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EDITORIAL

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# Review and Theory Symbiosis: An Introspective Retrospective

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**Abstract**

This paper presents a polylithic framework of review and theory development (RTD) papers. Based upon a reflective analysis of review papers that I have written, read, and/or reviewed, I build a framework suggesting four types of RTD papers: organizing reviews, assessing reviews, specific-theorizing reviews, and broad-theorizing reviews. The four types vary according to the research focus and research objectives, with research focus ranging from primarily description to the identification of gaps, and research objective ranging from primarily synthesizing to primarily theorizing. The framework and accompanying discussion are intended to provide scholars a perspective of the different ways that theory development and review papers intersect. The paper proposes criteria to help evaluate the quality of RTD papers and provides suggestions to authors on how to craft RTD papers.

**Keywords:** Literature Review; Typology; Theorizing; Review; Theory Development

Suprateek Sarker was the accepting senior editor.

## 1 Introduction

My first exposure to review/theory development papers was in 1989. I was a first-year, first-semester student in a PhD seminar on organizational decision-making. One of the papers we read was a working paper by the professor, Dr. George Huber, on the effect of communication and information technologies on organizational decision-making. The paper was subsequently published in the *Academy of Management Review* (Huber, 1990). There are times in life when one has a thought that seems to come from nowhere and that one never forgets. This was one of those times. After reading the paper, my first thought was: “Gee, that seems like a pretty easy type of paper to write.” I came to learn a few years later when working on a review paper on the impact of technology on managerial education (Leidner & Jarvenpaa, 1995) that the thought was not only ignorant and naive, but somewhat offensive and irreverent. That thought would return many times to haunt me as an ironic adumbration of the intense effort I would expend and of the intense struggles I would encounter in writing review/theory development papers (RTD) papers. I now most certainly recognize that RTD papers are anything but easy to write. Indeed, I consider them the most difficult of papers to write, for there are no data to rely on to fill pages and inform dialogue. Instead, the contribution of the paper is very much dependent on the unique ideas that the authors derive and the elegance with which the authors convey said ideas. I have worked on seven such papers with varying degrees of success, if success is measured by the publication outlet quality or citation level. I have served as senior editor on over 70 RTD papers (for the *Journal of the Association for Information Systems*) and have been a reviewer on countless others (primarily *MIS Quarterly*). In this editorial, I would like to offer what I refer to as an introspective retrospective. My objective is to look back on my experience as author, senior editor, and reviewer on RTD papers and draw on my introspection to offer a fresh conceptualization of these kinds of papers. By RTD papers, I mean papers whose data are comprised of literature and whose content includes both some elements of literature description and some elements of theory. I am not referring to literature review or theory sections of papers, nor am I referring to papers that develop theory using empirical data. My focus is on papers that are grounded in literature as their source of insight and inspiration. My account is necessarily retrospective in that it begins more than 25 years ago and, in some cases, I must rely on faded memories. It is introspective in that I attempt to not just recount, but also to reflect on my experiences.

## 2 Some Reflections on Existing Review Typologies

Of late, the field has experienced a surge in articles providing typologies and advice for potential review paper authors (e.g., Paré, Trudel, Jaana, & Kitsiou, 2015; Okoli, 2015; Templier & Paré, 2015; Schryen, 2015; Vom Brock, Simons, Riemer, Niehaves, & Plattfaut, 2015; Bandara, Furtmueller, Gorbacheva, Miskon, & Beekhuyzen, 2015; Rowe, 2014; Boell & Cecez-Kecmanovic, 2015), many drawing at least partial inspiration from Webster and Watson's exemplary perspective on review/theory as described in their 2002 editorial. These articles provide solid foundations for researchers aspiring to conduct reviews. In some areas, my vision of RTD papers and how best to conduct them may vary from the existing advice. While I do not intend to contradict or critique these articles, I will nevertheless offer an alternative perspective on issues that I find fundamental to the conduct and writing of review papers.

Let me begin with some observations on the typology of review papers presented in Paré et al. (2015). Paré et al.'s typology of nine ideal review profiles is quite helpful in distinguishing types of reviews and certainly merits reading and reflection. Prior to a recent paper submission, a coauthor and I were asked to classify our review paper according to the Paré et al. typology. With alacrity, we read and debated what our response should be. We quickly ruled out meta-analysis, realist, and critical review on the basis that we were certain we had not conducted a meta-analysis, and on the basis that we were not certain that we fully understood the meaning of realist and critical reviews. But we then had difficulty deciphering between narrative, descriptive, scoping/mapping, and theoretical reviews in particular, and qualitative systematic and umbrella reviews to a lesser extent. The typology felt somewhat monolithic in the sense that one's paper was supposed to fit nicely into one single category. Ours did not. I realize that the authors do not claim that all papers should fit into a single category, but there is a tendency for readers and, more importantly, for reviewers, to judge a paper according to how well it typifies an ideal profile, as well as to distinguish a hierarchy of profiles according to which some ideal profiles are more worthy than others. Indeed, we were asked to specify which of the nine types our paper was intended to be, not which hybrid form our paper took. In terms of our paper, we felt there were certainly aspects of it that were narrative and descriptive, but we also felt that we would be hurting our chances of publication if we claimed to be doing a narrative or descriptive review. Ultimately, our paper developed into a theoretical review, but there were still elements of narrative and description, without which our theoretical portion would have made little sense. Even within the single category of theoretical review, there seemed to be two different types—one type in which an existing theory was used to synthesize an existing body of research, and another in which a new theory was developed from a synthesis of an existing body of research. As it turns out, our paper did both. This left me debating how to reconcile and conceptualize the various roles that theory can play in a review paper and that reviews can play in theory development.

