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**DIALOGUE ARTICLE**

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# Scholarly Commentaries on Hirschheim's "Against Theory"

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## Abstract

This paper presents seven scholarly commentaries on Hirschheim's "Against Theory" essay published in this issue of the *Journal of the Association for Information Systems*. Each commentary is written by a renowned IS researcher. Following the individual commentaries is Hirschheim's response to the commentaries. Each commentary provides an insightful exegesis on theory in its own right and, collectively, the commentaries and response provide thought-provoking reflections for researchers in IS and beyond.

**Keywords:** Theory, Method, IS Practice

# Against, and For Theory: Provocations

**Dirk S. Hovorka**

*The only principle that does not inhibit progress is: anything goes.*
*– Paul Feyerabend, 2001*

Reflection on a field's own beliefs and practices is a defining characteristic which separates scientific inquiry from dogma or opinion. As digital phenomena intensify in scale and scope, as fundamental technologies evolve, and as information systems become increasingly intertwined through all aspects of modern life, reflections on how the field of information systems can progress are warranted and welcome.

In this provocative paper, Professor Rudy Hirschheim has tasked himself with analyzing the trajectory of the IS field and establishing the ground for a lively discussion within the IS field. The depth and breadth of Professor Hirschheim's historical perspective put him in a unique position for observing the state of the IS field at this point in time. He has many distinguished achievements, is a historian of the IS field, and has twice been awarded the JAIS Best Paper of the Year: First, for "A Glorious and Not-So-Short History of the Information Systems Field" (2012) and again, for his co-authored articulation of history as a research method (Porra, Hirschheim, & Parks, 2014). In his selection of Paul Feyerabend's critique of scientific practice as a focal point for this provocation, Professor Hirschheim opens a historical analysis of how academic fields are placed and evolve, and echoes Feyerabend's own provocation: *How do we want science to work*?

Professor Hirschheim is well aware that as an iconoclast, Paul Feyerabend himself carries a notoriety that is bound to spark strong reactions and discussion. Feyerabend held multiple academic positions, lectured globally, and was a prolific writer. As a scholar whose career was intertwined with the great scientific debates of the mid-twentieth century, Feyberabend's antagonists and foils included Karl Popper, Thomas Kuhn, and Imre Lakotos. His notoriety was based on his widely criticized *Against Method* and other arguments against the perceived unity and methodism of scientific practices. His critique of the primacy of method in science turned Kuhn's own concept of normal science into a problematic suppression of discovery. He argued that the emphasis on methods in periods of normal science encourages scientists to develop special-purpose adaptations of theory to concretize the known (Kuhn 1963). Feyerabend suggests instead that science progresses through the proliferation of new ideas which challenge the received view and may include "even the most outlandish product of the human brain" (Feyerabend, 1970). He characterized science as a struggle of alternatives and characterized mature science as one which "unites two very different traditions…the tradition of pluralistic philosophical criticism and a more practical tradition which explores the potentialities of a given material (or a theory of a piece of matter) without being deterred by the difficulties that might arise and without regard to alternative ways of thinking and acting" (Feyerabend, 1970). In interpreting his seemingly radical positions, it is important to retain a clear view of Feyerabend's project—the challenge to the orthodoxies of scientific practice *at that historical time*. He was not literally suggesting "anything goes" but rather sought to relax what he perceived to be the straitjacket of method and theory upon inquiry and scientific progress.

Professor Hirschheim places his critique at the present moment in (historical) time and reflects on the comparison of the trajectory of the IS field to that of operations research—a trajectory he does not view as favorable to progress or to the core phenomena of IS. After setting the historical landscape of the IS field, he follows the political implications for research practice imposed by the placement of IS in business schools and the subsequent demand for theoretical rigor. He argues forcefully that this historically grounded distancing from both the applied considerations of information systems and the practice community that could benefit from academic inquiry poses a significant risk to the viability of the IS field. But in concluding that all is not lost, Professor Hirschheim offers actionable changes through which researchers and the field at large can reconnect to relevance and the challenges of societal and organizational implications of new technologies.

That Professor Hirschheim intended a provocation is evident in his title "Against Theory…" and seven distinguished IS scholars have responded to his critical reflection on what many consider the premier accomplishment in IS—the kingship of theory (Avison and Malaurent 2014; Gregor 2014; Lee 2014; Straub 2009). These commentaries deserve a close reading as they take the reader through nuanced positions on how IS research communities can broaden their commitments on what contributes to disciplinary progress and what constitutes theory. Arguments are made that IS phenomena are now of concern to a wider group of stakeholders than are historically included. In addition, the field can learn from practice to increase intermediate and long-term knowledge outcomes by identifying overlapping areas of interest (ecotones) and by creating and disseminating understanding through action principles. Like Feryerabend's own work, the essay also elicits emotional responses that point out seeming contradictions in his argument. These responses present an appeal to step back and view Professor Hirshheim's challenge as a matter of concern to the field as a whole. The careful thought regarding the role of theory, the dangers of methodism, and the IS field's connections to practice initiate a valuable reconsideration of the values and goals of our own research and publication practice. While the responding essays take umbrage with details in Professor Hirschheim's argument, there is broad recognition, to paraphrase Shakespeare's *Hamlet*, that "Something is Rotten in the State of IS." These scholars see different underpinnings than Professor Hirschheim's for the field's existential angst, but each offers constructive actions that deserve careful consideration by the IS community.

Professor Hirschheim succeeds in engaging these scholars in a robust and sometimes emotional debate on the health of the IS field. I suggest that, whether you agree or not with the argument, such reflection is necessary for a field intent on studying rapidly changing yet durable world(s). Our methods, our theories, and our communities can become ossified and self-referential if we are not capable of loosening our grasp upon them (Holmström & Truex, 2011) and maintaining our imagination and orientation to the future(s). When academics speak only to each other and then only in abstract formalisms and esoteric jargon, it is little wonder that companies, policy makers, and individuals are unable to see the relevance of academic research. At the same time, our focus on corporate stakeholders, on economically oriented business goals, and on discrete, bounded "information systems," narrows our vision and our impact. The potential and perils of digitization certainly have implications for organizations. But digital phenomena are increasingly manifest in individuals' lived experience; in politics, humanities, medicine, and society at large; and in the way we perceive the environment. The emerging scale of digital phenomena and new socio-politico-ethico- technical configurations and processes are difficult to grasp using our current theories, concepts, and arguments. We can see renewed salience in Langdon Winner's warning that:

> *What we lack is our bearings.... Many of our standard conceptions of technology reveal a disorientation that borders on dissociation from reality. And as long as we lack the ability to make our situation intelligible, all of the "data" in the world will make no difference. (Winner 1978 p 7).*

By invoking Feyerabend's notoriety among the philosophers of science of his time, Professor Hirschheim challenges scholars of our time. In each of the six responding essays, exemplary IS scholars have taken a step back to gain perspective and reflect on our own practices outside the hurly-burly of publishing to ask: *How do we want our research to progress*?

# Against Theoretical Constraint: A Commentary on Hirschheim's "Against Theory—With Apologies to Feyerabend"

**By Frantz Rowe and M. Lynne Markus**

In "Against Theory," Rudy Hirschheim looked to Feyerabend's "Beyond Method" to ground his analysis of, and prescriptions for, the ills that face the IS field. Hirschheim asserts that we IS scholars have drifted away from our practice-oriented base, and that a fetish with theory is what has got us here. Hirschheim fears that the IS field will follow in the ruinous footsteps of operations research, another field he claims has become irrelevant to practice. Among the remedies that Hirschheim proposes is a return to engagement with practice that will foster "understanding," in contrast to theory, of the sort that practitioners use to solve real-world problems.

In this commentary, we start by stipulating that the IS field has indeed moved away from its practice-oriented roots, largely in the way that Hirschheim lays out: In response to criticisms from within US business schools and from academia more generally, IS scholars sought to increase legitimacy for their research by emulating the research practices of more established fields. We also agree with Hirschheim that, like other management fields, the IS field finds the concept of theory perplexing and devotes a fair number of journal pages to working through thorny questions like "What is theory?" and "What is a theoretical contribution?"

There, however, we depart from Hirschheim. We argue that it is not a fetish with theory that got us to this pass, but an overemphasis on method, and that it was overemphasis on method that caused the field of operations research to lose practical relevance. Second, we claim that the IS field's problems with theory are not that we fetishize it but rather that we do not sufficiently problematize the definition of theory that Hirschheim takes for granted. Third, we contend that Hirschheim takes the wrong lessons from Feyerabend. The solution to our distance from practice is not to try to acquire practitioner understanding, but rather to diversify our understanding of scientific theory, just as Feyerabend sought to diversify our understanding of scientific method. Finally, we suggest that framing our scholarly work explicitly within a broader description of a phenomenon or problem may go a long way toward helping practitioners appreciate our theoretical and empirical contributions.

## Feyerabend on Method: Many Things Go

Hirschheim chose philosopher of science Feyerabend to stage his essay against academic theory, because Feyerabend is (in)famous for his "anarchic" attack on scientific method. Feyerabend created—and possibly even courted—controversy in his debates with Karl Popper and other prominent philosophers over the meaning and place of method in science (Treiblmaier, 2018; Myers, 2018). Viewing his mentor's (Popper's) view of the scientific method as narrow-minded, Feyerabend discussed the methodological principles underlying pseudosciences like astrology and religious practices such as voodoo. His claim that "anything goes" in scientific research earned him the unflattering epithet "'the worst enemy of science' in the prestigious scientific magazine *Nature*" (Treiblmaier, 2018, p. 93).

Despite his extreme written contributions, Feyerabend's private views may have been more moderate. Interestingly, the title of the German translation of his text could be rendered as "Against Methodological Constraint" (hence the title of our commentary; Treiblmaier, 2018). Furthermore, he later claimed that he did not personally hold the view that "anything goes," stating that it was a position wrongly attributed to him by people with a strongly rationalist view of science (like Popper) (Treiblmaier, 2018). A better label for the implications of Feyerabend's arguments might be "disciplined methodological pluralism" (Myers, 2018).

We emphasize these points, because we believe that Hirschheim learned the wrong lesson from Feyerabend: Instead of being against theory, Hirschheim should be against "theoretical constraint" in the sense of the stifling definition of theory as a "relationship of variables" that he uncritically accepts.

