How and Why 'Theory' Is Often Misunderstood in Information Systems Literature

Completed Research Paper

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Abstract

IS theory accounts have increased our understanding of scientific theories. However, many influential theory accounts in Information Systems (IS) are influenced by the Received View of scientific theory (RV), which flourished in the philosophy of science around 1930-1969. The RV has been widely rejected in the philosophy of science, as the theory ignored much of the actual scientific context, and it mischaracterized several important characteristics of scientific research and theories. Although RV ideas were crafted for philosophers' philosophical purposes, and not for scientists' use, several IS scholars seem to believe that some of the RV theses represent actual (good or strong) scientific theories. Mistaking problematic logical reconstructions by philosophers for "real" scientific theories may seriously impede IS research. Recognizing how some of the RV theses may mislead the IS research will hopefully help us to value other types of research contributions that may not meet the current 'theory' conventions.

Keywords: Theory, received view, logical empiricism, logical reconstructions

Introduction

The importance of 'theory' has been widely inculcated in the information systems (IS) community. For example, a former editor of *MIS Quarterly* (MISQ) reports that a "required element" for any excellent paper is that it "sufficiently uses or develops theory" (Straub 2009, p. vi). Former editors of the *Journal of the Association for Information Systems* tell us that "all top journals in our field promote strong theory" (Tan et al. 2008, p. 44). In addition, it is stated that "design theorizing is an expected norm for DSR" (Baskerville et al. 2018, p. 362). The importance of theories in IS dates back to the first *International Conference on Information Systems* (ICS). In the first ICIS, Keen (1980) introduced the need for theories in IS, so that IS is not a theme but a "substantive field" (p. 9) that has "a scientific base" (p. 13). Importantly, as we understand Keen's (1980) view, it was influenced by his perception of the intellectual atmosphere in business schools: "We look muddled, messy and fraudulent to people in information economics and operation research" (p. 10). Especially in organizational and management research (OMR), 'theory' has been important: "If manuscripts contain no theory, their value is suspect. A primary reason, sometimes the primary reason, that reviewers and editors decide not to publish a submitted paper is that it contains inadequate theory" (Sutton and Staw 1995, p. 371). In addition, "making a theoretical contribution" is commonly required by OMR journals (Hambrick 2007, p. 1346). The curious story of (new) theoretical contributions seems to have been transferred to IS to some extent: "It is in the evaluation of the theoretical contribution that most reviewers become convinced, or not" (Straub 2009, p. vi).

IS theory views have increased our understanding of scientific theories. That being said, in this paper, we argue that (1) the role of 'theory' in IS, and (2) what is a scientific 'theory' in IS, often intellectually stems from a view, once widely held in the philosophy of science around 1930–1969, known as the Received View (RV) of scientific theory. What is known as the RV contains a number of theses by professional philosophers for philosophical purposes. Although the RV is commonly presented as a generic account of scientific theory, this view was typically influenced by philosophers' understanding of physics. Since 1960, the RV has been widely rejected by professional philosophers (Beatty 1980; Craver 2008; Suppe 1972). For example, Carl Hempel is regarded as the most prominent advocate of the Received View of scientific theories, yet Hempel himself rejected it (Suppe 1972). As the RV is now regarded as widely rejected, it is sometimes called the Once Received View of scientific theory (ORV; Craver 2008). Understanding several basic ORV ideas may shed new light on many influential views of IS theories. In addition, there is a risk that many IS scholars base their understanding of 'theory' on an outdated philosophy of science that has been widely rejected by philosophers, and which offers a questionable, if not misleading, account of scientific 'theory' and research.

In this paper, we describe some of the history of the ORV, and the way in which many ORV theses influence IS, although the ORV (as originally presented) is simplistic, and thus, utopian, even for physics. Similarly, ORV-influenced views may portray IS research settings as much more simplistic than they probably are. Moreover, the ORV may have misled some IS scholars to characterize a priori "theory" or "strong theory," which may, in fact, restrict the presentation of research findings that do not meet these requirements. Understanding the ORV may also help us see that "theories" and "theoretical contributions" may not be intrinsically valuable, but they have an instrumental role in serving higher aims in empirical research, which could be related to explanation, understanding, or prediction, for example. Requiring a "new theoretical contribution" may hinder these goals in some cases.

Critical studies, such as this paper, are important in science. Scientific doctrines should not be accepted blindly, simply because they are published in top journals, or are highly cited. Scientific doctrines should withstand serious scrutiny by the scientific community. This review indicates that some 'theory' ideas taken for granted in IS have not yet enjoyed a critical review. Although this analysis contains critiques (as philosophical studies in the philosophy of science usually do), this paper cannot do justice to many important issues pointed out by the papers we criticize.

The Received View of Scientific Theories in the Philosophy of Science

In this section, we introduce some of the historical background of the ORV, which shows how some key ORV advocates regarded their doctrines as *a logical reconstruction* for philosophical purposes only. Making this historical point is important, because it raises the question of why scientists (in this case, in IS) should use or impose logical reconstructions of research that are developed for philosophical purposes (and not for scientists' use). We are not aware of any theory account in IS that demonstrates this understanding.

Although key advocates of the ORV included Carl Hempel and Rudolf Carnap, "virtually every significant result obtained in the philosophy of science between the 1920s and 1950 either employed or tacitly assumed the Received View" (Suppe 1972, p. 4). The history of the ORV is complex and dynamic, and varies according to the philosopher (e.g., Hempel vs. Carnap). It cannot be explained in detail here. To understand some background of the ORV, it is helpful to briefly discuss Berlin logical empiricist Hans Reichenbach's (1920) method of *logical analysis* of science. The result of logical analysis was known as a *logical reconstruction*, which "endeavours to clarify the meaning of physical theories, *independently of the interpretation by their authors*, and is concerned with [the] *logical relationship alone*" (Reichenbach 1949, p. 293). Roughly speaking, with Reichenbach's method of philosophizing (the method of logical analysis of science), a philosopher analyzes a theory only in terms of logical relationships. What cannot be represented as logical relationships, therefore, is simply omitted, but the outcome can be different from what the original author said. Reichenbach's view was influential. For example, Hempel, who studied with Reichenbach (Milkov 2013), outlined "the logic of explanation" (Hempel and Oppenheim 1948), which is a major ORV account (Giere 1988). The logic of explanation is primarily concerned with the logical relationship alone, just as Reichenbach stated.

Carnap (1961) referred to "rational reconstruction," namely, "the searching out of new definitions for old concepts" (p. v). Elsewhere, Carnap referred to this as the "method of explication" (Carnap 1945a, 1945b, 1947, 1950). Carnap (1950) characterizes the method of explication as the process of "transforming a given more or less inexact concept into an exact one" (p. 3). The outcome "of explication is a new, more precise concept, a replacement for the original vague concept, *more suitable for philosophical purposes*" (Giere 1996, p. 340). For Carnap (1947), this is "among the most important tasks of logical analysis" (pp. 8–9). Hempel's model of explanation can be understood as Carnapian explication, according to Niiniluoto (2000). There are important differences between Reichenbach's logical analysis and the method used by Hempel and Carnap, however. Next, we discuss an important difference.

