An Unhealthy Obsession with Theory

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Abstract

As the flagship journal of the Association for Information Systems (AIS), the *Journal of the Association for Information Systems* (*JAIS*) invites the incoming president of AIS to write an editorial about issues facing AIS and its members. In this editorial, I argue that over the past 30 years, information systems research has shifted to what I see as an unhealthy obsession with one particular type of theory to the exclusion of other types. I believe this obsession is stifling new research and is preventing us, as a discipline, from leading technology innovation. The solution is simple: return to our roots and embrace other types of theory. The opinions in this editorial are solely mine, and do not reflect the official policy of AIS, or the opinions of my colleagues at AIS or those who serve at *JAIS*.

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1 Introduction

Over the past 30 years, I've seen a gradual shift in the nature of information systems research that I have come to believe has led us astray. My objective in this essay is to argue that, as an academic discipline, we have developed an unhealthy obsession with theory, an obsession that is stifling new research and is preventing us as a discipline from leading technology innovation.

Gregor (2006) defines five types of theory: (1) analysis (a descriptive framework); (2) explanation (understanding but no prediction or propositions); (3) prediction (propositions but no causal explanation as to why); (4) explanation and prediction (both causal explanations and testable propositions); and (5) design and action (a process theory for developing an artifact). Today, most IS academics would argue that Type 4

theory is what they mean when they say "theory" (cf. Sutton & Staw 1995; Whetten 1989).

Theory—and Type 4 theory, in particular—is a key tool in scientific discovery and contribution to knowledge. However, theory, in and of itself, is neither scientific discovery nor is it a contribution to knowledge. Rather, theory is just one way of describing these. Unfortunately, the IS discipline has gradually evolved to conflate these concepts so that they are often seen as synonymous: scientific discovery and contribution to knowledge are seen as the production of new theory—Type 4 theory, in particular.

In the sections below, I briefly describe how we ended up in this situation, the problems it has created, and what I believe is a better path forward for our discipline.

2 The "Maturation" of Information Systems as an Academic Discipline

Information systems (IS) is an applied discipline, more so than other disciplines like psychology, biology, or mathematics. Many IS researchers and most members of the Association for Information Systems (AIS) work in business schools; as such, the culture of the business school has helped shape the culture of the IS academic discipline.

From the beginnings of business schools as separate academic units founded in the early 20th century through their initial decades of existence, business schools focused on highly applied research (Association to Advance Collegiate Schools of Business, 1997). They studied new phenomena emerging in the business world and provided academic insight into what, up until then, had been viewed as practical problems. The Carnegie Report (Pierson, 1959) was highly critical of this focus and advocated shifting business research into a form that looked more like the theory-based research of our cousins in the arts and sciences. That shift, like many changes in academia, took decades to complete.

When I began my career in the 1980s, the IS discipline still valued research on new phenomena in the business world. Many of the seminal papers in *MIS Quarterly* (*MISQ*) (one of the few purely IS academic journals at the time) had deep roots in new phenomena and were very unlike the theory-based papers in arts and sciences, or even those in other disciplines within the business school that were gradually moving to theory-based research. Throughout the 1980s, *MISQ* was distributed to all members of the Society for Information Management (SIM), the leading association for practicing IS managers, as a benefit of membership, and the winners of the annual SIM competition had their papers published in *MISQ*.

The 1990s brought changes. Many leading IS academics became concerned that IS was not a real academic discipline because of its dual focus on theory and new phenomena in the world of practice. They were concerned that our colleagues in other business disciplines would not take IS seriously unless we too moved to focus on theory-based research. The editorial mission of *MISQ* changed, and other IS journals followed suit. Theory became the primary goal. Consequently, SIM ended its relationship with *MISQ* because its members no longer saw value in the research it published.

In the early 2000s, I was part of a group of academics that founded *MIS Quarterly Executive* (*MISQE*), a journal dedicated to practice-oriented research, and served as its publisher for 15 years. SIM embraced *MISQE* and began distributing *MISQE* to all SIM members. The divorce between theory-based research and practice-oriented research was clear, with most academic journals eschewing all research except theory-based research—especially those journals that "count" for tenure and promotion. The "maturation" of IS as an academic discipline was now complete with theory-based research dominating the "A" journals.

But unfortunately, this "maturation" did not stop where our business school colleagues stopped. Over the past decade, I have received many comments from reviewers and editors suggesting that this focus on theory has become an obsession. Contribution and discovery are specifically seen as the production and testing of Type 4 theory, not the other types of theory.

3 The Problem of Theory Obsession

In the 1980s, the focus was on understanding new technology-enabled phenomena and good research produced a contribution to *knowledge*, not a contribution to *theory*. Research was phenomenon-focused, and the goal was understanding the who, what, why, where, when, and how of the phenomenon, and then figuring out how to improve the technology or the way technology was used. Other disciplines call this "problem-based research" (e.g., Van de Ven, 2007). *MISQ* and other top journals primarily published phenomenon-focused research that used Type 1 theory to present frameworks for describing a phenomenon, Type 2 theory that described what happened in case studies, Type 3 theory that presented empirical results, and Type 5 theory that described how artifacts were designed and used, as well as Type 4 theory.

By focusing on new phenomena, IS research was cutting edge and innovative. IS research regularly led practice by presenting the first understandings of new phenomena such as decision support systems (Sprague, 1980), group support systems (Dennis et al., 1988), end-user satisfaction (Doll & Torkzadeh, 1988), IS as a competitive weapon (Johnston & Vitale, 1988), and so on.