## Method, not Theory, Is the IS Field's Fetish—As it Is for Operations Research

Theory may be a fetish, as Hirschheim claims, in some management fields, but it is not a fetish in IS. Instead our fetish is *method*, as it is for operations research.

Hirschheim cites Hambrick (2007), writing for the field of management, in support of his argument that the IS overemphasis on theory is leading us away from "understanding" and "rich detail about interesting phenomena" (Hambrick, 2007, p. 1348). We, too, have commented on Hambrick's paper (Rowe, 2011), only to point out how different the field of IS is from strategic management (Hambrick's specialty). IS scholars produce lots of empirical work (qualitative, as well as quantitative), but our *theorizing* about our interesting subject matter is limited and often drawn from other fields with little modification. In fact, what our field needs is *more* and better theorizing, not less, including "pure theory" papers that do not require the inclusion of new empirical data generated by rigorous scientific methods for publication (Grover & Lyytinen, 2015; Rowe, 2011, 2012).

Despite the need, pure theory development papers remain rare in the IS literature. Many special issues on methods have been published in the AIS Senior Scholars' Basket of Eight IS journals, but none soliciting pure theory development papers. Indeed, the 2018 *MISQ* special issue call for papers on "next-generation information systems theories" includes papers that theorize on the basis of empirical data! In our view, this downplays the value of theoretical papers whose quality depends on relevance and rational consistency, rather than on methodological rigor. We do not mean to say that empirically based theorizing is inferior to pure theory development. However, empirical theorizing is limited to *what can be observed*, and its strength then comes from the methodology, not from an intellectual speculation based on deductive thinking, intuition, or imagination (Rowe, 2018). By contrast, research grounded in critical realism (Mingers, 2004), social mechanisms (Avgerou, 2013), or causal mechanisms more broadly (Markus & Rowe, 2018) could enable pure theorizing about phenomenon that cannot easily be empirically observed.

It is true that pure theory development papers are difficult to write (Leidner, 2018). But the deeper problem may be our persistent insecurity in our academic legitimacy (described by Hirschheim), which we salve through an overemphasis on empirical methods and data. In our experience, methodological rigor is a prerequisite for publication in IS journals. Increasingly, we observe, editors are willing to jettison strong theory for papers that have good empirical contributions and potential theoretical implications (Agerfalk, 2013; Majchrzak et al., 2016). But we have seen no similar looseness over method.

If we have a fetish in IS, it is with method, not theory. And it was method, not theory, that diminished the glory of operations research (Otondo, forthcoming). Hirschheim makes this point at several points in his paper. His lengthy quote from Ackoff (1979) describes how OR became identified with mathematical models and algorithms rather than practical relevance. And in his Footnote #13, Hirschheim states explicitly that the fall of operations research had little to do with theory and much to do with its attention to rigor of method. (Ironically, it is an intense focus on method that has enabled operations research to return to prominence today, when practitioners have become enamored of data analytics!) Nevertheless, Hirschheim is disturbed by the ominous parallels he sees between operations research and our field. This is all the more reason to diagnose carefully what distances our field from practice and what can best be done to narrow this gap. In any case, overemphasis on theory is *not* to blame, and prescriptions based on the misdiagnosis that theory is at fault are sure to fail.

## The IS Problem Is Not Overemphasis on Theory, but a Narrow Understanding of Theory

In our view, it is not an *overemphasis on theory* but a *narrow definition of theory* that is responsible for distancing our field from practice. Hirschheim asserts, and we agree, that "the general consensus" and "the only type of theory that is acceptable (for scholarly journals) is one that views "theory [as] consisting of one or more functional statements or propositions that treat the relationship of variables so as to account for a phenomenon or set of phenomena" (Hollander, 1967, qtd. by Hirschheim). Hirschheim notes that this is a view of theory that reflects a positivist epistemology, but he doesn't rail against this limited view of theory. Instead, he asserts that the solution is to "stop focusing on 'theory' and focus instead on 'understanding.'"

We might agree with this prescription if Hirschheim meant embracing a broader view of theory that would include hermeneutic understanding, along with positivist and realist views (Markus and Rowe, 2018). But that is not what Hirschheim calls for, although he does paraphrase Wittgenstein (1953), stating that to have an understanding means to "be able to do things with regard to the phenomenon—to perform it, it comment on it, to answer questions about it." He also quotes Sandelands (1990) to bolster his argument that practitioner knowledge is not the type of understanding that can be conveyed by academic theory. Hirschheim believes that we need to understand and act the way practitioners do.

We agree that practitioners are concerned with changing practice, and that they do not need academic theory to help them do that (although we like to think that the right kinds of academic theory can help them. In fact, our conceptual frameworks may be what practitioners most value about academic research! [Lyytinen et al., 2018]). But learning to think and act like practitioners is definitely *not* what we should do, if we are to fulfill our role as scholars, while at the same time improving our relevance to practice!

It is very important to recognize that practitioner understanding, the kind of thinking that enables them to act, is very different from the kind of theorizing that we could do that would provide genuine support for practitioner understanding and action. Consider, for example, Lindblom's (1959) classic description of the ways that practitioners approach solving their problems. Instead of

> *tak[ing] advantage of any theory available that generalizes about classes of policies…the [practitioner] would set as his principal objective [a] relatively simple goal…. As a second step, he would outline those relatively few policy alternatives available to him. … In comparing his limited number of alternatives. … he would not ordinarily find a body of theory precise enough to carry him through a comparison of their respective consequences. Instead he would rely heavily on the record of past experience with small policy steps to predict the consequences of similar steps extending into the future.* (Lindblom, 1959, p. 79)

This is how practitioners understand, and this is how they get things done. But just because *they* think like this doesn't not mean that *we IS scholars* should (or even could) do so. We will not help practitioners by attempting to replicate the deep tacit understandings of the worlds they inhabit. But, we believe, we can (and sometimes do!) help practitioners by theorizing their experience in diverse ways, thereby providing them with alternative perspectives that they may then be able to incorporate into their successive and incremental "science of muddling through" (Lindblom, 1959).

## One Solution Is Diversity of Theory, Including Hermeneutic, But Not Practitioner, Understanding

The solution to the IS problem of distance from practice is not to jettison theory, nor is it to attempt to replicate practitioner understanding, as Hirschheim proposes. Instead, we argue, one solution would be to diversify and improve our theorizing about IS phenomena in ways that practitioners may find useful. In addition to theorizing as proposing relationships among variables, we can theorize by providing rich descriptions and hermeneutic understanding of practitioners' worldviews, and we can theorize by offering purely theoretical speculation about the unobservable mechanisms responsible for outcomes (Markus & Rowe, 2018). We can theorize by developing models of practitioners' problems and by articulating the logic of how IT-involved solutions work, when they do work (see Markus's comments in Galletta et al., 2019; cf. Markus, 2014). There are many ways to theorize, and what distances us from practice is our constraining preference for a narrow, positivist, understanding of what theory is. The solution is not less theory, but more and better theories about the problems of practice and IT's role in creating and solving them.

## Another Solution Is Framing Our Theorizing Within a Broader Problem Space

Another solution, we believe, is to articulate clearly in our writings how our efforts at theorizing and researching practical problems fit into the larger picture. It is unavoidable that careful scholarly work will tackle only a narrow slice of a phenomenon, but that is no excuse for presenting an article as the last, or even only, word on the subject. A study on cybersecurity might naturally focus on employee noncompliance with an organization's security policies, because evidence suggests this is a common source of hacks. But a comprehensive understanding of the problem would also require attention to the quality of the organization's policies and technological controls, as well as quality of enforcement. Similarly, however important task-technology fit might be to technology acceptance, it is only a small piece of the larger issue of organizational technology assimilation (Fichman, 2000, p. 111). This is not to say that that every research project or article should try to cover an entire domain or problem. Indeed, editorial emphases, space limitations, and other practical constraints would doom attempts at comprehensiveness to rejection or frustration (Rowe & Markus, 2018). However, it hardly takes more than a good paragraph at the outset of a paper to explain, for example, that employee noncompliance is only part of the cybersecurity problem and that fixing employee compliance alone cannot ensure cybersecurity. Framing our theorizing and research contributions in terms of a larger phenomenon or problem space can facilitate dialog with practitioners and promote additional research on neglected parts of the problem.

## Conclusion

Rudy Hirschheim is correct, we believe, in once again highlighting our field's growing distance from practice. And he is on sure footing by emulating Feyerabend's iconoclastic approach to exposing some of the narrow-minded views in our field. But his definition of the problem and his proposed solution are off target. He has not drawn the right lessons for theory from Feyerabend's "disciplined methodological pluralism" (Myers, 2018). The problem is not overemphasis on theory, but a narrow-minded definition of theory. The solution is not to replace theory with practitioner understanding, it is disciplined *theoretical* pluralism within a sufficiently broad and relevant problem space. The solution is not to reject theory, but to reject *theoretical constraint*! (With apologies to Feyerabend.)

# Commentary on "Against Theory: With Apologies to Feyerabend"

Sirkka L. Jarvenpaa

In the article titled "Against Theory: With Apologies to Feyerabend," Hirschheim (2019) paints theory as a culprit for the unsatisfactory state of academic research in the information systems field. According to Hirschheim, theory has taken the field on a journey that rarely produces insights that practitioners can use to solve their problems. As a discipline that is commonly housed in professional schools of business, engineering, or information, Hirschheim (2019) argues that research in information systems needs to produce knowledge that is incorporated into practice. Without contribution to practice, the field's long-term academic existence can become questioned. According to Hirschheim (2019), "'theory worship'... has become dysfunctional and is leading the discipline down a dangerous path toward irrelevance." In conclusion, Hirschheim (2019) states that "what I am against is the mindless obedience of making theory the only thing that matters in our research."

Although I share many of the concerns expressed in the article, I disagree that the culprit is theory per se. Hanson (1958) reminds us that all observations are theory-laden, whether we are implicit or explicit about it. In my view, Bacharach (1989) got it right: "It [theory] is no more than a linguistic device used to organize a complex empirical world."

