

## Communications of the Association for Information Systems

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Volume 42

Article 30

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5-2018

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Thomas Stafford

Louisiana Tech University, [stafford@latech.edu](mailto:stafford@latech.edu)

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### Recommended Citation

Stafford, Thomas (2018) "Rejoined and Regenerated: Response to Responses to “Reviews, Reviewers, and Reviewing: The ‘Tragedy of the Commons’ in the Scientific Publication Process”," *Communications of the Association for Information Systems*: Vol. 42 , Article 30. DOI: 10.17705/1CAIS.04230

Available at: <http://aisel.aisnet.org/cais/vol42/iss1/30>

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## Rejoined and Regenerated: Response to Responses to “Reviews, Reviewers, and Reviewing: The ‘Tragedy of the Commons’ in the Scientific Publication Process”

**Thomas F. Stafford**

J.E. Barnes Professor of CIS

Louisiana Tech University

*stafford@latech.edu*

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### Abstract:

The joy of writing critiques about the editorial process is doubly enhanced when engaged in debates such as this one wherein I find the most fascinating views, points, and counterpoints in correspondence to my own. I am simply pleased to say that the sterling colleagues who have chosen to respond to and rejoin my initial essays are highly skilled and insightful editors in their own right, and we all benefit from the colloquy that ensues in such discussions about how to improve peer reviews in our field of science.

**Keywords:** Reviews, Reviewers, Reviewing, Peer Review, Science, Tragedy of the Commons.

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This manuscript was solicited by the Department Editor for Debates, Karlheinz Kautz.

## 1 Essay

In this essay, I provide my thoughts on the series of response to my notion of the tragedy of the commons that, in my opening debate paper, I use as a metaphor for the review problem we editors find ourselves in these days. I offer these thoughts subject to the following caveats and concessions:

### 1.1 Caveats

- 1) As you read this essay, please understand that I have conflicts of interest with the following colleagues who have provided rejoinders to it: Edgar Whitley, Janice Sipior, and Stacie Petter. Whitley has reviewed my own work as part of his editorial duties during my earlier career. Professor Sipior appointed me to run *The DATA BASE for Advances in Information Systems* on my first, second, and current appointments to the editor's role at that fine publication. Lastly, I have the distinct honor to share duties during this current appointment as co-editor with the highly capable Professor Petter, Editor-in-Chief of *The DATA BASE*. That said, I just wanted everyone to know: I know these people and they know me. They know my thinking and I theirs. Filter their points and my own as well through that caveat.

I have never met Professor Avital in person, though I wish now that I had. I have no conflicts of interest with him other than the recently realized point that he has written on this same topic himself (the academic tragedy of the commons) several years earlier (e.g., Avital et al., 2015). I regret that I did not find his own paper on the tragedy of the commons and the research process before this point; I should have cited his work as part of my original essay. I also appreciate his ecclesiastical quote on the nature of "nothing new under the sun" in his original paper on the matter (Avital et al., 2015, p. 2). The tragedy of the commons has been with us from the start, I suspect, and I am only the most recent person to regretfully write about it.

And, then, Karlheinz Kautz, our associate editor for this process; I would not call it a conflict of interest what he and I have so much as a confluence of interests. I have found him a highly approachable editor for providing a ready platform in the debate section of *Communications of the AIS* whenever I wished to float a controversial idea to the field, such as I have in the past on the issues of review cycles and as I do now on the issue of journal reviewing service as a public good. Thanks, Karl, for letting me populate your pages with my philosophical and controversial musings; you do us all a great service by sponsoring such discourse.