Today, however, IS research seldom leads innovation; instead, we study industry innovations. As one of my more perceptive colleagues noted before he left the research world, IS research in top journals has shifted to doing autopsies on technologies that have been dead a few years.

By focusing on Type 4 theory, we have slowly but surely lost our focus on new phenomena, because most research on new phenomena does not start by producing Type 4 theory (Gregor, 2006). Research starts with a gradual understanding that advances, step by step, though conceptual frameworks, case studies, empirical results, and new artifacts. Type 4 theory

requires a fuller and more complete understanding of a phenomenon than do other types of theory. We seem to be afraid of publishing innovative articles that do not reflect the complete understanding of phenomena required for Type 4 theory. By focusing on Type 4 theory, research published in top IS journals has lost its focus on innovation and new phenomena that was the hallmark of our early days.

As a discipline, we get what we reward. As the top journals have shifted to focus on Type 4 theory, so too have our junior and midcareer scholars, who need articles in top journals for tenure and promotion. They have shifted their research away from cutting edge topics to more mature areas where Type 4 theory contributions can be found. These scholars then become the next generation of reviewers and editors who instill this focus more strongly. The discipline moves slowly but surely to a more extreme and narrow focus. And the discipline loses its way.

I am a product of my generation. I grew up in a discipline that valued research on new phenomena, so this is the type of research I was taught to value and the research I continue to do, even as the discipline has shifted around me. Much of my research still focuses on new phenomena that do not lend themselves to Type 4 theory. One indication of this is the review comments that I often receive. Reviewers often complain that I have not used a single "overarching theory" to guide the research or that my research integrates two or more theories rather than using one theory. This is because no "overarching theory" exists for the phenomenon. Likewise, reviewers sometimes complain that I am just testing an existing theory from psychology or management in a new environment (because in their worldviews, research is theory testing). The goal of phenomenon-focused or problem-based research is *not* theory testing; the goal is to understand a new phenomenon, and the existing theory is just a useful tool in this process.

As an aside, I'll note that if there is an "overarching theory," then there is a good chance the phenomenon is not new, and thus the research is more likely to be incremental, providing only minor contributions. I'll also note that I am not advancing the old debate between rigor and relevance, because my focus is on new phenomena that may or may not have immediate relevance to practice.

4 A New Direction

IS scholars, as well as management scholars and business school leaders, have come to realize that a narrow focus on theory-based research—some call it a fetish—is causing harm (Avison & Malaurent, 2014; Bisoux, 2018; Hambrick, 2007). There are increasing calls to move the pendulum back to the center so that academic research focusing on new phenomena,

especially those with societal impact, are valued to the same extent as theory-based research (Avison & Malaurent, 2014; Bisoux, 2018; Hambrick, 2007). Such a move will take time, likely decades, because the shift from our prior worldview took decades. Nonetheless, I am confident that at some point in the future, we in IS will follow our business school colleagues and recover from our obsession with Type 4 theory, perhaps reluctantly, since the generation of scholars then leading the discipline will have grown up in the current times.

But the question is, why wait? We can lead. I believe that it is now time for us as IS scholars to lead our business school colleagues back to a focus on innovative research on new phenomena that does not obsess over Type 4 theory. We no longer need to fear that our business school colleagues will think we are not an academic discipline because business school leaders themselves are calling on disciplines to make this change (Bisoux, 2018).

Change has to be led by our top journals, because journal articles are the coin of the realm when it comes to research value. We need to publish exploratory research on new phenomena, embracing the idea that other types of theory (i.e., Types 1, 2, 3, and 5) are just as valuable as Type 4 theory. To be clear, there is nothing wrong with Type 4 theory; it is just that a narrow focus on Type 4 theory to the exclusion of other theory types is what has driven us to this point.

Changing culture is hard because most top journals have hundreds of reviewers and editors independently assessing submissions and making publication recommendations and decisions. Effectively communicating a change in cultural values to such a wide audience is extremely difficult. In the short run, the most effective solution may be to create separate sections in top journals that focus on new phenomena, using reviewers and editors that have been hand-picked and educated on the new cultural values.

Conferences are another place where new and innovative ideas are typically found. A special section in top journals that routinely fast-tracks the most innovative conference papers to examine new phenomena with minimal consideration of Type 4 theory may be another option for jump-starting the recovery.

I believe that much of the historical drive that has led us to focus on Type 4 theory is rooted in fear—fear that our colleagues in other disciplines will not see us as a valid academic discipline, fear that our IS colleagues will not value our journal, or fear that someone will think that I as an editor accepted a "poor" article. However, as Straub (2008) points out, failing to publish a good article hurts a journal and the discipline more than publishing a poor article, because readers will ignore a poor article, but the discipline will miss

the knowledge that could have been gained from the good article.

We need abandon our fear of publishing papers that don't fit the Type 4 archetype and shed our unhealthy obsession with Type 4 theory. We need to return to our roots where the research in top journals published scientific discoveries and contributions to knowledge that focused on innovative new phenomena so we can once again lead practitioners by applying deep academic insight into new problems and opportunities that are not immediately amenable to Type 4 theory, such as climate change, fake news on social media, artificial and augmented intelligence, virtual and augmented reality, and so on. Please join us in refocusing IS on understanding new technology-enabled phenomena.

Acknowledgments

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