Hempel and Carnap abstracted Reichenbach's idea of logical reconstructions further. With Reichenbach's logical analysis, the "concepts were connected to particular scientific theories" (Giere 1996, p. 340). However, "Carnap and Hempel from the 1940s onwards, had no such connection to any scientific theory" (Giere 1996, p. 340). For example, Hempel's logic of explanation is not specific to a particular scientific theory (Giere 1996, p. 340). Such logical reconstructions, although they were once widely accepted, started to receive criticism. Especially philosophers, who had close connections to actual scientific practice, regarded these logical constructions as problematic. For example, Burian (1975) reports "the logical empiricists provide us with highly 'cooked' formulations of theories—logistically kosher rational reconstructions" (p. 6). To give another example, Feyerabend (1970) characterizes philosophy of science at that time as "beautiful but useless formal castles" (p. 183). Kuhn (1962) tells us how "a concept of science drawn from them [the ORV accounts] is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure" (p. 1). As a result of these logical reconstructions, the gap between logical reconstructions and the actual content in science increased considerably in the philosophy of science in (around) 1930—1970 (Giere 1996).

Suppe (1972, p. 14) sets the demise of the ORV in 1969, when Hempel publicly rejected RV theses. (The demise, and its exact time, is debatable, and is not relevant here.) Giere (1996) suggests that Kuhn (1962) inspired the philosophy of science to focus on real cases, and Kuhn's work turned to a philosophy of science that focused on the specifics of sciences (Giere 1996). Similarly, van Benthem (2006) sees that, owing to Kuhn, in the 1960 and 1970s (and since), philosophy of science "became less concerned with explicating the logical structure of theory, confirmation, and explanation and more concerned with actual scientific reasoning and the historical structure of scientific change" (p. 264).

One more detail of the ORV is critical for scientists. Many philosophers, of whom perhaps Giere is the most well-known, have argued that "no one [even philosophers proposing ORV thesis] argued that scientists actually use such a conception of theories [as presented by ORV]" (Giere 1994, p. 276). IS readers must understand that ORV ideas were made just "for philosophical purposes" (Giere 1994, p. 276). This is the case for Carnap, Hempel, and Reichenbach. Reichenbach (1951) explicitly noted that scientific progress would be hindered if scientists wasted their time on logical analysis or defining what is a theory: "Scientific research does not leave a man [a scholar] time enough to do the work of logical analysis, and that conversely logical analysis demands a concentration which does not leave time for scientific work—a concentration which because of its aiming at clarification rather than discovery may even impede scientific productivity" (Reichenbach 1951, p. 123). In turn, Hempel (1965, p. 412) noted that "these models [of scientific explanations; major ORV models] are not meant to describe how working scientists actually formulate their explanatory accounts." Carnap (1937) declared that "the philosophy [and not the actual sciences!] is to be replaced by the logic of science – that is to say, by the logical analysis of the concepts and sentences of the sciences, for the logic of science is nothing other than the logical syntax of the language of science" (p. xiii).

We want to highlight the following thesis modified from Giere (1994). *The logical analysis and resulting logical reconstructions were professional philosophers' tasks for specific philosophical purposes only*. We add specific philosophical purposes, because key advocates of the ORV (e.g., Carnap and Hempel) mainly limited philosophical analysis to logical analysis. For them, logical means "formal logic" or "logical syntax" (as Carnap called it), or "mathematical logic" (Giere 1988, p. xv). However, at the same time, for example, an *MISQ* editorial educates the IS audience that "there is general agreement about what a theory is (e.g., "a statement of relations among concepts within a boundary set of assumptions and constraints"

[Bacharach 1989, p. 496]) (Rivard 2014, p. vii). This "general agreement" seems to be influenced by the ORV. If general agreement on theory in IS is based on major ORV theses, then there is risk that the general agreement offers a flawed picture of scientific theory and research. For example, a trained philosopher should know that strictly imposing ORV concepts on science would seriously hinder scientific progress. The concern is that many IS authors may truly believe that the many ORV characteristics present genuine scientific theories, educate their students about such concepts, and impose these concepts as reviewers or editors. As we will point out, some *MISQ* editorials seem to implicitly or explicitly echo some thesis containing ORV assumptions. We are led to the conjecture that some logical empiricists' (and perhaps Popper's) logical reconstructions for philosophical purposes—the ORV—not only have led us astray but also have the potential to block important scientific research, which does not fulfill ORV theory ideas. Today, the ORV (as originally presented) is widely rejected as 1) representing actual theory used by scientists or scientific research, and 2) as logical reconstructions for philosophical purposes. But what are some of the rejected ORV theses, and how are they visible in IS? This is what we discuss next.

The Influence of the Once Received View Thesis in IS

In this section, we show how some influential IS theory accounts are similar to the ORV, and how ORV accounts, as they were originally presented, misrepresented actual science.

On the ORV and IS Theory Accounts

The paper is not a complete treatment of ORV or theory in IS and many details are omitted. Furthermore, we do not suggest that all IS theory accounts, even those mainly influenced by the ORV, are committed to all aspects of the ORV. (Length restrictions do not allow us to discuss each IS paper at this level of detail.). It is also possible that some theory accounts in IS are not primarily influenced by the ORV. Moreover, the ORV is not a fully homogeneous doctrine. Similarly, certain different IS theory accounts may be largely influenced by the ORV, although they may implicitly highlight somewhat different theory characteristics. For example, Weber (2003, 2012) highlights laws, while, for instance, Tan et al. (2008) do not mention laws. In addition, for some IS theory papers (e.g., Tan et al. 2008), the ORV influence seems to be through OMR literature. The influence of social scientists that are inspired by the ORV (e.g., Merton and Dubin) may also explain some of the ORV influences in IS. In addition, some IS theory accounts contain elements that are not ORV theses, although they display some OMR influence (e.g., Merton's middle-range theory). Some IS theory accounts contain elements that seem to require, in a way, more than the ORV required (see the why questions later). Examining such issues, albeit important, is beyond the scope of this paper. Finally, the possible misunderstandings of some ORV ideas by OMR/IS theory accounts cannot be discussed in this paper. Next, we describe some famous ORV theses, and how they are visible in IS.

The Main or the Only Vehicle of Scientific Knowledge Is Theories

According to the ORV, the most important task in science is to develop and test theories (Suppe 1972). For example, Popper (1934/1959) noted that "[t]he work of the scientist consists in putting forward and testing theories" (p. 31), and "the empirical sciences are systems of theories" (p. 37). In addition, Suppe (1972) was once certain that "if any problem in the philosophy of science justifiably can be claimed the most central or important, it is that of the nature and structure of scientific theories" (p. 3). He maintained that "theories are the vehicle of scientific knowledge and one way or another become involved in most aspects of the scientific enterprise" (Suppe 1972, p. 3). Suppe's life work was theory and theory structure (see Suppe 1977). Yet he later admitted that what he stated above was simply wrong: "Don't believe it for a moment! Today much of science is atheoretical, as it was then" (Suppe 2000, p. 109). Suppe (2000) concluded that theories are not the main vehicle of scientific knowledge. It is difficult to find many writers in the so-called best (AIS basket) journals who have questioned that theories are the main vehicle for scientific knowledge. For example, Avison and Malaurent (2014a p. 327) who reported "the theory fetish in information systems" research, quickly added that they do not suggest "atheoretical or theory free research. To us it suggests an anti-theoretical stance which we do not share. We are arguing here for theory light research" (Avison and Malaurent 2014b, p. 359).