Rather than theory per se, I argue that the culprit is how we use theory to isolate the information systems field rather than bridge it with other academic fields as well as with practice. Because of the information system field's continued anxiety, theory is used for practices of "turning inward, inbreeding, and introverting." These are the exact same words that Hirschheim (2010) quotes from Ackoff's (1979) paper lamenting how scholars in the field of operations research have "veered off the path of helping practitioners" and become obsessed with their mathematical models and algorithms. I argue that some of the dysfunctions described by Hirschheim (2019) are taking place because theory is used for inward-facing practices that reclaim boundaries rather than span boundaries.

We hear calls for "native" theories in information systems. What renders something a native theory? It is difficult to come up with reasons why practice would care if the theories are native unless the word "native" relates to novel, underresearched, or poorly understood problems. At times I wonder if the search for indigenous, or native, theories is nothing but a jurisdictional shield to isolate the field and sharpen the field's identity from within. Or are native theories important for scholars in the field of information systems to gain bigger audiences and more powerful roles in large interdisciplinary collaborations? Do native theories increase opportunities to link our work more effectively with those from other fields and contribute to the accumulation of knowledge and insight more broadly?

The search for IT artifacts or for digital materiality without the deeper understanding of social dimensions can turn out to be similarly protective moves. One can only puzzle over what understanding is improved by differentiating technology issues from other issues in which they are embedded. The sociotechnical perspective is viewed as fundamental in the IS field (Sarker, Chatterjee, Xiao, & Elbanna, 2019). The term "socio" is in front of the "technical" for a reason. The technology design may have failed, but often not because of the technology per se, but because of the social processes and circumstances involved.

Theories are also used as a language barrier. At times theories advanced in IS are composed of esoteric and nonstandard language that is inaccessible to scholars even within the IS discipline, and even more so to scholars outside it. Particularly in the field of IS, a very open conception of theories is needed and, indeed, exemplified in many excellent published works (see, e.g., Gregor, 2006). The role of theory is not to narrow conversation but to broaden horizons and deepen our understanding of both the depth and breadth of problems. We ought to be celebrating all forms of theorizing, including radical theorizing (Nadkarni, Gruber, DeCelles, Connelly, & Baer, 2018), as long as they are accessible to broad audiences, including those beyond academics.

The preoccupation with the past might be also contributing to the inward focus. If our theories were more future focused, they might be more useful in practice. I have recommended to colleagues and students an article by Alvesson and Sandberg (2011), which encourages problematization in framing research questions. However, this problematization does not necessarily help with newly emergent problems or future problems. Identifying the assumptions in the extant literature, articulating them and challenging them, can limit the view, even when such literatures go beyond a particular paradigm or work to search for commonalities in assumptions at a field level. Our understanding of problems should not start or end with existing academic or even practitioner literatures. Ronald Coase (1937) noted, "I made it all up myself." Only after formulating his basic ideas did he examine the prevailing literature on the topic. Engagement with the literature or with any one particular stakeholder group should be merely a stepping stone—not a rope from which we fasten a noose to kill the relevance of our research or our work in a dramatically changing world. Problem formulation requires broad engagement with varied stakeholders. It might also require developing the scenarios of the future. To create movement and influence with our research, perhaps the wisdom attributed to legendary hockey player Wayne Gretzky should be internalized: "It's not as important to know where the puck is now as to know where it will be."

One protectionist strategy is to discourage PhD students from taking on internships with industry during doctoral studies. It is viewed that somehow spending time with industry steers them to industry jobs or corrupts them with industry problems that are difficult to package as academic research. During my PhD studies, I completed an internship with one of the leading strategy consulting firms and this experience redefined my research as well as my teaching. Without that experience, I would not have received the teaching opportunities and had the confidence to venture out to emerging topics. Such internship opportunities will not only help students communicate their research to practice audiences but may also redefine their research and teaching.

I conclude by advancing a call for our increased engagement, not just with business practitioners but also with a broader set of fields and stakeholders. This recommendation is synergistic with the recommendations of Hirschheim (2019). Given that IT has penetrated all facets of society, the urgent need for us is to embrace a broader set of stakeholders as we seek to increase understanding through our research. Limiting our research to "business interests" is looking at the rearview mirror. Fortunately, Hirschheim (2019) looks beyond business enterprises and also brings up the importance of policy. Having a section on policy in our journals is a good stepping stone. Yet, just as "quality" should be a concern not only for the quality manager but for everyone in an organization, so should every article strive to speak to policy. Hirschheim (2019) cites King and Kraemer (2019), who write that "[policy] pertains to any systems of principles guiding decisions toward desired outcomes." Through principles, guidelines, and standards, practice can be influenced by research and research can be influenced by practice. Conducting research that influences such guidelines, however, is a major exercise of political astuteness and power. The exercise of political astuteness and power come to those who focus outward, rather than to those who build disciplinary walls and rely on inbreeding and introversion. Real societal impact requires convergence of many different disciplines and fields. Theory can be a useful tool to link conversations and span fields to integrate knowledge for important and compelling contemporary and future problems.

# Theory Commentary

By E. Burton Swanson

## Introduction

Following the example of Feyerabend's (1975) warning about scientists' preoccupation with methods, our colleague Rudy Hirschheim (2019) calls attention to information systems researchers' current absorption with theory and expresses similar alarm. Aiming to spark debate, Hirschheim worries that we as IS scholars have become subject to a kind of theory worship in our pursuits that likely puts us on a path to irrelevancy. He speaks therefore "against theory." His essay merits our attention. With others, I join in the discussion. I first place Hirschheim's worry in the broader context of other worries in IS academics. I then consider his worry of how IS practice learns or not from IS research, after which I turn the coin over and consider instead the worry of how IS research learns from IS practice. Having briefly examined these related worries, I conclude by offering a few kind words for theory.

## Worries in IS Academics

Having long been associated with the Senior Scholars of the Association for Information Systems and having attended many of its annual meetings at the International Conference on Information Systems, where we as elders discuss institutional matters of broad concern, such as recognition given to leading journals and published research articles, I have become very familiar with worries in IS academics that seem to pervade our ever-changing field. Many of these worries are reflective of the history of the field's development, as recounted by Hirschheim, and especially of the IS trials and tribulations in US business schools, which continue today, even as the field has become well established. Ongoing concerns over IS acceptance in academia, both narrowly within business schools and more broadly in research universities such as my own, have often seemed to me to motivate our frequently expressed concerns about IS research acceptance in practice. Especially in a professional school context, gaining such acceptance in practice has come to be embraced by many IS scholars as the obvious and principled means to secure our academic future.

## The Worry of How IS Practice Learns from IS Research

Hirschheim begins his essay by contending that we as IS researchers have somehow become unmoored from professional practice, seemingly pursuing theory for its own sake. He argues that knowledge useful to the practitioner and to practice itself has largely disappeared from the research cycle. He worries about how theory translates or not into practice, whether current IS theories have practical import, and whether IS scholars are accepted by practitioners. He briefly reviews the origins of the IS discipline in the context of the history of US business schools and faults the pursuit of theory in the attempt to achieve more scholarly acceptance in the wider university, claiming that it came at the cost of failing to provide valuable knowledge for practitioners. He suggests that we as researchers should focus not on theory and explanation, but on achieving useful practical understandings. After warning us not to lose our way as did operations research (according to some), he offers several recommendations for a course correction, and concludes by challenging the IS research community to take these recommendations up and involve itself more deeply in bringing about needed change.

There is much in Hirschheim's essay to agree with here. It's easy to concur that most practitioners have little interest in theory as such and that we have often taken theory overseriously in our research, especially in tying ourselves in knots over its presence or absence in publication submissions. Giving more weight to broader understanding, as contrasted with narrow formalized theory, seems like a good thing, to the extent it frees us up from our own dogma, although I would not let practitioners be the principal arbiters of usefulness. The constructive suggestion that we move toward more direct engagement with societal issues is particularly timely and important, as IS and ICT increasingly saturate most human practices.

But unlike Hirschheim, I confess I am not myself so worried about how IS practice learns from IS research, especially from its scholarly publications, the primary vehicles in which we theorize and communicate our investigative findings, first of all to ourselves. Apart from these publications, there are a variety of good ways that practitioners can learn from our research, not least through direct collaboration with it. Swanson (2014) describes rich pathways for such learning, and most of these do not entail the task of translating our research findings into practical understandings.

## The Worry of How IS Research Learns from IS Practice

A more interesting worry to me is how IS research learns from IS practice. For if we are to have anything to report of interest in our research publications, it should presumably be anchored in practice, the very focus of our studies.

It is important to consider what we are trying to accomplish with our IS research, as Constantinides, Chiasson, & Introna (2012) remind us. Every research undertaking reflects normative choices and value judgments concerning the ends of our efforts, which can be positioned in terms of the "highest good" that the community of inquiry seeks to achieve.

My own research mentor, C. West Churchman, sometimes contrasted the choices made by inquirers and deciders in terms of "immediate man" who acts in the here and now versus "historical man" who acts by taking the longer view. While most IS practitioners necessarily engage themselves in the here and now, most IS researchers have the luxury of taking the longer view, to the extent they wish to take it, even as they may be under pressure to deliver actionable findings to practitioners (not to mention gain tenure in their schools). My own worry of how IS research learns from IS practice centers on the extent to which we focus on today's pressing practitioner problems in the presence or absence of a longer view that informs the "highest goods" worthy of our efforts and best motivates our undertakings and gives weight to our purported research findings.

For IS researchers, the problem in taking the shorter view is exacerbated by the rapidly changing technology that is our focus. It is further compounded in a professional school context, in that knowledge gained by researchers is expected to serve the public interest, even in a business school. The very foundation of professional schools in a university presumes the preparation of practitioners who will commit themselves through their specialized knowledge gained to acting in society's best interests (Pelikan, 1992). Yet the ethos of the business school often conflates the pursuit of private profits in an idealized free economy with serving the broader public interest, weakening the professional commitment of its graduates, and increasing pressures to do research that speaks to shorter-term needs of firms rather than to longer-term needs of society.