- 2) Frankly, writing here about flaws in the editing process, I have to admit a conflict of interest with myself as an editor. I should be a more patient editor, I feel, but I have been editing for decades now, and I am plainly distressed at the flagging commitment of some reviewers out there. It harms the process of science to which I have given my life. As I say in my original piece, everybody wants to *get* a good review but not everybody wants to *do* one. Such a pity! Late performance, non-performance, and poor performance are issues that any editor would candidly agree hamper the quality of the publication process in our field. That is why I chose this platform to promulgate the philosophical wisdom I first learned as a doctoral student in cognitive psychology: human rationality is inherently self-interested. To not be a rationally self-interested individual who looks to maximize personal outcomes against group outcomes runs contrary to a certain and pragmatic extent to one of the more pragmatic benchmarks of human functionality. I might have said "sanity", but that assertion would likely draw further rejoinders to what I wanted to make as a simple, philosophical, and thematic point of metaphorical impact.
- 3) To wit, I write with an inherent bias: in our world of science and publication, one must give to get, and not enough currently give in the tradition of my revered mentor Carol Saunder's (2005) "diamond cutter" metaphor, which we saw in her editorship over *MIS Quarterly* in a prior decade.

### 1.2 Concessions

- 1) I am a long-time fan of the contemporary channeler of the Mark Twain spirit: public radio's Garrison Keillor. Keillor's favorite notion about his home town is that "all the men are strong, all the women are good-looking, and all the children above average". Not to risk lending a similarly gratuitous note to my recognition of collegial capabilities, but everyone who wrote a rejoinder to my original essay here is similarly strong, good-looking, and above average so to speak. Everybody has a point, and they make them quite well—better than I make my own in

my original essay, I think. The fact is, I consider that everyone who had something to say in rejoinder to my essay really knows more about this stuff than I do. As I frequently tell anyone who will listen on the matter of disciplinary service, it is the greatest gift in my work life to be associated with capable, motivated, and competent colleagues. I enjoy that particular working gift here to my great delight.

- 2) I am a chatty and colloquial Appalachian. It shows in my writing.

## 2 Responses to Rejoinders

With the above concessions, caveats, and various other prevarications, I have this to say after consideration of the rejoinders: everyone who writes in response to my original essay did a better job than I of explaining what Hardin's tragedy of the commons really concerns. I used the rubric as a highly accessible and necessarily inflammatory cultural metaphor of critical scarcity in deliberately telegraphic style (in order to make a quick and succinct point rather than explaining the philosophy behind Hardin's pronouncements). I call forth the metaphor of the tragedy of the commons mainly to make a point about problems with staffing the review process, and, in fact, deeper introspection on what Hardin said about self-interested exploitation of common resources leads to a couple of good points to ponder as the colleagues who write in response to my original essay in these pages note. To that end, I was not as informative on the intricacies of the actual commons rubric as I might have been. My colleagues provide fascinating points of view in that regard—about what Hardin meant and what it likely implies.

For example, Edgar Whitley (2018) quite correctly notes that Hardin's solution regarded the necessity of a new morality in response to an old problem. He continues that, in earlier years, moral suasion served to ameliorate the review/reward mismatch (did you see his point about the benefits of reviewing flowing mainly to the author?), and he makes his point well.

Whitley (2018) also says something interesting and insightful about the metaphor of grass as a grazing resource vis-a-vis reviewers as publication resources. Grass regrows each year, and, as he points out, so too do reviewers, but the replenishment process for both differs. Graduating PhD students freshen the reviewer crop, so to speak; except, as he aptly points out, they do not hit the ground running, whereas grass that regrows in the commons springs up with immediate utility. Newly graduated colleagues do not always know how to be good reviewers. PhD students rarely seek review work, it seems; they have enough to fill their days with seminars and learning how to get published (except I tend to think that reviewing *is* part of learning about the research process). I myself sought review work as part of my matriculation into the management information systems (MIS) field. In fairness, I had two chances to engage in this learning process because I foolishly sought two different business doctorates at different times in my career (I am terminally degreed in marketing and in MIS). So, I have spent a lot of time in my career as a doctoral student learning the ropes of the review process of a given field. In my initial days as tenure track colleague, transitioning from the marketing field to MIS, I was on the review boards of the *Journal of Marketing (JM)*, *Journal of Operations Management (JOM)*, and *Decision Sciences (DS)*. I was on those boards, however, because I specifically applied to join as opposed to waiting to be asked. I did so because I wanted to get experience and understanding in the review process and because my professional aspiration was to edit journals one day. Not everybody will choose that route to fame; most just want to hit *MISQ* or *ISR*. I can appreciate that.