Weber (2003, p. iii) reports that "most research articles published in *MIS Quarterly* have proposed a theory." Therefore, it may be surprising to some IS readers, albeit not for natural scientists, how rarely the prestigious natural scientific journals contain articles that actually test or propose a (specific) theory. Some editors of IS journals have replied to us that such articles are from immature sciences. (Such statements are understandable, if one's philosophical understanding stems from ORV.) However, proposing (or testing) a theory is hardly a maturity criterion. Making this case is straightforward. Who wants to deem contemporary cancer research (in biology, biochemistry, and medical oncology), presented in the most prestigious journals, immature? *Cell* is arguably the most prestigious journal for cancer biology and biochemistry. From January 2012 to January 2017, the journal published 990 articles that examined cancers in biology and biochemistry. When looking at these 990 articles, only 14 mention a specific theory at all anywhere in the article. None of these articles specifically test a specific (or named) theory. None of these articles propose a new theory. Let us move to medical research. The *New England Journal of Medicine* (NEJM) published 985 cancer-related articles between January 2012 and January 2017. None of these studies actually tested a specific (named) theory. No new theories were proposed.

Consider the claim that "developing theory is what we are meant to do as academic researchers and it sets us apart from practitioners and consultants" (Gregor 2006, p. 613; Gregor 2014). Assessing this claim requires one to say what counts as "developing theory," and showing that "academics" engage in the activity, while practitioners and consultants do not. Length restrictions allow us to consider just one possible interpretation of this claim. If one interprets the claim as developing a theory means proposing a specific theory or named theory, then most of the cancer biology and biochemistry or medical oncology scholars—even those who publish in the best journals (such as *Cell* and *NEJM*)—are "practitioners and consultants," and not "academic researchers" (cf., Gregor 2006, p. 613). Moreover, according to this specific interpretation, they are not doing what they "are meant to do as academics." This is because most of them never develop a theory, according to their own words. This interpretation does not sound convincing. We should not seriously believe that cancer biologists publishing in *Cell* are "practitioners and consultants," and not "academic researchers," simply because they are not developing theories, according to their own words. If most scientists, generally speaking, are not testing specific (named) theories, and are not proposing new theories, in the most prestigious scientific journals, then what are they doing? That is an issue which philosophy of science has tried to describe during the last 60 years or so. Suppe (2000) claims, "Today, models are the main vehicle of scientific knowledge" (p. 109). Van Benthem (2012) reports that heavy use of isolation characterizes science. What they refer to are complex issues, but we provide an answer later. Before that, we need to understand more about the ORV, and its influence in IS.

Scientific Theories Consist of True Laws

The ORV regarded sciences as (mainly) nomothetic. That is, scientific theories contained law(s): "A theory, as the term is actually used, is a set of laws" (Putnam 1974, p. 125). The major ORV account that explained how things actually happened with nomothetic settings (a world governed by laws) was Hempel and Oppenheim's (1948) nomological-deductive (D-N) model. Popper (1934/1959) outlined similar ideas. For this reason, therefore, the deductive-nomological method of explanation is also sometimes called the Popper-Hempel theory of explanation (von Wright 1971). Roughly speaking, the D-N model explained an observation by logically deducing it from laws and the initial conditions (Hempel and Oppenheim 1948).

Given that it was once widely believed that scientific theories contained laws (the ORV), it is understandable that IS authors also refer to a 'law' and its synonyms (e.g., nomothetic, nomology, and nomological). We provide several examples of influential IS sources. According to one source, "the theory we seek to build in essence is an attempt to articulate *a law*" (Weber 2003, p. iv, emphasis original). The most-cited design science article separates two research streams, behavioral science and design science. The former "seeks to develop and justify theories (i.e., principles and laws)" (Hevner et al. 2004, p. 76). For the latter, "laws are crucial components of design-science research" (Hevner 2004, p. 88). Orlikowski and Baroudi (1991, p. 10) assign "universal laws" to positivistic IS scholars. As a final example, Bhattacherjee (2012) reports that "[s]cientific knowledge refers to a generalized body of laws" (Bhattacherjee 2012, p. 2), and "the goal of scientific research is to discover laws" (Bhattacherjee 2012, p. 3).

It is worth emphasizing that the standard meaning of 'law' is universal, i.e., exceptionless (Craver 2008): "Traditionally, the word 'laws' has been reserved for universally applicable, exceptionless generalizations"

(Teller 2004, p. 731). The case exemplar for laws was "Newton's law of gravitation, originally claimed to apply to all bodies through the universe at all times" (Darden 1996, p. 409). No such laws exist in IS. For instance, how can statistical statements (which obviously are not 100% exceptionless) be regarded as (universal) laws? Hempel, for example, explains in various writings how probabilistic or statistical hypotheses cannot be regarded as D-N (exceptionless) laws (e.g., Hempel 1966, p. 54). Despite 1) this and 2) Hempel's (1965, p. 412) warning that D-N does not describe how scientists even in physics "actually" explain, it is implied that IS positivists follow D-N (Evaristo and Karahanna 1997). Realizing the difference between 1) D-N laws or Popperian laws and 2) statistical statements is important. With laws, the results do not vary from one setting to another, or from one individual to another. If the results vary, then the law is not a true law. The generalizability of statistical statements, let alone qualitative studies, is different. Their applicability is expected to vary case by case basis at the individual level.

Another a common criticism of the ORV is that it simplifies actual science (Burian 1975). For example, *MISQ* editorial statements, such as, "No prizes are awarded for being second to discover a scientific law" (Rai 2017, p. vi), may hint that only new laws are primarily valued (and the tests of the laws are not valued). Such statements can lead to misunderstandings. Importantly, most papers, even in the best natural science journals (even in physics), do not present a new law (or new theory). Value research does not end by proposing a law. Natural scientists can easily devote several (even hundreds of) years to studies that test various aspects of the law. For example, "the main elaboration of the Newtonian theory took about 200 years" (Burian 1977, pp. 37–38). Einstein's theory of general relativity was published in 1919, and in 2018, physicists still reported confirming an aspect (Abuter et al. 2018). Moreover, "theories, like people, are born, grow, develop, adapt to new circumstances, have powers which wax and wane, etc." (Burian 1975, p. 6). Neither the idea that only new laws are valued nor the idea that one paper has to present a new theoretical contribution can do justice to these cases.

Furthermore, the ORV's idea of true universal law was challenged. Philosophers of life sciences reported that the best candidates for laws in life sciences, including Darwin's theory of evolution, could not constitute laws (Beatty 1980). Later, philosophers of biology reported that laws have "few, if any, applications in neurobiology or molecular biology" (Machamer et al. 2000, p. 7). Instead, "a satisfactory explanation requires providing a description of a mechanism" (Machamer et al. 2000, p. 1). (For Machamer et al. (2000), laws are not mechanisms; see Craver 2008). The ORV laws were also challenged in physics. "Newton and his followers in the eighteenth and nineteenth centuries…regarded…laws as general truths about the world" (Giere 1988, p. 76), but many philosophers realized in the 1960s and onward that this is not the case. Scriven (1961) noted that "the most interesting fact about laws of nature is that they are virtually all known to be in error" (p. 91). Later, Cartwright (1983) famously reported how even the fundamental laws of physics either 1) make true claims, which apply only in highly idealized counterfactual settings, or 2) make false claims about how things are in actual settings. Why is this the case? We will discuss this point soon, but first, we explain the way in which many IS theories are presented.