In order to better understand this relationship between theory and review, I thus began my process of introspective retrospection. Unlike Paré et al's rigorous deductive approach in which they first analyze papers about review papers, and claims within review papers, to identify ideal review types and then assess how well published review papers meet the standards of the ideal review types, my approach is highly inductive. Rather than looking for differences in review paper types, I look for similarities, and rather than looking at what authors claim review papers should do, I consider what authors and reviewers seem to embrace. I then induce a framework of RTD papers. I call my classification a polylithic framework of RTD papers because it does not assume that RTD papers fit nicely into one category or another, but instead assumes that RTD papers are highly malleable and vary more in degree of emphasis than in type.

## 3 A Polylithic Framework of Review/Theory Papers

Figure 1 depicts four RTD types. The types are based on the research objective and the review focus. To clarify, allow me to propose a photography metaphor. Any photo has both an objective and a focus—the objective may be to capture the beauty of a sunset; the focus may be narrow (the sun setting over a tree top) or broad (the sun setting over an ocean) depending upon the photographer's perspective and choice. As with photos, with RTD papers, there are merits to different foci and objectives.

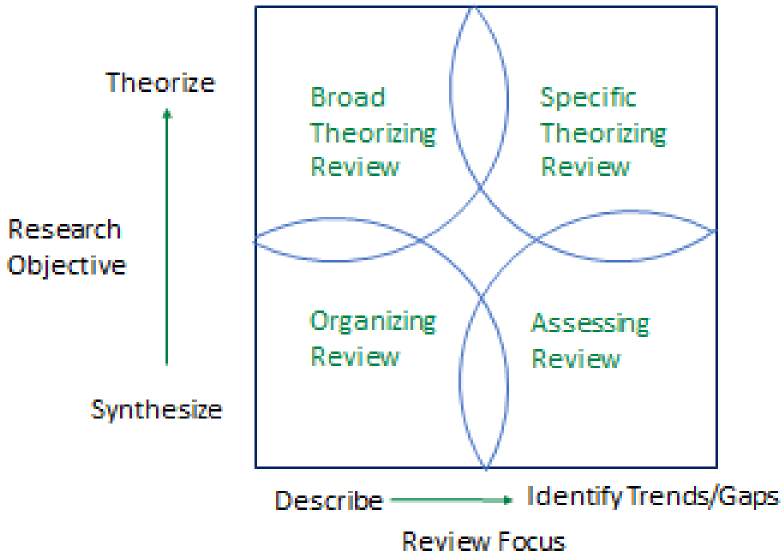


**Figure 1. A Polylithic Framework of RTD Papers**

In the case of RTD papers, the objective ranges somewhere between literature synthesis to theorizing from the literature. Some RTD papers have as the research objective to synthesize the literature. For such papers, the emphasis is not to use the synthesis as a basis for further theorizing, but to contribute to the body of knowledge by providing an insightful synthesis of the literature. Other RTD papers have as an objective to theorize on the subject of the reviewed literature. These papers necessarily begin with a literature synthesis, but the objective is to use the synthesis as a springboard for theorizing. Most are somewhere in the middle of this range with some synthesizing and some theorizing. I place research objective on the y-axis of Figure 1.

RTD papers also have a focus. The focus ranges from describing the literature to identifying trends and/or gaps in the literature. When one focuses on describing the literature, one is interested in identifying what has been done and what has been found. A good description does not summarize literature; rather, it weaves together literature while offering observation and insight above and beyond the literature itself. This is a highly creative process when done well. RTD papers with a focus on identifying trends/gaps also describe what has been done and found, but they aim to use the description to identify patterns in the research stream and to uncover what is missing within the stream. RTD papers whose review focus is description tend to cover broad and/or emerging phenomena, whereas those whose focus is on trend and gap identification tend to have well-defined parameters. This is partly a matter of pragmatics: identifying trends and gaps assumes that one has thoroughly examined all relevant literature, else the gap identified might merely represent a gap in what the author read, not an actual gap in the literature. Hence, papers that aspire to trend and gap identification have a much higher threshold for inclusiveness (e.g., including all literature on a topic) than those that aim at description. In part because of this need to be inclusive if one has a research focus on gap identification, gap identification is likely to be associated with much more defined—e.g., narrow—topics than are descriptions. I place review focus on the x-axis of Figure 1.

Figure 1 is intentionally amorphous. The lines between RTD paper types are only a matter of degree. The bottom left portion of Figure 1 comprises RTD papers that I refer to as *organizing reviews*. These are RTD papers with a focus on description and an objective of synthesis. Such papers most often cover very broad topics or phenomena. The literature may be extremely large and come from several disciplines. For such reviews, a comprehensive literature search that covers everything written on the topic is unrealistic. Hence, organizing reviews do not claim to be comprehensive, but do try to make a large stream of literature understandable. Papers that extend along the y-axis from synthesizing to theorizing, while maintaining the review focus of description, strive to theorize based upon the synthesis and description. I label these papers *broad theorizing reviews* because the resulting theory is intended to bring together a stream of research and is typically covering a phenomenon, as opposed to a gap within a phenomenon. Such papers often begin as organizing reviews but emerge into broad theorizing reviews. The bottom right portion of Figure 1 comprises RTD papers that I refer to as *assessing reviews*. Such RTD papers provide a synthesis of trends and/or gaps identified within a research stream. An assessing review will most often be much narrower in focus than an organizing review, based on the need to ensure that all relevant literature on a topic has been consulted. The upper right portion of Figure 1 refers to *specific theorizing reviews*. Such RTD papers typically begin by identifying a gap in the literature and then seek to provide a theoretical filling of the gap. Hence, they are more specific than the broad theorizing reviews. Specific theorizing reviews will most often theorize about one particular gap identified, rather than multiple gaps. This is again primarily a pragmatic decision because to fill a single gap with theory is already an arduous task. To try to fill multiple gaps with theory could quickly lead to an unmanageable paper.

