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Editorial Commentary

Some Guidelines for the Critical Reviewing of Conceptual Papers *

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Some Guidelines for the Critical Reviewing of Conceptual Papers

As Senior Editor of the *IS Research Perspectives* Section of the *Journal of the Association for Information Systems* (*JAIS*), I am often asked about the section; in particular about the kinds of papers that are or are not appropriate, how we decide what is and is not appropriate, and, specifically, how we evaluate the submissions we receive. It is on the last point I would like to offer some thoughts, as evaluating the kinds of submissions we receive is challenging to say the least. But before, I present my thoughts on this; a little background to the section is warranted.

Overview of the IS Research Perspectives Section

The *IS Research Perspectives* is a special section of *JAIS* whose overall goal is to publish articles that address critical issues that shape the IS research tradition, carry an underlying message for the field's research mission, and are thought-provoking and insightful. We also welcome controversial and speculative papers as long as they are well argued. Articles should help those in the IS field to come together and reflect upon the community, its organization and mission. While most published articles deal directly with IS research in the broadest sense, we welcome articles that assist the disparate sub-communities of our field to better understand one another: to help overcome what has become a significant "communication deficit."

Like other academic disciplines, IS has been driven both by the dynamics of its own internal academic traditions and external pressures. To paraphrase Banville and Landry (1989), this has resulted in an adhocracy where there is not always a shared, well defined, and common mission. We are divided by different paradigms, different research interests, and different perceptions of what the field should or should not include. We also cannot agree on what the preferred approaches are for developing IS in practice. This in turn has led to the current situation, where one IS research community sees little "relevance" in the research of another, because each subscribes to its own "socially constructed truth." Benign tolerance or indifference is all too common, perhaps, because that is the most that one can expect from different truth communities. Many would no doubt agree that the first challenge IS academics face is to make their research more relevant to each other before they can make it more relevant to external constituencies. For this to happen, both the quantity and quality of communication among different research communities would have to increase. This means that we must devote more effort in discipline-wide discourse to achieve a better understanding of the differences between us, and based on this, work toward a greater synthesis of ideas and integration of results by building on each other's work. But given that the different research communities have their own outlets, generally targeting for their own audiences, it is hardly surprising that a communication deficit exists. Moreover, we have a habit of developing specific languages, jargon, and expressions which hinder communication between the community which is familiar with that language, and those who might be interested but are not part of that community. A possible example of this is the language used by the advocates of Actor Network Theory (ANT) which is considered impenetrable by those outside the ANT community.

The current situation is one in which research results are reported but are only understandable to the insiders who spend an extraordinary amount of time with the literature and research findings of a specific community. This time commitment tends to make it virtually impossible for most researchers to keep up with related work in other areas. Add to this the different preferences of what constitutes "good research," and it is easy to see why the findings of one research community are typically not known or valued by another, let alone by researchers from other disciplines or practitioners. This also hampers reviews and review policies, as many times they appear to be random lotteries with varying outcomes depending upon whom the authors happen to receive as reviewers on their submissions. We are stuck in one corner of the literature and lose sight of the greater, overarching issues. In fact, currently, there is little broad-based debate on identifying overarching issues let alone on exploring them.

Here, the *IS Research Perspectives* section can help: by providing a forum where this debate can occur. We believe that the IS discipline already has the motivation for engaging in such discussions, but needs direction in addressing the communication deficit. We believe we offer one such vehicle.

When the *IS Research Perspectives* section was first implemented in 2003, the idea was to provide the opportunity to comment on disciplinary-wide topics that did not fit the mold of traditional papers using traditional methods and/or using empirical data. One area where we felt *JAIS* could be at the forefront was in redefining institutional publication practices concerning the concept of rigor in research. We felt rigor needed to be augmented to include a wide range of scholarly inference and evidence giving. We also felt it needed to include the linking of detailed models or hypotheses to broader theory so as to arrive at expanded categories of knowledge. Such a discussion could contribute substantially to communicating across the narrow boundaries of our preferred research communities. Other institutional publication practices should also come under scrutiny. And this, indeed, is one of the primary goals of the *IS Research Perspectives* section.

