

What's in a Contribution?

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Juliet's soliloquy, in which she despondently ponders "Tis but thy name that is my enemy; ... What's in a name? That which we call a rose by any other name would smell as sweet" (Shakespeare, 1984, p. 869), would have us believe that words are independent of the entity they represent and that by changing the word, the entity described by the word remains unchanged. Juliet laments that a mere word—in this case a name—stands in the way of her happiness. Is not the word itself meaningless? Would that this were true.

There is a powerful word that has taken center stage in many reviews, a word that justifies the rejection of many papers from our top journals, a word that seems to mean different things to different readers, a word that is profoundly difficult to describe let alone define, a word that ignites despondence in many an author. That word is "contribution." Papers are routinely rejected on the basis of "insufficient contribution" or its cousin, "insufficient theoretical contribution," with little explanation as to what makes for a sufficient, convincing, or strong "contribution." Authors are told that it is their responsibility to demonstrate a sufficient contribution, even as the nature of contribution remains ambiguous. We are better at describing what is not a contribution—the addition of a moderator to a well-tested model, the testing of a theory in a different context but within the same general theoretical boundary, the description of a phenomenon without a theoretical interpretation, the development of hypotheses without an overarching theoretical lens, and so forth—than we are at describing what a contribution is, save to say that a contribution goes beyond that which we describe as being "not a contribution."

In attempting to provide some clarity to the meaning of contribution, editorials and commentaries on

scholarly research and theory in IS and related disciplines consistently rely on words like "novel," "interesting," and "insightful" (Sutton & Staw, 1995; Le Pine & King, 2010; Corley & Gioia, 2011; Davis 1971; DiMaggio, 1995; Bergh, 2003). These same words pepper many a review and are frequently preceded by such other words as "not very," "what's," "I don't see anything." Even as the words "interesting," "insightful," and "novel" are used to try to elucidate the meaning of contribution, they themselves are wrought with ambiguity—What makes something interesting? What makes something insightful? Is all novelty equally good?

Some have gone to great lengths to define "interesting" by describing such attributes as counterintuitive, paradoxical, contrarian, surprising, unexpected (Alvesson & Sandberg, 2011; Davis, 1971). Insightful has been described as "important and relevant," offering something "new," or changing the way "researchers think about a subject" (Bergh, 2003). Novelty is rarely defined, but is associated with "original" (Agerfalk, 2014) or "quirky insights" (Oswick, Fleming, & Hanlon, 2011). It seems that even though individuals in social situations gravitate toward similar others in terms of appearance, beliefs, and behaviors (McPherson, Smith-Lovin, & Cook, 2001), in our scholarly reading, our minds are piqued by that which is dissimilar to what we know, that which forces us to think differently, that which makes us uncomfortable. Mind you, it must be similar enough that we can make sense of it, but it must be different enough that it stimulates us to expand our mental horizons. In this never-ending pursuit of interesting via novelty, we certainly risk overlooking genuinely useful knowledge and creating the equivalent of an arms race for new ideas, a race that has few winners.