How, then, should IS research learn from IS practice, while taking the longer view? In an earlier essay, Ramiller, Swanson, & Wang (2008) provide a simple framing with which to answer this question, presenting an institutional view of overlapping discourses among the IS researcher and practitioner communities, as shown in Figure 1. Here the overlap of discourse constitutes an "ecotone," or transitional zone of mutual discursive interaction, supportive of exoteric research that informs both communities as distinct from nonoverlapping researcher discourse, allowing for esoteric research that informs primarily the research community. From this framing, it should be clear that cultivating and expanding the discourse ecotone and engaging in exoteric research that speaks to both communities is a primary means for IS research to learn from IS practice and vice versa. What might be less clear is that, admittedly, esoteric IS research is every much as needed for the research community as a whole to learn and thrive, not only in its own interest, but in the interest of IS practice, taking the longer view. For in its absence, IS research goes silent in its unique space and gives up its own professional claim to specialized knowledge about how best to learn in support of IS practice. It yields management of the larger discourse stage entirely to practice and weakens its own authority in the academic education of future professionals. In doing so, it also weakens its own standing in the larger research university, where bridges in discourses are desirably built across disciplines and fields of knowledge, not only to practice.

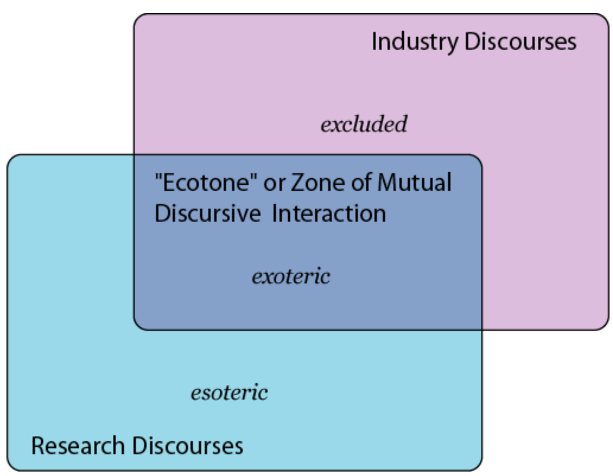


**Figure 1. Discourses in Research and Practice. Adapted from Ramiller et al (2008).**

How does esoteric IS research with its theoretical leanings learn from IS practice at all, given that it locates itself primarily outside the discourse in the ecotone? Most obviously, IS researchers of an esoteric bent can spread the risk associated with their ignorance by spending at least some portion of their time in the ecotone. They can mix it up with their exoteric research colleagues and IS practitioners and attempt to communicate findings from their own research as best they can. They can listen a lot and ask questions. They can struggle to explain the significance of their work for practice, not only through hand-waving in articles which will be read primarily by academics (see Ramiller & Pentland, 2009), but in conversations with practitioners, facilitated by the field's porous boundaries, both within academia and between academia and practice. But, as is currently often the case, IS researchers given to theorizing can also gain the indulgence of practitioners and locate themselves as careful learners in practice itself. That they will subsequently report their esoteric findings primarily to other researchers through scholarly publications does not necessarily mean that practice will not ultimately benefit, as long as the IS academic community keeps an appropriately skeptical eye on what is being learned collectively over the longer haul (as reflected in Hiirschheim's present challenge to us here).

On balance, theorizing is the friend of taking the longer view of things in our research, or at least it should be. Here there is much for us to discuss, as we have been doing for some time, as Hirschheim recounts (see his references). With all of this as backdrop, I offer a few kind words for IS theory and attempt to put it in its rightful place, or at least where I prefer to see it.

## A Few Kind Words for Theory

My own view of theory is rather a romantic one. It is a broad notion that a scholar can fall in love with explaining and gaining understanding of how one thing leads to another in the world in which we live. It often has an ephemeral quality that is difficult to get one's arms around, as it were. But it most definitely attracts. One wants to spend time with it, lots of time, as long as the romance lasts.

Just to be clear, what theory is not is a formal causal model. While such a model may be informed by theory and can be built and examined in a particular study, it yields, at best, fragmentary insight in need of narrative accompaniment. Research employing such models amounts to no more than interpretative ("qualitative") research by another ("quantitative") name. Which is not to demean it, but rather to place equivalent demands on it, notwithstanding whatever claims are made about the rigor of the study. Currently, many of our IS research efforts seem devoted to causal modeling (Gregor, 2006, positions this work in theorizing more broadly).

Spending some time with a theory is one of the joys of academic pursuits for those so inclined and should be. In my own case, I have spent considerable time theorizing around the esoteric concept of organizing visions (Swanson & Ramiller, 1997), and have tried to communicate much of what I and others have learned through multiple studies in an article directed to practitioners (Swanson, 2012) as well as in executive education and related venues. Most recently, I have been attracted to theorizing about information systems in the broader context of technology (Arthur, 2009; Swanson, 2017)—in particular, as informed by rather esoteric practice theory (Schatzki, 2002; Swanson, 2016). This is very much a romance in its early and uncertain stages. Wish me luck. But I offer no apologies for theorizing.

Practice theoretical studies offer a good example of rather esoteric scholarly work deeply committed to practice itself, in particular, in workplace settings (see Barley & Kunda, 2001). Nicolini (2009) describes what he calls a "package" of method and theory for engaging in organizational ethnography:

> *The package of theory and method requires first that we zoom in on the details of the accomplishment of a practice in a specific place to make sense of the local accomplishment of the practice and the other more or less distant activities. This is followed by and alternated with a zooming out movement through which we expand the scope of the observation following the trails of connections between practices and their products. The zooming in and out stops when we can provide a convincing and defensible account of both the practice and its effects on the dynamics of organizing, showing how that which is local (for example, the doctors' and nurses' conducts on one site) contributes to the generation of broader effects (for example, sustaining or upsetting the historical hierarchical relationship between the medical and nursing professions).*
>
> *I call this a "package" to emphasize that for studying practices one needs to employ an internally coherent approach where ontological assumptions (the basic assumption of how the world is) and methodological choices (how to study things so that a particular ontology materializes) work together. For example, studying practices through survey, or through interviews alone, is not acceptable for researchers. These methods are, in fact, unsuitable for studying work practices, as they are not faithful to the processual ontology that underpins an ethnography of practice research.* (Nicolini, 2009, pp. 120-121)

Note in this somewhat lengthy excerpt the argued necessity of theory for this research in conjunction with the longer social view taken, as well as the methodological dictates that follow. Consider that in the absence of theory more broadly in our research, we are hard put to specify methods at all. To be "against theory" suggests that we might as well be "against method" too. This leaves us not much to talk about in our academic space outside the ecotone.

## Conclusion

To sum up, I argue that the IS academic research community has an existential problem in which its exoteric and esoteric endeavors must be continually reexamined and reconciled in achieving the "highest good" sought by all. The danger is always that one of these two forms will come to assert itself and will largely drive out the other, to the detriment of the research enterprise as a whole. Hirschheim justifiably worries about the dysfunctional role that "theory worship" can play in favoring esoteric research over its exoteric partner. He argues therefore against theory, in the interest of better serving practice. Would he strangle esoterica altogether? Surely not, despite the provocation. In his conclusion, Hirschheim acknowledges that theory has "an important role to play" in our endeavors. With this in mind, here I have offered a few kind words for theory in the concern that to the extent we would abandon it, we risk impoverishing whatever research findings we think we have to offer practice in the longer run.

# From Theory Worship to Action Principles: A Commentary on Hirschheim's "Against Theory: With Apologies to Feyerabend"

By Mary Lacity

## Introduction

Thank you for the invitation to comment on Rudy Hirschheim's provocative essay. Much of his essay argues that information systems (IS) researchers should produce understanding that contributes to practice. He writes:

> *My plea, therefore, is that instead of focusing on what contributions one's research makes to theory, we should focus on the contributions one's research makes to understanding—What new insights does the research generate, in particular as they relate to changing or helping practice? Do the insights resonate with practitioners? How would these insights change the way practitioners see particular problems, particular solutions?*

Hirschheim makes four recommendations for the IS field. My commentary expounds on one particular recommendation—namely, his call for a return to engagement with practice. Hirschheim's essay briefly mentions my work with Leslie Willcocks on action principles as an example of understanding produced by practitioner-focused research, stating "For Willcocks and Lacity (2016), Understanding translates into 'Action Principles.'" In this commentary, I explain what action principles are, explain how they are coproduced with practitioners through the process of inquiry, demonstrate the immediate contribution action principles make to practice, and discuss how action principles can contribute to theory. It should be noted that I am not against theory, I am against theory worship. My account is purposefully "confessional" (Van Maanen, 1995) and self-reflective, and it is my hope that IS PhD students and assistant professors will find the ammunition and courage they need from Hirschheim's essay, accompanying commentaries, and a recent article by Wainwright, Oates, Edwards, & Childs (2018) to pursue action principles and other practitioner-focused research approaches.

## Action Principles

**Action Principles Are Practices That Explain the Results Found in Real-World Implementations.** An action principle can be expressed in the following form: *According to n participants in m contexts, action X produced result Y*. Action principles are cocreated with practitioners through the process of inquiry to articulate, understand, and provide meaning for associating actions with outcomes within a particular organizational context. They are lessons learned from practices enacted in the contexts studied: "Interpretive researchers tend to focus on meaning in context. They aim to understand the context of a phenomenon, since the context is what defines the situation and makes sense of it" (Michael Myers, 2013, p. 39).

Action principles are empirical findings expressed in a way that other managers can consider applying within their own organizations. However, action principles are not "laws," "prescriptions," or even "best practices." Whereas "best practices" imply that mimicry is always recommended and will always produce similar results, we do not assume that an action principle will be effective in every context. Rather, we offer them to practitioners for their consideration; a thoughtful practitioner decides the extent to which action *X* would likely produce result *Y* within his or her organizational context.

Examples of action principles from my recent research on enterprise adoptions of blockchain technologies include:

- According to 2 participants in 2 organizations, creating a cryptocurrency (i.e., an AltCoin) for internal use was an effective way to build blockchain awareness to a large number of employees.
- According to 12 participants in 7 organizations, participating in multiple blockchain consortia was an effective way to avoid technology lock-in.