We invited the IS academy to submit articles taking a stance on any debatable issue of interest to our community and with lasting scholarly value. But we especially welcomed submissions that attempted to address this communication gap. We encouraged articles focusing on high-level, institutional analyses of the field (e.g., future of the discipline), theoretical concerns, methodological and philosophical issues, as well as interdisciplinary analyses. In addition, we invited articles suggesting how we should assess our journals, provided the authors went beyond simple atheoretical postures and suggested how methodologies could be justified and, ideally, how their work fit into a larger picture of theory development within scientific communities. So far, we have been pleased with the response. The number of submissions to the *IS Research Perspectives* section has been growing annually, and we have informally heard that most of you value the papers published in this section.

This brings me to my main point. It is not so much that we need more submissions (although we certainly would welcome them), we need more individuals capable of reviewing submissions to this section. And that is where the real problem lies.

Having been the senior editor of this section for more than two years now, I would like to share some thoughts on what reviewing for these types of manuscripts involves. Whilst there are similarities with reviewing traditional research articles, there is enough of a difference to merit clarification. Because the submitted papers are typically conceptual, they require a somewhat different mindset to evaluate the quality of the authors' arguments. The arguments are based less on data in the traditional sense, but involve the assimilation and combination of evidence that may come from a variety of sources.

I do not want to reinvent the wheel in terms of how to perform traditional reviewing, as there are many good 'how to review papers" articles written by many well-known scholars (*cf.* Daft 1995; Pondy 1995; Lee 1995, 2000; Zmud 1998; Benos et al. 2003; Saunders 2005). Yet these papers focus on the more traditional research article genre, i.e., empirical and mostly positivist submissions, while I wish to focus on how to review conceptual or philosophically motivated, rather than empirical, pieces. Evaluating such papers is challenging, not so much because they are more complex, but because they generally do not fit the mold of empirical research articles to which we are more accustomed. Too many researchers (at least in the IS domain) seem to think that any non-empirical paper is simply an essay and devoid of deeper scholarship. Nothing could be further from the truth. More than once I have received comments from reviewers claiming a paper is nothing more than an essay, implying essays are little more than opinions. But aren't all papers "opinions" in one form or another? One cannot categorically state that papers based on opinions are unscholarly. It is what these opinions are based on, how they are supported, and how they are formulated that makes them more or less believable; and ultimately, whether we ascribe to them the label "contribution to knowledge" that is important. Indeed, some of the most influential and widely cited papers in the field are of this type, e.g., Mason and Mitroff (1973), Ives et al. (1980), Kling (1980), Orlikowski and Baroudi (1991), Benbasat and Weber (1996), and Klein and Myers (1999). Part of the reason for my commentary piece is to show that reviewing such papers requires a broad critical eye to ensure that the accepted papers do make a significant contribution to knowledge.

Reviewing Issues

Many scholars feel that reviewing papers is more an art than a science. In the seminal book *Publishing in the Organizational Sciences,* Cummings and Frost (1995) attempt to address questions such as: What makes a good review? How should one undertake a review? What should one look for in a paper? How does one assess the level of contribution of a paper? How novel is the contribution? Does the paper successfully build on what has been done in the past? More fundamentally, how does one distinguish between a good paper and a bad one? Although it is not necessarily easy to ascertain a good paper from a bad paper, there are some accepted guidelines to help. Unfortunately, many of these guidelines work for knowledge claims that are based on empirical data. But what if the claims are conceptual and focus more on offering new ways to think about a phenomenon? How does one judge such claims?

The purpose of this commentary is to summarize how to undertake a critical review of such papers and offer some guidelines. These guidelines are not meant to be a cookbook, but rather a brief introduction to the art of critical reviewing. I use the word "art" advisedly since critical reviewing is not a mechanistic process, but more of a craft. It is a subjective exercise that one hopes to get better at the more one does. My intention is to stimulate readers to think about how to review and evaluate any paper they read, especially if it is non-empirical. Any paper — whether submitted or published — intrinsically claims to make a contribution to knowledge. Does it? Your job as you read the paper is to assess whether you believe it does or does not. If so, is the contribution noteworthy? If not, could it become noteworthy, and if so, how? While this assessment is inherently subjective, that does not mean it is not rational or that we cannot come to an agreement on the grounds upon which the assessment is based. In fact, just the opposite: it should be highly rational. And, that is what I attempt to show with this commentary.

