

On Crafting Effective Theoretical Contributions for Empirical Papers in Economics of Information Systems: Some Editorial Reflections

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Abstract. The terms theory and theoretical contributions evoke mixed reactions in the information systems discipline, especially among empirical researchers in the economics of information systems (Econ-IS) area. Although some see such contributions as the *raison d'être* for academic scholars engaged in research, others feel that the discipline has developed a fetish for theory, with reviewers and editors often demanding an unreasonable level of theoretical contributions for empirical manuscripts to succeed in the review process. Moreover, there exists a great deal of diversity in the conception of what constitutes a reasonable theoretical contribution, especially within empirical work, across editors and reviewers, leading to frustration with the review process and disappointment with editorial decisions. Given the different types of theoretical contributions that may be suitable for a given manuscript and recognizing the changing nature of empirical work within Econ-IS, we attempt to shed some light on theoretical contributions within empirical Econ-IS research, paying attention to their nature, types, and impact. Specifically, we start by reflecting on the typical theory-related comments we have seen in review packets that we generalize to a set of critiques often related to empirical papers. Subsequently, we provide a working definition of a theoretical contribution and the components that make up such a contribution. We then propose a taxonomy of theoretical contributions typically observed in *Information Systems Research (ISR)*. Based on this taxonomy of contributions, the typical critiques observed in empirical Econ-IS papers, and a set of published papers, we provide some broad guidelines for how authors may craft an effective theoretical contribution for submission to *ISR*. We also discuss a pathway for manuscripts that do not (seek to) offer significant theoretical contributions. Such manuscripts are welcome, but we believe that a very high bar of practical impact must be met for them to succeed in the review process. Based on the guidelines and suggestions made here, our hope is that authors and evaluators will participate in the review process with a shared understanding of the elusive notion of theoretical contributions.

Keywords: empirical research • IS economics • theoretical contributions • guidelines

Introduction

Academic publications serve as crucial vehicles for advancing knowledge within their respective fields, with theoretical contributions often playing an indispensable role in their impact. However, as fields mature and expand beyond their original boundaries, there is a pressing need to revisit the baseline assumptions and institutional norms that exist within the field and update them accordingly. In this editorial, our attempt is to re-examine what constitutes a theoretical contribution when it comes to empirical research within the broad “economics of information systems” (Econ-IS) area in information systems (IS) research

and to provide guidance to the community on how authors may position their work within this tradition in the most suitable way possible. The area generally identified as research in Econ-IS emerged as an identifiable subfield roughly in the early 1990s, catalyzed by the first Workshop on Information Systems and Economics (WISE) that was organized in Cambridge, Massachusetts in December 1989. Econ-IS, as a field, is heavily informed by scholarship in economics, sociology, industrial organization, organizational psychology, and strategy. As a result, the theoretical paradigms invoked in empirical papers within Econ-IS typically come from these reference

disciplines, contextualized to the IS domain as needed. Therefore, in writing this editorial, we also consider arguments about what makes for a compelling theoretical contribution from these affiliated fields.

In this editorial, we try to answer several pointed questions that emerge during the review process within *Information Systems Research (ISR)* in particular. These issues often lead to negative review outcomes and significant frustration within the author community; our hope is to establish a baseline understanding of these concepts to help guide young scholars in developing their research while also providing clarity to reviewers and editors about how they may evaluate papers submitted within Econ-IS. In doing so, we work within two artificially imposed yet useful boundaries; first, we direct our attention to papers that fit within the broad domain of Econ-IS, even though many of our suggestions can be applied to papers that fall outside this realm. Second, we focus primarily on empirical research, and we do not consider analytical modeling or computational papers, which can be found within Econ-IS, in our conceptualization. These two conditions were chosen to ensure that our recommendations remain reasonably proximal to the target body of research. Within these conditions, we try to answer the following common questions that arise during the review process at *ISR*.

- Is theory relevant for empirical Econ-IS research? Why are theoretical contributions needed for scholarly empirical research?

- What exactly constitutes a theoretical contribution in Econ-IS?

- Do all papers need to have a strong theoretical contribution? Are there levels of theoretical contribution we observe in the literature that can help authors/reviewers formulate/evaluate empirical manuscripts?

- Can a paper be considered viable for publication in leading disciplinary journals, such as *ISR*, if it is empirically strong but does not offer a clear theoretical contribution?

One of the issues in IS research and indeed, any social science research is the problem associated with “weak paradigm” fields (Glick et al. 2007), wherein there is often no consensus on what represents high quality in a given research paper. Weak paradigm fields are also characterized by review processes that are viewed as being somewhat unpredictable and stochastic in nature, which can undercut the process of systematic development of knowledge that is needed for any field to grow and thrive. We hope that the insights provided in this editorial are able to reduce, to an appreciable degree, the extent to which Econ-IS is viewed as a weak paradigm field. In that sense, the target audience for this editorial includes authors of empirical papers within Econ-IS who target journals such as *ISR* and who often tend to be at the receiving end of comments from review teams criticizing these papers for a lack of

an adequate theoretical contribution. This editorial is also aimed at editors and reviewers seeking to establish some level of shared understanding of what we believe represents a reasonable theoretical contribution.

Of course, the review process at leading journals, like *ISR*, retains, as always, a degree of judgment on the part of the evaluators that is hard to eradicate, especially in a fast-moving and dynamic field like IS. The goal of this editorial is to hopefully help authors prepare stronger manuscripts for submission and reduce the “random error term” involved in the review process. The trends in empirical methods, data accessibility, and theoretical development in the economics of IS community are also continuously evolving, in part driven by newer data sources, econometric methods, and machine learning techniques. Furthermore, the IS field is, in general, influenced by the diverse theoretical paradigms and data sources that have emerged in the last two decades, leading to a rich intermingling of insights from various reference disciplines. In such a dynamic environment, individual taste and subjective judgments do carry weight. However, we hope that both authors and reviewers would benefit from the guidance that we provide here, albeit modest, on the theoretical contributions that can or should be expected in manuscripts submitted to *ISR* and other similar IS journals.

This editorial is structured as follows. We first provide a brief overview of theory and the role of theory in research, offering a working definition of a *theoretical contribution* specifically adapted to empirical research within Econ-IS. Having presented this perspective, we describe a typical set of critiques that authors tend to encounter in the review process based on our experiences as editors at *ISR*. As a way of addressing these critiques, we provide a simple conceptualization of the types of theoretical contributions we have observed in the journal thus far, with some examples of recent papers published in *ISR* and with some elaboration of their attributes as well as strategies for authors aiming to achieve these types of contributions. We then highlight some other attributes of papers that could make them viable or even attractive, even under conditions when a manuscript has minimal theoretical contributions. Finally, we conclude with some summary guidelines for authors and reviewers.

Theory and Theoretical Contributions

The terms “theory” and “theoretical contributions” evoke a wide range of reactions in the IS and related scholarly communities, irrespective of the research tradition they are associated with, such as qualitative, quantitative, and design science (e.g., Gregor 2006, Avison and Malaurent 2014, Sarker et al. 2018, Abbasi et al. 2024). A significant proportion of scholars, particularly

gatekeepers in journals, tend to view theory as “king” (Avison and Malaurent 2014), thereby rendering theoretical contributions as an essential element of scholarly work and the “scientific endeavor” (Colquitt and Zapata-Phelan 2007). Hitt and Smith (2005), among many others, highlight some of the reasons for why theory is considered to be important. First, they remind us of Kurt Lewin’s assertion that “nothing is so practical as good theory” and that “[t]heory can advance science by providing cohesion, efficiency, and structure to our research questions and design” (Hitt and Smith 2005, p. 2). Furthermore, they note that “good theory helps identify *what* factors should be studied and *how* and *why* they are related ... [it] also states the conditions and boundaries of relationship,” making it central to the advancement of a field (Hitt and Smith 2005, p. 2). In addition, theory offers a level of abstraction that can help enhance the transferability of insights across contexts, making the implications of empirical research more generalizable and robust (e.g., Sarker et al. 2023).

In sharp contrast, other scholars express frustration when faced with constant demands for theory and theoretical contributions in manuscripts undergoing review, and they wonder whether our community has developed a “fetish” for theory, with a tendency to dismiss papers that are light on theory as unworthy (Avison and Malaurent 2014). Within this view, theory can act as a “straitjacket” that limits flexibility in thinking while also leading to a biased view of a phenomenon. Moreover, an excessive focus on theory can limit the dissemination of empirical results and patterns that can serve as a critical foundation for subsequent theorizing, especially in fast-moving areas (Miranda et al. 2022). The overemphasis on theoretical contributions has also been criticized by scholars, noting that “theoretical generalizations” (that represent broad abstractions) are often so general that they tend to “conceal far more than they reveal” (Prasad 1997, p. 109). Thus, in sum, there exists a significant difference of opinion in the field on how important or necessary theoretical contributions are, particularly so with respect to empirical papers.