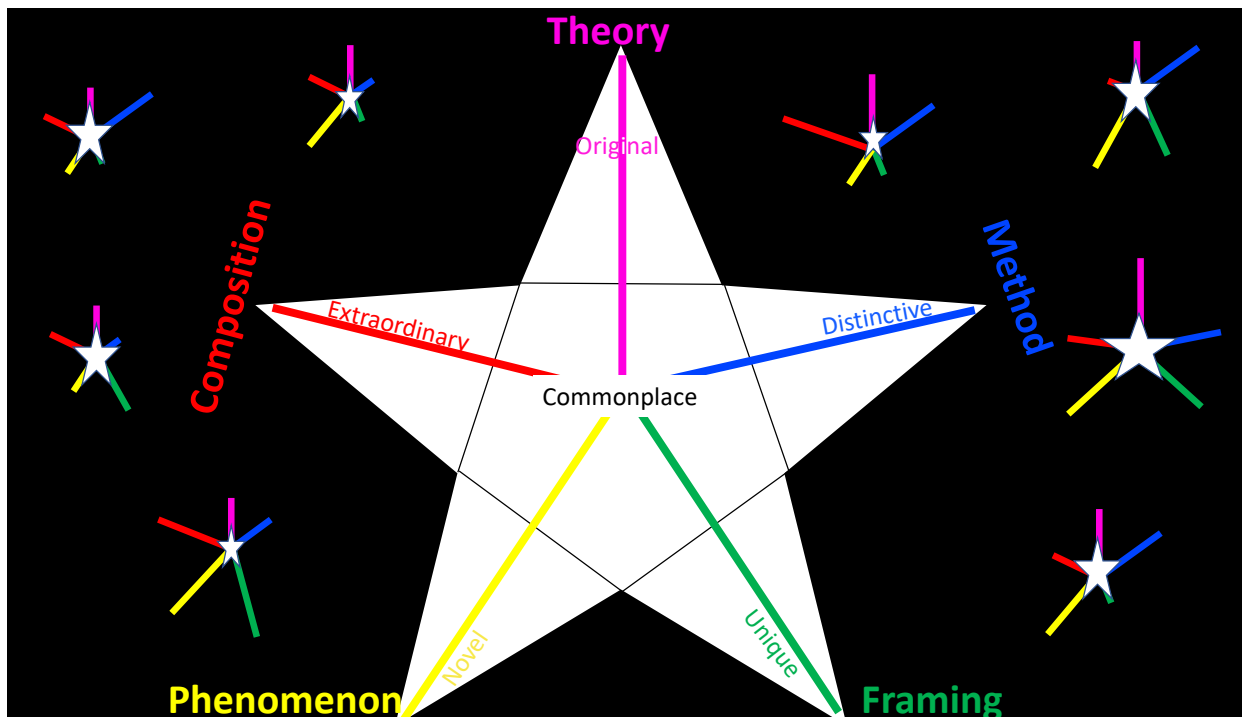


Figure 1. A Five-Point Assessment of Contribution

Seemingly employed with wanton abandonment by reviewers and editors alike, the term “contribution,” as well as its accoutrements “novel,” “interesting,” and “insightful,” stands in need of greater clarity, or alternatively, a moratorium on its use. In this editorial, I will target the former with the objective of providing some clarity to the word. We need a language that helps us understand, both as authors and as readers, when we have and when we do not have something worthy of our attention and deep reflection—in short, when we have a contribution.

It is my belief that reviewers often cite “insufficient contribution” as a rejectable offense when, in reality, there is no “fatal flaw” in the paper so to speak; rather, from a holistic perspective, there is simply nothing distinctive about the paper that sets it apart. It is often less about things the authors did than things they did not do. It is much easier to critique with precision those mistakes that a paper did make than it is to express with clarity that which a paper is missing. Hence, reviewers and readers employ the broad and vague “contribution” word to indicate that a paper, missing something intangible, leaves the reader with a feeling of general malaise. Given that both attention and time are limited resources, readers need to conclude a reading with a sense that their time was well spent, that the attention they devoted to the paper was worthwhile. Contribution, as experienced by readers, is the sense that a paper is worthwhile to read.

Here, I present contribution as a pentagram, or a five-point star. The interior pentagon portion of the

pentagram represents the commonplace, meaning the way things are commonly undertaken in a given time period. Many things that are commonplace today were innovative a decade ago. As one extends outward toward each point, one finds something original, distinctive, unique, novel, and/or extraordinary (see Figure 1). I use these words interchangeably because I believe they are all indicative of the reader’s desire to be exposed to something that makes the attention and time he or she devotes to reading a paper worthwhile. Authors must attract reader attention and interest by shining, so to speak, on more than one of the points. There are different ways to make a contribution by shining on different combinations of points. There is no single best way, illustrated by the irregular pentagrams of various shapes and sizes in the background of Figure 1.

Beginning with the theory point, the use of a theory that is very mature in the IS discipline would be positioned in the pentagon within the pentagram (see Figure 2). It is quite common to use mature theories and there are certainly excellent reasons to do so—the theory is well understood and requires less effort to explain and justify; there are existing, valid measures; the theory itself has a legitimacy in the field, and so on. Using a mature theory is often the right choice. However, for authors who are using mature theory in their work, their theory contribution will likely revolve around testing a mature theory using quantitative data or using a mature theory to interpret qualitative data, contributions that will be difficult to position as novel or interesting.

