practical needs] for their [research] problems succeeded merely in validating and explaining, not in improving, techniques developed earlier and without the aid of science.*

This outcome suggests that, even if IS researchers were to implement Benbasat and Zmud's second recommendation ("IS researchers should look to practice to identify research topics. . ."), they might succeed only in explaining what practitioners are already doing. Certainly, research that systematizes and makes explicit what practitioners are already doing can be relevant by helping to promote the dissemination of successful practices; however, this mechanism for achieving relevance is different from the instrumental model of relevance (which I explain below) underlying Benbasat and Zmud's recommendation.

Kuhn's concept of "normal science" is also instructive. It refers to the activities in which natural scientists typically engage. He writes (1996, p. 25): "There are, I think, only three normal foci for factual scientific investigation, and they are neither always nor permanently distinct." First, for a scientific community, there is the activity of "determination of significant fact," which refers to the facts that the community's predominant theory can explain.[3] Second, there is the activity of "matching facts with theory"; this involves "those facts that... can be compared directly with predictions from the [predominant] theory" (p. 26). The third and last activity is "articulation of theory," which "[c]onsists of empirical work undertaken to articulate the [predominant] theory, resolving some of its ambiguities and permitting the solution of problems to which it had previously only drawn attention" (p. 27). *The point here is that the activities in which natural scientists typically engage are theory-driven (i.e., driven by needs to refine the theory so that it pre-*

[3] "Attempts to increase the accuracy and scope with which [these facts] are known occupy a significant fraction of the literature of experimental and observation science. Again and again complex special apparatus has been designed for such purposes, and the invention, construction, and deployment of that apparatus have demanded first-rate talent, much time, and considerable financial backing."

dicts more accurately), rather than practice-driven (i.e., driven by needs to resolve real-world problems). One might attempt to force a natural-science research effort to be practice-driven, but this is what Kuhn discussed as the ineffective efforts of Kepler and Carnot (see above).

None of this is to argue that research taking a natural-science approach must necessarily be irrelevant to practice. Some natural sciences, as mentioned above in the quotations of Kuhn, eventually had beneficial impacts on real-world needs and problems; however, these particular fields reached such a stage only by passing through the theory-driven (not practice-driven) activities of normal science, where there was no guarantee that the field would ever produce results useful in solving real-world problems.

Therefore, whereas research conducted in the manner of the natural sciences can be (for long-run impact) one of the strategies that the IS research community takes in its pursuit of relevance, it need not be and should not be the only strategy. After all, not even all the natural sciences have produced results with relevance to practice. Our community of IS researchers might, therefore, consider conducting inquiry not only in the manner of the natural sciences, but also in other ways, such as the manner of the professions.

Ramifications of Instrumentalism for Relevance

I interpret the "instrumental model of practice" to be the model-in-use in Benbasat and Zmud's rationale for their nine recommendations. I define the instrumental model of practice as including the following elements. A researcher formulates, tests, and validates a theory that specifies independent variables, dependent variables, and the relationships among them. In doing this, the researcher is careful to make sure that, first, the dependent variables represent the outcomes that the practitioner is interested in achieving and, second, the independent variables represent factors that not only indeed influence the outcomes but also can be manipulated or changed by the practitioner. A practitioner could then apply the theory by manipulating the independent variables in order to achieve the desired levels in the dependent variables. Of course, all of this presumes that the practitioner's problem was clearly definable in the first place. Furthermore, the practitioner is the "customer" of research, and the researcher is depicted as the "producer" of it. Finally, for the researcher's theory to be immediately useful and also for the researcher's long-run research program to be fruitful in producing additional useful theories, the researcher should follow as many of Benbasat and Zmud's nine recommendations as possible.

Certainly, the instrumental model of practice applies in some situations, but it does not apply in all situations.