There is also, unfortunately, no clear consensus on what constitutes a theoretical contribution or how one crafts such a contribution. A simple definition of theory is “a set of statements, verbal, symbolic or mathematical, that identifies what constructs are important and how they are related to each other, as well as identifies the conditions under which they should be related or not” (Campbell 1990). Sutton and Staw (1995) define theory more broadly as follows: “[T]heory is the answer to queries of why. Theory is about the connections among phenomena, a story about why acts, events, structure, and thoughts occur. Theory emphasizes the nature of causal relationships, identifying what comes first as well as the timing of such events.” Sarker et al. (2018,

p. 759) summarize some of the conceptions in the literature, that range from “a coherent framework with identified variables and relationships (Gregor 2006)” to a “lens” or “scaffolding” (Walsham 1995), or even as “enlightenment” (DiMaggio 1995, p. 391). Mintzberg (2005, p. 360) sees theory (explanations) “along a continuum, from lists.. to typologies.. to impressions of relationships among factors.. to causation between and patterns among these relationships, to fully explanatory models.”

Within the IS literature, Gregor (2006, p. 611) provides a “taxonomy” of theory types: “(1) theory for analyzing, (2) theory for explaining, (3) theory for predicting, (4) theory for explaining and predicting, and (5) theory for design and action.” With such variety in the definition/conception of theory within IS research itself, it is only natural that there would be even less agreement on what the term “theoretical contribution” might mean (Colquitt and Zapata-Phelan 2007) and what kind of theoretical contributions may be considered suitable or adequate for a given paper. Yet, many journals in IS and related disciplines continue to demand significant theoretical contributions in the manuscripts submitted for publication. It is, therefore, worth providing a working understanding of what constitutes a theoretical contribution within the domain of empirical research in the economics of IS tradition. We note that the goal here is not to be exhaustive but to characterize what we believe to be the central tendency (i.e., capturing the most common elements of theoretical contributions as observed in the journal and our experience in managing manuscripts for the journal).

In the context that we are interested in—empirical Econ-IS papers—pure theoretical development is rare. Pure theoretical arguments used within this tradition of research tend to come from affiliated fields, often from theories/models in economics (games of information, contract theory, models of competition, labor and human capital, production theory, and so on) or from research in strategy, sociology, and organizational theory (population ecology, social networks, institutional theory, and information processing). In most cases, Econ-IS scholars adapt arguments taken from these to provide *three* critical aspects that might contribute to a paper’s theoretical engagement. Therefore, for an empirical paper, we propose that a theoretical contribution might comprise the following three components in some measure depending on the objectives of the paper.

Component 1. The theoretical contribution has an overall “theoretical narrative” or “theoretical paradigm” into which the context of the study fits well; this narrative is important because it creates a boundary within which the study and its implications have to be evaluated.

- Papers can benefit from being positioned within a theory; theory serves as a “venue” where scholars have conversations using a common lens. A group of scholars using the same lens to examine and make sense of the same portion of the world is actually working within the same theoretical narrative. Theory thus provides a shared frame for scholars to meet and debate, incorporate new ideas, and agree or disagree with each other on a phenomenon. It is useful for a scholar to claim the venue so that readers are clear about where the work presented seeks to offer a contribution.

- As an example, a study of online dating platforms can be viewed through the lens of signaling and information asymmetry, thereby creating a clear link to prior work in information asymmetry and signaling theory (Bapna et al. 2016). Similarly, a paper on employee training programs in information technology (IT) companies can be viewed through the lens of human capital theory, again allowing the researchers to remain consistent with an identifiable body of knowledge (Bapna et al. 2013). In these cases, the appropriate theoretical lens is evoked to create a boundary for the analysis and claims of contribution. Clearly specifying this lens also allows the right editor and review team to be assembled for the review process, thereby reducing the odds of a mismatch between the paper and the evaluating team.

Component 2. The theoretical contribution has a clearly identifiable set of relationships between the constructs that are being studied, where some are designated as key independent or exogenous variables; others are denoted as key dependent or endogenous variables; and contextual variables are denoted as moderators, mediators, or important boundary conditions.

- Along with the overall “theoretical narrative,” the above-mentioned constructs prevalent within that narrative need to be delineated and defended robustly within the specific research context because the same construct can act as a dependent, independent, moderating, or mediating variable in different contexts within different theoretical narratives. For example, a simple construct, like firm IT spending, can be viewed as an exogenous, endogenous, or moderating variable in different models depending on the theoretical narrative in use.

- In some cases, a subnarrative within the broader narrative may be more relevant and proximal for the purposes of theory construction. For instance, although information asymmetry may be the broader theoretical narrative for a given paper studying secondary markets, a paper focusing on adverse selection may need to only invoke the subnarrative pertaining to adverse selection and its treatment in the paper (e.g., Alhaili et al. 2022, Lin et al. 2023). Therefore, a more focused

description of adverse selection is likely to be more applicable than a discussion of the overall information asymmetry paradigm. Similarly, if the leadership in online communities is being studied through a social networks perspective, specific elements of social networks will need to be invoked and discussed (Johnson et al. 2015) instead of a detailed discussion of social network theory or online communities across all possible contexts. Thus, subnarrative selection becomes an important process in helping to craft a compelling theoretical contribution for a paper.

Component 3. The theoretical contribution has a clear description of what *new links*, relationships, or insights are being added to the underlying theoretical narrative within which the entire paper is set.

Some papers are largely about the independent variable; a new independent variable is being added to an existing body of work, and the theory building in the paper is based on explaining “why” this new variable is needed and “how” it works within the existing nomological network within that narrative. An example of this is the introduction of “practical intelligence” in understanding software development team performance (Langer et al. 2014). In other cases, a new outcome variable is added to the body of work, and again, theory building addresses relevant “why” and “how” questions; examples include the study of prostitution trends in the United States (a new outcome variable for IS research) emerging from the introduction of Craigslist (Chan et al. 2019) and the study of telework adjustment (a new construct) emerging from the coronavirus disease 2019 pandemic (Hou et al. 2023). In yet other contexts, a new interim variable or boundary condition is added to the theoretical paradigm, such as recent work studying commission caps in matching platforms (Li and Wang 2024). In some cases, the “new” insight is generated by new forms of empirical analysis, more advanced empirical methods, or newer forms of variable construction that can help the field cast existing models or theories in a new light. This form of research allows for stronger causal inference or allows for policy simulations through counterfactual analyses. An example of this approach is Pan et al. (2019), where the authors develop a new measure of new entry threat, a construct that has often been used in strategic frameworks of competitive dynamics but has never been adequately measured. The authors use text mining to develop and validate a measure of this construct for subsequent use in theoretical models of innovation and competition to generate new insights. In all of these cases, the reader should be clearly informed about what “new” knowledge or insight is being added to the specific reference theoretical lens through the paper and its analysis. Given this overview of an empirical paper’s theoretical contribution, we

can rather simply answer the broad question of whether theory is relevant for empirical research in the affirmative. We believe that theory is relevant to virtually all types of empirical research published in *ISR*, with a few exceptions that we discuss later. Theory, as described above, helps position research within a larger nomological network of prior research while also serving as a guide for the research process. Furthermore, it helps clarify gaps between the extant theory and newer empirical findings, thereby allowing for the development of new theories. In other words, our view is that theoretical engagement can serve several roles; it can provide an “input” to empirical research, allow the researcher to follow an appropriate “process” for conducting the empirical analysis, and help contextualize the “output” back within the appropriate theoretical paradigm. Broadly speaking, the presence of theory in its various forms and its applicability across domains and time set empirical research in IS apart from other forms of empirical inquiries and reporting that tend to be data driven.

It is worth noting that although the three components discussed above can greatly help a paper establish/sharpen its theoretical contribution, papers can vary in terms of how much emphasis is laid on adhering to these components. An exploratory study examining an emerging phenomenon may not build on pre-existing variables and relationships (Component 2), but it may aim to discover patterns embedded in data, subject to subsequent scholarly validation, that could potentially provide insights for future theory development. Although acknowledging the fact that IS research cannot be equated to natural science, we note that an increasing number of studies in the natural sciences have adopted this genre of research and rely on artificial intelligence techniques to reveal the underlying patterns and mechanisms. Also, in order to provide timely implications to real businesses, IS scholars can, in certain circumstances, choose to be light in the overarching theory (Component 1) and instead, focus on evidence-based inferences that emerge from analyzing big data gathered from business processes. In such circumstances, the authors would need to provide a clear justification for adopting a particular approach to (and the level of) theoretical contributions. *ISR* is a big-tent journal that strives to be open to innovative research of all genres while not disregarding disciplinary practices.

Next, we outline and elaborate on the typical problems that empirical IS papers experience in the review process, keeping the three components (given above) in mind. Thereafter, we offer a taxonomy of theoretical contributions and suggestions for authors and reviewers.

Typical Issues Faced by Empirical Papers in Review

We summarize the common issues identified in empirical papers in Econ-IS with respect to their theoretical contributions below in Table 1. Column 1 presents a short description of the broad “critiques” of papers (critiques 1–6 (C1–C6, respectively)) often raised by editors and reviewers, whereas columns 2 and 3 provide further elaboration on these issues.