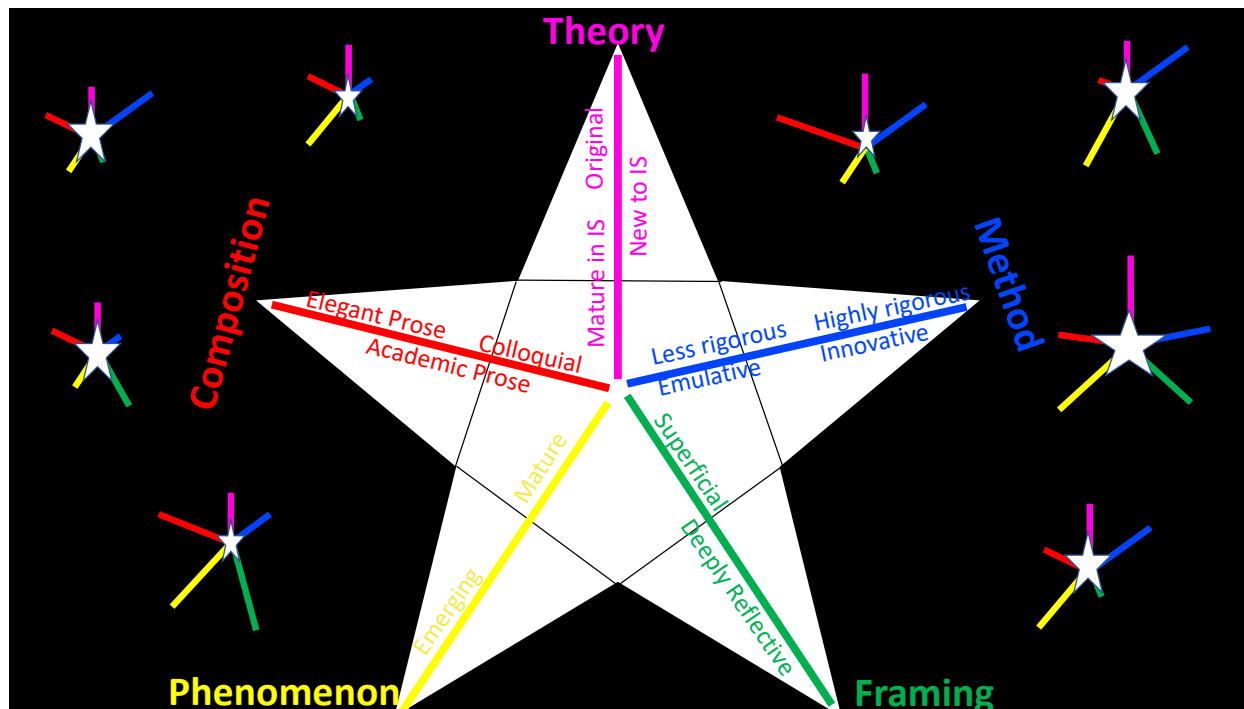


Figure 2. From the Commonplace to the Distinctive

Something more novel might be the use of a theory that is itself not new, but that is new to IS. Authors might be the first to apply a theory from another discipline to help explain some phenomenon in IS. Through applying a new-to-IS theory, authors may be able to demonstrate insights into the phenomenon that were not possible with mature-in-IS theories. In this case, the paper moves beyond the more common “mature to IS” theory to a “mature but new to IS” theory, offering the paper the potential to shine somewhere closer to the midpoint of the theory point in the pentagram.

The greatest potential for originality in the theory point would be the development of a new theory. The new theory might be a result of blending established theories or might be entirely novel. Developing new theory is imbued with challenge; otherwise, it would be far more common than it is. When one seeks to develop a new theory, one is judged not just on the basis of whether the theory is interesting, but also on the basis of many other criteria for “good” theory (Bacharach, 1989; Corley & Gioia, 2011)—is it falsifiable, is it useful, is it original, is it revelatory, is it prescient?