Conceptual papers emphasize assumptions, premises, axioms, assertions, etc.; and these need to be made as explicit as possible so they can be evaluated. But how should they be evaluated? One useful framework is the one offered by the British philosopher Stephen Toulmin (1958) in *The Uses of Argument.* Using Toulmin's framework, one can assess the strengths and weaknesses of the arguments used by a paper's authors. For Toulmin, there are six aspects of an argument: three necessary components and three optional ones. The necessary components are *claims*, *grounds,* and *warrants*. (For this paper, I am going to omit Toulmin's optional argument properties, i.e., *qualifiers, rebuttal,* and *backing*.) *Claims* refer to the statement or thesis that the authors are asking the reader to accept as true. An example might be the assertion that for an information system to be successful, one needs to have user participation in its design. The *grounds* (*or support*) is the method of persuasion used by the authors and is comprised of data plus the reasoning behind the claim. Fundamentally, this is the evidence or grounds by which the claim is supported or justified. The support for a claim may take the form of facts and statistics, mathematical proofs, expert opinion, examples, explanations, prior literature, and logical reasoning. In the case of the claim for user participation, the authors might cite a variety of research articles that show user participation leading to successful systems. A *warrant* links the data (grounds) to a claim. Ostensibly, warrants are the assumptions or presuppositions underlying the argument. They are often unstated or implied, and typically not debated. In the case of user participation, a warrant might be the belief that organizations want to build successful systems. The job of the reviewer is to assess whether the claims made by the authors are: (1.) understandable (intelligible); (2.) substantiated (believable); and (3.) significant (makes a worthy contribution to knowledge). That is what critical reviewing is all about.

Keep in mind, when I speak of "critical" I do not mean "negative." Many reviewers feel that to be "critical" they have to tear paper apart. When reviewing a paper, you should not only be searching for flaws in the paper, but also how such flaws could/should be overcome. In other words, the reviewer has to be constructive as well. How could the paper be improved? What would its value be if it were improved? Identify both strengths and weaknesses. The ultimate aim is to contribute to quality control: to safeguard the audience interest and also to help the authors make a contribution. Virtually no one can write an excellent paper in the first round. Papers mature into worthwhile products only through critical review as many experienced eyes pore over them. Outside reviewers play a vital role here. They help the paper's authors better craft their arguments, their thinking, their way of presenting evidence, their conclusions, etc. Reviewers must, therefore, guard against narrow-mindedness. They must be open to new ideas: new ways of thinking, new ways of presenting evidence, new insights, and the like. This is even more important when reviewing conceptual papers. They also have to guard against their egos taking over the review process. No authors want to have reviewers write derogatory remarks about their paper or to have the reviewers dictate what the paper says. Reviewers need to be diplomatic and constructive, yet clear and concise. While the onus is on the authors to make their arguments intelligible and believable to the reader, the reviewer should be polite and constructive no matter how bad he/she feels the paper is. Harrison (2002) notes that reviewers shouldn't take themselves too seriously. In fact, he offers what he terms a manuscript author's "Bill of Rights" in terms of expectations of a reviewer. He also suggests there might be a parallel list of author responsibilities to a reviewer. In any event, in this business, it is good to have 'thick skin'!

On the whole, I am not sure that there can be a definitive introduction to reviewing, because undertaking and then writing a good review is a creative process, as is the writing of a good paper. Just as a paper should be logically consistent, offering a coherent set of intelligible and believable arguments and contentions, so should a review. Moreover, what a reviewer looks for in a paper — quality of arguments, coherence, contribution, data, analysis, etc. — are the same no matter what type of paper it is, although conceptual papers do pose more problems, as there is no real *pro forma,* which, in principle at least, exists for empirical papers. Nevertheless, in the following, I offer a rough set of guidelines to get started. They are structured in eight areas: (A) introduction, (B) content, (C) presentation and structure, (D) theoretic foundation, (E) data analysis/ interpretation /argumentation, (F) results, and (G) conclusions. Many of these guidelines are general and relate to any type of paper being reviewed. Others - especially sections (D), (E) and (F) - are focused more on conceptual papers.