As is evident here, many submissions that fail in the review process where the theoretical contributions are implicated do so for reasons primarily associated with the three components of a theoretical contribution outlined above. On one extreme, a paper may lack a coherent and clear theoretical narrative within which the research is embedded (C1 in Table 1); the authors may have pulled opportunistically from multiple theoretical backgrounds without establishing a clear overall theoretical perspective through which the paper can be evaluated. C2 in Table 1 addresses an ineffective theorizing effort, wherein some theoretical arguments are made in the paper but not in a compelling, coherent, or rigorous manner. C3 and C4 in Table 1 are associated with manuscripts that offer inadequate theoretical contributions to the specific IS/IT context at play and back to the reference theory, respectively. C5 in Table 1 is a common occurrence where the paper does not seem to offer anything new theoretically but offers insights that are already well-established in the literature, leading to the “what’s new here?” comment in reviews. Finally, C6 in Table 1 identifies papers where the empirical contributions are the primary focus, while the theoretical contributions, if any, are relatively muted. The relationships examined empirically in these papers may be based on prior theory that exists but has not been tested as yet in a rigorous manner. For each of these six critiques, the authors either have not addressed one or more of the three components (identified above) regarding a reasonable theoretical contribution in adequate measure or have done so but in a cursory manner.

Although Table 1 summarizes the common issues encountered in empirical research, it is worth considering how these problems may be rooted in a more general taxonomy of theoretical contributions that can help guide authors and reviewers. The idea of a taxonomy of theoretical contributions can help authors understand how to position their papers as they are crafted and also evaluate the appropriate responses to reviewer comments received through the review process. In the next section, we provide a simple taxonomy of theoretical contributions for empirical papers observed at *ISR*.

Table 1. Typical Critiques Pertaining to Theoretical Contributions in Submitted Manuscripts at *ISR*

Key issues or critiques	Primary critique of the review team	SE, AE, and reviewers: Reactions and dilemmas
C1: Lack of an overarching theory or narrative in which the paper is located (inadequate treatment of Component 1)	The authors examined an interesting issue, but the theoretical treatment lacks a clear theoretical narrative, making the empirical analysis (and findings) appear ad hoc and unfocused.	<ul style="list-style-type: none"> The authors opportunistically draw from multiple theoretical backgrounds and literatures, creating a “laundry list” of variables and constructs for their analysis. This approach results in a lack of clarity and coherence in their story-telling, and it fails to contribute to relevant scholarly discourse. Reviewers are unclear about the theoretical perspective being used. It is also unclear who the reviewers should be for the paper because it appears to span multiple perspectives. Such papers invite inconsistent reviewer reactions that are difficult for the SE and the AE to resolve, leading to further confusion. The SE and the AE need to sift through the mix of reviews and decide whether the “remixed” set of theories indeed represents a theoretical contribution, especially when the reviews are generally negative. The modal decision here tends to be a rejection.
C2: Weak theory development (inadequate treatment of Component 2)	The arguments leading to the hypotheses offered in the paper are weak and logically incomplete. There are inadequate explanations for the central mechanisms that are postulated in the paper.	<ul style="list-style-type: none"> The authors provide weak arguments in support of their hypotheses. Often, the theory section includes many references and arguments, but it is not done in a cogent and coherent manner. The reviewers respond with a negative review, claiming that the arguments are speculative and not compelling. These judgments are often shared across the review team, even if there is an element of subjectivity in them. Offering an invitation to revise and resubmit can be troubling for the SE and the AE as well as for the authors because the refurbished theoretical arguments may not be empirically testable or even feasible. The result is an impasse.
C3: Lack of engagement with and a contribution to the IS literature (inadequate treatment of Components 2 and 3, with specific reference to prior IS research)	The authors used IT/IS as a general background to test a general theory without attempting to adapt to an IS context.	<ul style="list-style-type: none"> The SE and the AE need to bank on the collective wisdom of the review team to evaluate the theory development and/or suggest concrete ways to make the theoretical arguments more compelling. The authors claim that developing a general theory makes sense to both academia and management. The reviewers, however, demand insights and theory development <i>specific to IS/IT</i>. The SE and the AE need to make a call as to whether and to what extent an IS-field journal needs to offer IS-specific theoretical contributions and to what extent general theories are likely to be of interest and relevant to the IS/IT audience.
C4: Lack of a relevant theoretical contribution back to the original theoretical narrative (inadequate treatment of Components 2 and 3, with specific reference to the reference theory)	The authors borrowed arguments from a theoretical narrative in another field to analyze an IS phenomenon, but it merely provided a replicative study of the theory just in another context.	<ul style="list-style-type: none"> The authors focus on motivating the need to solve an IS problem (a real business problem), and that borrowing the reference theory makes sense. The reviewers, however, expect a contribution to the development or an extension of the original reference theory itself.
C5: Inadequate theoretical insight generated by the paper in general (inadequate treatment of Component 3)	The authors conducted rigorous tests of theoretical relationships proposed in prior research or developed (and tested) a set of hypotheses that are largely unoriginal and add little (if any) new insight to the existing theoretical narrative (“ <i>We already know all this from prior research</i> ”).	<ul style="list-style-type: none"> The SE and the AE need to evaluate whether enlarging the boundary of the original theory (by including an IS context) is sufficient or even necessary. Is adding an IT-specific element relevant to the original theoretical narrative? Alternately, is it enough to adapt that theory to the IT context? The authors motivate the study by pointing to prior work based on conjecture and/or case studies that lack a systematic body of evidence. Or, the authors focus on small tweaks to existing theories without building out fresh insights. The reviewers remain unsatisfied with the novelty or originality of the work. The authors provide some cosmetic work in the revision, which failed to satisfy the review team. The SE and the AE need to guide the authors to clearly expand the theoretical contributions beyond what appears largely derivative. The review team may provide ideas or suggestions to enhance the paper’s contributions, without clear consideration of their empirical feasibility. In such cases, the possibility of the paper going through without a significant reworking of the theoretical contribution is small.

Table 1. (Continued)

Key issues or critiques	Primary critique of the review team	SE, AE, and reviewers: Reactions and dilemmas
<p>C6: The empirical results is the key contribution of the paper, with minimal theoretical engagement or contribution (inadequate treatment of Component 2, with Component 3 primarily driven by empirical results)</p>	<p>The authors focused on using empirical data and tests to demonstrate the usefulness of a solution to a practical problem.</p>	<ul style="list-style-type: none"> • The authors motivate the study by the practical importance of the research question; however, the reviewers question whether empirical contributions and practical value alone were sufficient to meet the bar of a top journal. • As to the practical impact, the SE and the AE need to assess practical impacts consistently and offer guidelines to help sharpen the claims of practical impact made by the authors. • For a manuscript with minimal theoretical engagement, the SE and the AE need to keep a high threshold for its practical impact, which may exceed the expectation of the authors.

Note. AE, associate editor; SE, senior editor.

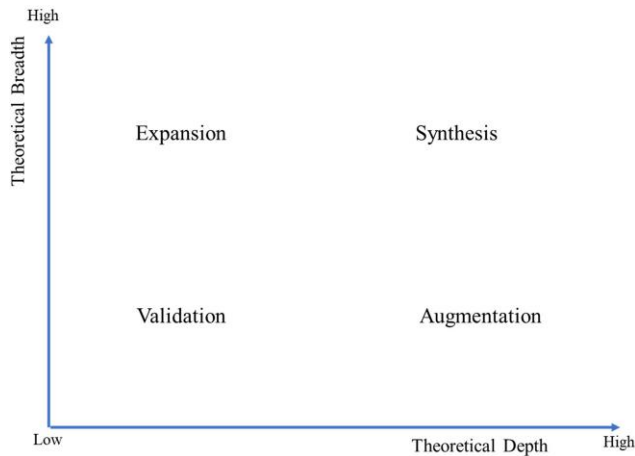
A Taxonomy of Theoretical Contributions in Empirical IS Research

To promote a clearer understanding of theoretical contributions, particularly among empirical Econ-IS researchers, we propose a taxonomy that highlights different types of theoretical contributions typically seen in *ISR* submissions. As mentioned before, our discussion is not intended to apply to pure analytical modeling or computational papers; we consider papers that are largely empirical in nature that build off of models provided in the paper to guide hypothesis testing. We note, however, that this taxonomy is not exhaustive (i.e., there may be manuscripts that “fall through the cracks” or show up in more than one category). That said, our experience suggests that a vast majority of *ISR* submissions that use empirical methods within Econ-IS can be categorized into one of the types that we discuss below.