## 3.1 Comparing this Framework to Others

Allow me to briefly distinguish this framework from some others. In an insightful editorial, Rowe (2014) describes three types of reviews—a descriptive review, a new framework-based review for understanding, and a theory-based explanatory review. These are distinguished based on the primary goal of the paper, with the goals being either analysis and description, explanation, or prediction and prescription. Other papers about reviews also discuss the issue of the review's overarching goal (Paré et al., 2015) or purpose (Okoli, 2015). Like Rowe (2014), Paré et al. consider three goals: summarization of prior knowledge, data aggregation or integration, explanation building, and critical assessment of extant literature. Okoli lists six purposes: to analyze the progress of a specific research stream, to make recommendations for future research, to review the application of one theoretical model in the IS literature, to review the application of one methodological approach in the IS literature, to develop a model or framework, or to answer a specific research question. These assessments of goals are helpful and can produce useful typologies, but they can sometimes be confusing in that the categories may overlap. For example, in making recommendations for future research, one might first need to analyze the progress of a specific research stream. Or, one might review the application of a particular theoretical model in the IS literature in order to answer a specific research question. Likewise, in building an explanation, one might first engage in summarization of prior knowledge or in data aggregation. Or before one can critically assess the extant literature, one must first summarize or aggregate it. And in order to provide an explanation, one must first provide an analysis and explanation. Typologies must not necessarily provide mutually exclusive categories, but when there are overlaps, it can be difficult for authors to clearly explain where their work fits within a typology, and it can create issues for reviewers who might unintentionally apply incorrect criteria for assessing the paper. For this reason, my framework illuminates degrees of emphasis, moreso than distinct, ideal profiles. Each of the four RTD types in Figure 1 could form the basis of a top journal publication, However, each type should be judged according to what it is, rather than according to what it is not. I will describe criteria of evaluation for the four RTD types below. First though, it is important to understand the role of theory in the four RTD types.

## 3.2 The Theory-Review Symbiosis

An important issue in any review is that of contribution. My notion of contribution departs somewhat from that of others. Schryen, Wagner, and Benlian (2015) in their thought-provoking theory of knowledge for literature reviews, describe five potential contributions: synthesis, adopting a new perspective, theory building, testing theory, and identification of research gaps. My framework considers some of these elements not as a paper's contribution per se, but as the research focus or research objective. My notion of contribution stems from the argument that contribution, in the minds of reviewers, is most often associated with theoretical contribution. Thus, to understand contribution, we must uncover the role of theory in reviews and/or the role of reviews in theory. In reflecting on this relationship between theory and reviews, I see four possibilities as depicted in Figure 2. In the bottommost portion of the Figure, theory informs the review whereas in the topmost portion, the review informs the theory.

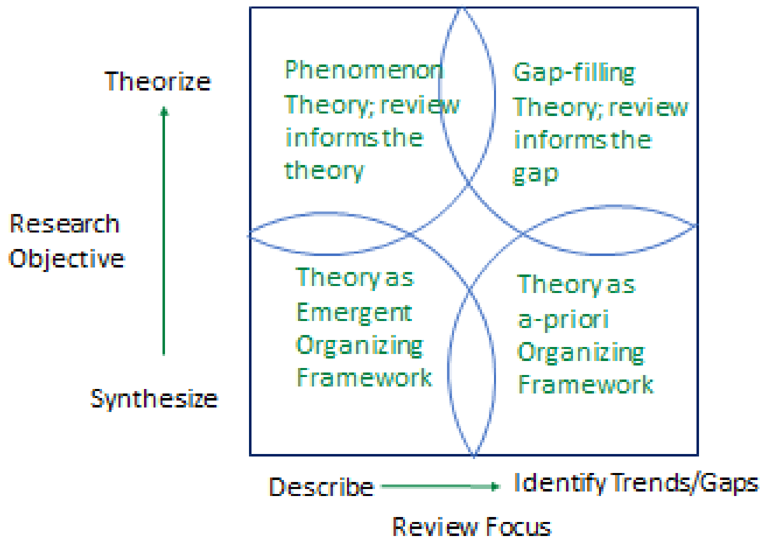


**Figure 2. Theory-Review Symbiosis**

In the *organizing review* paper (Figure 1, bottom left), theory may take the form of an emergent framework that is used to synthesize the literature. This may be an existing theory that is not selected a priori but rather emerges as an effective lens within which to synthesize the literature as the authors conducted their analysis. This would be akin to an interpretive qualitative analysis, in which the authors consider various theories that help explain their data before choosing the theory that best explains their data. By applying the existing theory to analyze the stream of literature, the authors are able to extract insights and uncover assumptions that might otherwise be undetectable. Alternatively, the authors might create their own framework to synthesize the research when existing theory does not effectively capture the literature stream. This would be akin to a grounded theory approach. This approach might be particularly well suited to reviewing a fairly new phenomenon. I often find that reviewers expect review papers to be organized around a preexisting theory; however, we should remain open to the possibility that some phenomena that are in need of reviews may be more amenable to a grounded theory, or emergent framework, than to the application of a preexisting one. It is the authors' responsibility to justify the choice made. In this latter case, the emergent framework is an important contribution of the *organizing review*.

With *assessing review* papers (Figure 1, bottom right), theory is used as an a priori organizing device. Authors decide in advance that they will code the literature according to a particular framework or theory. They are then able to identify those areas of the theory that have been understudied and those that have been overstudied. This is effectively a positivist approach to the literature analysis. Authors might also have a particular theory as the focus of their review. In this case, they will examine how this theory has been applied within a discipline. Their review may assess those aspects of the theory that have been thoroughly examined versus those that have been ignored (e.g., for example, see Mizruchi and Fein, 1999). Such reviews focus less on extracting insights and uncovering assumptions, and more on the explicit coding of the literature according to the existing a priori theory in order to identify those relationships that have been fully studied and those in need of greater attention.

*With specific theorizing review*s (Figure 1, upper right), a theory is proposed that is intended to fill a gap identified during the process of gap identification. In this sense, the review of the literature informs the gap but does not inform the theory. The theory is developed using a separate stream of literature or a separate analysis of the reviewed literature with a specific focus on extracting insights relevant to filling the gap. Authors of specific theorizing reviews must either first identify the gap themselves (thus performing an assessing review), or they must rely on a previously published review that identifies the said gap. The theory contribution of a specific theorizing review is the development of a gap-filling theory.