I believe that there are often circumstances in which one of our responsibilities as academicians is to be the conscience for our practitioner colleagues and, indeed, for society in general. Our research, for instance, could lead us to believe that the profession's systems development methodologies are oppressive to users. In publishing such an article to expose this exploitative practice (for a similar instance, see Beath and Orlikowski 1994), we researchers would hardly be approaching practitioners as "customers" to whom we would be catering. Here, we would not be handing a theory to practitioners for them to apply in the tasks that they themselves have defined. Instead, we would be seeking relevance by criticizing and changing the "customers" themselves. In other words, research can be relevant not only in the sense intended by the instrumental model of practice, but also in the sense intended by critical social theory, where false consciousness and inappropriate work relationships are brought to light. Interestingly, in this situation, practitioners would not be the judge of the relevance of our research, and indeed, their non-acceptance and non-utilization of it would not indicate that it lacks relevance. Furthermore, a critical-social-theory researcher could even question the sort of relevance delivered by the instrumental model of practice to the extent that an instrumentally produced theory could be used to perpetuate false consciousness and inappropriate work relationships.

Then there is the situation in which a practitioner's problem is not clearly definable. The practitioner's organizational task environment is murky, and the variables are not even known. In such a situation, we can invoke relevance through our roles as teachers. For instance, when I conduct class discussions on Harvard Business School cases, I present each case to my students as if it were a "simulation" of an organizational situation. I teach the students (who are current and future practitioners) what I know, where what I know comes from the research that I have read and done. What the students learn through the case discussions can make a difference to the actions they take in the actual organizational situations they encounter in the future. Similarly, as Lyytinen states in his own commentary on Benbasat and Zmud, textbooks (which distill our research) can also provide an avenue to relevance. Hence, through our teaching, research can achieve relevance, albeit not through the instrumental model of practice or the associated nine recommendations that Benbasat and Zmud offer.

None of this is to argue that Benbasat and Zmud's positivist and instrumental approach is irrelevant to practice. The point is simply that their prescribed guidelines represent only one way by which relevance can be achieved.

The Need for a Broad Approach to Empirical Research on Relevance

It is not enough for senior IS researchers to call for relevance in IS research. We must also call for an empirically grounded and rigorous understanding of relevance in the first place.

For example, Benbasat and Zmud usefully cite a 1980 article by Peter Keen to support their statement that "IS researchers have been less successful than their colleagues in other business school disciplines in developing a cumulative research tradition"; however, I know of no recent empirical research, positivist or otherwise, that attests to the accuracy of that statement today. Indeed, until rigorous empirical research convincingly establishes the respective states of cumulative research in all the different business-school disciplines, no assessment comparing other IS researchers with other business-school researchers may be properly made. For another example, consider Benbasat and Zmud's statement, "one tends today to observe a lack of relevance to practice in IS research"; my reaction is that a survey, field study, documentary analysis, or other rigorous empirical study must be done to procure evidence for this statement (where, of course, the result of the empirical study could even be that IS research *is* relevant to IS practice). However, until these empirical studies are done, the extent to which IS research is relevant to IS practice remains, objectively speaking, unknown.

Research on the topic of relevance to practice would need to accomplish more than just provide empirically grounded statements on the state of relevance of IS research today. Such research could also explore all the different forms that relevance can take, in addition to the forms presumed in the instrumental model and in critical social theory. Such research might also look for parallels between how relevant research has unfolded historically in professions such as medicine, law, engineering, and architecture, and how it might therefore unfold in IS. Above all, it would not be enough for such research to offer speculative philosophical ponderings; such research would, of course, have to be relevant in its own way too.

Finally, I would like to return to Applegate's case study featuring the predicament of Assistant Professor Marilyn Moore. What advice might a senior faculty give her? If positivist research were the only option available, then she would have no choice but to engage in the research activities of "normal science," which are theory-driven, not practice-driven. (One could attempt to stage an act of what Kuhn calls "revolutionary science," but this act would not be feasible at the individual level, especially if the individual is an untenured assistant professor.) As for the recent rejections of her submissions to some research journals, the positivist advice to her would be to continue to do positivist research, but harder and better than before. And then, if she were to succeed eventually in publishing her research, it might or might not achieve relevance to practice, in the same way that the results of different natural sciences (according to Kuhn, above) sometimes do, and sometimes do not, achieve relevance to practice.

However, positivist research is not the only option. At the same time, in order for young scholars such as Marilyn Moore to be regularly and responsibly advised to pursue alternative forms of scholarly research, such research would need to be better recognized and institutionalized. This, in turn, is a challenge that senior members of the academic IS research world would have the responsibility to take up.

References

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