Looking back to my early years as a junior assistant professor in MIS, I probably suboptimized my career trajectory by doing so much reviewing since it took time from my own publication productivity. I did make tenure and achieved full rank at the school where I started my MIS career, but I feel it might have gone better had I just stuck to my research. Still, I feel we do the field a service when we require our students to learn how to seek and prize review work regardless of its lack of extrinsic rewards, and I was proud at the time as a new assistant professor to be on Ruth Bolton's board at *JM* or Rob Handfield's board at *JOM*. Whitley (2018) makes this same point on the rewards of reviewing versus the career penalties that accrue from service dedication well, for which I am grateful. We Appalachians have a saying of admiration over a well-turned point of discourse: "Wish I'd said that!".

I turn now to Janice Sipior (2018) and her rejoinder. Sipior edits her own journal and has overseen the editing of my own journal for several decades as well. Thus, she knows a thing or two about editing and reviewing. For that reason, it has been a treat working with her these many years. I find the references to sociological theory in her rejoinder quite interesting, not the least because I was a sociology student back in the day. I find even more interesting her reliance on studies of review process issues found in *PLoS*, the

*Public Library of Science* (an eminent and well-read online journal of the physical sciences) of which I know quite a bit from my time editing *Decision Sciences* and dealing with the review cycle issues there. If the *PLoS* examples and citations that Sipior uses in her rejoinder had been in mind at the time I wrote my original essay, I would have leveraged them with a will. I was, indeed, already aware from a previous editorial experience of the example. As it is, the Appalachian in me has again to say: "Wish I'd said that!"

At *Decision Sciences*, I had a senior editor (SE) from the medical field, and, when we spoke about the issue of review cycle times, he strongly suggested that I look to the operating model of *PLoS* since the review process ran well, reviewers reviewed papers quickly, and his medical sciences colleagues considered *PLoS* a premier publication. He also indicated that a professional and *paid* editorial staff operated *PLoS*. One takes from the *PLoS* example that the journal runs so well because it *pays itself to do so*. The parallel notion of financially compensated reviewing is something that comes up in another rejoinder, which I turn to shortly.

Aside from the highly accessible *PLoS* example of compensatory publication management, another revelation about ways to improve the review process came from my editorial vision at *Decision Sciences* to expand into the economics and finance fields for further catchment and impact growth. In this venture, I became familiar with the publication processes in the "money" fields. What I learned from the SEs that I appointed to my board is that finance and economics journals have no problems obtaining good reviews on time. They *pay their reviewers a stipend* and generate the wherewithal to do so by *charging significant submission fees* from their authors at each submission event. So, for example, finance professors might find themselves paying \$500 to submit to a top-tier journal in their field. When they get lucky and land a revision, they must pay another \$500 to resubmit. And so on. But, the reviewers respond quickly (typically within three weeks or so) because they receive more than just ethical truisms for their service. They get paid; it makes a big difference. Trust me: human economic self-rationality is a powerful motivating force.

Getting to Sipior's (2018) point about the sociology of science and the norms and mores of the review process, the point to consider regards the reciprocity norm that Squazzoni, Bravo, and Takács (2013) give voice to. It forms the underpinning of polite human discourse. I would agree with the power of the reciprocity norm in most circumstances, but I do have some psychological wisdom to offer in counterpoint. The notion of mindlessness in human behavior (Langer, Blank & Chanowitz, 1978) suggests that, when one person makes a polite request that fits social norms even if for a nonsensical favor, one typically gains acquiescence because of the normative pressure of reciprocity in which individuals typically accede to polite requests. To say, "will you perform this review for me because I need you to do it" is syntactically similar to "may I break in line in front of you, I just need to make some copies", and yet the outcome differs. People do not tend to do reviews just because one asks them nicely (we likely do not yet have a reciprocity norm for reviewing to serve the essential moral suasion role). Reviewers who one asks for help might accept the assignment and then not follow through (it happens a lot to me), they might accept the assignment and return the work late (also quite frequently), or they might accept the assignment and turn in cursory performance (not unusual to find either). Or, worst of all, they might simply say, "No, I'm just too busy writing something important that I need to submit myself right now" (which happened almost verbatim in a recent request for specialized review services of a topic area expert).