With a *broad theorizing review* (Figure 1, upper left), the review informs the theory. I refer to this theory contribution as phenomenon theory because typically, such a theory would not set out to fill a specific gap, but would seek to offer a broad theory of an emergent area or a new theory within an established area. The literature reviewed in such cases directly informs the theory. Some authors might choose to first offer a full organizing review before then developing their theory. Papers without some form of organizing or assessing review would be considered as "pure theory."

As shown in Figure 3 below, RTD papers are malleable. One might begin with an aim to describe/synthesize, but eventually write a paper that develops a broad new theory. One might also begin with an aim of developing a broad new theory but ultimately end with a synthesis/description. Or, one might begin with an aim to describe, but through revision and additional literature searches, emerge to focus on identifying trends/gaps. The various areas can serve as inputs to each other, although a specific gap-filling theory has the weakest tie to the literature base from which the review originates. Thus, if one develops what one believes is a very nice gap-filling theory but the reviewers do not appreciate it, there may be little that can be salvaged from the theory unless one can enlarge it into a broader theory.

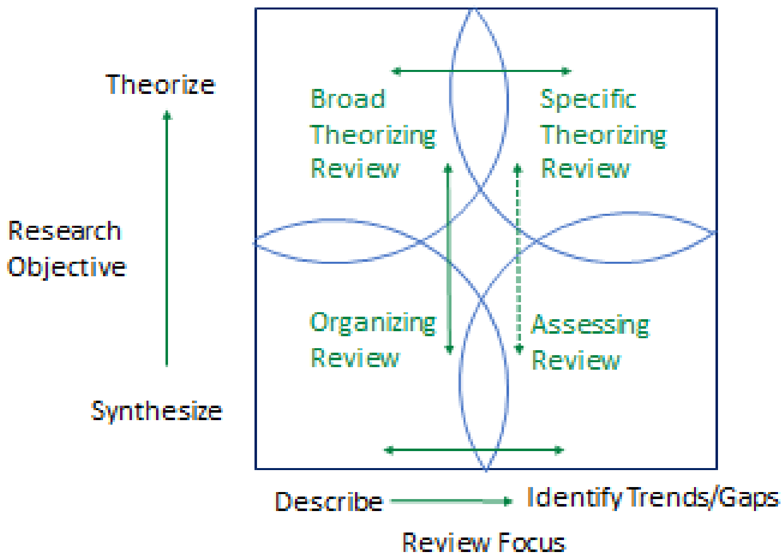


**Figure 3. Theory-Review Relationship**

## 3.3 Concrete Examples of Papers in the Framework

Having now provided the concepts within my framework of RTD papers, but before describing criteria for evaluation, I will provide some concrete examples based upon my own experience. In Figure 4, I attempt to classify the review papers on which I have worked, as described below.

Leidner and Jarvenpaa (1995) is an RTD paper about IT in education. Its objective is to synthesize the research on learning and technology and lay a foundation for theorizing. It is therefore midway on the y-axis. The focus of the paper is description rather than trend or gap identification. It is therefore toward the left side on the x-axis. This paper uses extant theory—Zuboff's informating paradigm—as an organizing device within which to synthesize the research on technology in education and to surface the educational assumptions implicit in the research. The paper then presents a taxonomy of the potential impact of IT on learning. Whether a taxonomy is a theory, or not, is open to debate, but I suggest that the theoretical contribution of the paper lies with the taxonomy that emerges from the literature review.

Alavi and Leidner (2001) is an RTD paper about knowledge management (KM) and knowledge management systems (KMS). At the time of its writing, KM was a new topic in the management literature. There was very little literature on KMS in the top IS journals, but there was a great deal of literature spanning multiple disciplines that was relevant to the study of knowledge in organizations. Alavi and Leidner's objective is to synthesize the research from these diverse streams of research and to provide the foundation for theorizing about KM and KMS. Various figures are used to explain relationships between tacit and explicit knowledge exchange within and between individuals and groups. The initial paper built propositions; in response to reviewers, the final paper replaced the propositions with questions. Paré et al. (2016) classify this paper as a narrative review, but I somewhat disagree. From my perspective, the figures, and the surrounding text, used to synthesize the literature form a theoretical contribution. The figures go beyond mere description to synthesis and novel insight. Although the initial submission was higher on the theorizing scale on account of the directly stated propositions, the ultimate paper nevertheless makes a theoretical contribution albeit a theoretical contribution that lies with the synthesis (figures synthesizing the research) rather than the research questions per se.

Leidner and Kayworth (2006) is an RTD paper about culture in IS research. I classify this paper as a specific theorizing review. The topic is more narrowly focused than either the Leidner and Jarvenpaa or Alavi and Leidner papers. In terms of the objective, this paper develops a specific theory of various forms of IT-culture conflict in which culture is construed as values held by individuals that can be embedded in technology and work practices. The paper first describes the literature using as an organizing device the basic framework of IS design, development, use, and outcome. Within this framework, the paper focuses on explicating major trends in the IS research on culture. As is common with such papers, we counted how many papers fit various topics and looked to explain discrepancies in findings. The gap we identified—that there was little work in the area of values embedded in IT—then led us to develop a theory to fill this gap: the theory of IT culture conflict. This theory explains how values embedded in IT artifacts, as well as values held by developers and users, may work in harmony, or disharmony, to effect outcomes.

The resulting theory in Leidner and Kayworth in no way resembles the theory from the original submission of the paper. One of the reviewer complaints about the theory in our original submission was that it drew from the papers that we had reviewed. This complaint was initially baffling to me as an author. What, I thought, is the point of the literature review if we are not allowed to draw upon those same articles to formulate our theory? I later came to appreciate the delicate balance between presenting the past and projecting the future with the latter aim sometimes requiring a fresh set of literature to guide the way. In other words, it might not always be possible to extract sufficiently novel insights from one's assessment of the literature to support theory development that appeals to readers as new theory and not just as a summary of the literature. Thus, for the new theory in Leidner and Kayworth (2006), we drew from a completely different stream of research than was used in the literature assessment portion of our paper.