In constructing a taxonomy of theoretical contributions observed in empirical papers in IS, we build on prior work and use primarily two dimensions—*theoretical breadth* and *theoretical depth* (Colquitt and Zapata-Phelan 2007). *Theoretical breadth* refers to the range and diversity of theoretical perspectives and approaches used to explain phenomena. It captures how broadly a paper explores and applies various theoretical lenses to understand or address problems. The key aspect of theoretical breadth is the rigorous and compelling integration of these diverse perspectives rather than their mere aggregation. This approach promotes a more holistic view by combining a variety of conceptual ideas and fostering connections across multiple perspectives. Because IS research is, by definition, a discipline that sits at the intersection of several reference disciplines, it is often necessary to introduce and align perspectives from multiple theoretical paradigms in a coherent manner. *Theoretical depth*, on the other hand, indicates the thoroughness with which a single theoretical framework is examined, extended, and tested using empirical methods within the paper. It reflects how deeply a paper delves into and explicates the nuances and subtleties of a particular theory to understand or address problems. The focus of theoretical depth is on the detailed and robust application of one theory, demonstrating profound expertise and a nuanced grasp of that specific theoretical perspective.

Based on these two dimensions, we find that approaches toward developing theoretical contributions observed in empirical studies roughly fall into four categories, reflecting the depth and breadth of their theoretical engagement. We label these categories as *validation*, *augmentation*, *expansion*, and *synthesis* (Figure 1). It is essential to clarify that this taxonomy pertains to only the theoretical aspects of manuscripts and does not seek to capture their overall quality, novelty, or originality in any way; we address these aspects

Figure 1. (Color online) A Taxonomy of Theoretical Extension in Empirical IS Research



later. We acknowledge that studies that offer valuable empirical insights, even in the absence of explicit theoretical contributions, can significantly influence the field; we address these papers later as well. It bears repeating that because of the complexities and variations involved in the approaches, not all articles can be neatly categorized; we present this typology solely to foster a systematic and a more nuanced understanding of theoretical contributions within the empirical Econ-IS area. We discuss our taxonomy below.

Validation

This category typically includes articles that present significant and timely empirical findings, putting relatively less emphasis on generating new theoretical contributions or breaking new theoretical ground. These studies primarily aim to validate predictions without extensive theoretical groundwork, often derived from existing theories that may not be specific to IS and/or that have not been tested before. Typically, they examine established models in novel IT-driven contexts or domains to verify that they perform as anticipated (Whetten 1989). Validation research is important because first, the process of validation holds theoretical significance, albeit in a limited sense. This form of research helps introduce a given theory to explain a phenomenon and can lead to the refinement or eventual development of (new) theory by confirming or challenging pre-existing theoretical assumptions and relationships. Validation can assist in identifying the boundary conditions for the applicability of the focal theory in different contexts. Research findings that validate theories provide a solid foundation for enhancing the understanding and prediction of phenomena/behaviors and enabling theory-informed recommendations, interventions, and practical applications.

Validation research can provide multiple forms of insight to the community. At baseline, validation research tests existing theory, helping to validate or invalidate theoretical relationships and examine boundary conditions. In addition, validation papers can help with effect size quantification in some cases and testing the efficacy of recommendations that may be derived from existing theories for individual or firm behavior. Interventions and managerial implications based on existing theoretical analyses in the literature need to be tested using data before they can be either enhanced theoretically or abandoned as being ineffective. Validation research helps with these goals, helping to clarify the impact of existing theory on human and firm behavior through rigorous empirical testing but within an existing theoretical paradigm.

In linking validation research to the components of a theoretical contribution discussed earlier, we argue that papers adopting the validation approach should focus on strengthening Components 1 and 2; they should be placed within a clearly identified theoretical narrative and should be expansive in terms of what specific relationships are being studied empirically within this theoretical narrative. Because the focus is often on testing existing theoretical relationships and their boundary conditions, locating this research within a well-defined theoretical paradigm is essential. Furthermore, given a limited focus on theory building per se, it is imperative that authors clearly enunciate the study variables and their boundary conditions as well as their linkages to existing theoretical work. Component 3 is not likely to be particularly novel, and it will largely emerge from a strong focus on Components 1 and 2. The focus of Component 3 likely comes from the insights obtained from the theory testing in the form of a clearer boundary condition of when the theory works or does not work, the direct applicability of the theory, and the significance and magnitudes of effects, which bring practical relevance, allowing future researchers to estimate the applicability and effects of theory in different conditions and contexts.

Several recent papers published in *ISR* provide instances of validation research, exploring a variety of topics relevant to IS scholars while also pushing the envelope within the discipline. These papers typically focus on addressing interesting and time-sensitive issues by leveraging existing knowledge about IT processes, artifacts, or digital environments while staying within the methodological boundaries of the economics of IS research. Drawing on routine activity theory from criminology, Park et al. (2021) demonstrated that ride-sharing can reduce crime in urban areas, particularly those related to sexual assault and rape at times when taxis are less available. The authors argue that ride-sharing provides a more reliable and timely transportation service, which significantly reduces the risk of sexual

crimes, a prediction that falls out of well-established theories in criminology and economic geography. Within IS research, the authors provide evidence for the societal benefits that accrue from ride-sharing. The paper thus addresses a topical and impactful research question while also validating existing theoretical predictions in the literature from both IS and criminology. Using a similar approach but in a different context, Oh et al. (2022) explored the relevance of self-presentation theory (Baumeister 1982) in explaining news-sharing behaviors on social media platforms. The authors utilize an existing theoretical framework to validate its applicability in the context of social media content sharing.

Validation-based research thus reports on highly impactful and relevant research questions and forms an important part of the literature within Econ-IS. This research focuses primarily on empirically validating well-established theories in new IT-facilitated environments while also exploring various boundary conditions and heterogeneities in the identified effects. It tends to pay limited attention to extending these theories beyond their original frameworks. In essence, although exceptions may exist, articles in this category generally focus on the *contextual validation* of existing theories and can provide significant value to the field through this approach.

Augmentation

Articles in this category seek to enhance the depth of existing theories through a more extensive examination of postulated relationships in prior theory. Such examination often comes in the form of a deeper analysis of potential moderation and mediation relationships as well as an examination of various specifications and structural relationships within the existing theoretical narrative. Path dependency plays a crucial role in the progression of academic research, especially in the study of IT artifacts. As technologies move through their life-cycle stages, their effects on individuals, organizations, markets, and societies become increasingly complex and multifaceted. This complexity necessitates more than just a foundational understanding; it calls for a nuanced theoretical perspective to grasp the evolving impacts comprehensively.

Although initial theoretical analyses often provide a broad theoretical foundation for exploring causal relationships involving IT artifacts, this foundation can sometimes remain a “black box.” This occurs because of insufficient detail and a lack of nuanced understanding of the nomological network—the system of relationships among key constructs involved in the IT phenomena under study. The “black box” can be opened up through moderation, mediation, latent variables, or specification. Although not uniformly applied to all cases, moderation typically explores how the strength or direction of technological effects changes under

different conditions. Mediation, on the other hand, can capture the mechanisms through which technologies influence outcomes, showing intermediate processes that may explain their impact. Similarly, latent constructs aim to capture some unobserved characteristics in the process that would influence the outcomes. Finally, specification may refine theoretical constructs to more precisely capture the nuances of technologies in question in different contexts.

The augmentation approach is particularly effective in addressing two critical challenges: (1) when existing research does not fully capture the complexity and depth of IT artifacts and their impacts (C3 in Table 1) and (2) when theoretical frameworks are applied to the IS domain without appropriate adaptation (C4 in Table 1). Fundamental to crafting theoretical contributions, the augmentation approach involves introducing substantive moderators and mediators to augment existing theories, relationships, or processes in IT-related contexts or digital environments. As outlined by Whetten (1989), the introduction of new moderating, mediating, or specifying variables illuminates “when,” “how,” or “for whom” a causal relationship or process operates. For instance, augmentation strengthens an existing theoretical narrative by focusing on specifying boundary conditions, thereby enhancing its depth vertically within a theoretical paradigm rather than expanding their theoretical scope. Consequently, this approach often results in qualitative enhancements in understanding the limits of a theory rather than broadening its applicability by incorporating adjoining theories (Whetten 1989). The augmentation approach becomes essential when current frameworks and empirical findings are insufficient to capture nuances or underlying mechanisms behind phenomena.

In terms of the components of a theoretical contribution discussed above, augmentation papers have to critically establish Component 1; the theoretical narrative of interest has to be established clearly and unambiguously up front. Furthermore, Component 2 is also critical because this entails informing the reader about the manner in which the existing theory is being augmented, including which key variables or new relationships are being added, transformed, or modified in this examination of the existing theory. Finally, Component 3 remains important as a way of establishing how the initial model is being revised and why the paper’s insights are important. Readers have to be informed suitably about what the new and augmented view of the theoretical narrative is as a result of the paper’s empirical results.

We consider examples of recently published papers that fit this particular form of research. Li et al. (2022) exemplify the augmentation approach in their study on how recommender systems influence behaviors leading to actual consumer purchases. In doing so, the

authors try to open the black box of the recommender systems by examining the intervening process of consideration set formulation—the mental short list that consumers create before making a purchase. By focusing on this mediating effect, the authors provide a more in-depth understanding of how recommender systems impact consumer decisions. This study vertically adds to the theoretical model of consumer behavior in the context of recommender systems, enriching the literature with a more detailed view of these influences in a well-designed mediation analysis.

