There Is Nothing So Practical As Good Theory: Exploring the Conjecture

A special issue edited by Mike Metcalfe

A general theory of IS

What is Theory?

Design Theory (3 articles)

Pragmatism: looking at problems with multiple perspectives

The seven papers that make up this special issue (6:2) are the result of much hard work and some visionary thinking by its special issue editor, Mike Metcalfe. Prof. Metcalfe has long sought to push the boundaries of IS research thinking. He was one of JITTA’s early authors and one of its early editors. In the role of editor, he helped to bring to the journal a number of interesting papers. In this issue, Metcalfe has organized the review of many submissions, winnowing them to seven very special papers about theory in IS research. As a group the papers ask each of us to pause briefly from our work-a-day efforts to understand phenomena in our focused domains to think broadly about the theory that many of us are trying to create, expand, challenge, test and practice.

In the lead paper, Hamilton (2004) argues for the need for a general theory of information systems, a broad “set of umbrella concepts” that would define the discipline. Such a theory could provide the mechanism through which the discipline’s audience could understand it and come to appreciate its value. Marxism, for example, has been such a theory, but not the only one, that has provided this role for the sociology discipline.

Marxism has been a foundation for much social thinking, particularly in Europe and Asia, for more than 130 years, in spite of its manifestations in several of the world’s most brutal regimes and in spite of the manifest foolishness of several of its major component concepts. This may be because of the comprehensiveness of Marxism and its ability to be extended, modified, and adapted to suit a variety of cultures and environments. Also Marxism follows from two plausible assumptions: all social processes are based on economic value and people are naively rational in their decision making.

Could IS be the focal point of a major social theory? Certainly IS structures permeate society and are beginning to form the structure through which much of individual and organizational work and play is performed. Hence, they’re very comprehensive. Clearly IS structures evolve to allow their adaptation to a variety of cultures and environments. Could we develop a theory of IS based on bounded rationality and the centrality of information to human behavior?

IS academics are well placed to make use of a general theory to increase the discipline’s influence on society, the economy, and business. More effort should be invested into theory-oriented research on the social

impacts and implications of IS developments, Hamilton claims.

What is theory? Metcalfe (2004) seeks to define the concept of ‘theory.’ He starts with the meaning of the word “theory,” finding many alternative current and historical meanings in dictionaries, among professions, and for various purposes. Then he turns his attention to research, mostly in the management research literature, but also in other disciplines, such as the natural sciences. With so many definitions, he asks, perhaps the meaning of the term, ‘theory’, is so diffuse that that it is no longer useful. Everything is not lost, however; Metcalfe is able to discern a common meaning among all of these definitions that lead to an understanding of what theory is. He explains this understanding in terms of six concepts: perspective, explanation, argument, evidence, generalizability, and in-theory.

With the third paper, we reach the core of the functional objective of the special issue: the practicality of theory. Information systems research is naturally applied research, says Martin (2004), so our discipline is a very good context in which to understand the relationship between theory and practice. Theory and practice should produce the same or similar results. Isn’t that the purpose of theory? A theory describes the universe and predicts how it will behave. Practice instantiates the theories in real artifacts. Theories have their limitations, however, especially in the social sciences or in an applied social science, like information systems. Social science research often explains only a small part of the observed phenomenon, leaving most of it to ‘observed error’ (it sounds as though the universe is at fault for not behaving properly). Martin explains that much of the practical limitations for theory come from pragmatic considerations, such as resource constraints, organizational politics, laws, and management style. If we want theory, and with this Martin means also our profession of research, to work well for practice, we’ll have to develop ways for research and practice to actively collaborate, such as “reflexive practice.” Theory must incorporate practical constraints.

Walls, Widmeyer, and El Sawy (2004) turn to the questions of design theory, specifically investigating the extent to which their own framework for information systems design theory (ISDT) has been or can be effectively used as the framework for building theory. They discover four distinct levels of use among 26 articles that reference it, ranging from using it as a “cloak of legitimacy” to using research to “[enhance] the richness of ISDT itself.” They finish up with four recommended strategies to enhance the efficacy of ISDT. Their effort is interesting because, as they point out, design science is underrepresented in IS literature, unlike in the literature of our discipline’s second cousins, electrical, computer, and industrial engineering. This under-representation is unfortunate in a discipline that is inherently applied. Like engineering and unlike the natural sciences and pure social sciences, the IS discipline systems exists because of the practical value that it can create.

Goldkuhl (2004) continues the discussion of design theory by leading us through an inquiry about how to ground design theories. He explains that grounding falls into three process types, internal, empirical, and theoretical, with several sub-processes. These processes contribute to the multi grounding of design theory a process that contributes to the validity of design theory and may be used to describe different ways that design theory can be generated. Grounding theory drags it in the direction of the practical as, in the grounding process, the effects of Martin’s practical considerations have to be taken into consideration.

According to Hooker (2004) there cannot be a theory of design, in the sense that there are theories in the core academic disciplines in the natural and social sciences, but there can be supporting theories. This is because design, by its nature, is an attempt to move from functional descriptions to physical descriptions and artifacts. There could be a theory that describes how designers work or how they should work, but this would not be a theory of design, but one of design practice. So what would design theory be? It could consist of computational models, where the designing could be supposed to be done entirely by machine, thus carrying out the theory without a human practice component.
Since design inherently involves incomplete descriptions, e.g., IS requirements specifications or architectural drawings, it could be a theory of incomplete specification. A theory of incomplete specification could be quite exciting for IS. The problem of the incomplete specification a very important and underresearched one in an era in which IS design is extensively outsourced offshore and in which global virtual project teams and organizations are used often used to produce product, process, and infrastructure solutions. Such coordination mechanisms invariably involve incomplete specification.

Since design starts with a functional description, it might be appropriate to develop teleological theories of design, i.e., theories that relate to the purpose of the intended artifact. This suggests a (currently nonexistent) “teleological science.”

Ian Mitroff (2004) rounds up the seven papers, seeking to help us all to think better about research problems by encouraging us to look at them from a variety of perspectives. He contrasts the usual assumptions of 19th and 20th century scientists and problem solvers (objective problem definitions, disciplinary problem ownership, clear solutions) with those proposed by William James, an influential early 20th century pragmatist. The earlier assumptions still guide thinking in many knowledge realms, leading, for example, to stovepipe research disciplines and the legalistic solution to society’s quarrels. He proceeds to lead us through several examples to show how these contrasting views of the nature of knowing can affect outcomes in real life and can affect the value of what we know and do. A new way of thinking can help us to deal effectively with the “wicked problems” of our life and our research.

REFERENCES