The Schultze and Leidner (2005) paper epitomizes an assessing review. Albeit also a paper in the general area of knowledge management, this paper has a narrower focus than the Alavi and Leidner (2001) knowledge management review. In the case of Schultze and Leidner (2002), the objective is to synthesize KM research in IS with a particular focus on identifying how knowledge has been conceptualized in the IS literature on KM. The paper draws from an a priori framework (Deetz, 1996) and a coding protocol to code and synthesize the literature. Of all the review papers I have worked on, this was the most comprehensive in the sense that it consulted every potential article on KM published in a select set of IS journals over a stated time period was consulted. Otherwise, the conclusions drawn based on the trends identified would have been highly suspect. The contribution of this paper is to synthesize the body of knowledge according to an a priori theory, and in so doing, to provide fresh insights into how IS researchers moving forward might conceptualize and study KM.

The final published paper appearing in Figure 4—Balozian and Leidner (2017)—is an RTD paper on the topic of IS security policy compliance. The paper's objective is to synthesize the research on IS security policy compliance with a focus on describing the research. The paper does not apply an a priori theory to organize the literature, but rather allows the themes to emerge from the review. The paper does identify trends, such as the portion of papers employing various theories to explain compliance behavior and the distribution of papers by method, and also identifies some gaps in the literature. The paper does not attempt to fill any particular gap or to provide a framework for future research, nor do the themes that emerge fit nicely into an original framework within which to synthesize the research. Hence, it remains toward the bottom of the y-axis.

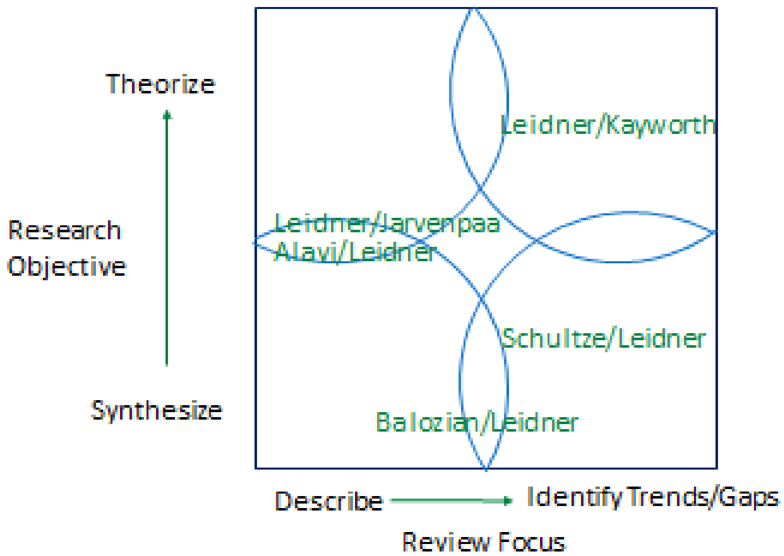


**Figure 4. Personal Assessment of Review Papers I Have Coauthored**

It would be remiss not to note that the published versions of these papers often varied considerably from the originally submitted versions. All but the Schultze and Leidner paper began as broad theorizing reviews but evolved through various revisions. Leidner and Jarvenpaa, Alavi and Leidner, and Balozian and Leidner all sought to develop theory in the early rounds of review. The propositions were eliminated from all three. Leidner and Kayworth began as a broad theorizing review but the original theory was completely scrapped for a new, much more specific theory aimed at filling a gap. Authors should not confine themselves to a specific objective and focus. If the review process carries a paper in another direction, they must be open to the journey. On the other hand, reviewers should also be careful to not demand incompatible movement. For example, trying to shift a paper from an assessing review to a broad theorizing review would be tantamount to asking the authors to write a completely different paper.

Having described the framework and provided some examples based on my own work, I now shift attention to the evaluation of RTD papers. Here, I draw from my observations of reviewer comments on both my own submissions, as well as on those of RTD papers that I have handled for the *Journal of the Association for Information Systems* in order to describe attributes that I perceive reviewers to expect from review papers.

## 3.4 Evaluating the Quality of Review Papers

Figure 5 summarizes the attributes for evaluation of RTD papers. Beginning with *organizing reviews* (Figure 1, bottom left), what I have observed is that for a paper to succeed in presenting an effective description and synthesis, it must be both interesting and informative. Readers must feel that after reading the description, they do not need to consult the original papers. While this satisfies the informative criteria, it is not enough: the description and synthesis must spark interest. Interest is most often sparked by offering insights beyond that which the literature itself presents and by uncovering assumptions in the literature. It is not uncommon for reviewers to point out that a paper describes what has been studied, but fails to either describe what has been found or to provide insights beyond the explicit findings from the literature itself. Such papers are consistently rejected or returned to authors for a reanalysis. In essence, they do not meet the interesting or informative criteria.

For *assessing reviews* (Figure 1, bottom right), reviewers expect that the paper be comprehensive and convincing. This means that the authors must persuade readers that they considered all relevant literature (comprehensive), and must persuade readers that their method for searching, coding, and analyzing the literature was appropriately rigorous such that readers can trust the authors' assessment of the literature (Paré et al., 2016). An effective means of convincing readers is to provide evidence of a systematic approach. A systematic approach is one that is carefully described (e.g., transparent) and reproducible. A systematic search and coding strategy helps convince reviewers of the validity and trustworthiness of the trends/gaps identified. Assessing reviews must also satisfy the interesting and informative criteria because they also describe and synthesize. However, the means of achieving informativeness and sparking interest are different. In the case of assessing reviews, this is accomplished by uncovering unexpected trends and gaps, whereas with organizing reviews, this is more often accomplished by identifying assumptions, by synthesizing findings in a new way, or by offering the authors' interpretations of the findings in the literature.