Each action principle can be illustrated through the craft of organizational storytelling (Daft, 1983), often peppered with participant quotations. For example, quotations that support the latter action principle include:

> *At this stage in the game, we're not informed enough to pick a winner. There are lots of people vying for this strategic high ground, so I think it's important for us to engage in places and keep our fingers on the pulse of all of them rather than try and pick a winner at a way too early stage.* (Head of a blockchain CoE for a global financial services firm).

> *So, from a strategy point of view, it's early days. We're probably in the*

> *situation that all the other big financial institutions are at the moment. Nobody's really backing one horse. We're all trying to get to know as much about it as possible and see where it takes us. All we know is that it's going to be extremely disruptive.* (IT Consultant and Architect for a bank based in Africa).

A short accompanying narrative can be used to illustrate each action principle (see the Postscript below for an example).

Our approach to creating action principles emerged as a bricolage of personal research experiences and ideas from the work that inspired us. My personal heroes and heroines include John Searle (1995, 2010) for his ideas on the social construction of reality and institutional facts; Anthony Giddens (1984) for his ideas about the duality of human agency and societal structures; Gibson Burrell and Gareth Morgan's (1979) magnum opus on organisational paradigms beyond functionalism; Rudy Hirschheim's work on IS paradigms with Heinz Klein and Tim Goles (Hirschheim & Klein, 1989; Goles & Hirschheim, 2000); Richard Daft (1983) for his elegant essay on research as craft; Jeffrey Pfeffer (1981) for his views on stakeholders, power, and politics; Clayton Christensen (1997) for his deep insights on the practices of innovation; Gerald Susman and Roger Evered (1978) for arguing the scientific merits of action research. I greatly admire Allen Lee (1991) and Michael Myers (2013) for their support of qualitative research and multimethods in IS research; Thomas Davenport (1993, 2018); David Feeny (1998; Feeny & Willcocks, 1998); M. Lynne Markus (1983); and Jeanne Ross and Peter Weill (2002, 2004) for their numerous contributions to practice. (My co-author Leslie Willcocks also has his heroes and heroines that influenced our work and we certainly also influenced each other.)

As social scientists, we view practitioners as thoughtful agents capable of action based on free will, power, intelligence, emotion, creativity, and self-reflection, but who operate within the liberations and confines of their environments. Like Anthony Giddens (1984, p. 3), we believe practitioners are able to express reasons for their actions: "To be a human being is to be a purposive agent, who both has reasons for his or her activities and is able, if asked, to elaborate discursively upon those reasons."

Practitioners are capable of describing practices and their consequences, but they must be asked. Therefore, interviews are our most frequently used data collection method. Our craft as researchers is to help refine research participants' reflections and to find a common language to express our shared understanding of the associations they make between actions and results.

I share my own journey to encourage IS researchers entering the field to apply and succeed with an action principles approach. In 1987, I matriculated into the PhD program in business administration with a major in MIS at the University of Houston. I was uninspired by the "table-versus-graph" and other behavioral laboratory experiments that were considered state-of-the-art scholarship back then. Fortunately, Rudy Hirschheim joined the University of Houston in 1988. He had just come from Templeton College at Oxford University and brought with him a philosophical and qualitative research tradition that was new to many US IS programs. His PhD seminars provided the philosophical justification (and thus the courage) to contribute to practice. I wanted to study large IT outsourcing (ITO) contracts—in particular, the nearly billion-dollar deals that were occurring at the time. Why were these companies signing megadeals? How could a provider that needed to earn a profit margin deliver IT services that were better, faster, and cheaper than in-house service delivery given that the providers were obligated to use the same IT assets and were not allowed to fire anyone for a year? Why were so many disputes emerging and how were they being handled? These questions seemed vastly more interesting than whether to display a table or a graph on a user interface.

ITO could not be studied in the lab; ITO could not be studied with quantitative tools because there were only a handful of phenomena to study. I used interviews, case studies, and action research (I went to work as a consultant for TPI on the Enron-EDS ITO account) to investigate ITO. Rudy chaired my dissertation. He ran interference for the faculty member who demanded to know the theoretical contribution of such a study. I appropriated transaction cost economics (Williamson 1975; 1991) and the political view of organizational decision-making (Pfeffer, 1981), but I was really seeking an understanding of an emerging practitioner phenomenon.

We generated deep insights into ITO practice. Not only were we able to answer the research questions, we uncovered surprises (Daft, 1983) in the form of myths, metaphors, and realities. (We admired Morgan's [1986] use of metaphors). For example, we found that the internal IT Department was often able to achieve similar cost reduction results promised by ITO providers without contracting with them. We further documented the practices that reduced IT costs in the organizations we studied, such as data center consolidation, resource optimization, and charge-back implementation to curtail runaway user demand, as well as many more practices. At the time, we did not use the terms "action principles," but what we produced could be readily translated into the form: "According to n participants in *m* contexts, action *X* produced result *Y*." For example, our finding on cost savings achieved could be expressed as follows: "According to 12 participants in five organizations, the internal IS department was able to achieve better business results (i.e., lower costs) on their own, without relying on an ITO provider."

Practitioners began noticing our work. The British Computer Society invited me to present to a crowd of 300 in 1993. Leslie Willcocks, then a research fellow at Templeton College, presented next. Leslie, Guy Fitzgerald, and David Feeny were using similar qualitative methods to study British ITO deals. I spent 1994 working with Leslie and David at Templeton College, and our combined research papers (various author permutations of Feeny, Hirschheim, Lacity, and Willcocks) were published in the *Harvard Business Review*, *Sloan Management Review*, and scholarly books aimed at practice as prescribed in Hirschheim's essay.

Following our ITO research, we used the same approach to study offshore outsourcing, prison sourcing, impact sourcing, rural sourcing, application service provision, cloud computing, business process outsourcing, robotic process automation, cognitive automation, and blockchains. After nearly three decades of practitioner-focused IS research, our action principles approach became more formalized as follows:

**Let Relentless Curiosity Motivate the Research.** The commonality across our research projects is that we studied emerging phenomena that generated many perplexing questions that piqued our curiosity. None of them began with a search of the existing academic literature for something to study. Encouraged by Richard Daft (1983) to avoid well-formulated a priori hypotheses (that typically produce "small" returns on knowledge in his view), we studied contexts that were uncharted, ambiguous, and complex—what fun! According to Daft, the "quality of work" should be measured by the "intensity of the surprise" of the findings. As Daft explains:

> *If we have a good idea about what the research answer will be, if we understand the phenomenon well enough to predict and control what happens, why bother to ask the question? If we are to acquire knowledge that is truly new, then we do not know the answers in advance. The significant discoveries, the good science, requires us to go beyond the safe certainty of precision in design* (540).

**Study the Early Bellwether Adopters.** Action principles fieldwork began with the study of early organizational adopters of business and technical innovations. We wanted to understand what drove their decisions, the actions they took during the entire journey, and the outcomes they experienced from multiple perspectives—top managers, middle managers, line employees, and customers. We made contacts at practitioner events sponsored by professional associations, consulting firms, and service providers. For ITO, the early adopters we studied included Kodak, Enron, Continental Airlines, and Inland Revenue. For BPO, the bellwether adopters included British Petroleum, Microsoft, and EMC. For robotic process automation (RPA), we studied companies like Telefónica O2, Ascension Shared Services, and Virgin Trains. For blockchains, we studied companies like J. P. Morgan, State Street and BNP Paribas (as well as 35 others and counting).

**Cocreate Action Principles.** Our interviews typically begin with a very simple statement: Tell us your story from your perspective. Across interviews within a context, we find a common narrative, reveal differences, and begin the process of formulating the action principles. Anything we write needs to be reviewed and approved by participants until we come to a common understanding. Across research projects, I estimate that we have generated between five to ten action principles at each organization we studied, sometimes based on a single interview with a key participant.

As we interviewed more participants across more contexts, we build tables that map action principles across contexts. The participants reviewed and provided feedback as the data built across contexts. The approach is iterative; action principles may be added, reworded, or combined with subsequent rounds of data collection. As evidence accumulates, action principles become more "robust" when the practice holds up over multiple contexts. However, frequency is not necessarily an indicant of importance or impact. Sometimes it's the "according to 1 participant in one context, action *X* produced result *Y*" that resonates with practice. As an example from our research on blockchains, "according to one person at one large financial institution, allowing people to pay with Bitcoins in the employee cafeteria signaled to employees that senior management considered cryptocurrencies to be legitimate." While we found no other example of that across the 30 other firms we examined for this particular study, it is still a powerful finding—a "surprise" from our practitioner audience that we did not expect.

**Make a Theoretical Contribution When There Is Something Truly Insightful to Say That Is Backed by a Powerhouse of Action Principles.** Going back to our 1990s ITO research and the subsequent research by hundreds of scholars, TCE was the most frequently appropriated theory to study IT outsourcing decisions (Dibbern, Goles, Hirschheim, & Jayatilaka, 2004). It is *the* "make or buy" theory, leading to two Nobel Prizes in economics; one for Ronald Coase in 1991 and one for Oliver Williamson in 2009. However, TCE logic failed to explain much of what we found in ITO practice. Practitioners were routinely outsourcing highly specific assets characterized by high levels of uncertainty, measurement difficulty, and ambiguity. We began looking across other ITO work and found similar results in 64% of findings on asset specificity. We examined the reasons authors gave when TCE logic went counter to their empirical ITO findings. Authors most frequently blamed themselves, or more precisely, blamed their research methods. It took us over two decades to get these insights published in one of our field's top academic journals (Lacity, Willcocks, & Kahn, 2011; Lacity & Khan, 2016). Once reified, prestigious theories seem to be "untouchable": it takes a vast number of action principles over many years and across many contexts to question them. A path forward is to build endogenous theories from action principles rather than borrow them from other disciplines (Avison & Malaurent, 2014).