In another example that highlights the use of moderation, Guan et al. (2023) draw on expectation confirmation theory to investigate the effects of customer-generated images (CGIs) on postpurchase satisfaction in the context of online commerce as measured by product reviews. They leverage the augmentation approach by examining how various factors, such as the aesthetic quality of images and reviewer face disclosure, moderate the effects of CGIs on product reviews. Their work adds to the literature by introducing new moderators and empirically testing their efficacy while also building theoretical links to the current literature to justify the addition of these variables. Finally, the work of Li and Wang (2024) represents an example of the augmentation approach using specification. The authors draw on the general theory of power asymmetry between platform owners and participants, drawing from the large literature on double-sided platforms. By specifying large delivery platforms (i.e., Door Dash) as platform owners and local restaurants as platform participants, they examined how a specific intervention—a cap on commissions—implemented by owners affects matching dynamics and platform participation. In summary, articles in this category contribute to a moderate level of theory building (Colquitt and Zapata-Phelan 2007) by augmenting the theoretical depth of existing theories through the introduction of moderation, mediation, and specification.

Expansion

Papers within this category introduce *alternative* theoretical perspectives to examine a phenomenon already explained by empirically validated causal relationships in existing theories, thereby expanding the footprint of theory provided in the paper. Theory functions as a vital lens through which we observe, explain, and predict various phenomena. The choice of theoretical lens profoundly influences our interpretation and understanding since each theory brings its unique perspective and focus. Given the complexity inherent in IT artifacts and their impacts, relying solely on a single theory often proves inadequate for a comprehensive understanding of the underlying context. Embracing a diverse range of theoretical perspectives is often essential to grasp the multifaceted nature of these challenges. Expansion thus

aims to broaden or expand the spectrum of theoretical explanations for the phenomena under investigation by introducing “entirely new points of view” (Huff 1999). This approach initiates fresh conversations rather than merely contributing to current ones.

By expanding the theoretical landscape, expansion-oriented papers not only enrich our understanding but also, guide more nuanced and balanced decision making for scholars and managers regarding the adoption, deployment, and regulation of these technologies. The expansion approach is particularly useful when addressing critical issues often faced in IS research, such as the lack of novelty (C5 in Table 1), and potentially, limited theoretical impact (C6 in Table 1). In IS research, there is often a struggle to present novel insights within traditional theoretical confines. The expansion approach can address this issue by combining existing ones innovatively or introducing new theoretical lenses. This infusion of fresh perspectives can uncover new dimensions of IT artifacts, revitalizing narratives with fresh perspectives that are both innovative and relevant. Likewise, this approach enriches our understanding by introducing alternative viewpoints, thereby enhancing their explanatory power, relevance, and impact.

In terms of how expansion is linked to the components of theoretical contributions discussed above, it is clear that Component 1 becomes particularly important, especially because these papers tend to move beyond a focal theoretical narrative by including alternative perspectives. Therefore, it is incumbent on authors to ensure that these theoretical narratives, both focal and ancillary, are identified unambiguously. Component 2 entails discussing the choice of variables from multiple theoretical narratives and arguing for their seamless and logical integration, and hence, it is more difficult in expansion papers than in augmentation papers, where only the theoretical paradigm is explored. We find that Component 2 is a significant obstacle for authors attempting expansion papers, especially because a viable balance has to be found in extending key variables from one theory to another. If done successfully, Component 3 is paradoxically easily achieved; the new insights emerge rather organically from the very nature of expansion-oriented papers. The critical challenges in such papers typically lie in achieving the required goals in Components 1 and 2, respectively.

We briefly discuss some papers that use the expansion approach. Drawing from rational addiction theory in economics, Kwon et al. (2016) provide alternative theoretical explanations for technology-induced addiction related to, for instance, mobile games and social network apps. Medical scientists have long treated technology-driven addiction as a chronic disorder stemming from biological or neurological predispositions. Social scientists, on the other hand, have conceptualized addiction as an irrepressible response driven by the dynamic

interaction between heredity and the social environment. In contrast to these medical and sociological explanations, Kwon et al. (2016) utilize economic theory to conceptualize and validate the phenomenon of “app-diction” (i.e., addiction to mobile apps) in the context of mobile games and social network services.

In yet another example of expansion-oriented research, Gopalakrishnan et al. (2022) theoretically expanded our understanding of the factors influencing the adoption of interorganizational systems (IOSs) by bringing together multiple perspectives. Although previous theoretical assessments of IOSs primarily focused on a trust and cooperation-based perspective of interorganizational relationships, the authors employed arguments from transaction cost economics (Williamson 1979) as an alternative lens with which to view IOSs. When viewed through these combined perspectives, the authors show how central constructs, like technological modularity of firms that participate in IOSs, need to be re-evaluated for their role in ensuring efficacy. Essentially, by combining multiple perspectives, the authors provide a more nuanced and qualified view of IOSs and their adoption, especially during times of technological change. Their treatment goes beyond simple validation because the theoretical contribution in the paper provides arguments for why one theoretical perspective may be inadequate or inaccurate while bringing in viewpoints from an alternative perspective. Thus, the theoretical footprint of the paper is significantly larger than would be expected in a validation exercise. In summary, studies in this category strive to enhance the breadth of theoretical explanations for IT-related phenomena or artifacts through the *horizontal expansion* of current theoretical understanding.

Synthesis

Our final category, synthesis, includes papers that amalgamate concepts from multiple theoretical sources to construct comprehensive frameworks that explain the dynamics of relationships or processes involving the phenomenon under study. Synthesis, closely aligned with theory building, may involve introducing new constructs or significantly reconceptualizing existing theories (Colquitt and Zapata-Phelan 2007). However, it is important to note that theoretical synthesis goes beyond the mere physical addition or aggregation of multiple theories—an approach that does not “substantially alter the core logic of the existing model” (Whetten 1989, p. 492). Instead, synthesis entails a “chemical” integration of two or more theories to create a framework that profoundly reshapes our understanding of phenomena by “reorganizing our causal maps” (Whetten 1989, p. 493). Through this approach, both the depth and the breadth of theoretical understanding are substantially enhanced and expanded (Colquitt and Zapata-Phelan 2007). By chemically combining multiple perspectives and deeply

exploring them, researchers can offer more nuanced, novel, and impactful insights. However, achieving both depth and breadth poses challenges, which is why empirical IS research in this category remains relatively uncommon.

Given the combination of key elements from both the *augmentation* and *expansion* approaches, synthesis-oriented papers are especially valuable in tackling critical challenges in IS research. This approach can effectively address the various issues identified in Table 1, including challenges with theoretical integration (C1 in Table 1), inadequate theoretical explanations and weak underlying mechanisms (C2 in Table 1), insufficient adaptation to IS contexts (C3 in Table 1), lack of novelty (C5 in Table 1), and the absence of significant theoretical contributions (C6 in Table 1). By synthesizing diverse theoretical perspectives with both depth and breadth, this approach addresses weakness in existing mechanisms, adapts theories deeply to IS contexts, introduces novel insights, and ensures significant theoretical integrations and contributions. Furthermore, it promotes creativity and innovation in theoretical contributions by blending diverse viewpoints. Ultimately, this approach ensures that research contributions are theoretically significant, offering deeper and novel insights and broader applications.

When linking synthesis papers to the three components of a theoretical contribution described above, we note that authors attempting synthesis papers will need to pay attention to all three components. Because synthesis involves theory building, often across multiple theoretical perspectives, the threshold for achieving Components 1 and 2 is understandably high. Authors will need to identify the multiple theoretical narratives that are at play clearly while also ensuring that the appropriate constructs are considered within each narrative and combined in a competent manner. Furthermore, Component 3 in such papers will span new insights on both new theory as well as empirical analysis, and hence, it can be expected to be more challenging for authors. For these reasons, as mentioned above, it is perhaps understandable that relatively few published papers can cleanly be associated with synthesis.

A good example of synthesis may be seen in recent work by Andrade-Rojas et al. (2024), where the authors explore the challenges faced by small- and medium-sized enterprises (SMEs), particularly concerning technology and government support deficiencies. They position IT as a transformative means to mitigate these innovation hurdles. Through a masterful synthesis of theoretical narratives, including absorptive capacity (Cohen and Levinthal 1990), open innovation (Chesbrough 2003), metaroutines, and practiced routines from IT perspectives, they construct a comprehensive framework that elegantly illustrates how IT-facilitated open and closed innovations can assist SMEs in

overcoming these challenges. At the core of their contributions lies the enriched conceptualization achieved through the fusion of absorptive capacity and innovation theories, reinvigorated with novel IT constructs: information technology use for closed innovation activities (ITC) and information technology use for open innovation activities (ITO). By imbuing these concepts with fresh insights and synthesizing them with metaroutines and practiced routines, the authors propose a powerful strategy. This synthesized theory suggests that a paradigm shift, which is characterized by digitalizing rules, procedures, and norms driven by ITC and ITO, offers SMEs a pathway to resilience and competitiveness in an increasingly dynamic landscape. Through the amalgamation of multiple theories and the introduction of new constructs, this paper significantly contributes to theory both in depth and breadth.