Much of the advice that I see in papers purporting to assist authors in conducting reviews focuses on developing systematic literature search and assessment strategies. I find such advice to be most relevant for assessing review papers. For example, Okoli (2015) provides eight steps that authors should take when writing review papers, and suggests that review papers should be explicit in describing each step, should be comprehensive in the first four steps (identify the purpose, draft the protocol and train the team, apply practical screen, search for literature), and should be reproducible in the first two and seventh steps (identify the purpose, draft the protocol and train the team, and synthesize the studies). These steps provide excellent guidance if one aspires to write an assessing review paper. But for papers not aspiring to be assessing reviews, this advice can constrain rather than aid. One does not need a comprehensive literature base or a convincing coding scheme to conduct a highly interesting and informative description and synthesis of the research (organizing review). In fact, forcing such a protocol on authors of organizing reviews or broad theorizing reviews might constrain the creativity needed to extract insight and uncover assumptions.

Okoli's paper codes 23 review papers. Interestingly, Okoli codes Alavi and Leidner's (2001) paper as having only completed stages 1 (identify purpose) and 8 (write the paper). Of course, one must keep in mind that just because an author does not report a step in a paper does not necessarily mean that the author did not conduct that step. That aside, I don't dispute Okoli's observation of Alavi and Leidner's (2001) failure in steps 2 through 7. Admittedly, the first time I saw the paper's poor performance on Okoli's criteria, I felt rather disappointed that our paper fared so poorly on the evaluation criteria. But on reflection, I suggest that for the particular objective/focus of the Alavi and Leidner paper, these steps as explicated by Okoli were not necessary to create the synthesis and insights that emerged from the analysis. I use this example to hopefully persuade reviewers that one need not apply a checklist of steps to each review paper one assesses. One must take into account the particular objective/focus of the review paper as I have outlined in the above figures.

Moving on to broad theorizing reviews and specific theorizing reviews (Figure 1, upper left and right), the criteria for evaluation rely heavily on the creative capacity of authors. Ultimately, if one theorizes in a review paper, one must convince reviewers that the theory meets the standards for good theory (see Rivard 2014 for an excellent perspective on theory building). I will not delve into the topic of good theory here because it has been well-explicated elsewhere (Rivard, 2014; Feldman, 2004; Sutton and Staw, 1995; DiMaggio, 1995; Weick, 1995; Gregor, 2006), but I do wish to emphasize that review papers that have a theoretical component—which includes most review papers submitted to elite journals—will be judged in part according to the quality of the theory contribution. In addition, such papers must be articulate, imaginative, and insightful. By articulate, I mean that broad theorizing reviews and specific theorizing reviews must employ an elegant style of writing. I have seen reviewers comment about dull textbook style writing, about getting bored reading the manuscript, and about having trouble staying focused. At the core, these are as much complaints about the writing as about the content.

Readers are accustomed to a standard paper format of introduction, literature review, theoretical development, method, analysis, discussion, implications, limitations/conclusions. Readers' brains are conditioned to know what to expect. This facilitates their reading and even if they are getting bored in one spot, motivates them to continue or just skip ahead because they know what to skip. RTD papers do not have this luxury of consistency of structure. Each RTD paper is different. I therefore opine that the bar for writing quality is much higher in an RTD paper than a non-RTD paper: the authors must keep the readers engaged with words without having the advantage of being able to conform to a preexisting mental model in the mind of the reader. By imaginative, I mean that specific theorizing reviews and broad theorizing reviews must create images in the minds of the readers. They must provide examples and logic with which the reader can relate. They cannot simply state the obvious, no matter how powerfully and elegantly; rather, they must provide a vision of something new. Finally, by insightful, I mean that specific theorizing reviews and broad theorizing reviews must provide a reader with thoughts that the reader would not have had without the authors' analysis. The reader needs to have a "voilà!" experience wherein his or her perspective of the research is challenged, is provoked, is awakened.

In addition to these shared attributes, a broad theorizing review should also be integrative—the theory should effectively integrate the knowledge that was previously dispersed in the literature. If the theory departs substantially from the reviewed literature or only integrates a small portion of it, the reviewers will perceive the theory as fragmented and the contribution as weak. Finally, a specific theorizing review should also be filling—it must fill a gap by introducing new or previously unconnected constructs.

The articulate, imaginative, and insightful criteria are difficult to achieve. The IS literature is neither renowned for its elegant sophistry, nor for its inspirational messaging. Part of the reason for the dearth of published review papers that our field witnesses, in comparison to the number of papers relying on more traditional data (surveys, experiments, panel, case study), may be not so much that far fewer review papers are attempted, but that the expectation for articulation, imagination, and insightfulness are much higher for review papers than they are for other types of papers. I laud these expectations. Review papers are among the most highly cited papers for any journal. The standards for their publication should be high, particularly in elite journals. And although I recognize that proving that one's paper is insightful and imaginative is challenging, I encourage authors to pursue these criteria as they craft and polish their papers. Figure 5 displays the evaluation criteria discussed above and the quadrants in which I find such criteria most applicable.

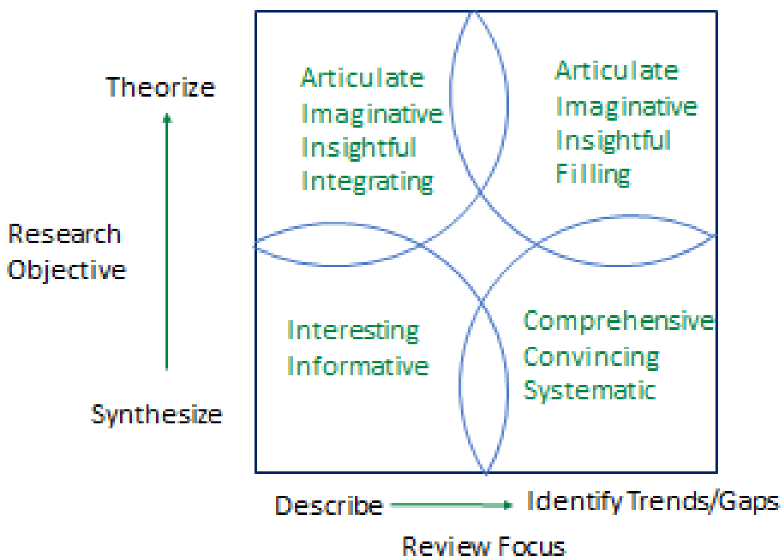


**Figure 5. Evaluation Criteria for RTD Papers**

## 4 Some Advice for Authors

Authors often look for advice on how to write a good RTD paper. Any specific guidance that I would offer authors would ultimately relate to how to work toward fulfilling the criteria upon which review papers are judged—i.e., the nine attributes listed in Figure 5, not all of which are appropriate for each type of review paper. Some specific recommendations follow. Many of these recommendations may be helpful for writing other types of papers, but I suggest that they are particularly salient for writing RTD papers.