A) Introduction

The *Introduction* sets out the motivation and purpose of the paper. It tells the reader why he/she should be interested in investing the time to read the paper. It sets up the *claims* that the paper will be making.
1. Is the paper interesting? Interesting for whom? Why would the reader find it interesting?
2. What is the paper's purpose? Is it clearly stated early in the paper? If not, can you intelligently guess at the purpose? If it's not clear, interpret the purpose in your own words and make the point that the authors need to be clearer about this. The burden to be clear about the purpose is on the authors! If the purpose is ambitious, are the limitations made clear? Sometime it is appropriate to talk about the problem rather than the purpose of a paper.
3. Often, even if the problem or purpose is stated clearly, the authors need to convince the audience that it is an important one; that it is worth the readers' time to read the paper. Hence, ask the authors how legitimate it is to take up this problem and how well the problem is motivated. Have the authors made the problem seem important and in need of research? Have other people addressed the same problem and missed important angles, or have circumstances changed so that prior approaches are no longer valid or appropriate? What is going to be new in this paper?
4. How well does the structure fit with the purpose? For a longer paper, the authors should preview its structure near the end of the introduction.
5. Who is the audience? Are the style and language appropriate for the audience, or have the authors not thought through who their audience is and what might be of interest and understandable to the selected audience? (Is the audience an academic one, a practitioner one, or both? Is the audience single discipline, cross-discipline?)

B) Content

Content refers to the specific contribution to knowledge that the research makes (or is supposed to make). This is where the *claims* are actually articulated. The claims need to clearly stated and understandable.
1. Is the paper adequately positioned within the context of what has been done before? This refers to the adequacy of the literature review. Have key reference areas been omitted? If so, have the authors

justified why they were left out? Has the literature base used in the paper been sufficiently well motivated?
2. Is the thesis advanced in the paper valuable for the field? Why?
3. What is or are the (potential) contribution(s) to the problem? Or, how much of the purpose is achieved?
4. If there are research questions, are they clearly stated? Are they well motivated? Is it clear why these research questions were chosen and not some others? If there aren't research questions, what is the paper supposed to do?
5. Did the authors relate the paper's line of reasoning and contributions or results to pertinent prior contributions? Is the literature review reasonably adequate (complete and clear)? Does the paper fit within a particular research stream? Which research stream and why that research stream?
6. Which claims or solutions are advanced? What types of evidence are used to support the claims? Is counter-evidence considered? How penetrating (thorough and sharp) is the reasoning to weigh conflicting evidence? Are crisp and enlightening examples or analogies suggested to help grasp difficult points? Would additional examples, vignettes or other forms of explanations help?
Basically two types of evidence exist: empirical (i.e., cases, multiple observations as in surveys, statistical samples, anything coming from the five senses) and non-empirical (personal beliefs, contemplation, concepts, anything not coming from the senses). Another important aspect of evidence is that of coherence; more specifically, coherence with an established body of knowledge. For example, the claim of the Immaculate Conception is coherent with Christian theology, but not with biology. A biological discussion of this claim would obviously employ different definitions and different types of arguments than a theological one.
7. Are some meaningful conclusions or recommendations advanced? And, in whose eyes might they be meaningful? In other words: If the paper's main line of argument is accepted, what difference does it make? Keep in mind that what are considered significant differences will vary with different disciplines, cultures, and research communities.
8. Given that a paper is like a set of building blocks where the blocks (i.e., arguments) have to be coherent and build upon one another, one has to ask: Is the basic strand of argument used throughout the paper logically consistent and believable?

C) Presentation and Structure

Presentation refers to the logical sequence of the arguments' presentation. It also involves the rhetorical style used by the authors, or how the *claims* are articulated to the reader. They must be presented in an intelligible way.
1. Is there a clear structure, one that has a place for everything (leaves nothing out that is of importance) and also puts everything in one place (avoids redundancies)?
This idea is related to the principles of unity and cohesion. Unity requires that everything said in the paper relates to the overall topic (no orphaned ideas) and that all ideas or claims are supported (no widows). Cohesion means everything hangs together in a logical flow of ideas or claims, supporting arguments, and examples. For a paper to be coherent, it should use important terms with one consistent meaning and not use different words for the same concept. For example, if the word IS is introduced as referring to both organizational and technical elements, then a different word is needed for the hardware and software supporting an IS (it could be "computer system" or "technical subsystem"). If the authors use words inconsistently, coherence suffers and the reader cannot follow what is being said.
2. Is the language clear and intelligible for the audience? Generally academic papers are written by and for scholars with an interest and background in IS. Correct grammar and spelling and good style need to be observed.
3. Does the writing flow easily or is it something that the reader has to fight through? Does the writing style keep the reader's interest or put him/her to sleep? Keep in mind certain subjects may be inherently more difficult to discuss and, hence, the notion of "fighting through" might vary depending on the audience, reference discipline, and/or research topic. Also, papers written by authors whose native language is not English may not express themselves very well. But it is their job, nonetheless, to ensure the paper is readable and understandable. It is not the job of the reviewer to edit/re-write a non-English speaker's text!