In yet another example, Adamopoulos et al. (2018) use the synthesis approach in their research on social media by integrating insights from social media and marketing research with the five-factor personality model widely used in psychology. The authors apply the widely used five-factor model to social media discourse in understanding how individuals may influence others through their social media postings. Their study also incorporates elements of machine learning: training an algorithm to assess the big five personality traits from social media data. This algorithm is then employed in a quasiexperimental setup to examine how Electronic Word of Mouth (eWOM) from users with specific personality traits can influence others on these platforms. The research is a prime example of synthesis of diverse literatures, empirical methods, and disciplines, including computer science, marketing, IS, and psychology. It showcases the value of synthesis by deriving a novel explanation for the role that eWOM played in marketing based on the personality of creators, which differs significantly from prior research that heavily depended on analyzing the textual contents of eWOM.

Our proposed typology of theoretical contributions, as shown in Figure 1 and described above, is focused on identifying approaches that authors may choose to position their papers prior to submission to *ISR*. The goal here is to assist empirical IS researchers with clearly delineating and crafting suitable theoretical contributions. Table 2 outlines the key characteristics of each category, including its theoretical orientation, appropriate use, key benefits, relevant critiques, and examples. Although the taxonomy provided in Table 2 describes the types of theoretical contributions, we did not categorize papers by their overall quality or value. Scholars may choose to adopt a specific template for their submitted work to *ISR*, but the manner in which the paper achieves its goals and provides value to the field and more broadly, to industry and society remains distinct from the type of theoretical contribution. In other

words, our taxonomy identifies the modes of theorizing that may be chosen by authors but not an evaluation of the final product. The quality and the value of the submission (within a category of theoretical contribution) remain attributes that are evaluated by editors and reviewers within the review process, and they remain distinct from the form of theoretical contribution chosen by authors. However, do novelty and practical impact matter in these contexts? We discuss these issues in turn below.

Novelty and Originality

The novelty of a study determines its long-term impact (Uzzi et al. 2013), and typically, it encompasses both aspects of theoretical development as well as empirical rigor in addition to the choice of a research question. A previous editorial (Gupta 2018) discusses two dimensions of paper quality: (1) *q-quality*—the quality of an idea or treatment in a paper that has a “discovery” nature to it and tends to answer the difficult “why” question that is common to the social sciences—and (2) *r-quality*—the quality of the execution of the paper, which may be improved through the review process by robustness tests, better data sets, linkages to existing theories, and so on. Gupta (2018) writes that a useful way to think about the two forms of quality is in a lexicographic manner (i.e., the paper should be judged on its *q-quality* first), and only if it passes muster should *r-quality* be considered. We submit that *q-quality* points to the spirit of pursuing novelty and originality within the paper in both the theoretical contributions as well as the empirical methodologies and results.

Novelty and originality represent an approach to theory building that introduces elements that were not previously associated with that theoretical narrative (Leahey et al. 2023) or provide scientific discoveries with unique knowledge that were not available from previous studies (Shibayama and Wang 2020, p. 410). With the research domain, novelty of work has long been associated with the growth of knowledge; new discoveries, new techniques, and new laws or principles of social and natural sciences have all been viewed as novel and instrumental in growing a discipline or field beyond its current boundaries (Kuhn 1962). Originality thus complements novelty in a very specific way; novelty allows for the same phenomenon to be studied or viewed in a new and fresh way, whereas originality allows the scholar to bring in something new, in terms of data, methods, forms of testing, instrumentation, or experimentation, that was not used or available before (Mulkey 1974, Guetzkow et al. 2004). Through original and novel research, “excess content” is created in a field that then helps broaden and refine existing explanations within the field, thereby allowing the discipline to move toward newer areas of exploration.

Table 2. Four Categories of Theoretical Contributions in Empirical IS Research

Approach	Orientation	Relevant use	Key benefits	Examples
Validation	Contextual validation (i.e., validation or invalidation of existing theories within IT-driven contexts or environments)	<ul style="list-style-type: none"> Addressing issues associated with time-sensitive and high-impact technologies Investigating urgent practical applications Existing theories fail to address the core issues directly 	Provides theory-informed actionable insights rapidly, enhancing the impact of research	Park et al. (2021), Oh et al. (2022)
Augmentation	Vertical augmentation (i.e., augmentation of existing theories through moderation, mediation, or specification to provide a deeper understanding of IT-related problems)	<ul style="list-style-type: none"> Deepening exploration through mediation, moderation, and specification to enhance existing theories Uncovering the underlying mechanisms that drive causal relationships Improving the adaptation of theories to IS contexts 	Adds depth and introduces new dimensions to current theories	Li et al. (2022), Guan et al. (2023), Li and Wang (2024)
Expansion	Horizontal expansion (i.e., provision of an alternative theoretical lens to existing ones to broaden the theoretical understanding of IT-related problems)	<ul style="list-style-type: none"> Tackling complex issues that demand multifaceted perspectives Examining the dual nature of emerging technologies and their sociotechnical impacts Introducing innovative perspectives to solve existing problems 	Captures the breadth of impacts and implications thoroughly	Kwon et al. (2016), Gopalakrishnan et al. (2022)
Synthesis	Conceptual fusion (i.e., seamless integration of multiple concepts and theories to derive new constructs and provide novel explanations and solutions for complex IT-related problems)	<ul style="list-style-type: none"> Integrating diverse theoretical perspectives into a unified framework Analyzing emerging technologies through fresh and innovative lenses Generating nuanced and impactful theoretical insights 	Develops innovative and comprehensive frameworks with both depth and breadth	Adamopoulos et al. (2018), Andrade-Rojas et al. (2024)

The taxonomy of the four types of theoretical contributions is related to the novelty/originality in that, everything else being equal, a validation study is lower, compared with the other three, in the potential to bring in new theoretical elements. A validation study, however, can still feature remarkably high novelty. A notable exemplar is the use of computational linguistics to depict the competition landscape for a firm (Hoberg and Phillips 2016). Based on the novel approach to competitive interactions, new insights surface through validating/invalidating previous theories regarding the firm’s competitive moves. This example showcases the distinction between a study’s potential to make a theoretical contribution (as analyzed in Table 2) and its novelty and originality. The latter is broader and can be achieved through engaging in novel and original thoughts, jumping in a blue-ocean topic, and inventing or using new methodological instruments (data, algorithm, instrument, etc.). On the surface, it may be argued that synthesis or expansion has a higher probability of generating novelty and originality, especially in theoretical contributions, and indeed, this may well be the expectation of a given

review team when it receives such manuscripts for review. However, we contend that beyond the taxonomy of theoretical contributions per se, there is still a process of evaluation for novelty and originality (aligned with q-quality) inherent in the review process that cannot be undermined.

Practical Impact

Earlier, we had asked the following question. “Can a paper be considered viable if it is empirically strong but without a clear theoretical contribution?” The answer is yes. *ISR* is willing to publish research having *unquestionably compelling impact* on practice (although the focus of this editorial is on theoretical contributions, and to date, cases of published papers with practical impact being the major highlight are relatively few). We have suggested in Table 2 that “validation” and “expansion” are two approaches for a manuscript with an empirical focus to enhance its theoretical contribution. Alternatively, the manuscript can still be crafted to be potentially publishable by sharpening its practical impact. In this section, we provide some thoughts on when and why this can happen at *ISR*.

Practical impact may be measured by *the extent to which a paper influences the status quo, affecting our understanding of phenomena and our ability to predict behaviors and future events*. A study has a practical impact if and only if its results can influence practitioners and policymakers *directly* through actionable implications or insights. In the context of IS research, there exist a number of stakeholders, such as policymakers and industry leaders, who can be directly influenced by the outcomes of research. Entities, such as the National Institutes of Health, the U.S. Congress, the U.S. Patent Office, and the Department of Education and Commerce, have sought and received advice and guidance from published IS research, to name just a few policy-making entities. Furthermore, large platform firms, like Meta, Microsoft, Amazon, and Google, continue to work together with IS researchers on projects that provide very direct and actionable advice on strategic issues facing their everyday operations. Outside the United States, firms such as Alibaba (China), Infosys Technologies (India), Samsung (South Korea), and DBS Bank (Singapore), to name just a few, have worked with IS researchers collaboratively in addressing practical problems through joint research projects. We believe that papers that are a result of these collaborations and provide direct and practical impact should continue to be solicited and published at *ISR*, when found appropriate. We provide some examples of papers that have taken this pathway below.