The ORV as a Statement View

According to the ORV, "theories systematize phenomena by exhibiting deductive and inductive inferential relations among their descriptive terms" (Craver 2008, p. 56). The ORV is an axiomatic system, and deductive, and contains linguistic structures (Craver 2008). As mentioned, Reichenbach's (1949) logical analysis "is concerned with logical relationships alone" (p. 293). In OMR, Bacharach (1989) views "a theory as a statement of relations among concepts," which is "no more than linguistic device" (p. 496). Then a theory is "a system of constructs and variables" (Bacharach 1989, p. 498). Gregor (2006) talks about "relationships among the constructs. These may be of many types: associative, compositional, unidirectional, bidirectional, conditional, or causal" (p. 620). More generally, many IS theory accounts seem to view theories as relationships between the *explanans* (plural *explanantia*) and the *explanandum*. The D-N model regards explanation and prediction as *explanans* and *explanandum* relationships. For example, in the case of the technology acceptance model (TAM), ease of use is an explanans, while IT use is the explanandum. Often, explanantia in IS are called independent variables (IVs), laws, predictors, propositions, determinants or processes. The explanandum is often a dependent variable (DV). For example, Keen (1980), at the first ICIS, required "the dependent variable," to portray IS as scientific. Gregor (2006) describes IS theories as "statements of relationships" (often hypotheses or propositions) between "primary constructs." Likewise, Weber (2003) notes: "The theory we seek to build in essence is an attempt to articulate a *law* (or less formally an association or statement) that relates the value of two components" (p. iv, emphasis original). Finally, Tan et al. (2008) report that most IS theories "are attempts to build justified and valid knowledge claims that seek to explain causally why something occurred by means of an outcome, criterion, or dependent variable in the context of specific conditions that are captured as a set of antecedent variables denoted as independent or mediating variables" (p. 41).

How about theory contextualization (Hong et al. 2014)? We cannot discuss Hong et al. (2014) in detail here. According to one interpretation, Hong et al. (2014) add "context" to the "statements of relationships". Other ORV similarities also exist. For example, it has been widely reported since the 1960s that scientific theories contain various *assumptions* (that many of them are purposefully false). Generally, ORV accounts either missed or downplayed the role of these assumptions. The underlying assumptions of a theory do not have role in Hong et al.'s (2014) work. Moreover, the first principle is "grounded in a general theory," namely, that "context-specific research could be built on a general theory" (Hong et al. 2014, p. 117). Although the Hong et al. (2014) work is not the D-N model of explanation, some similarities are notable. As we understand Hong et al. (2014), using D-N terms, contextualized theories seem to be subsumed by "general theories." The idea that specific or contextualized theories are built on general theories seems to tacitly assume a reductionist and a nomothetic D-N worldview (the world is governed by laws). It is not clear how non-nomothetic accounts, such as mechanism-based explanations, fit here. Hong et al. (2014) say that "contextualization may serve as the starting point of new universal theories" (p. 114). Universal is 100% exceptionless. While the ORV assumed that theories are universal, theories based on statistical hypotheses or statistical generalizations are difficult to depict as universal theories.

Viewing theories as consisting of "statements of relationships" is understandable, if the major source for theory is the ORV. The ORV is commonly referred to as the "statement view" of theories (Giere 1994, p. 276). Unfortunately, ORV accounts generally misrepresent or neglect important aspects of scientific theory, such as dynamics or complex reticulated connections, because the ORV depicted theories as static (Craver 2008). Moreover, "the theory must be represented physically in some way" (Gregor 2006, p. 620). In many contemporary science journals (pick, say, *Cell* or *NEJM*), not one paper presents the theory (as whole). In IS, an *MISQ* editorial reports how most *MISQ* papers "have proposed a theory and then tested it either in whole or in part" (Weber 2003, p. iii). IS readers may further have inferred that theories lacking "rigorous specification of all their parts" in fact "do not constitute theories" (Weber 2012, p. 6). However, some textbooks in the philosophy of science inform us that modern scientific theories are rarely presented as a whole; "[r]epresentations of theories in the wild are also often partial or incomplete" (Craver 2008, p. 59). The requirement of the "rigorous specification" of all parts also has difficulties. For example, Reichenbach (1949) explains how Einstein "left his theory open to misunderstandings and erroneous interpretations" (p. 291). Assuming that Reichenbach (1949) was correct, one might argue Einstein's theory of relativity did not meet the criterion of rigorous specification of all its parts. Despite that, Einstein's theory of relativity is widely viewed to constitute a theory. Moreover, the idea that IS theories are sometimes presented as a whole (Weber 2003, p. iii) may hint that the theories themselves are static. However, "a theory is a growing, developing entity, one which cannot be understood as a static structure" (Burian 1977, p. 35). Craver (2008) explains how scientific "theories are also frequently incomplete as they are cobbled together over time" (p. 59). Newton, for example, presented various versions of his theory, "differing not only in their structure and in their known empirical consequences, but also in their implicit empirical consequences—most obviously in the specialized force laws which they allow and in the range of cases which they comprehend" (Burian 1977, pp. 37–38). Finally, some IS theory accounts, which imply that theories are presented as a whole (e.g., Weber 2012), are at odds with science proceeding with the isolation that is advocated as a hallmark of modern science (van Benthem 2012, p. 784). The method of isolation means, for example, that one research stream specializes or focuses on a highly narrow aspect of the 'theory' (or some phenomenon). We discuss isolation later.

Scientific Theories Describe a Real Phenomenon, and They Are Evaluated (Accepted or Rejected) Against Real-World Observations

The ORV held that theories described actual things in the real world, and theories were true or false in comparison to real-world observations, or "feature[s] of the world" (Beatty 1980, p. 72; Craver 2008; Suppe 1972). As Gregor (2006) describes, "Popper…held that theorizing, in part, involves the specification of universal statements in a form that enables them to be tested against observations of what occurs in the real world" (pp. 614–615). Many influential IS sources report, similarly to the ORV, that scientific theories correspond to real phenomena as such. To start, "a theory is an account that is intended to explain or predict some phenomena that we perceive in the world" (Weber 2003, p. iv). He maintains that "[t]heories provide a representation of someone's perceptions of how a subset of real-world phenomena should be described" (p. 3). "By theory, I mean a particular kind of model that is intended to account for some subset of phenomena in the real world" (Weber 2012, p. 4). "By phenomena, I mean someone's perceptions of facts in the real world" (Weber 2012, p. 4). Similarly, in a seminal article, "an MIS case study refers to the examination of a real-world MIS as it actually exists in its natural, real-word setting" (Lee 1989, p. 34). Weber and Lee advocate a natural science perspective. For example, Lee (1989) claims that his idea meets "the standard of the natural science model" (p. 34), while Weber's (2003, p. iii) account is influenced by physics. Moreover, a seminal article reports how positivists (assumed to present the natural science view) believed "that there is a one-to-one correspondence between the constructs of a researcher's model and the events, objects, or features of interest in the world" (Orlikowski and Baroudi 1991, p. 9). As a final example, "[i]t is not theory – until it…explains the…observed connections in the proposed elements of the theory *(and world)*" (Tan et al. 2008).