There are many contributions to be made on the theory-side of the star: testing a theory, filling a gap in a theory, contextualizing a theory, extending a theory, importing a theory to IS, developing a theory. The key to shining in the area of theory is to offer a new perspective or a new way of interpreting a phenomenon, thus offering insights that are unexpected, surprising, or counterintuitive (e.g., nonobvious) and making individuals pause to reflect about something that they

would not otherwise have thought about and about which they are glad that they did. A paper may make a theoretical contribution without altering or extending a theory. The theory contribution might be that the authors find a new way to use the theory or a new application of the theory, uncover an important, overlooked assumption of the theory, or derive an insight into the theory that had previously been dormant. Authors would do well to reflect on the two or three most interesting insights that they were able to derive as a result of using the theory they used, even if the insights themselves are not theoretical in nature, and should build their claims for theory contribution around these insights without trying to extend their claimed contribution into too many tangential streams of literature, the connection to which might be very thin and thus unconvincing. In truth, making distinctive theory contributions is very difficult and authors often debase their theoretical contribution by exaggerating the implications of their research for theory.

The second point in the pentagram is method. Just as with theory, with method, there is an opportunity to shine and to extend beyond the common to the distinctive or novel. Inside the pentagon would be methods that are less rigorous and/or methods that are highly emulative. Moving outward would be methods that are highly rigorous and/or innovative. It might not sound obvious that readers expect novelty in method, but method sections that read like they could have been extracted from any number of other papers and pasted into the paper in question will sound robotic. In today’s

environment, a single-respondent, single-point-in-time survey method using data from Qualtrics and PLS analysis would be commonplace, as would a qualitative case study that relied exclusively on standard procedures from first-generation qualitative method references (Sarker et al., 2018a, 2018b) to describe its approach with an almost script-like precision, a lab experiment relying exclusively upon an undergraduate student sample, or an econometrics paper that pulled data from a single website.

Authors who find new/interesting sources of data, use data that span points in time, invoke new approaches to data analysis to derive deeper insight, or combine methodological approaches in new ways are more likely to attract harsh reviews when their deviation from the norm appears forced or excessive, but are also more likely to attract adulation when their deviation from the norm results in a new form or a higher quality of data and/or analysis than was previously possible. Papers that are commonplace in terms of both theory and method are not likely to make it beyond the desk screen at elite journals. This does not imply that the research is not important, but simply that elite journals are elite precisely because they publish distinctive research.

The third point of the star represents a paper's framing or the literature base from which the paper draws and in which the paper is positioned. Framing ranges from the superficial to the deeply reflective and involves a single stream or multiple streams of research. Superficial framings tend to provide long and thorough descriptions of what has been studied in a given area, but do not derive novel insights. They might identify research gaps, but only gaps in what has been studied, which are not necessarily gaps in what is known. Superficial framings often rely on single streams of literature. Slightly more distinctive would be a framing that explains past research in terms of that which has been found, as opposed to that which has been studied. Such framings are likely to strike readers as acceptable yet not intriguing. When more than one stream is included, the two streams are often covered independently rather than woven together. The most deeply reflective framings are those that derive insights from the past findings in a domain, and typically in more than one relevant domain, and then synthesize those insights to build a captivating and novel frame in which to encase the current paper. Part of what makes such framings unique is not each individual stream, but the consideration of the different streams together. Papers are rarely able to break out of the pentagon portion of the framing point if they rely on one stream of literature for their framing. Particularly in the field of IS, part of the artistry of framing a paper is weaving multiple streams of research together to produce something novel.

Moving to the phenomenon point of the star, some papers deal with very mature phenomena whereas some

deal with emerging phenomena. Both are important. Indeed, there are scholars whose remarkable careers have been built delving into a given phenomenon and researching it over many years, from the time it was emerging through the time it was quite mature. There are also scholars who have built equally remarkable careers by constantly being on the forefront of emergent phenomena. A challenge for authors studying both mature and emerging phenomena is satisfying the "what's new" question—emerging phenomena often appear to be freshly labeled reincarnations of previously studied phenomena.