As an example of a recent award-winning paper with significant practical implications, Gunarathne et al. (2022) used secondary data from Twitter to identify instances of racial bias in business to consumer (B2C) interactions within the airline sector. The authors show bias in customer service responses to complaints registered on Twitter against the airline companies, with users who are Black customers receiving fewer responses to their complaints compared with similar White customers. Importantly, these results vanish when visual cues about the customer's identity are withheld, thereby providing direct implications for practice. Unlike disciplinary journals such as *ISR*, broader outlets, such as *Science* and *Proceedings of the National Academy of Sciences*, have a greater capacity to publish research with impact that *primarily* lies on the practical side. One recent example of this type of work is by Watson et al. (2024), who provide direct evidence showing that the introduction of digital monitoring (using mobile smartphones) systematically reduces the extent to which police officers self-report instances of their interactions with citizens. The results of the study, as reported, have direct implications for how digital technologies may and should be used in sensitive contexts, such as policing, especially in crowded, urban areas, like New York City.

We suggest that practical impact can be characterized by two dimensions. First, the impact can be characterized

by *the significance* of the identified effects in the paper (i.e., how profoundly it affects people's understanding and behaviors) measured in terms of certainty, confidence, direction, and magnitude. Obviously, significance of a work can be influenced by what is considered a "burning" or "enduring" issue in business and/or society at a given time. A statement of these potential effects and their implications for future practice should be included in the paper so that reviewers and editors can make the appropriate assessments. Ideally, these statements should incorporate quantifiable data or evidence so that suitable editorial evaluation of significance can be made. Second, impact can be characterized by the *scope* of the effect (i.e., the breadth of the impact in terms of the number and varieties of entities or contexts affected). Here again, clear statements of the scope of the implications on practice will be needed to establish that the paper does indeed "change the conversation" within a field of practice or industry. We believe that for purely (or predominantly) empirical papers that seek to make it through the review process at *ISR*, a relatively high bar in terms of practical impact would have to be met. As authors seeking to "thread the needle" using practical impact, it would be necessary to provide arguments addressing how the empirical analysis in the paper provides both *significance* (in terms of the magnitude of effect) and *scope* (in terms of the number of entities affected).

A recent *ISR* paper (Gao et al. 2021) can help illustrate the scope and significance of practice impact. Gao et al. (2021) investigated the impact of fundraising on a crowdfunding platform by a kindergarten to 12th grade (K12) school's teacher on the academic performance of students. The entities that are directly affected are teachers and students of K12 schools; in particular, those in schools that face a reduced public budget for education can directly benefit from this research. According to the authors, public funding for K12 education has continuously declined during the past several decades, making the opportunities on crowdfunding platforms particularly relevant. The paper's implications extend to a significant number of stakeholders, including school administrators, policymakers, and political entities. Indeed, the paper addressed a question that remains central in the education sector in the United States. Should K12 teachers be encouraged to use crowdfunding platforms to raise funding? The authors write that "while the Austin Independent School District in Texas and the Chandler Unified School District in Arizona openly encourage their teachers to seek donations online, 67 school districts in Ohio (Cachero 2019) as well as Nashville's public school system (Del Valle 2019) explicitly forbid their teachers to do so" (Gao et al. 2021). The authors provide vital information to inform this debate; they

show that the average test scores of students whose teachers receive crowdfunded donations increase by 1.934 points relative to those of students whose teachers do not receive crowdfunded donations. The authors also show that this effect is not trivial because even small amounts of crowdfunded support (approximately \$16.00) can have a significant impact on student outcomes. This paper serves as an illustrative example of research with practical impact given the scope and breadth of its implications.

When we juxtapose the types of theoretical contributions discussed above and the role of practical impact outlined here, we can combine these quite simply, as shown in Figure 2 below. On the *y* axis, we plot *practical impact*, whereas the *x* axis shows the types of *theoretical contribution* approaches in increasing order of value. The indifference curve represents the overall value of a research project. As we can see, to be viable as a potentially publishable paper in journals such as *ISR*, in the absence of significant theoretical contribution, we expect the practical impact to be truly noteworthy. Likewise, even though we expect all Econ-IS empirical manuscripts to offer practical implications, a high level of theoretical contribution may be able to compensate for less than stellar practical impact. Overall, we suggest that the goal of the research activity would be to find ways to move the work beyond the indifference curve to the upper right corner of Figure 2 (that is, to produce research with higher levels of value with respect to practical impact and our understanding of theoretical contributions).

Summarizing our arguments here, we believe that for studies that resort to the possibility of getting published based on high practical impact without a significant theoretical contribution, the threshold for practical impact has to be set at a much higher level than would be for papers that also aim to provide a theoretical contribution. Correspondingly, even papers with strong theoretical contributions would benefit immensely by being responsive to questions of practical impact on firms, society, and individuals. Papers that offer both types of

value represent, of course, the ideal type, and they can remain a goal for all scholars to aspire to. This logic is displayed in Figure 2.

Guidelines—Crafting Compelling Theoretical Contributions in Empirical IS Research

Having discussed the components of a theoretical contribution, the typical pitfalls that authors face as they craft theoretical contributions for their manuscripts, and the taxonomy of theoretical contributions typically seen in empirical Econ-IS papers, we now provide a short set of guidelines to assist authors and reviewers as they navigate this process. Because providing a theoretical contribution largely remains an essential part of a submission to top-ranked journals like *ISR*, we hope that these specific guidelines are helpful as heuristics when papers are being crafted. In the interest of clarity, we structure our guidelines by the issues/critiques discussed in Table 1, and we link these to the three components of theoretical contributions discussed earlier.

As authors work toward crafting their theoretical contribution, we urge authors to consider the following three questions clearly.

1. The “what?” question (context)

• Providing Component 1

Is the specific context in which the research is situated described clearly and unambiguously? What is the specific theoretical narrative (subnarrative) that is the most relevant to the study? Is this theoretical lens used cleanly and convincingly to frame the study?

• Addressing C1

Ensure that any confusion about the overall “narrative” or the overarching theoretical lens for the paper is resolved in the paper.

Identify the target audience for the research in terms of the academic community, stakeholders, and practice-based audience for the paper.

Figure 2. (Color online) Research Value as a Function of Practical Impact and Theoretical Contribution