The IS views are understandable, if one's understanding of the philosophy of science is based on logical empiricists (e.g., Hempel) or Popper, until the 1960s. However, since the 1960s, there has been a realization that countless exemplars of scientific theories do not, after all, apply (without modification) to the real world. For example, Giere (1988, p. 90) reports that "general laws, such as Newton's law of motion…are not really statements about the world." Moreover, "when one examines physical theory and considers the development of the theorizing in a certain field of enquiry, one is struck by the fact that the theories of physics do not seem to be about the world at all" (Harre 1996, p. 223). Similarly, Cartwright (2016) argues that "scientific principles…do not describe what actually happens" (p. 335).

Moreover, Wayne (2011) notes how an "explanation in physics relies essentially on idealizations (idealized models) of physical systems, and the explanations themselves contain false statements about the both the explanatory relevant features of the physical system and the phenomenon to be explained" (Wayne 2011, p. 831). As a concrete example, Boyle's law of gases is true only for ideal gases (Scriven 1961). Similarly, as Suppe (1972) notes, "the gas laws (e.g. Boyle's law and Charles' law) describe the behavior of ideal gases, not real gases" (p. 12). Ideal gases do not have a one-to-one correspondence to any real gas in the world. Moreover, "Newton's first law…refers to what happens to a body that is subject to no external forces, but there are probably no such bodies" (Ladyman 2013, p. 358). As a result, "there is not a good fit between the claims made by the laws of science and happenings in the world" (Chalmers 1993). The simplest law describes the simplest things falsely: "falling objects, such as Autumn leaves, rarely descend to the ground in a way that conforms to Galileo's law of fall" (Chalmers 1993, p. 196). Finally, in modern economics, "models involve idealizing assumptions that are strictly false." (Mäki 2002 p. 11). For example, some economic models have the assumption of perfect knowledge, which "is of course false of all actual consumers or producers; it is indeed true of nothing actual." (Pemberton 2005 p. 36).

According to an IS source, "models use constructs to represent the real-world contexts of the design problem and solution spaces" (Baskerville et al. 2018, p. 362). However, in sciences, a model is "a specially prepared, usually fictional description of the [real] system under study" (Cartwright 1983, pp. 158–159). Moreover, "the word 'model'" suggests "the failure of exact correspondence with reality" (ibid.). Similarly, Chalmers (1993) reports that "[t]he models that our theories are able to handle are deliberate falsifications of reality." and "the theory may then accurately describe the workings of the model, but the model does not describe the phenomena" (p. 200).

To recap, many IS theory accounts assume (perhaps owing to the ORV) that scientific theories or models correspond to the real world, when, in fact, scientific theories or models hold in some isolated and idealized data, which contain purposeful falsifications of the (assumed) real phenomenon (Cartwright 1983; Chalmers 1993; Giere 1988). Giere (1998) claims that "an adequate theory of science must reflect this fact [e.g., understanding that theories have purposefully false assumptions] in its most basic concepts" (p. 78).

Guarding Against Several Basic Misunderstandings

It is important to guard against possible misunderstandings. First, we have not said that scientists never use the term theory, or that "theory" has no importance in sciences. Scientists certainly have used the term "theory." We question the idea that theory is always the main vehicle of scientific knowledge (Suppe 2000). Moreover, if one examines some of the biggest names in the history of physics, then one may conclude that they proposed theories. For example, Popper's "scientific hero" was Einstein, whom Popper cited more than 100 times. However, if one reads the best journals in natural sciences or medical research, such as *Cell* or *New England Journal of Medicine*, most of the articles do not propose new theories. We can do good science without a reference to named theories, and without proposing a new theory. The 'theory,' and theoretical contribution (see below), may be less important in sciences than many IS authors believe. Second, the ORV is not about quantitative or qualitative research. As mentioned, many ORV accounts had difficulties accounting for statistical studies, because they viewed theories as consisting of exceptionless laws. Third, why do we use natural science and medical research examples? First, all the theory concepts discussed in this paper, and actually, many other philosophical concepts alluded to in IS, originate from philosophers who were philosophizing about natural sciences. For example, what *MISQ* editorials (such as Rivard 2014) regard as a "general agreement about what a theory is" refers to Bacharach's (1989) work, which is based on the work of ORV scholars; who, in turn, philosophize the logic or theory structures of natural sciences. Similarly, many IS authors allude to universal theories (e.g., Hong et al. 2014), and it is questionable to what extent truly universal theories exist outside physics. Furthermore, some former or current EICs of *MISQ* refer to laws, as do many other influential IS authors (e.g., Bhattacherjee 2012; Hevner et al. 2004). Or it is claimed that most IS research follows the deductive-nomological model of explanations (Evaristo and Karahanna 1997). All of these concepts stem from the philosophy of natural sciences, especially physics. Discussing these concepts benefits from understanding their origins in natural science. Second, our argument amounts to the following. The concepts we discuss in this paper come from professional philosophers' logical reconstructions of physics, but (as reported in this paper) they would be questioned, even in physics, let alone in life sciences (e.g., in cancer biology). For example, these ORV views make laws (of physics) appear much simpler and more generalizable than they actually are in physics. Furthermore, Popper (like so many of his contemporaries, especially in the 1930 and 1940s) apparently misunderstood that physics theories contain true universal law(s). We should not believe that the natural sciences or IS phenomena are that simple. More precisely, ORV-influenced concepts may confuse, or hinder, some general good scientific practices, which we discuss next.

How Rejecting Some of the Key Theses of the ORV Helps IS Research

Thus far, we have shown that many 'theory' accounts are problematic. Next, we highlight important debates in the philosophy of science and IS, and how the modern philosophy of science, followed by the ORV, can shed new light on many important IS debates.

From Theories Accepted or Rejected in Real Settings to Idealized Models

In the 1970s, the semantic 'theory' view (which followed the ORV) championed that idea that "theories do not consist of empirical claims, much less laws of nature" (Beatty 1980, p. 400). The point with the "empirical" is that theories are not necessarily tested against real-world observations. Theories are tested against a model or some data, which, generally speaking, fail to have "exact correspondence with reality" (Cartwright 1983, p. 159). As Chalmers (1993) notes, "it is correct to say that our theories are applied to models rather than to descriptions of real situations" (p. 202). Moreover, the role models for theories or models, whether we look at physics, economics, molecular biology, either 1) purposefully omit important difference makers in the model or 2) purposefully add falsehoods (e.g., a perfect knowledge in some economic theories) that do not exist in real-world settings. This practice is well-known and perhaps necessary, yet IS theory literature seems to be silent about it. Let us take Nagel's (1961) *structure of science* as an example, because it is cited by many influential sources that inspire IS, such as Bacharach (1989) and Gregor (2006). Nagel (1961) reports the following that is not discussed by Bacharach (1989) and Gregor (2006): "It is common if not normal for a theory to be formulated in terms of *ideal concepts* such as the geometrical ones of straight line and circle, or more specifically physical ones of instantaneous velocity, perfect vacuum, infinitely slow expansion, perfect elasticity, and the like" (p. 131). Ideal concepts do not exist in the assumed "real" world. Modern philosophy of science widely regards these "ideal concepts" as purposefully "simplifying false-hoods" (Levy and Bechtel 2013, p. 243).