What are we then to do? One suspects we might more seriously consider a compensatory model of reviewing, which Michel Avital (2018) makes in his brilliant rejoinder. Anybody who can mix Hardin's (1968) tragedy of the commons with Ouchi's (1980) special views of transactional economics in organized endeavors has my full attention. Avital does more than make a superb point: he reasons and develops it elegantly in the scientific meaning of the term (i.e., succinctly and meaningfully) (particularly his Figure 1). Where we editors have for so long relied on "team spirit" and "clan-based" organizational strategies to normatively regulate our adherence to peer-review expectations and performance, the increasing volume of research and available publication venues have rendered good will seemingly impotent as a regulatory mechanism of the field's review service. Operational regulation in Avital's view becomes the next plausible alternative when good will and social norms fail (the bureaucratic "rules and regulations" approach of a strong management hierarchy as I construe it from a transaction costs perspective). I suspect most journals *do* operate on bureaucratic norms now (i.e., on a rules-based ethic) and that they sanction those who do not adhere to the formal standards regarding review duties by eschewing their service in the future (to the extent that being excluded from review assignments might seem a sanction). Avital's recommendation is not unlike Rick Watson's "reviewer certification" suggestion (Gray et al., 2006); keeping tabs on performance is an interesting idea, the only counter argument being "who does the work?"

Though I construe organizational operation from Avital's (2018) Figure 1 as "bureaucracy" from the transaction costs perspective, he clearly has the notion of "pay for play" fee-based publication and compensated reviewing (as opposed to the momentary negotiation of the value of services on a spot market) as the natural outgrowth of such formalized rules-based approaches. I see the value in that.

His notion of a blockchain-mediated compensation system ensures that those who do good reviews also receive good reviews. It is a "coin of the realm" argument and highly compelling: a review system, he notes, is not just a public good but a market. And, with all markets, transaction costs pertain. Taking the transaction costs perspective further, Avital (2018) actually refers to the "hierarchy" as Ouchi (1980) construes it rather than the transaction costs notion of "markets", which has a distinct operational meaning in comparison to the marketing-oriented mainstream notion of marketplaces. No matter, his point is compelling: if we can set up a blockchain system whereby reviewers can accumulate credit for performing satisfactory review work, we can then accord a new cryptocurrency status as a way to pay "review fees" in the journal-submission process.

I like it. The notion is still in its early stages and unspecified details remain, but it is a workable idea if we could generate sufficient disciplinary buy-in, and, as he says, we are just the sort of field to engage in digitally innovative mechanisms. The mechanisms of how one "mines" such a currency remains to be seen, but Avital's (2018, p. 649) epitomizes the operative transaction in stating:

*In the envisioned token-based peer-review scheme, authors pay submission fees with peer-review coins (PRC) and reviewers receive PRC for their services. Thus, PRC is a cryptocurrency that fuels a peer review market in which PRC is exchanged, paid, and earned for review services.*

I am willing to hear more about that; the key issue I wonder concerns a chicken-and-egg notion of where the first coins to prime the system will come from. It is a little thing: the first author to pay for review services with PRC will have to have found a way to generate some coins with which to fund the transaction. Once the author pays for a few reviews, the reviewers have earned coins they can then appoint to purchasing review services for their own research as authors and a self-sustainable model begins to emerge. However, the problem of how we get to the first author with coins is what I wonder about.