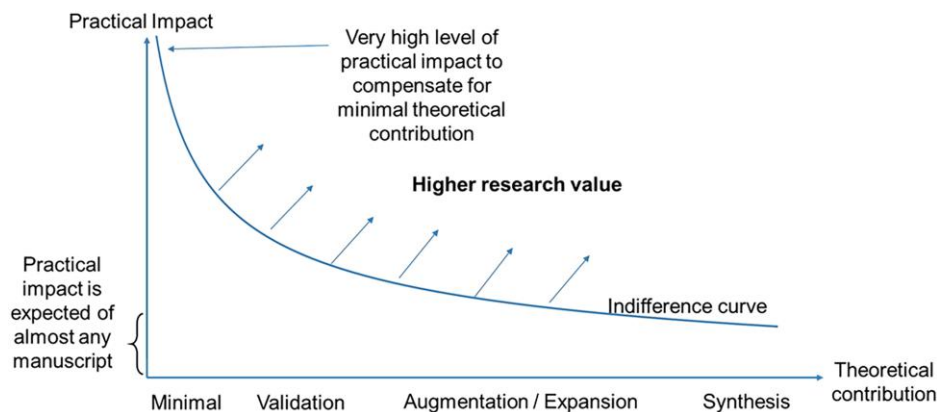


Table 3. Some Guidelines for Authors, Reviewers, and Editors

Guidelines for authors	Guidelines for reviewers	Guidelines for editors
<ul style="list-style-type: none"> • The taxonomy offered in Figure 1 represents a simple yet effective guide to authors as they consider how to position their work for review. Therefore, as a first step, authors should consider, carefully, the kind of approach they wish to adopt in their work and the ramifications of the chosen approach. Signaling this choice can help better match reviewers and editors to the paper. • Authors should refrain from claiming that new methods provide a theoretical contribution and should be viewed as such. Authors should ideally separate theoretical arguments from the econometric methods used to test these arguments. • The review process always involves judgments by reviewers and editors. Therefore, gathering pre-submission feedback on theoretical contributions is always preferable, especially at workshops and conferences. • A common issue encountered in the review process is a combination of C2 and C3 A strong theoretical contribution, especially in augmentation and expansion, comes from a deep understanding of the relevant theoretical narrative. Therefore, authors are urged to engage deeply with the relevant theoretical paradigm of choice when crafting a strong theoretical contribution. • Papers providing practical impact are expected to articulate a clear and compelling case for the same, as shown in Figure 2. Authors should clearly note their focus on practical implications in their manuscript in their abstract and cover letter so that the manuscript can be evaluated appropriately. 	<ul style="list-style-type: none"> • Theoretical contributions for empirical IS-Econ papers vary in type, magnitude, and objective. The taxonomy proposed allows authors to choose and appropriately craft papers accordingly. Paper should be suitably reviewed within the domain of the taxonomy chosen by authors. • Feedback to authors in terms of theoretical contributions may be delivered more easily by using the three components identified earlier. This focused feedback allows authors to better identify where remedial action is needed. Note that all papers may not need all three components in equal measure. Depending on the type of papers, the importance of different components may vary. • Papers that are primarily empirical in nature are potentially publishable if they are timely, address a question of practical importance, and have significant policy implications. They can be reviewed as such, without necessitating significant theoretical contributions. However, the threshold for what constitutes a contribution via practical impact should be maintained at a high level. • Unless papers are submitted and clearly demarcated as practical impact papers, all other papers require some articulation of a theoretical contribution. Theory remains relevant for motivation, framing, and sense-making. • Finally, whether a paper is able to deliver a significant theoretical contribution within its stated objective (reflected in the type chosen by authors) is subject to a certain level of taste and evaluation by individual reviewers and editors. The guidelines provided here are not meant to eliminate this important aspect of the review process. Thus, reviewers should be empowered to use their judgment of a paper's theoretical contribution and honestly provide this feedback. 	<ul style="list-style-type: none"> • Editors should consider the match between the type of papers the authors have chosen to write and the suitable form of theoretical contributions provided in the paper. Decisions on rejections or revisions should take into account the choices made by authors in a holistic manner, based on the type of paper submitted. • Feedback on the theoretical contributions in a paper may be better received by authors if they are described and evaluated using the three components identified earlier. Rejection or revision decisions should ideally be based on the extent to which the manuscript addresses these components (as appropriate) competently, in addition to empirical issues. • Editors should recognize that empirically driven papers focused on topics of practical importance are also welcome at the journal, and should be viewed as representing a legitimate form of research. However, the bar for practical implications should be high in terms of its significance and scope of effects. The cover letter and abstract would typically indicate if a manuscript is focused primarily on practical implications. Even within such papers, some links to extant theory may be elicited through the review process, as per editorial feedback. • Editors can provide guidance and feedback to authors on when a manuscript may be suitable for a specific type of paper within our taxonomy. This feedback may be important in the case of papers claiming practical impact so that desk rejections can be avoided.

2. The “how?” question (logic and execution)

• Providing Component 2

Within the theoretical narrative, what are the key relationships/variables being studied? What are the relevant constructs being studied: identify the key independent variables, key dependent variables, and contextual variables (moderators, mediators, latent variables or boundary conditions).

• Addressing C2

Provide strong and compelling arguments for the relationships between the independent, dependent,

and contextual variables while remaining embedded within the predominant theoretical narrative(s) of importance.

Consider engaging more deeply with the relevant community to gather feedback and advice on crafting a convincing set of arguments to support the postulated relationships between the variables.

3. The “what’s new?” question

• Providing Component 3

What is new in this research, and on what basis is it considered new to the literature?

- Addressing C3–C6

Provide evidence of a strong set of insights back to the broader IS community or the reference discipline from where the theoretical narratives were adapted.

Consider the extent to which the results and the theoretical linkages made in the paper are “new” to the literature, and make this case strongly in the writing of the paper.

Ensure that the insights are not rehashed from prior work. Avoid the “*We already know all this from prior research*” critique from reviewers or readers.

Gather feedback from the academic community to evaluate the answers to the “*what’s new?*” question.

Addressing these broad questions will help authors position their work more effectively to address expectations about theoretical contributions in their manuscripts. In addition to these, we also provide more pointed guidelines for authors, reviewers, and editors based on our work in this editorial. These are shown in Table 3 and are guided by our experience in managing empirical papers at the journal over several years.

Conclusion

What is a theoretical contribution, and when does a paper make a compelling theoretical contribution to be worthy of publication? These questions, although highly relevant and common in academic publishing, remain a source of significant confusion for authors working on empirical Econ-IS research. Empirical research within Econ-IS operates at the intersection of several reference disciplines, including economics, sociology, psychology, industrial organization, and computer science. This interdisciplinary nature of the field may contribute to the ambiguity surrounding what constitutes a reasonable theoretical contribution. As the standard for empirical methodologies has risen and new data sets are more easily available, young scholars have risen to the challenge in terms of using the latest empirical methods to address their research questions. However, crafting an acceptable level of theoretical contribution to complement sophisticated econometric analyses continues to be a major challenge, particularly for early-career scholars. In this editorial, we have sought to provide some understanding of, and guidance on, this particular problem by combining the experiences and viewpoints of several senior editors who have worked in this area for a number of years.

Like any research project, we believe that it is important to set boundaries and establish reasonable expectations for the reader of this editorial. Therefore, our ideas and suggestions in this paper are bounded by certain choices that we have made. First, we have largely tailored this editorial to scholars who work in the

economics of IS area, for whom econometric and statistical analyses combined with mathematical modeling tend to dominate PhD coursework. The practice of crafting a compelling theoretical contribution has, as a result, perhaps not received enough attention, making it a particularly weak point for scholars attempting to navigate the review process at top journals. Second, we focus on theoretical contributions in empirical papers while intentionally leaving out modeling and computational papers. Third, our work here should be viewed as an attempt at applying some structure to an ill-defined problem rather than as a definitive and exhaustive model of publishing. In other words, we do not, and cannot, claim that the taxonomy of theoretical contributions that we present is the last word on the topic; we do not intend this taxonomy and its implications that we highlight to become the final word or an “iron cage” in the institutional context of IS publishing (DiMaggio and Powell 1983). We expect authors, reviewers, and editors to still exercise judgment and agency in their work as they consider the guidance offered here.

In our analysis, we start by establishing a simple truism; theory and theoretical contributions are indeed relevant to empirical research in Econ-IS submitted for review at a journal like *ISR*. This is consistent with the age-old wisdom attributed to Immanuel Kant: “*Theories without data are empty, data without theories are blind*” (Harrington 2005, p. 5).

However, not all papers need to provide the same type or form of theory building. Theory plays an essential role in advancing knowledge; therefore, researchers need to think about contributions on this front in ways to help us understand and explain phenomena, make sense of things, assess and predict future outcomes, and provide a foundation for future work. To help authors and reviewers, we first propose a working definition of a theoretical contribution, building on prior work that has addressed this question in other disciplines (e.g., Hitt and Smith 2005, Colquitt and Zapata-Phelan 2007). Each of these three components is an important building block to providing a clear theoretical contribution. Subsequently, we describe the typical sets of issues that we have encountered in our work at *ISR* (shown in Table 1), and then, we provide a taxonomy of the four types of theoretical contributions that we observe in our work at *ISR*.

The four types of theoretical contributions we observe—validation, augmentation, expansion, and synthesis—vary in terms of their theoretical depth and breadth, and they are also largely independent of methodological choices, data availability, or the state of maturity of the body of knowledge. However, these types of papers may have downstream implications for how authors should make choices about the

theoretical treatment in their work; the ability to draw attention to specific aspects of their work; and even more pragmatic considerations, such as choices of editors and reviewers as well as the form of submission (*Full Paper* versus *Research Note*). To the extent that mismatches between what is promised in the paper versus what is actually delivered to reviewers are minimized, we believe that authors are less likely to be disappointed in the review process. Moreover, understanding the implications of the approach chosen for a paper can also help editors and reviewers appropriately evaluate and guide submitted papers, thereby helping to create a more equitable and constructive review process.

A question that is often asked in conferences and workshops is whether pure empirical papers, with no clearly discernible theoretical contributions, are acceptable. In this regard, it is worth mentioning that there are quite a few scholars in IS who suggest that the field has a “fetish” for theory, which tends to straitjacket creativity and agility in scholarly work (Avison and Malaurent 2014). In the specific case of empirical research in the economics of IS, we do acknowledge the importance of empirical papers that address research questions of substantial practical importance. In addition, empirical papers can be viewed as early indicators of lacunae in theory that can then subsequently be explored more deeply. We argue that such empirical papers, with minimal theoretical contributions, can be viable submissions to the journal for publication consideration (and be eventually published), *although infrequently to date*. However, to be viable, the paper must meet a high threshold of practical scope and significance, and authors would benefit from making a clear case for how the paper has a significant practical impact.

Finally, we do not downplay aspects of successful publications that have been discussed in prior work, such as originality and novelty (Gupta 2018, Leahey et al. 2023). Papers that are judged as being original and novel, whether emerging from the theoretical treatment or the empirical methodologies, are always appreciated (Gupta 2018). Within each of the paper types in our taxonomy, we believe that there will exist papers that vary with respect to novelty and originality, judgments regarding which we believe are best left to the editors of the journal at this time.

In closing, we hope that this editorial helps authors submitting their manuscripts to journals such as *ISR* to craft more compelling papers, anticipate challenges in the review process, and effectively respond to concerns raised, leading to greater satisfaction with the review process, regardless of the outcomes. Similarly, we trust that our editorial will also help reviewers and editors offer a more constructive and thoughtful approach to evaluating papers, especially when

assessing theoretical contributions within submitted papers. Finally, we note that as fields evolve and change, we will continue to revisit questions of theoretical contributions in the years to come. At this point, IS research is well positioned to be at the forefront of empirical rigor and impact. Adding strong theoretical contributions will give it a well-deserved trifecta and help advance knowledge in a more coherent and systematic manner.

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