For the last 60 years or so, philosophy of science has tried to understand such ideal concepts, and the reasons cannot be discussed in one article. One reason is that virtually in all "empirical" sciences, the simplest real phenomenon is simply too complex and dynamic for any theory or model to handle, without omission of difference makers, simplifications, and falsehoods (Chalmers 1993; McMullin 1985). One other rationale for such activity is because research methodological treatment of the complex phenomenon is not possible without the introduction of deliberate falsehoods or simplifications, which may misrepresent the actual phenomenon (McMullin 1985). Provided that complexity or research methodological treatments are the chief reason for deliberately false assumptions in natural sciences, then social science and IS models or theories may also contain falsehoods. For example, virtually any actual phenomenon in biochemistry may contain several hundreds of dynamically changing connections between entities and activities; yet no model can cope with this. This could be also the case in IS. Popperian or logical empiricists' science can lead some of us to believe that an IS phenomenon is simple, when it may not be. It is easy to see examples. A recent *MISQ* editorial refers to "well-established theories or models" (Rai 2017, p. vi), while others (e.g., Weber 2012, p. 6) seem to require that theories should not be called as such unless they are complete. But a reader of modern philosophy of science learns that "[e]very theory we have proposed in physics, even at the time when it was most firmly entrenched, was known to be deficient in specific and detailed ways" (Cartwright 1980, p. 160). Or consider: "all our current best theories, including General Relativity and the Standard Mode of particle physics, are too flawed and ill-understood to be mistaken for anything close to Final Theory" (Hoefer 2016). If one critically reviews the assumptions underlying the theories used in IS, one may realize that many assumptions are purposefully false, if the results are expected to be applied in actual settings (and not in counterfactual conditions).

A task remains for IS philosophy: to unveil which deliberate falsehoods are added to the models, theories, and methods used in IS. Another important task is to introduce the method of isolation to IS. We argue that this a major reason why in indeterministic sciences (e.g., molecular biology or cancer research), theory does not commonly appear in a single paper, and has mainly a pedagogical purpose. Modern science has increased the depth and complexity, so that a "simple theory" contains hundreds of complex and reticulated mechanisms. To reach such complexity, scientists need to focus on a mechanism, or its subpart, in a single study. This is called isolation. For example, the second author has examined, for example, the P450 enzymes in one type of cancer. As the name indicates, there are hundreds of P450 enzymes, and hundreds of different cancers. The same approach could be relevant in IS (with certain necessarily modifications).

What if There Are No True Laws in IS

Consider what an *MISQ* editorial claims to be type III errors: "The answer to the Question Is Derivative to Current Understanding...paper as taking what is well known and reiterating it in a different context. Some of our editors refer to this type of paper as 'affirming that gravity works in my kitchen.'...Reevaluating well-established theories or models in different contexts and presenting evidence that further affirms the validity of the theory or model do not represent scholarly contributions at the level expected at MISQ" (Rai 2017, p. vi). This problem is referred to as "affirming that gravity works in my kitchen" (Rai 2017, p. vi). This "policy" would be understandable (with certain reservations) with laws. If the world is governed by laws that are true at all times and in all places, then one might believe that there could be a law that exists everywhere, including one's kitchen. The deductive-nomological model (Evaristo and Karahanna 1997) can mislead us in that direction (Cartwright 1979). However, many philosophers of physics have questioned such laws, even in physics: "general laws...are not really statements about the world" (Giere 1988, p. 90). Cartwright (1980) claims that laws tend to hold (without modifications), only under very special (ideal) conditions, such as a perfect vacuum.

When we expect that a phenomenon is not governed by laws, this *MISQ* policy becomes problematic. We give a medical research example, because we assume readers are familiar with medical treatments and drugs. Typically, it takes 10 to 15 years to develop a cancer drug (Djulbegovic and Guatt 2017). And despite all these efforts, there are countless examples of treatments that were approved as a treatment for one purpose, and then were reevaluated for another purpose (and approved for another purpose), and later rejected for the first purpose. Or in some cases, the treatments were later rejected for all medical purposes. Let us move to a concrete example (Zhou et al. 2013). (1) Thalidomide was first introduced to prevent morning sickness in pregnant women in the 1950s–1960s (Zhou et al. 2013). Unexpectedly, (2) the drug had deleterious side effects, and more than 10,000 babies were born with severe limb malformations and congenital defects of the kidneys, ears, eyes, and heart (ibid.). The drug was taken off the market. Later, (3) thalidomide turned out to be an effective treatment for another disease (*erythema nodosum leprosum*), establishing a new indication for the use of this drug (ibid.). Since then, thalidomide, or its variation, has been studied in the context of various different diseases, with varying results (ibid.).

To summarize, the curious story that theories become "well established," and then apply so uniformly across contexts that further tests in different contexts are not valued (Rai 2017), seems to be influenced by a nomological worldview (utopian laws). Reality is different. An (empirical) phenomenon is too complex for theories or models to handle. To cope with the complexity, a scientific theory or model simplifies the phenomenon (e.g., so that it can be studied statistically or using some other methods), which typically results in mispresenting the phenomenon. As a result, in real (complex and dynamic) settings, scientific theories or models are expected to face anomalies, and varying success. To simplify a complex story, an important approach (used in life sciences) is to build various models for each 'theory' or approach. The importance is not to "validate" them with singular studies, let alone one-time studies. It is important to view that each model or theory has a track record. It consists of all tests of the theory through models. Instead of asking whether a theory is "well established" or "validated" (Rai 2017), we should examine what the track record tells us. These numerous reruns in different contexts and replications set the track record for each theory or model. If there are no 100% deterministic laws, then the track records are expected to vary within the same context and across contexts. How much do the track records vary? Well, this is precisely what the reruns and replications in different contexts are expected to tell us. The reruns also tell the limits of our theories or models. Alas, there is a risk that if replication is not valued (cf., Rai 2017), then the accumulation of this track record is hindered.

What Is a Theory Is Not an a Priori Issue, but Largely an a Posteriori Matter

As Hartmann (1998) reports, "it turns out to be difficult to exactly define what a model is and how it ought to be distinguished from a theory. Quite often, the terms are used interchangeably by scientists. The term 'model' is, however, frequently preferred. Scientists tend to be reluctant to name their constructs a theory" (p. 101). One can examine such top natural science journals as *Cell* and *Nature* in vain to find articles describing what is the structure of the theory. Yet we learn from philosophers that a 'theory' or 'model' has been shifted over the years (Laudan 1977). Moreover, the semantic 'theory' view (which followed the ORV) holds that "any language will do for describing exactly what the models are like" (Cartwright 2006). Scientists have even used cartoons (Craver 2008).