### 4.1 Allow the Objective, Focus, and Topic to Evolve

Authors are often encouraged to identify their purpose as the first step in their literature review. I agree that knowing one's purpose is important, but I argue that the purpose may evolve as one proceeds through the search and analysis process. The purpose, in my conceptualization, is the research objective and research focus as presented in the above figures. What I see is that the purpose of many review papers changes over time. The purpose might start out very broad but become quite narrow, or vice versa. The purpose evolves as one begins to read literature and develop insights. Not only might the purpose evolve, but so too might the topic itself. Initially, a good topic is one that the author feels passionate about, or at least highly interested in. I strongly advise against conducting a review for the sake of publishing a paper. The amount of work is enormous, and if one is not highly interested in the general topic, it will be very difficult to sustain the momentum through several iterations of revision, and even more difficult to derive insights and get one's imagination churning.

## 4.2 Challenge Assumptions

One way to create insight and show imagination is by uncovering assumptions. I recall once reviewing the literature on CIO leadership. Something that struck me was how many IS papers relied upon leadership theories drawn from the literature on CEOs as the basis for the study of the CIO. It seemed to me as though the IS authors assumed that the CIO as a leader is similar in nature to the CEO. In questioning this assumption, I found myself pondering whether the CIO is more dissimilar than similar to the CEO in that the CIO has very little power over the people whose behavior is most instrumental to the CIO's ultimate success (e.g., the users). The CIO can neither punish nor reward to any significant degree (e.g., through promotion, bonuses, or firing) those whose use, or nonuse, will ultimately render a system successful or unsuccessful. I considered whether a more appropriate analogy might be to compare the CIO to the pastor of a church or the director of a nonprofit association, neither of which can directly control the behavior of the major constituents, e.g., members, without whose cooperation the pastor or director becomes irrelevant. This would then lead to an entirely different theory base being used to interpret the CIO leadership literature.

Identifying assumptions is an activity for which one must allow time to not just to read, but to actively reflect and engage with a body of literature. I do not believe that this process can be effectively routinized. In the paper "Generating Research Questions through Problematization," Alvesson and Sandberg (2011) implore researchers to challenge the necessary presuppositions underlying a subject matter in order to develop new research questions. One might go further and even challenge the assumption that research begins with research questions. Indeed, I find that research questions often arise at the end of a project rather than the beginning, whereby authors pose the research questions in a paper that they know their data can answer. Assumption finding is as much about understanding the importance of what is left unsaid as it is about interpreting what is said.

## 4.3 Avoid Mechanizing the Process

Some of the papers providing advice on writing review papers suggest that a literature review should follow a systematic method in which the procedures by which it is conducted are explicitly articulated and, as such, reproducible (Okoli, 2015). For example, as discussed above, Okoli provides a "systematic guide to literature review development" in which he explicates eight steps. These steps include identifying the purpose, drafting the protocol and training the team, applying the practical screen, searching the literature, extracting the data, appraising the quality, synthesizing the studies, and writing the review. He advises that one must be explicit, comprehensive, and reproducible in steps 1-2, explicit and comprehensive in steps 3-4, explicit in steps 5-6, and explicit and reproducible in steps 7-8. As mentioned above, while these steps may be very relevant to assessing reviews, the danger arises when authors, or reviewers, treat these steps as essential for all types of RTD papers and/or adopt the mindset that the approach should always be reproducible with the same outcome by another team of authors. I have seen authors obsess over reproducibility and have enjoyed some lively debates about creativity versus reproducibility. The process of conducting a review, to me, is as much an art as a science.

My personal observation is that the process of developing a literature review is iterative, not linear, and that it is not always necessary, or advisable, to attempt to be explicit at each stage. Nor is it always possible to reproduce steps, even in stages 1, 2, and 7. Others emphasize the iterative nature of reviews (Bandara et. al., 2015; Vom Brocke et al., 2015), yet also emphasize the notion of reproducibility and explicitness. That an iterative process should also be reproducible strikes me as a non sequitur. Not only is it difficult to record each decision in an iterative process, rendering the explicitness necessary for reproducibility challenging, but it is equally daunting to recreate the logic behind the insights inferred from an analysis such that another would arrive at the same insights from reading the same set of articles. There are cases when reproducibility is possible, such as when one has a narrow topic and counts how many papers from a set of well-specified literature fit into which category. As such, assessing reviews should indeed be reproducible. But reproducibility is neither necessary nor sensible for the other types of review papers. If two researchers read the same set of papers and have precisely the same set of observations, then one might aver that their observations are neither very novel nor unique. In fact, novelty is often mentioned as a key to effective theory building (Oswick et al, 2011; Whetton, 1989; Bacharach, 1989; Corley and Gioia, 2011). Without novelty, it is virtually impossible to fulfill the expectations for imagination and insightfulness expected of broad theorizing and specific theorizing reviews.