In addition to addressing the "what's new" question, authors must go beyond communicating the nature of the phenomenon of interest to describing its importance and relevance. Authors who are able to ground a phenomenon in a real-world problem facing organizations or society are better able to build a case for contribution than those who present a phenomenon without providing evidence of its importance to individuals, organizations, and/or society at large. Theory can intersect with phenomena in cases where theory is the phenomenon. The latter is a special case of the phenomenon whereby researchers take a mature or emerging theory and use an emerging phenomenon to demonstrate weaknesses in the theory and advocate for either new theory or modifications to the existing theory. In this case, the authors must be able to provide evidence that the theory is of such importance to the field that an examination of the theory, even absent a real-world problem addressed by the theory's application, merits attention.

The fifth and final point on the star is that of composition. In Feldman's 2004 editorial, he observed: "If the writing is unclear and tortured, it makes it virtually impossible that reviewers will put in the time and effort to try to make sense of new ideas. It is the author's affirmative responsibility to ensure that the writing quality enhances, rather than detracts from, the theoretical contribution of the work" (p. 567). There are a variety of composition styles. At the center of the pentagram, I place the colloquial style. A colloquial style of writing is a conversational style. Indistinct and not altogether appropriate for an elite journal, a colloquial style is unlikely to be used except in cases of commentaries or research essays, but even then, a more formal style of prose is encouraged. Academic style is a more scientific and formal prose than the colloquial style and exists at the border of the pentagon. Barring careful attention, the academic style can quickly become a somewhat mechanical, textbook-like style that does little to motivate a reader to continue reading. Perhaps because it is both common and mechanical, academic prose risks making interesting ideas appear jejune. Some authors take academic prose to the extreme and compose prose that reads as though it were written from a presentation, using point 1, point 2

(subpoints 1, 2, and 3), point 3 (subpoints 1 and 2) and so forth repeatedly throughout a paper as if to force feed their thoughts into the reader. In such papers, the presentation of the content, rather than the content itself, risks leaving the reader bored instead of enlightened. Excellent copyediting, such as that provided by Monica Birth, the copy editor for *J AIS*, can help papers move out of the pentagon portion of composition, but this happens only after a paper has been accepted. Authors must themselves be attentive to the effects of their composition during the submission phase and cycles of revision.

Elegant prose is the most distinctive style. Elegant prose requires meticulous attention to each word, combination of words, flows of thought, transitions in thought, sentence structure variety, and so forth, attention that many authors are no longer able to muster by the time they have completed a draft of their paper. Elegant prose is certainly more appropriate for pure theory papers than is a colloquial or academic style. When one does not have data to help build a contribution, but only ideas, then the quality of the writing will figure strongly into the conveyance of the ideas and resultant contribution perceived by readers. I sometimes wonder if the dearth of original theory from which the IS field suffers is related more to a general lack of experience in the art of elegant prose than to a paucity of talented theorists. Ultimately, I agree with and slightly modify Feldman (2011): composition quality should intensify rather than distract from a paper's contribution.

Even though conducting a research project is a largely scientific endeavor, the preparation and revision of a paper is largely an artistic one. The paper must tantalize the reader, arouse the reader's curiosity, and raise the reader's intellectual acuity such that the reader finds the paper worthwhile to read. Reviewers will see a potential contribution and will be willing to work with authors to shape this contribution when they feel that the time spent reading a paper was worthwhile. It is my contention that reviewers and, more generally, readers find articles that shine in multiple areas worthwhile to read. Papers that do nothing scientifically wrong but that fail to shine in any area—those that fail to breach any edge of the pentagon within the pentagram—are often the ones that endure repeated cycles of review and revision only to be eventually rejected, much to the authors' dejection as well as the reviewers' frustration. A paper need not shine in each area and, indeed, if every article were distinctive in each area, then novelty itself would eventually become commonplace. However, a paper on track to publication in *J AIS* must create distinction, e.g., shine, in multiple areas. Therein, to me, lies the meaning of "contribution."

