The Rhetorical Ecology of Peer Review: Changes in the Digital Age

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Not long after Harvard announced its support of an open access (OA), web-based repository for academic publications, a number of newspapers around the U.S. ran alarmist headlines bemoaning the death of peer review, as if open access publication would undermine the celebrated process of reviewing academic work by others in the same field. It is peer review that sets the gold standard for academic publication for most researchers, including writing scholars, and it has been celebrated as a vetting process, as a part of academic initiation and professionalization, and as a feedback mechanism that generally leads to improved writing. Although peer review of scholarly publications is not dying and some of the fear-mongering may be driven by profit-conscious publishers, peer review practices are indeed changing in the digital age, and the changes have implications for us as teachers of writing. We need to take note of the ways in which the revolution in digital publishing has altered the landscape of peer review, the tools for communal witnessing, and our notions of audience, text, and possibly of science and other bodies of knowledge.

Unlike the general public, most teachers of writing are already predisposed to believe that peer review is a rhetorical process, a dynamic, socio-historic process that inevitably undergoes change in response to new conditions. Most of us are predisposed to believe that, when one set of conditions shifts, such as changes in electronic publishing, we need to anticipate- and deal with- a ripple effect throughout the system of prevailing practices known to us as readers, writers, archivists, and teachers. Most of us are predisposed to believe that changes in electronic peer review practices will make more evident than ever the multi-faceted, complex rhetorical systems in which literate activity takes place.

Familiar as most of us are with rhetorical principles in theory, though, some of us have only a vague understanding of rhetorical particulars in historical context. Arguably, we will serve our students better if we are well informed about the history of peer review and if we have a

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language fit to describe the emergence of factual knowledge in various historical contexts. We believe that Marilyn Cooper (1986) and a host of writing scholars, including Jenny Edbauer Rice (2005), provide us with such a language when they refer to writing contexts as “ecologies” or “rhetorical ecologies.” The better we understand the rhetorical ecologies of peer review, the better positioned we may be to engage our students in thoughtful, responsible evaluation of sources, something that is more important than ever in the digital age.

The following case for the “rhetorical ecologies of peer review” is developed in four sections: In the first section, I define the ecological perspective. In the second section, I describe peer review in three ecologies: that of the Royal Society of London in the seventeenth and eighteenth centuries (when scholarly peer review practices took shape); that of the heyday of print journals in the twentieth-century world of “big science”; and that of new review practices at the dawn of the digital age (See Appendix 1 for a chronology of some changes in digital culture). Throughout, I address various selective pressures that delimit the circulation of texts in these ecologies—not only peer review, but also archiving practices, cost, time, and reward structures. In the third section, I review studies of variation in peer review and argue that all too often variation is recognized but the rhetorical purposes driving the variation is not. This failure to recognize the rhetorical, dynamic features of peer review is what leads me to discuss possible solutions. In the final section, I review a few ways that writing teachers might foster more critical reading habits among students. Although the thrust of this essay is argumentative, I extend Edbauer Rice’s concept of “rhetorical ecologies” to peer review and argue that the dynamic, rhetorical dimensions of peer review are poorly understood. I illustrate the claim with both existing scholarship and our own IRB-approved case material, including surveys and interviews with colleagues in engineering. Finally, I suggest some implications for teaching.

I: The Ecological Perspective

Edbauer Rice’s term “rhetorical ecology” aptly fuses two scholarly traditions, one rhetorical and the other Darwinian. When Edbauer Rice fuses the two traditions, though, she uses one tradition to critique the other. In particular, she uses the term ecology, with all its associations of fluid, dynamic, shifting processes, to critique discrete rhetorical terms, such as Lloyd Bitzer’s (1968) term “rhetorical situation,” one that Edbauer Rice finds too static. Just as
Edbauer Rice attempts to open the rhetorical term “situation” with the term “ecology,” I attempt here to open the phrase “peer review.”

I am hardly the first to do so. A generation of genre theorists and activity theorists have made similar claims, whereby “writing” is understood not as a discrete action completed by an autonomous individual in a single moment, but rather as an activity that functions within a complex network of interacting variables, a network that could easily be described as an ecology. Moreover, a number of writing scholars have explicitly invoked the metaphor of “ecology,” including Marilyn Cooper (1986) with “The Ecology of Writing,” Margaret Syverson (1999) with *The Wealth of Reality: An Ecology of Composition*, Clay Spinuzzi (2002) with “Modeling Genre Ecologies,” and Jenny Edbauer Rice (2005) with “Unframing Models of Public Distribution: From Rhetorical Situation to Rhetorical Ecologies,” among many others.