One can find IS papers explaining what a strong theory is (e.g., Tan et al. 2008). There is a problem if we (in IS) define the characteristics of a good, high-quality, or strong theory *a priori,* and then the accepted papers have to meet the criteria, so that they are strong or good theories. By *a priori,* we mean that a strong theory is defined independent of the research context and the research findings. However, "[i]t is not the job of a theory of science to legislate, a priori, the form scientific reasoning must take" (Giere 1988, p. 96). We take, as a key problem, by defining an accepted theory *a priori*, that the target phenomenon (or data) and the specific research goals do not define a theory. If we impose an *a priori* defined strong or good theory, then any phenomenon or data, and various research goals, have to be presented so that they meet the *a priori* defined "strong theory." For example, if a theory has to be presented as IV and DV relationships, or statements of inferential relationships, then many mechanism-based explanations cannot be adequately presented (Craver 2008). Again, if a theory has to be simple, then a complex phenomenon has to simplified, just for the sake of meeting some theory criterion. But this is not what is happening in the natural sciences or medical research. For example, biochemical pathways (Thagard 2003) can contain hundreds of complex parts, interacting with each other. Modern molecular biology and biochemistry would not have been developed if "theories" cannot be complex, and have to be represented as statements of static relationships. This point could be relevant in IS (with certain necessarily modifications). Our point is that what is a theory in a given field is largely an *a posteriori* issue. This means that the data on the phenomenon often "define" what a theory is. If the object of investigation, or what explains it, is complex and dynamic, then the theory can reflect that. However, often the 'theory' or model has to omit some aspects (which may result in misrepresentation of the phenomenon) to manage the complexity.

Reconsidering the Requirement of a New Theoretical Contribution

Not "making a theoretical contribution" is reported be a key reason why papers are rejected in OMR (Hambrick 2007, p. 1346) and in IS "top journals" (Straub 2009, p. vi). What is the link between a "theoretical contribution" in IS and the ORV, if any? The distinction between "theoretical and empirical" is important in the ORV, which Carnap (1936) famously proposed. The basic account is as follows: Theoretical entities are unobservable (e.g., electrons and molecules), while empirical entities are observable (e.g., a blue table). Then, theories are called as such, because in the modern natural sciences, theories often contain theoretical statements, i.e., statements on unobservable(s). (Whether IS theories contain typically unobservables cannot be examined in this paper.) Although distinguishing theoretical from empirical may appear trivial for IS readers, drawing the dichotomy turned out to be a difficult task. The proposed definitions contradicted scientific practice. For example, in a classical attack on the ORV, *What Theories Are not*, Putnam (1962) reports that "the *problem* for which this dichotomy [theoretical/empirical] was invented…does not exist [in actual science]" (p. 241, italics original). Darden (1976) informs us that "as is by now commonplace knowledge, providing a clear distinction between observational and theoretical terms has proved difficult" (p. 243). Suppe (1977) continues: "The distinction [theoretical/empirical] has not been successfully drawn, and what is more cannot be drawn in any plausible way on the basis of ordinary usage of the terms in natural scientific language. The only way the distinction could be drawn is artificially in a reconstructed language" (pp. 86-87). Carnap knew all this well. For the distinction between theoretical and empirical, Carnap used his method of explication, discussed previously, and used artificial languages, rather than actual scientific language used by scientists. As far as we understand, Carnap did not claim that scientists use the distinction. In fact, to our understanding, ORV philosophers, such as Carnap or Hempel, did not claim that a scientific paper must make a (new) "theoretical contribution." If one reads the most prestigious journals of the philosophy of science (e.g., *The Philosophy of Science* or *The British Journal for the Philosophy of Science*), there is virtually nothing written *philosophically* on "theoretical contributions" in the whole history of the journals (a few papers mention it, but they do not provide a philosophical treatment of it). It is an open question why "theoretical contributions" often seem to be valued over "empirical" in IS, which cannot be answered in this paper. (A reply that it is unreservedly copied from OMR does not explain why.)

It is possible that for some IS (or OMR) authors the meaning of *theoretical* is somewhat different from the philosophers the authors cite. Nevertheless, the *theoretical* (in IS/OMR) could be influenced by the ORV. For example, the requirement of "theoretical contributions" in IS implies that pure empirical contributions (lacking "theoretical") are at risk of being unacceptable (e.g., Straub 2009; Tan et al. 2008). This suggests a distinction between theoretical and empirical, which we see in some IS theory accounts (e.g., Tan et al. 2008). What counts as "theoretical" in IS, and how is it different from "empirical"? We first briefly discuss Bacharach (1989), because this work is reported as presenting a "general agreement" on theory in IS (Rivard 2014).

Bacharach takes self-evident that theories consist of "theoretical statements", but he is less clear about what makes statements theoretical. As we understand Bacharach's (1989) view, theoretical statements are not "descriptions" that tend to answer "the question of what" (p. 497, 498). Instead, theoretical statements answer the following questions: "how, why and when" (Bacharach 1989, p. 497, p. 498). In addition, Gregor (2006) holds that a theory that explains "provides an explanation of how, why, and when things happened" (p. 619). These views may appear to have an ORV influence. The major ORV account on explanation starts by declaring that "to explain the phenomena in the world of our experience, to answer the question 'why?' rather than only the question "what?"" (Hempel and Oppenheim 1948, p. 135). Hempel maintains that scientific explanations should "go beyond a mere description of its subject matter" (ibid). Despite some similarities, key ORV philosophers (e.g., Hempel) would have deemed requiring all "how, why *and* when" restricting even in physics. Other concerns also arise. To what extent do IS theories, which explain (or explain and predict), satisfy "how, why, and when"? To give a simple example, "ease of use" may not explain "how" or "when" IT is used. Does ease of use answer the question of "why" IT is used? If we now answer, "because it is easy to use," this answer, we take it, is not a satisfactory answer for why questions. The point is not only that the ORV's why questions require pointing to a law (Hempel and Oppenheim 1948), which ease of use is not. Whether it is a law or not, we should like to know what it is about the ease of use that make users use IT. We would also like to know what system characteristics constitute "ease of use" (the objection is modified from von Wright 1971). It could turn out that many influential IS theories or models do not meet the criterion of "theoretical" in terms of "why"; not to mention "how, why, and when." This is not a critique of IS research. We are complaining about imposing philosophical concepts on scientific use uncritically. Other difficulties exist. For example, Tan et al. (2018) call for "a strong theoretical logic," which they separated from "empirical" and "brute empiricism" (Tan et al. 2008, p. 42). What is the strong theoretical logic? One interpretation is the following:

Strong theory [and assumingly "a strong theoretical logic"] demands deeper analysis of why and how the propositions connect as a whole.... The essence of theorizing is not discovering significant correlations, but finding a deeper explanation. It is often the "why" behind the ascribed relationships that is bereft of deeper thinking. (Tan et al. 2008, p. 42)

As we understand Tan et al.'s (2008) view, a strong theoretical logic includes answers to why questions, while "empirical" logic and "brute empiricism" (e.g., statistical results) do not. For the sake of the argument, let us omit all the difficulties mentioned, and let us just assume that theoretical logic includes answering "the why behind ascribed relationships" (ibid., p. 42). But even in this case, requiring a new theoretical contribution by each study may prevent good science. Due to length restrictions, we can give only four types of examples. First, studies that compare the effect of two (or more) previously proposed intervention approaches may not contain a new theoretical contribution, and could be classified as "brute empiricism." However, this is good science. For instance, medical researchers want to know whether treatment X is better than Y in treating different cancers, albeit such a study may not constitute a new theoretical contribution in terms of "why seeking" explanations (or predictions). Design science, actions research, or IS experiments can use the same idea to see if a certain approach, intervention, or design is better than another. Second, combination studies are highly important in science, albeit they may not constitute a new theory or theoretical contribution. They can be classified as "brute empiricism." To give an example, diffuse large B-cell lymphoma (DLBCL) is a lymphoma. Generally, combining rituximab and chemotherapy for treating DLBCL is more effective than chemotherapy treatment alone (Coiffier et al. 2002). There is (perhaps) no new theory contribution, but such studies are valuable. Again, design science, actions research, or IS experiments can use the same idea.