## How Do Action Principles Address Hirschheim's Call?

Hirschheim is concerned that much of business research is moving from the research cycle "Problem → Research → Theory → Knowledge → Practice → New Problems" to "Problem → Research → Theory → New Problems." He notes that the knowledge and practice elements have "disappeared," and provides four guidelines for bringing knowledge and research back to the research cycle:

1. Broaden the aperture of what legitimate IS research should include.
2. Change the way journal editors handle "applied" research.
3. Bring back books as an accepted and valued publication outlet.
4. Return to engagement

Hirschheim's first three recommendations require institutional and structural changes that are difficult for new IS scholars to influence. My commentary primarily provides insights into the last point. New IS scholars can "return to engagement" because we do get to choose our research topics and methods (even if we don't get to choose whether our papers or books will be published and/or valued). However, individuals do not have to choose between practice and theory. As outlined above, research can include both, albeit on different time horizons. Therefore, I suggest one addition of "theory" to Rudy's final prescription for a research cycle: "Problem → Research → Understanding → Practice → Theory → New Problems."

In my experience, it may be years before someone has something theoretically profound to say beyond "small returns" (Daft, 1983). However, PhD students and assistant professors are advised to consider theoretical lenses that might help frame or inform their subsequent practitioner findings. Academics cannot have something profound to say theoretically in the future if they do not start thinking about theory early in their careers. In the meantime, scholars will be fulfilled and satisfied when they can confidently share with practitioners the understanding gleaned from action principles research.

## Final Thoughts

By any measure—citations, publications in top journals and scholarly books, leadership positions in the academic community, and numerous awards and recognitions (including a LEO!)—Hirschheim is an elite IS academic scholar. It takes someone of his stature to attempt to influence the course of an entire discipline. He didn't have to write this essay; he could have eased into an eventual retirement filled with electric guitar playing and tennis matches. So why did he so boldly put to words the conversations many of us have outside of the public view? I believe he did so to inspire the PhD students and assistant professors just entering the field. I assert this based on my own experiences described above. I hope by illustrating the Lacity-Willcocks action principles approach, we might, in turn, inspire the next generation of IS scholars.

## Postscript

The following short narrative illustrates the action principle: "According to 12 participants in 7 organizations, participating in multiple blockchain consortia was an effective way to avoid technology lock-in."

BNP Paribas, the second-largest bank in the Eurozone and among the ten largest banks worldwide, participated in both large and small consortia and invested in several fintechs in order to influence, learn, and contribute to blockchain initiatives. According to Jacques Levet, head of transaction banking, EMEA at BNP Paribas, "The way we go about investing in blockchain is really multifaceted since nobody knows today which players will prevail…you cannot put all your eggs in one basket, so we have a very diversified approach with whom we work on the blockchain." For Levet, a large consortium like R3 was very valuable because it brings many financial institutions into the conversation. As Levet explains, "R3 is very useful because it's a way to organize discussions between the banks. Banks have historically not been very good at doing that on their own, so having a third party who organizes that is quite useful." BNP Paribas also joined two smaller consortia, with the goal that the banks will eventually define standards and create a request for proposal (RFP) for fintechs to develop specified blockchain applications.

# On the Limits of Theory and Theorizing in Information Systems

Andrew Burton-Jones

It is a privilege to be invited to comment on Hirschheim's paper, "Against Theory: With Apologies to Feyerabend." Hirschheim's thesis is that the IS discipline, like many others, has responded to the need for academic legitimacy by focusing on the rigor of its research and, especially, by focusing on theory. He argues this focus on theory is problematic.

I will begin with a general comment. It is risky to entitle a paper "Against *X*" if the author is not against *X*. I say this because, if I interpret Hirschheim's paper correctly, he is not against theory, but rather against the unsophisticated, slavish, and mindless use of it. This is a different and uncontroversial point. The problem with using a title that differs from the arguments within a paper is that the paper can be hard to follow because the arguments slip and slide as the author tries to stay true to the title while also trying to say something else. I made a similar point in my response to Treiblmaier's (2018) paper (Burton-Jones, 2018). I am glad, therefore, to be able to comment on this paper. Through the ensuing dialogue, I hope Hirschheim's argument will become clearer and will have a positive impact on the field.

In the remainder of this commentary, I offer three critiques of Hirschheim's position. I then discuss his recommendations and offer two additional recommendations of my own.

My first critique is that the paper makes overly broad-sweeping claims about the IS discipline. For instance, in describing our discipline's focus on theory, Hirschheim writes: "Rarely are there instances of an academic paper getting published without a section entitled 'contributions to theory'…the whole world expects the focus to be on theory." While I agree that top journals tend to focus on theory, I do not believe they do so as thoroughly as Hirschheim states. Journals make decisions through their editors, and editors' views differ. There are healthy debates regarding the role of theory in our journals, as seen in recent editorials (Gupta, 2019; Rai, 2016), and in discussions in the empirical communities (Johnson, Gray, & Sarker, 2019; Maas, Parsons, Purao, Storey, & Woo, 2018) and the design communities of our field (Baskerville, Baiyere, Gregor, Hevner, & Rossi, 2018; Gregory & Muntermann 2014; Rai 2017a). While Hirschheim briefly alludes to these complexities (e.g., in footnote 21 of the paper), he largely paints a simpler and less accurate view of the field.

My second critique is that Hirschheim gives insufficient credit to the open-mindedness of IS researchers. While he gives credit occasionally (e.g., in footnote 20), he generally treats IS researchers as unsophisticated and inflexible. For instance, he claims that Bacharach's definition of theory is the "general consensus." While this definition is influential, it is not general consensus. Other definitions abound and IS researchers have critiqued it previously (Mueller & Urbach, 2017). Similarly, Hirschheim states that the only view of theory accepted in IS journals consists of: "one or more functional statements or propositions that treat the relationship of variables so as to account for a phenomenon or set of phenomena." This ignores the major contributions of interpretive scholars (including Hirschheim) who enabled us to use theory differently (Klein & Myers, 1999). Hirschheim goes on to urge a focus on understanding over explanation, but such issues have already been discussed in various ways by IS researchers (Hovorka, 2004; Lee, 1991), as has the need to be open-minded when theorizing (Burton-Jones, McLean, & Monod, 2015). Overall, while Hirschheim draws inspiration from Feyerabend to argue that we should be more open-minded in our use of theory, I agree with Treiblmaier (2019, p. 91) that "the IS community is already far more Feyerabendian than it might [realize]." Of course, we could be more open-minded, but that is a different point.

My third critique concerns the purported negative consequences of focusing on theory. Hirschheim claims the field of operations research evinces how badly we might fare if we continue down the theory road. This is unconvincing because, as he acknowledges in footnotes 10 and 11, operations research may not be experiencing the problems he alludes to, and these problems (if they exist) may not relate to its focus on theory. While the operations research field would be different had it followed the trajectory Hirschheim recommends, it is hard to know if it would be more successful. More generally, it is hard to accept Hirschheim's view because we are not shown any data on the effect of theorizing on a discipline's success. Longitudinally, the study by Colquitt and Zapata-Phelan (2007) shows that more engagement in theorizing is associated with greater impact. However, this evidence is not causal. We might instead consider cross-disciplinary comparisons (e.g., comparing the IS discipline, which appears to be theory-heavy, with related disciplines such as human computer interaction or clinical informatics that appear to be theory-light). However, it is hard to make cross-field comparisons of success because different fields inevitably have different paradigms. History suggests that any given paradigm might be replaced with another one and judging the "success" of any paradigm is fraught with problems (Kuhn, 1996). Contemporary sociologists of science suggest that we might not even know enough about the practical use of theory to know how best to use it to improve research (Swedberg, 2017).

Despite my concerns above, I empathize with Hirschheim's frustrations. I have had my share of tortuous review processes in which reviewers try to force-fit the paper into their views of theory. I have also read too many papers that say they "contribute to theory" when they are merely engaging in "Salt Passage Research" (Pencil, 1976). As an immensely respected IS scholar, Hirschheim has earned the right to express his exasperation. Such papers have a venerable tradition (e.g., Dunnette, 1966).

While I empathize with Hirschheim's frustrations, I have mixed views regarding his recommendations. While I support all of them, I would add nuance to each one.

Hirschheim's first recommendation is to broaden the aperture of what legitimate IS research should include to include policy work. I support Hirschheim's call for policy work, but I was surprised by his recommendation because, in my view, this is something we already do. Policy questions are an important part of IS research (e.g., Cheng, Bandyopadhyay, & Guo, 2011; Y.-K. Lin, M. Lin, & Chen, 2019). More policy work would, of course, be valuable.

Hirschheim's second recommendation is for journal editors to change how they handle "applied" research. I support this recommendation, but it deserves some nuance. To illustrate, when I read many "applied" journals, my impression is they are often written for a certain "type" of practitioner—the type who likes immediately actionable advice, 2*2 grids, and seven steps to success. I have not met many successful practitioners like that. And when I have used "applied" articles in MBA classes, many of my students (practitioners) have found them superficial. In my view, there is a large cohort of reflective practitioners in the IS field who engage in theorizing and who want to work with theory, just as in other fields (Reed, 2008). Some practitioners even see value in moving between practitioner and academic boundaries and identities over time. I currently have two doctoral students doing so (both of whom are successful practitioners) and, far from avoiding theory, they are actively engaging in theorizing and challenging my understanding of theory and good theory. Based on these experiences, I would not support moving to "applied" research if that implies oversimplifying research. Rather, I support initiatives at our journals to link academic work with practitioner-oriented communications (Gupta, 2017; Rai, 2017b) and to produce research that can have a strong impact on practice (Barrett & Oborn, 2018; Davidson & Barrett, 2018).

Hirschheim's third recommendation is to bring back books as an accepted and valued publication. I support this, but I would add that many academics in our field are writing books. In fact, my impression is that IS scholars are writing more books now because the issues they study are so relevant (Bailey & Leonardi, 2015; Brynjolfsson & McAfee, 2014; Ghose, 2017; Leonardi, 2012; Mithas, 2015; Parker, Van Alstyne, Choudary, & Foster, 2016; Sundararajan, 2016; Kane, Phillips, Copulsky, and Andrus, 2019). It is likely that the rewards from books differ from the rewards from journal articles, extending beyond academia alone (Harel, 2007, p. 5-11). Thus, even if a particular university does not reward the publication of books, it may well be in the interests of academics to write them anyway.

Hirschheim's final recommendation is for academics to return to engaging with practice. Once again, I support this. I would simply add that Hirschheim's criticisms appear to have a North American focus. While he mentions that levels of engagement differ in different regions, it is possible that the US, for instance, is actually an outlier rather than representative of the mean. In Australian academic circles, for instance, "engagement and impact" are the orders of the day, and I suspect this will only increase over time. Of course, engagement and impact are complex topics and there is an active literature on them (MacIntosh et al., 2017).