Lastly, I now turn to the rejoinder from my colleague Stacie Petter. I think she and I have a similar ethical and moral stance because she has framed the entire issue of the tragedy of the (reviewer) commons in the Judeo-Christian ethic of service to others. "Do unto others as you would have reviewer two do unto you" is a phrase so well turned, so insightful and to the point, that I sure wish I had said it myself!

I have a similarly altruistic sense for why I engage in the publication-support process. I consider that doing reviews for the love of the work is, at heart and at center, the true answer to our problem of the review process absent the possibility of instituting Avital's brilliant blockchain compensatory token-based model. I say that out of emotional conviction; I freely admit to the compelling arguments of the entire group of rejoinders that things have gotten out of hand and some sort of solution seems needful. It would be naïve of me to expect an entire field of practice to become, overnight, egalitarian and selfless in their contributions to the common good. Certainly, I mean nothing derogatory by that either. I have always been, as a student of human psychology, convinced that the economic "rational man" of classic economics is a force of nature that is hard to overcome. I think that self-interested motivations, the heart of self-preservation, in some pragmatic way indicate human functionality. Those who do not look after themselves will not be around to look after others for long. On the other hand, of course, those who look exclusively to themselves with no thought to others do not produce as much societal good as they might potentially do.

I honestly favor Petter's views so highly because I work closely with her. Her dedication to doing well and helping others has helped me and countless others as we have edited *The DATA BASE for Advances in Information Systems* together. At the same time, I know her to possess a keen intellect (who among us cannot quote chapter and verse from her treatise on model misspecification?). To that end, I found her taxonomy of the range of self-interests that authors and reviewers possess to be delightfully informative. In her Figure 2, in particular, I think one finds real food for thought: reviewers do the work out of duty (my motivation) or because they feel privileged to be consulted as a topic area expert (another motivation of mine just lately). The cross-continuum of power/burden worries me a little but only because I do not like the idea of "reviewer as gatekeeper", and I surely want to avoid any notion that review work is burdensome.

Even so, I do not think she precisely means that when she uses "burden". If one reads closely, one can discern her notion that any good reviewer will carefully consider an assignment based on the consideration that doing the job well entails expending resources that take away from other work to be done. Reviewing, alas, is a zero-sum game in terms of personal productivity—at least in the short run. The willingness to put off other important work in order to render prized and valued disciplinary service to scientific others also reminds me of Carol Saunder's diamond cutter metaphor.

In fact, precisely because Saunders suggested my name yesterday to a colleague in search of a difficult topic area expert (marketing, go figure, it was my first academic gig), that I must sadly, but necessarily, say at this point, "I'm sorry, I must close for now. I have a review that I need to go attend to."

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## About the Authors

**Thomas F. Stafford** is Professor Emeritus at the Department of Information Processing Science, University of Oulu, Finland. During his career he has served as a professor at the University of Jyväskylä and at the University of Oulu. Before his retirement, he also worked for ten years as a part-time scientific head of INFWEST/INFORTE programs, which are joint efforts of a number of Finnish universities to support doctoral studies in IT. He has also served in various editorial positions in IS journal, including *Communications of the Association for Information Systems*, *European Journal of Information Systems*, *Information Systems*, *Information Systems and e-Business Management*, *Information Technology and People*, *Journal of the Association for Information Systems*, *MIS Quarterly*, and *Scandinavian Journal of Information Systems*. His research has broadly focused on the theoretical foundations of information systems, IS development methods and approaches, organizational analysis, implementation and acceptance of information systems, and design science research in IS. He has published in journals such as *Communications of the ACM*, *Communications of the AIS*, *Data Base*, *European Journal of Information Systems*, *Information & Management*, *Information & Software Technology*, *Information Systems*, *Information Systems Journal*, *Information Systems Research*, *International Journal of Information Management*, *Journal of MIS*, *Journal of the AIS*, *MIS Quarterly*, and *Omega*.

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