Third, showing that something does not explain, or work well, may not provide a new theoretical contribution if theoretical means, among other things, a why-seeking explanation. For example, showing that (say) the TAM or UTAUT fails to explain computer game use may not contain a new theoretical explanation. However, this is good science. Perhaps the easiest-to-understand examples are, again, one type of study of medical treatments, whose only purpose is to show that a certain treatment does not work with certain diseases. Fourth, countless contributions or shifts in scientific thinking were due to "empirical," or "brute empiricism," rather than "theoretical," reasons. For example, breast cancer and gastric cancer are "theoretically" histologically adenocarcinomas. The former has a certain cancer growth–promoting, called HER2, signaling pathway. Trastuzumab is an antibody that inhibits the HER2 signaling pathway, and it is used as a treatment. Then it was found that HER2 signaling is also sometimes active in gastric cancers. However, trastuzumab gives different results for breast cancer and gastric cancers, although both are adenocarcinomas with HER2 signaling (Bang et al. 2010). The contribution here is not "theoretical," but instead, "empirical" or "brute empiricism"; yet this is good science. One may object to these cases by saying that theoretical reasoning (e.g., answering why-seeking questions) applies to *only* theories or studies that explain (Tan et al. 2008, for instance, seem to refer to theories that explain). First, even if we accept this idea for the sake of the argument, then we should reconsider following the statements to apply only theories that explain: "it is the evaluation of the theoretical contribution that most reviewers become convinced, or not" (Straub 2009, p. vi). Second, the objection fails in the sense that it cannot explain how "brute empiricism" (cf., Tan et al. 2008) has, in many cases, led to the improvement of theories that either 1) explain or 2) explain and predict. For example, in IS security, Rogers' protection motivation theory (PMT) is widely used. Originally, Rogers (1975) theorized a number of things, such as multiplicative relationships. However, as the statistical results did not support these relationships, Rogers (1983) omitted the multiplicative relationship assumption. It is difficult to portray this as a new theoretical contribution, as discussed above. It seems that the PMT from 1975 to 1983 seems to have been revised for reasons including "brute empiricism," as the change was due to the statistical results.

Although there is a need to analyze what is “theoretical” in IS, our first proposal is that the strict requirement for a new theoretical contribution should be loosened in the case of empirical IS research. There is also a risk that a theoretical contribution becomes valued intrinsically as the end itself in IS. However, in empirical sciences, perhaps the theoretical has mainly an instrumental role in serving higher aims, which could be related to explanation or prediction. It may turn out that achieving these aims does not always require new theoretical contributions. In fact, requiring such may hinder these objectives. For example, generally, scholars in medical oncology do not require new theoretical contributions. Scholars ask about better improvement effect rates (“prediction accuracy”). If another treatment can give a better average treatment effect with fewer side effects, then this is an important contribution. The point is to improve the average treatment effect rates, not to make a new theoretical contribution. A “new theoretical contribution” is valuable only insofar as it leads to an improved treatment effect rate. Generally, a similar approach (with certain necessarily modifications) works in IS research that uses some kind of intervention, for example, through action research, design science, or experiments. For example, IS security behavioral research can also ask whether the treatments (e.g., online training or email campaigns for improving password behavior) provide better treatment effect rates than those currently offered by the existing research. Imposing the requirement of a “new theoretical contribution” can prevent such research.

Conclusion

We argue that many of our beliefs about (1) what is a scientific theory, (2) and its role in science, stem from the philosophy around 1930–1960s, known as the ORV. Generally, ORV theses are professional philosophers’ logical reconstructions of scientific research, aimed at analyzing scientific theories in terms of logical relationships alone. Key ORV advocates (e.g., Reichenbach, Hempel, and Carnap) did not require scientists to formulate their theories in line with these logical reconstructions. However, it is precisely these logical reconstructions for professional philosophers’ purposes that some IS and OMR scholars deem the source for a good or strong scientific theory. As a result, what are sometimes commonly regarded as “generally accepted” characteristics of theory in IS are based on philosophers’ simplifications, and in some cases, misunderstandings of the philosophy of physics. These professional philosophers’ simplifications are widely rejected, as they have been presented, in the philosophy of science, as they seem to misunderstand theories (e.g., laws) in physics, let alone in life sciences. Mistaking philosophers’ logical reconstructions for philosophical purposes for “real” scientific theory is understandable, and perhaps, it is partly learned from some OMR authors (e.g., Bacharach). Many theory accounts in IS are understandable, if one’s primary understanding from science stems from logical empiricists (e.g., Hempel) or Popper, until 1970. It may be that mistaking philosophers’ logical reconstructions for philosophical purposes, we conjecture, can be also found in other philosophical concepts used in IS.

In contemporary life sciences, perhaps no single article presents a complete *theory*, because even the simplest candidates for theories are too complex, and dynamic, for one paper to cover. The progress happens in isolation: A study examines one highly narrow part of the “theory”—not in a real setting, but in a counterfactual setting (e.g., laboratory, data, or model). Studies do so, because the real settings are too complex (indeterministic) for theories to handle. For this and many other strategic reasons, scientific models or theories must introduce or assume various purposefully falsehoods. In turn, many influential IS theory accounts hold that theories and models represent a true phenomenon. Isolations tactics could be also relevant in IS. Moreover, a possibility exists that the requirement of a (new) theoretical contribution may block good IS research. We suggest that theoretical contributions should not be regarded as intrinsically valuable. Instead, “theoretical contributions” serve several higher purposes (in empirical sciences), which could be, for example, related to an explanation, understanding, or predictions. Finally, many IS theory accounts, as a “statement view of theories” (such as the ORV), are useful as idealized “reconstructions,” for pedagogic purposes. Outlining a normative criterion of theory is not an *a priori* matter, which cannot be done fully independently of the research context. That does not mean that we cannot refer to theories. But then the context (e.g., data about the phenomenon), and our goals, should play a large role in what our theories look like—and not the ORV accounts. Several tasks remain for IS philosophy, including explaining in detail 1) isolations and idealizations in IS, 2) to what extent certain IS theories offer “why explanations,” 3) the importance of theoretical, 4) key differences between universal laws, statistical claims, and mechanism-based explanations, and 5) what counts as an explanation or prediction.

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