Overall, despite supporting Hirschheim's recommendations (with the above nuances), I am not convinced that they will really address the problem he sees—that IS academics are hyperfocused on a particular view of theory. Rather, I believe that the issues Hirschheim is seeing are partly symptoms of deeper issues, at least some of which are unsolvable. I say this because theories are simply "nets cast to catch what we call 'the world'" (Popper, 1980, p. 59, qtd. in Mueller & Urbach, 2017, p. 353), Just as Straub, Hoffman, Weber, & Steinfield (2002, p. 228) wrote that measurement is impossible because we cannot "capture a moonbeam and hold it in our hands," theorizing is impossible because we cannot "catch the world in a net." Theories are always problematic (Kaplan, 1964/1998, pp. 351-356), just like every part of research (McGrath, 1981). This is true whatever approach we take to theorizing, and it will prove consequential even if we follow all of Hirschheim's suggestions. In short, I believe Hirschheim might be railing against the limits of research as much as the specific issues to which he points. Of course, this is not a reason to accept the status quo, but it should be borne in mind. If it is true, I see two additional ways to respond.

First, rather than engage in too much critique, we could focus on championing those who are making great strides in the sophisticated and mindful use of theory, to motivate more such work. At the risk of failing to mention many success stories, a handful that come to mind are:

- Lukyanenko et al.'s use of theory to improve the design of systems (Lukyanenko, Parsons, & Wiersma, 2014; Lukyanenko Parsons, Wiersma, & Maddah, 2019)
- Ho et al.'s use of theory to influence the users of systems (Ho & Lim, 2018)
- Larsen et al.'s use of theory to improve research practices (Larsen & Bong, 2016)
- Berente et al.'s combination of top-down and bottom-up theorizing to understand well-known IS phenomena (Berente, Lyytinen, Yoo, & Maurer, 2019)
- Miranda et al.'s combination of top-down and bottom-up theorizing to understand emerging IS phenomena (Miranda, Kim, & Summers, 2015)
- Sarker et al.'s sensitivity to the historical and future use of theory (Sarker, Chatterjee, Xiao, & Elbanna, 2019)

The list could go on. Of course, Hirschheim has done pioneering theoretical work throughout his career too. In fact, I found it ironic that his article was entitled "Against Theory" when his treatise on systems development and data modeling, which is very theoretical, has been so inspirational for my own work (Hirschheim, Klein, & Lyytinen, 1995). I hope journal editors, book publishers, and academic department chairs will continue to champion those engaging in creative theoretical work. We need more rather than less of it.

My second recommendation is simpler but harder. Rather than engage in too much criticism or self-doubt, we might just try to ignore the rat race, the rankings and metrics, and the limits of science, and simply focus on following our own scientific ideals (Berg & Seeber, 2016; March, 2011), whether for or against theory, while keeping a good sense of humor. In that spirit, I will end with the opening quote to Chalmers' (1976) well-known account of science: "Like all young men I set out to be a genius, but mercifully laughter intervened" (*Clea,* Lawrence Durrell).

## Acknowledgment

I thank Gongtai Wang for helpful comments on an earlier version of this response.

# Against "Against Theory" with Apologies to Hirschheim

Viswanath Venkatesh

Rudy Hirschheim is an icon and influential scholar who has contributed to the IS discipline in so many ways, especially through his research. Like many others, I too "grew up" in the PhD program reading the works of Rudy, as he paved the road of knowledge in our field by asking the tough questions and challenging assumptions. I mean, has anyone else used the word "myth" in the title of papers as many times as Rudy has? I suppose if one has not met Rudy, one would think Rudy is a myth not only because of what a fine scholar he is, but also because of what an amazing person he is. It is thus truly an honor for me to be invited to write a response to Leo Award winner Rudy Hirschheim's essay, invited as part of the journal's initiative to allow for such amazing scholars, who have effectively received a lifetime achievement award, to share their thoughts, largely without the shackles of editors and reviewers reigning them in. That said, I was a reviewer of the essay—knowing that the essay would be published and knowing that my goal as a reviewer was to help the author make the essay as good as possible without altering his core message. Now, there is a thought for all reviewers to embrace: help authors make their papers as good as possible without altering the core message of the paper! I know that if it were not for that task assignment, I would have come down like a ton of bricks on the essay because my fundamental disagreements were simply too many to count. But, given my task assignment, I was, over time, able to formulate a better review that allowed for the essay to become a balanced contribution to the dialogue about the role of theory. As a side note: when I was reading the masterpiece that is Hirschheim and Newman (1991), published in the first volume of *Information Systems Research*, I would have never imagined that I would be writing what is, effectively at least, the makings of a rebuttal to an essay by The Rudy Hirschheim himself.

Rudy's thesis in his essay against theory is that *theory* is not important, *understanding* is. I organize my response into five sets of reactions that I had as my thoughts about his essay and my response crystallized—seething, outrage, irritation, worry, and calm.

## Seething: Rudy is Hypocritical

Reading Rudy's essay several times, both the initial version as part of the review process and the final version, truly had me seething. Rudy has so many influential papers that built theory from interesting cases and presented influential research agendas that have been instrumental in driving IS research forward that I could only possibly conclude that this was truly hypocritical behavior. Here are just four examples of his theory-anchored works: Hirschheim (1985) discussed epistemology underlying IS as a core vehicle to think about how we create knowledge in this field. Hirschheim and Klein (1989) discussed four paradigms of IS development and noted that their article "provides a new vehicle for theorizing about the nature, purpose, and practice of information systems development." (p. 1199). Hirschheim and Newman (1991) were challenging assumptions before the idea of challenging assumptions was cool—the interested reader is invited to read Alvesson and Sandberg's (2011) all-too-radical-and-cool call to eschew typical gap-spotting work in favor of fundamental assumption challenges. In this millennium, Dibbern, Goles, Hirschheim, and Jayatilaka (2004) provide an excellent framework organizing the literature on outsourcing as a way to guide future work. How can someone who built his career doing such exceptional work, publishing in the best outlets and fully leveraging the crutch of theory, suddenly call the field to eschew theory? (The interested reader is referred to Rudy's website at Louisiana State University [https://www.lsu.edu/business/sdeis/profile-viewer.php ?un =rudy] for his selected publications and is invited to examine how many times the word theory is used in just the titles alone; a study of the papers would suggest Rudy is not just for theory, he loves theory). Why? Because he is hypocritical—why else? This was the response to my seething phase.

## Outrage: Essays Like These Are Irresponsible and a Disservice to the Community

I can see it—every doctoral student who is struggling to develop theory and identify theoretical contributions can now cite Rudy Hirschheim, among others, as the reason to ignore theory entirely and declare victory in the name of understanding or insights. Somehow, highly successful scholars, who built their careers masterfully leveraging, extending and building theory, not to mention charting the course for future theory development, seem to want a swansong in saying something bad about theory. Other such examples, aside from Rudy's essay, such as Hambrick (2007), Greenwald, Pratkanis, Leippe, and Baumgardner (1986), and Locke (2007), come to mind. Such essays are a disservice to the community in that they suddenly serve as a key marker for us to no longer focus on theory in a substantial way as an anchor to the knowledge creation process and the reporting process. What prompts such irresponsible behavior? I just could not figure it out as I went through my phase of outrage.

## Irritation: When Did Theory Become a Bad Word?

From the muted opposition to theory in PhD classrooms to vocal opposition in conference panels, to scathing articles, like Rudy's essay, theory has somehow become a bad word. It is seen as something that hinders progress, prevents the emergence of insights, and creates shackles that hinder the study of exciting new problems. Hambrick (2007) cites three examples (Baker & Pollock, 2007; Helfat, 2007; Miller, 2007) of papers that led to significant insights because theory was not leveraged. Rudy paints a doomsday scenario for IS, akin to what operations research went through decades ago, if we are wedded to theory. All of this suggests that theory is somehow the problem. The use of theory, the recombination of prior knowledge effectively (e.g., Uzzi, Mukherjee, Stringer, & Jones, 2013), and putting forth a research agenda grounded in theory have been essential not only to some of Rudy's most influential works, as noted earlier, but also to mine as well. Hence, my irritation with someone vociferously opposing theory.

## Worry: What Did Theory Ever Do to Us?

Slowly, it started to dawn on me. Rudy is not really opposed to theory. He is worried about the state of research, the state of scholarly pursuits, and the state of our journals—and most importantly, the people in the field. Blaming theory, rather than the scholars pursuing theory, is like saying that guns kill people while absolving the killers of any guilt. Isn't the real problem the way that people are "using" theory? Perhaps Rudy worries about scholars using theory as a hammer to regulate what work is pursued, how it is pursued, and what will ultimately be published. Perhaps Rudy is worried that we are failing to do an effective job of teaching theory development skills to our students, i.e., our next generation of scholars. With the unfolding of new phenomena due to digital transformation, Rudy is surely worried that the shackles of theory will hinder progress and understanding, which is his clearly stated goal for us, for research.

Ultimately, Rudy's essay does not really argue that theory is bad. The world behind the text as I see it is that it is the appropriation of theory that is bad; it is the broken review process and the misguided view of expectations that is dangerous to the field. Rudy calls for us to seek to publish good research that solves important problems. Our journals may have a misguided emphasis on what constitutes publishable work and that is what I view as Rudy's main concern. If eschewing theory will allow us to publish better work, he is for that. Before him, Weick (1995) called for us to publish intermediate products (what he termed outputs of theorizing) and suggested that the expectation that every engagement in a research endeavor will lead to a theoretical contribution is unreasonable. Like Rudy, the state of where we are causes me to worryCalm: Rudy is Brilliant—He Wants Us to Stop and Think!