Before closing, I would like to express my gratitude to the *J AIS* editor-in-chief selection committee—Soon Ang, Andrew Burton-Jones, Ola Henfridsson, Sirkka

Jarvenpaa, Jan Recker, Suprateek Sarker, and Carol Saunders—who have given me this opportunity to serve *J AIS* as editor in chief. I am deeply grateful for their encouragement and confidence in me. I am also very grateful to the supportive board of senior editors, associate editors, editorial board reviewers, managing editors, and the copy editor, for their willingness to work with me in the pursuit of developing, polishing, and publishing exemplary IS research. And, I am indebted to the previous editor in chief, Professor Suprateek Sarker, from whom I have learned so much.

In concluding this editorial, I would like to make two important announcements.

***J AIS* Promise Review Option**

I am pleased to announce the introduction of a new review process option, *J AIS* Promise. The *J AIS* Promise option is intended for the highest quality of original submissions. *J AIS* Promise submissions will undergo a first-round review with an SE, a blind AE (e.g., the AE will not be aware of the authors' identity) and, where deemed appropriate by the SE, one or more blind reviewers. Subsequent rounds of review will be handled by the SE and AE only.

After the first round, the senior editor will either offer a conditional accept or will reject the paper. The conditional accept may require one or more rounds of major revision, but the authors will have the commitment of the SE and AE after the first round of review to work the paper toward eventual publication in *J AIS*. In short, the aim of the *J AIS* Promise option is for authors to submit their most promising research to *J AIS* and for *J AIS* to promise to commit (or reject) after one round of review.

Authors interested in the *J AIS* Promise review option should specify this in their submission cover letter to the editor in chief. Authors are encouraged to describe in their cover letter what they feel makes their paper particularly exceptional. Such a description may help the editor and senior editor determine whether the paper is indeed appropriate for a *J AIS* Promise review. Unlike traditional reviews in which review teams often need one round of major revision in order to determine whether or not they see a potential for contribution, with the *J AIS* Promise review, this potential must be evident in the first round. A high standard will be applied to the initial screening of *J AIS* Promise submissions and papers that are rejected either in the initial screen or after the first round will not be eligible for resubmission as a regular submission. Thus, the *J AIS* Promise review option is only recommended for the highest-quality original submissions.

J AIS Reviewer Hall of Fame

The work of reviewing is among the most invisible and selfless of all service work for the IS community. I am pleased to announce the inauguration of the *J AIS* Reviewer Hall of Fame. The *J AIS* Reviewer Hall of Fame will honor those individuals who, over the course of the journal's history, have reviewed at least 25 papers. Our initial group of inductees comprises 25 scholars, 18 of whom have conducted between 25 and 39 reviews, 6 of whom have conducted between 40 and 49 reviews, and one who has conducted over 50 reviews! Seeing the amazing dedication and commitment that these scholars have each shown to *J AIS* and, more broadly, to the IS community, has marked me deeply. They are quintessential scholars upholding the values of service to the community. Each July, I will analyze the data in the Scholar One review system and update our Hall of Fame. Our first induction into the Hall of Fame will take place at the Hawaii International Conference on System Sciences in January 2021. Please take a moment to congratulate, and thank, our initial inductees. It is with the utmost of gratitude that I thank them for their commitment to advancing the mission of *J AIS* to publish exemplary IS research.

Acknowledgments

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50+ Reviews

Chuan Hoo Tan, National University of Singapore

40-49 Reviews

Hillol Bala, Indiana University

Christy Cheung, Hong Kong Baptist University

Traci Hess, University of Massachusetts

Stacie Petter, Baylor University

Juliana Sutanto, Lancaster University

Ryan Wright, University of Virginia

25-39 Reviews

Andrew Burton-Jones, University of British Columbia

Sutirtha Chatterjee, University of Nevada

Mike Chiasson, University of British Columbia

Ben Choi, Nanyang Technological University

Amany Elbanna, Royal Holloway University of London

Shuk Ying Ho, Australian National University

Dirk Hovorka, University of Sydney

Weiling Ke, Clarkson University

Huigang Liang, University of Memphis

Ramiro Montealegre, University of Colorado

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Shirish C. Srivastava, HEC Paris

Monideepa Tarafdar, Lancaster University

Jason Bennett Thatcher, University of Alabama

Daniel Veit, University of Augsburg

Eric Walden, Texas Tech University

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