Moreover, the very recording of each decision made during a search, in order to later make these decisions explicit, can disrupt the search itself. As one is searching literature with a certain set of keywords, one often realizes that the search should be modified. By the end, it is quite possible that an author has lost track of precisely what keywords were used and in what combination. The only way to avoid this is to record at each step precisely what terms one is using, and in what combination, and with what database. In some cases, this might be appropriate, but I do not necessarily advocate for this all the time. I have found that when one is in the course of searching a database for literature or in the course of reading literature, one often gets into a certain groove or flow (Webster, Trevino and Ryan, 1993) wherein thoughts are moving quickly, and the mind is processing and responding to what it sees and diverging from the original plan but reaping positive results. To distract this flow by self-recording every key term one is using and every decision one is making all so that one can recount for a review panel precisely how one conducted the search might completely disrupt the creative flow. In such a state of flow, one should not risk disrupting one's concentration and creative thought by stopping to record everything one is doing.

This is what I mean when I suggest that authors should avoid mechanizing the process. If it becomes a mechanical exercise in which all decisions are made in advance and one merely executes the plan, one has eschewed the creative processes that are essential to an effective review. At the risk of sounding unscientific, my encouragement to authors would be to not necessarily follow a rigorous protocol in their initial literature search and analysis, but to be prepared to reverse engineer when the time comes.

## 4.4 Don't Look for Shortcuts

Authors of review papers need not read every word of every paper that ultimately forms their panel of papers for the review. There are indeed shortcuts in terms of focusing on specific sections in a paper. For example, if one is conducting a review of how grounded theory research conducts the analysis, then one might only read the method and analysis sections of the papers. However, when it comes to synthesizing literature, there are no shortcuts. One cannot synthesize on the basis of only reading a handful of papers and then adding references later on. One cannot synthesize if one has only read a small portion of the papers selected for the review. I strongly suggest that at least one author should have read all the articles, except in the case of assessing review papers. In the case of assessing review papers, authors can divide the papers once they have developed an accurate coding protocol. For the other three types of RTD papers, having one author who is familiar with all of the literature consulted will have tremendous benefits in terms of extracting insights. In the case of specific theorizing reviews that are predicated on an assessing review, one author might read all the papers for the assessing review and then another author might read all the papers that help form the basis of the specific theorizing review.

## 4.5 Embrace the Wolf

A common characteristic of most stories that endure the test of time is that there is invariably an antagonist, often a person, object, or situation that is completely unexpected and even unimaginable. If one takes a beloved story and removes the antagonist, then one is often left with a story that might be sweet and might entertain once, but it surely would not leave a legacy or endure beyond a single telling. Even when writing for an academic journal, if there is nothing unexpected, unusual, or previously unimagined by the reader/reviewer, it is more likely that the reader will focus on the negatives of the paper rather than the positives. When interpreting a body of literature, I try to look for the wolves. The analogy is from *Little Red Riding Hood*. Without the wolf, the story is about a young girl wearing a red hat who wanders through the German forest to visit her grandmother. Without the wolf, the girl visits her grandmother and returns home. Without the wolf, the story is sweet and innocent perhaps, but not much of a story. The wolf brings a new dimension. The wolf adds imagination to the story. The presence of the wolf poses discomfort for the reader, raises questions for the reader—didn't the girl's parents know that there were wolves in the forest? Why didn't a parent accompany the girl? Even in the absence of an accompanying parent, is it wise to walk through a wolf-infested forest in bright red and carrying food? Such questions provide the very basis for whatever insight might be derived from the story. One can certainly go too far in drawing an analogy between writing a literature review and writing a fairy tale, but my suggestion is that authors embrace those elements of discomfort in the literature, seek them out, describe them, and incorporate them into their synthesis. This will go a long way toward enhancing the insightfulness and imagination of the paper.

## 4.6 Read Classic Literature

In my experience, to write at a high level, one must read at a high level. I have always enjoyed classic literature. Aside from merely enjoying reading good literature, I find that it helps spark creative thoughts and helps improve my own writing. I would argue that tacit-to-tacit knowledge exchange is possible as one reads very well-written literature. One might not realize that one is also learning better sentence composition, but one may at least partially absorb some of the tacit knowledge that forms the basis of exemplary writing.

### 4.7 Dedicate Yourself

Lastly, my advice to authors of review papers is to dedicate research time completely to the literature analysis, synthesis, and writing for six months. Many researchers try to increase their chances of publication by working on a large quantity of papers. If you are taking a lead role on a review paper, my experience is that you need to be able to focus exclusively on that project for at least six months, and to plan on an additional six months for the first revision and possibly six months for the second, with less time for subsequent rounds. If you are actively involved in several other projects and if those other projects are on unrelated topics, you are unlikely to be able to dedicate the amount of concentrated time that is necessary for the successful shaping of an elite review paper. Would that we each had a cabin on Walden Pond in which to recluse ourselves during the intense periods of analysis and reflection, but barring such luxury, we must at least provide our minds, if not our bodies, the time and space to iterate through searches, readings, reflections, and writing.

## 5 Conclusion

My aim in this editorial has been to present a conceptualization of review papers that is built on the notion that all reviews have an element of description and all reviews have an element of synthesis. Some reviews take the description to the point of identifying trends and gaps, and some review papers take the synthesizing to the point of developing new theory, creating several types of review papers. I do not intend to portray one type of review paper as necessarily superior to another. Rather, the quality of a review paper depends less on its type and more on its attributes. This editorial is not meant as a guide for authors on how to conduct a review but more as an inspiration for where to set one's sights. Neither is this editorial intended to provide a checklist for reviewers on what to evaluate in a review, but rather to elucidate different forms that review papers may take, each of which has a unique contribution to make. I do not claim to provide a definitive account of review papers, but I do hope the account is insightful and useful. For those aspiring to write a review paper for an elite journal, the demands from reviewers are great, yet so too are the rewards for authors. Authors stand to gain in-depth insight into a phenomenon, to lay a foundation for their own future work as well as that of others, and to have a decided impact on the development of theory and the conceptualization of a phenomenon within and across fields. Ultimately, as in art and music, as in architecture and fashion, the challenge is to provide something new and different, not so new and different so as to render the past unrecognizable, but new and different enough to render the future imaginable.

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