4. Is there an appropriate use of figures and tables? Would additional figures or tables make the paper more concise and enhance its readability? Could the paper's presentation be enhanced by using additional rhetorical vehicles, such as metaphors? Are there sufficient examples to help the reader understand the arguments?

D) Theoretical Foundation

Theoretical foundation refers to the theories, frameworks, or underlying concepts that are used to guide the research. Different disciplines have diverse bases for accepting or not accepting various theoretical arguments. Theoretical foundations relate to *warrants* — the assumptions and beliefs that lie behind the claims. They provide the *raison d'etre* for the *claims*.

1. What is the underlying theoretical basis? Different disciplines will expect (require) different literature bases, different ways of arguing (i.e., rhetorical styles and examples), different values and beliefs, and so on. If the discipline is, for example, philosophy, the style and structure of arguments are likely to be different than if it was computer science.
2. Is there a good fit between the problem and the theoretical basis? (Would another basis provide more insight for the chosen audience?) Are there good reasons why a particular theoretic base was chosen?
3. What about other, possibly conflicting, theoretical bases? Why were these not chosen?
4. Is the paper faithful to the chosen theoretical basis? (Is the underlying model, structure, or framework employed by the paper an appropriate application, subset, or extension of a theory?)

As an example, if the underlying foundation is historical, then the arguments would require their appropriate positioning in supportable chronological ordering. This could be done through literature citations and/or evidence from other sources: stock prices at particular points in time, company announcements, etc. Mason (2004) used the theoretic lens of history to highlight the lessons learned from the implementation of Lyons Electronic Office (LEO). Mason uses the legacy of LEO as a way of understanding how it influenced the development of the IS field. One can see where Mason's theoretical lens provided a good basis for understanding the IS field.

E) Data Analysis/Interpretation/Argumentation

In speaking about *data* here, it does not necessarily have to be empirical data. Data can come from many sources, and the reviewer has to be open to consider whatever type of data the authors may use. Moreover, data analysis/interpretation is broader than the application of some statistical technique; it refers to how whatever data used in the research is analyzed and/or interpreted in a rigorous fashion. In the Toulmin sense, this refers to the *grounds* or *support* for the *claims*.

1. Are the assumptions, premises, axioms, assertions, and arguments presented in the paper made explicit enough to be tested by whatever analytical/theoretical technique that is applied? What form of testing (or justification) is being applied, and does it make sense?
2. What is the unit or the units of analysis? Is the chosen unit of analysis sensible? Would a different unit or units of analysis be more appropriate?
3. Has the data or evidence and its analysis been adequately described; is it clear how the authors have undertaken their analysis? Do the steps taken during the analysis or interpretation seem logical and sensible, or would another approach have been better?
4. Is the evidence (i.e., grounds) offered rigorous? Can the reader buy into the process that was used to support the authors' claims, or does it seem superficial and ad hoc?
5. Fundamentally, are the assertions made adequately supported by the analysis/interpretation/argumentation? Such support for an assertion may take the form of facts', mathematical proofs, concrete and conceptual examples, published literature, expert opinion, and logical reasoning.

Again, keep in mind when I speak of data I am referring to the arguments and/or concepts and/or literature used to support the claims made by the authors. Data need not be empirical or collected by the authors. As an example, consider the Lyytinen and King (2004) paper on the academic legitimacy of the IS field. The authors' data analysis consists of deconstructing the arguments used by proponents regarding the need for a theoretical core for a discipline to be legitimate. The authors then propose their own basis (i.e., argumentation) for academic legitimacy. Lyytinen and King's arguments were clearly rigorous and substantive even though their data analysis/interpretation was of a conceptual nature.

F) Results

Results refer to the output of the research inquiry. Having undertaken the research, this constitutes what was actually produced by the authors. In the sense of Toulmin, this is where the *claims*, *grounds* and *warrants* all come together in one coherent unit. Metaphorically, this is “where the rubber meets the road.”

1. How are the paper’s results presented? Are they intelligible, do they make sense, or are they counterintuitive? (Of course there are times when the results don’t make sense, and it is the job of the authors to offer plausible explanations as to why this is so.)
2. How are the paper’s results positioned with respect to what is already known? Are the results interesting and new, and do they offer new insights for future research?
3. How believable are the results? Are the arguments used to support the results believable and, hence, likely to be acceptable to the audience? Is the internal logic of the results consistent and coherent; is there a clear chain of evidence to support the results (i.e., do the conclusions follow from the evidence)? Do the results support the conclusions (e.g., external validity)?