I agree with the core of Rudy's brilliant essay: we must stop and think, and we must focus on understanding. I agree, subject to the understanding (no pun intended) that we are in the business of building cumulative knowledge about abstract, enduring phenomena, and that this body of knowledge will continue to grow through what is primarily our normal scientific efforts, i.e., incremental steps forward, with paradigm-breaking ideas occurring every few years (30 to 40, per Kuhn [1970]). Our journals, our reviewers and especially our editors should embrace this reality and support scholars in these endeavors. Theory is that linguistic device, tool of rhetoric, abstraction aid, (the reader is invited to insert other rhetorical, linguistic devices to describe theory that fits their own worldview) that helps us achieve this goal. I feel a sense of calm that theory is here to guide us—we just need to figure out what it means to us, and the beauty of theory is that it can mean different things to different people. As long as we, as scientists, focus on building cumulative knowledge, the role of theory is simply indispensable.

## My Final Word

I firmly believe in the role of theory. At the same time, I do not subscribe to a narrow or positivist definition of theory. I view theory, like many others, as something that aids our understanding, as a linguistic device to organize our knowledge in a systematic way, as a way to generate insights, and so on. I believe it is a representation of reality and provides us the with necessary scope to guide our investigations. It helps us determine the lens we will use either at the front end (deductive approach) or at the back end (inductive approach) to organize our upfront thinking and acquired knowledge. Such rhetorical tools and devices are essential as we make our way through this complex world of unique phenomena, seeking to understand them in abstract terms so that one research endeavor can inform the next, and we can build a body of cumulative knowledge about our understanding of the world, thus guiding organizational and societal functioning to a brighter tomorrow. In closing, I agree with Rudy that research is about understanding—and to that I add, theory is about understanding.

# Postscript:
# Thoughts on the "Against Theory" Commentaries

**Rudy Hirschheim**

I would like to thank Suprateek Sarker (past editor in chief of the *Journal of the Association for Information Systems*), for the opportunity to offer my thoughts on a concern I've had for some time about the field—the reverence (and to my way of thinking, its misplaced reverence) it ascribes to *theory*. I also want to thank the various commentators for their thoughts on my "Against Theory" essay. I have now had the opportunity to read them over and wish to briefly respond to some of the points made in these commentaries.

In reading them I was delighted to see the breadth and depth of the authors' analyses. These were well-crafted and thought-through commentaries from some of the IS field's most preeminent scholars. In the commentaries, one can see agreement, disagreement, the offering of new insights, reflections on old ideas, attempts to ascertain what my exact purpose was in writing the essay, suggestions on how my thoughts might be modified, added to, subtracted from, and so on. There are clearly many different viewpoints—both positive and negative—on my essay. This was precisely my intention when I wrote the essay: challenge the field to reflect on where we are, how we got here, and where we might head in the future. My hope was to start a debate, not to offer a detailed solution. And while I did offer some recommendations, they were in no way meant to be definitive. They are, as it were, an opening strategy which should be further refined and added to. The original version of the paper offered no recommendations, only a hortatory appeal to reason. The reviewers quickly dissuaded me of this approach saying more needed to be written; I needed to offer some recommended actions for the field. This became my four-point action plan, i.e., (1) broaden the aperture of what legitimate IS research should include; (2) change the way journal editors handle applied research; (3) bring back books (and essays) as accepted and valued publication outlets; and (4) return to engagement. These guidelines are, of course, controversial, but that was the intention of the paper all along!

The focal point of the discussion in my essay is the field's infatuation with "theory." The focus could have also been "method," perhaps more closely following Feyerabend's core arguments. It could have been on the field's "body of knowledge" or what comprises its "core" (if such a thing even exists). It could have been on the field's desire for "objectivity," which to me is largely illusory. In the end, I chose to focus on "theory" because that is what has been bothering me for some time. The incessant call to produce "theory" for a paper to be recognized as a "contribution to knowledge" has seemed to take on a life of its own. I wondered why this explicit or implicit policy occurred, when did it start, and what its potential result would be. To me, this inexorable drive toward "theory" had become dysfunctional. Hence my essay.

Of course, as has been pointed out in several the commentaries (e.g., Venkatesh's "seething" reaction: "Rudy is hypocritical"), much of my work has involved the use of "theory" and "theoretical lenses." So how can I argue *against* theory when my work actually embraces it? As noted in my Conclusions section, it is not that I am against theory per se, what I am against is the mindless obsession of making theory essentially the only thing that matters in our research. Focusing solely on theory significantly constrains the practical and intellectual avenues a researcher can explore because many of these avenues do not lend themselves to the kinds of inquiry that theory-driven research demands; or should I say, the "theory-driven research" that the IS community seems to embrace. I do not deny that theory has an important role to play in research. But the field has taken too narrow a view of what theory is. This is the point made by Rowe and Markus when they note: "the IS problem is not overemphasis on theory, but a narrow understanding of theory." While I would likely take issue with the sentiment "the problem is not overemphasis on theory" (I believe it is), I do agree with the second part of the statement, i.e., the problem is a "narrow understanding of theory." Moreover, Rowe and Markus claim: "Theory may be a fetish, as Hirschheim claims, in some management fields, but it is not a fetish in IS. Instead our fetish is method." I strongly disagree with this. In fact, one might argue that the IS field is far more open to accepting a variety of methods—qualitative, quantitative, conceptual, design science—than it is about theory. It is here that the field has developed a restrictive view of what theory is, or should I say what passes for "theory." This point is made in Jarvenpaa's commentary when she writes: "I disagree that the culprit is theory per se. Hanson (1958) reminds us that all observations are theory-laden, whether we are implicit or explicit about it. In my view, Bacharach (1989) got it right: "[theory] is no more than a linguistic device used to organize a complex empirical world." I couldn't agree more. So why has the field chosen to view theory in such a constricted way? This is captured eloquently by Swanson, who states: "Just to be clear, what theory is not is a formal causal model. While such a model may be informed by theory and can be built and examined in a particular study, it yields, at best, fragmentary insight in need of narrative accompaniment." This is why I called for the field to recognize the importance of "understanding," rather than simple "theoretical explanation." This is what Lacity argues for in her action principles. For her, "action principles are practices that explain the results found in real-world implementations." Such principles emerge: "as a bricolage of personal research experiences and ideas from the work that inspired us." I believe these action principles provide a mechanism for how IS researchers can engage with practitioners and move the field forward. This is the focus of my plea for the field to "return to engagement." But how can one develop such a "bricolage of personal research experiences" especially as they relate to IS practice? Perhaps Jarvenpaa provides an answer when she writes: "It is viewed that somehow spending time with industry steers them to industry jobs or corrupts them with industry problems that are difficult to package as academic research. During my Ph.D. studies, I completed an internship with one of the leading strategy consulting firms and this experience redefined my research as well as my teaching. Without that experience, I would not have received the teaching opportunities and had the confidence to venture out to emerging topics." This was a similar path to the one taken by Lacity. Thus, industry internships, industry assignments, etc. should be considered as part of the overall PhD experience.

While I found Burton-Jones' call for a more "nuanced" approach to my arguments informative, it is not clear how far "nuance" takes us. For example, he notes that "my first critique is that the paper makes overly broad-sweeping claims about the IS discipline." Indeed, it does. My position is that any discussion of a collective body of individuals who call themselves "IS academics" or the "IS field" has to be categorized as an archetype—a highly simplified form that embraces powerful conceptions of an ideal or character type. These ideal types do not exist as "real" entities, rather it is their properties, exhibited (to a greater or lesser degree) in existing entities, that give the archetype meaning. Without such "highly simplified but powerful conceptions" it would be difficult if not impossible to talk about a discipline. Burton-Jones might be correct in saying my statements about the field are too broad and sweeping but I have tried to explain why I hold these views and where they come from. It is my hope that they will resonate with the reader. I also have to take issue with his comment: "I will begin with a general comment. It is risky to entitle a paper 'Against *X*' if the author is not against *X*. I say this because, if I interpreted Hirschheim's paper correctly, he is not against theory, but rather against the unsophisticated, slavish, and mindless use of it." Actually, my point is not so much the use of theory, but how the search for and emphasis on theory has become dysfunctional for the field. Perhaps a better argument or way to think about this is through Rowe and Markus' call for "disciplined methodological pluralism," although I would modify this to "disciplined methodological and theoretical pluralism."

Additionally, I must also take issue with Burton-Jones' third criticism concerning "the purported negative consequences of focusing on theory." Specifically, he asserts:

> *Hirschheim claims the field of operations research evinces how badly we might fare if we continue down the theory road. This is unconvincing because, as he acknowledges in footnotes 10 and 11, operations research may not be experiencing the problems he alludes to, and these problems (if they exist) may not relate to its focus on theory.*

Actually, my point was that OR's intense focus on method to the detriment of everything else has contributed to their thorny and uncertain future. In the case of IS, I am concerned about the same singular focus—only in IS it concerns theory rather than method.

Lastly, I have to hand it Venkatesh who with wit and aplomb captured the essence of the issue:

> *I firmly believe in the role of theory. At the same time, I do not subscribe to a narrow or positivist definition of theory. I view theory, like many others, as something that aids our understanding, as a linguistic device to organize our knowledge in a systematic way, as a way to generate insights, and so on. I believe it is a representation of reality and provides us with necessary scope to guide our investigations. It helps us determine the lens we will use…. Such rhetorical tools and devices are essential as we make our way through this complex world of unique phenomena, seeking to understand them in abstract terms so that one research endeavor can inform the next, and we can build a body of cumulative knowledge about our understanding of the world, thus guiding organizational and societal functioning to a brighter tomorrow. In closing, I agree … that research is about understanding—and to that I add, theory is about understanding.*

I couldn't have said it better!

In closing, I would like to thank all the reviewers who commented on various drafts of the essay, and especially Dirk Hovorka, who acted as senior editor for the manuscript. Without their valuable inputs and

significant insights, this essay (and its arguments) would have been half baked (although some readers might believe it still is half baked!).

I would also again like to thank the commentators for all their efforts in putting together cogent and coherent arguments. After reading the commentaries and reflecting on what was said, I was delighted to see the broad range of thoughts, opinions, suggestions, intellectual challenges, and desire to not dismiss theory out of hand! While I do not necessarily agree with all the points made in the various commentaries, they are thought provoking, well written, and informative. They form an excellent backdrop by which to assess and reflect upon the arguments made in my essay. My goal was to get the IS community to think about and engage in a debate on the myriad issues facing the IS field. If these commentaries are any indication, we are off to a great start. Let the debate continue!

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