Intelligibility, believability, and interestingness are inextricably linked, and ultimately relate to the validity of the research. Although all research projects must produce results that are intelligible, interesting, and believable in order for the community of scholars to agree that they make "contribution to the state of knowledge," the criteria are perhaps more subjective and open to negotiation in conceptual or philosophical papers. *Intelligibility* is addressed by considering the following: Have the authors made sure that knowledge claims made about the research topic are clearly stated, understandable, and coherent? *Interesting*ness deals with the question: Does the research add different insights and/or provide a new way of thinking about the research topic, or does it just state the obvious or reiterate what we know already? Finally, *believability* might be addressed in three ways: (i) providing direct quotations from the data (e.g., literature) the authors hope the reader can agree, or at least understand, their stated interpretation; (ii) building a level of coherence in the chain of arguments used and the concepts developed, which the reader can appreciate and relate to; and (iii) making sure that the results "make sense" and form a reasonable basis by which to think about the research topic. All papers must address the issues of intelligibility, interestingness, and believability in a satisfactory fashion.

Referring back to the Lyytinen and King (2004) article, the authors “results” in the form of an alternative conception of academic legitimacy based on a Market of Ideas as the center or core of the discipline are presented in such a way that the three criteria of intelligibility, believability, and interestingness are clearly evident.

G.) Conclusions

Conclusions should be the extrapolation of what was learned from the research. Many authors use the Conclusions section of their papers as summaries, simply repeating what they did. However, Conclusions should be the section where the authors take the opportunity to discuss what the results conceptually mean and what the implications are for research and practice. There are several ways to address this:

1. Do the paper’s conclusions successfully address the so-called "so what" problem? Can the authors defend their results from the counter-argument that the results prove nothing or imply no behavioral change from the view point of practice and research? Are the results trivial? Are they truly new, and if so, how? Horatory appeals to reason are not enough. The authors need to suggest what to do. For example, simply saying “stop global warming” is insufficient. They need to offer recommendations and actionable strategies or policies.
2. Do the authors describe how their results could be used by different audiences? What are the implications for practice? What are the implications for future research? Are the limitations of the research clearly stated?

As an example, consider the Hirschheim and Klein (2003) paper where we explored whether the IS field was or was not in a state of crisis. We concluded our analysis offering various options that were available for overcoming the internal communications deficits noted in the paper. These included changing the way the field thinks about generalizations, changing the institutional publication practices, focusing more on understanding the field's organizational stakeholders, and developing new knowledge creation and transformation networks.

Final Thoughts

While the review process is valuable for the institutional dissemination of all scholarly work, one is tempted to say it is especially valuable for knowledge claims not using empirical data, as these types of papers are largely based on conceptual thinking and argument. Traditional articles based on quantitative or qualitative data can typically be evaluated using a somewhat standard review template and by looking at weak points in sampling, statistical analysis, operationalization, or research design. The same cannot be said of a conceptual paper — it only stands on the strength of its argument and the originality of its thinking. And it is here where reviews can really add value.

In sum, reviewers can play a key role in helping to mold the authors' ideas into a coherent and effective message. Submitted papers improve through the review process. And, while it doesn't always work to the authors' liking (e.g., rejected papers, endless rounds of revisions), and it does have its drawbacks (length of the review process, can't disseminate ideas quickly, difficulty in getting novel ideas accepted, etc.), it is generally regarded as the best mechanism available. The review process represents the community's best efforts at assuring that knowledge creation and dissemination are done effectively. A good review process ensures that the best ideas get exposed and published and, it is hoped that good ones do not get mistakenly weeded out. Indeed, it is probably true to say that a good publication is constructed among authors, reviewers, and editors. Each must take his or her responsibility seriously. Each must also realize that the system only works when everyone contributes effectively. If you submit papers to any top level journal, you must be willing to perform review duties as well, and perform them in a timely and effective fashion. Not only is this necessary as your fair share and contribution to the community process, it also helps individuals learn how to build strong conceptual and theory papers. And even if you have not submitted papers to the journal, we might ask you to review papers for us if you possess the expertise needed to evaluate a particular submission. The journal not only needs good paper submissions, but good reviewers who complete good reviews. For those who have performed such duties for us, we thank you. For those yet to be asked, be patient. Your time is coming.

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