Although I am indebted to this body of work and its collective critique of static, mechanical perspectives of writing, I find understated the elements of an ecology that are most relevant to peer review: processes of selection. In any ecology, there are multiple selective pressures acting on variation, and if one changes or disappears, others emerge or take on new functions. Ideally, that research accepted as “knowledge” (or scholarship accepted as valuable) is research or scholarship that has survived multiple processes of selection.

Peer review is an important selective pressure in scholarly rhetorical ecologies, but it is not the only selective pressure - it is not now and never was a one-shot, all-or-nothing affair. Other selective pressures include archiving practices, economic constraints, time, “impact,” and available technologies, all of which might limit or shape which texts remain in circulation and for how long. Although a change in one selective pressure may not be a concern, any change still warrants attention, for a change in one pressure will inevitably lead to changes in others. At the dawn of the digital age, we have some new selective forces as well as some old ones functioning in new ways.

Among the selective forces functioning in new ways are academic librarians and general readers. Librarians were gatekeepers earlier, too, but their roles are much more pronounced in the digital age: They are forced to make economic decisions about archiving practices on an
unprecedented scale, decisions that have enormous consequences for our collective memory (as will be discussed in subsequent sections). Likewise, general readers had some gate-keeping role in earlier ecologies, but a limited one. Because general readers are accessing and responding to more primary research, they too are having an impact on research practices, and some of that impact can be quantified and used by other gatekeepers, including university faculty who evaluate peers for promotion and tenure based on the impact of scholarship. Although general readers have access to more primary research, they also have access to an unprecedented volume of unfiltered information. In the digital age, general readers have a much greater burden of selection and critical judgment, given the wealth of available information. To help readers make critically informed selections, we as rhetoricians have a greater responsibility than ever to teach what Krista Ratcliffe (2005) calls “rhetorical listening.”

At the dawn of the digital age, we have not only more information than ever, but a wider and more diffuse community of readers, readers who function informally as virtual witnesses, even if not formally as peer reviewers. The informal witnessing of readers in the digital age recalls some of the informal communal witnessing that existed at the dawn of the scientific revolution when academic peer review practices took shape.

**II: Three Ecologies of Peer Review**

**A 17th Century Ecology: Communal Witness of the Royal Society**

New peer review practices emerged in the late 17th and early 18th centuries, when the scientific revolution was underway and new standards of evidence were required to persuade a body of people that something might be true. Gone was knowledge claims based on revelation or disputation alone. One group interested in new standards of evidence was the Royal Society of London, which eventually produced one of the first scientific journals, the *Philosophical Transactions of the Royal Society*. While members of this community needed to be persuaded of the accuracy of reported evidence, they could not all observe the evidence directly. Steven Shapin names the indirect process of reading and evaluating an experimental report “virtual witnessing,” the process, for example, by which Robert Boyle persuaded other members of the Royal Society that he had experimental evidence of what I now accept as Boyle’s law (about the inverse relationship between pressure and volume in gases):
“Experimental reports rich in circumstantial detail were designed to enable readers of the text to create a mental image of an experimental scene they did not directly witness. I call this ‘virtual witnessing’, and its importance was as a means of enlarging the witnessing public. The notion of a ‘public’ for experimental science is, I argue, essential to our understanding of how facts are generated and validated” (Shapin, 1984, p. 481).

Among others who have traced the role of virtual witnessing, essentially peer reviewing, in knowledge formation is Charles Bazerman (1988), whose book *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* has been a landmark in rhetorical studies. As Bazerman tells us, the early British peer reviewers were basically gentlemen who had enough leisure to pay attention to new theories and experiments in natural science; they were not initially trained or certified in any particular discipline. They made up a community of witnesses, but only in a very loose sense. While the number of scientists and scientific papers doubled approximately every fifteen years from the beginning of the seventeenth century, yet to come were the specialized journals now available in academic libraries. Yet to come were specialized communities of readers in newly emerging sub-disciplines across the arts and sciences, each with their own journals, specialized review, and shared standards for review.

The loose community of amateurs that characterized virtual witnessing in the seventeenth and eighteenth century has parallels in the digital age, although today’s web-savvy amateurs have not supplanted highly specialized readers - they merely co-exist in overlapping ecologies. If one similarity between the Royal Society and some contemporary rhetorical ecologies is the looseness of the community of virtual witnesses, another is the form in which they communicate. The genre out of which the experimental article developed was the letter, which gave way eventually to less narrative, more thesis-driven arguments that became what we now recognize as experimental, theoretical, or scholarly articles. While thesis-driven, monologic accounts still dominate academic scholarship today, the web culture may be turning back via blogs and pre-print discussion sites to something more letter-like and dialogic. Again, the more dialogic genres available today have not supplanted more tightly-constrained, monologic genres, but they co-exist in overlapping rhetorical ecologies.
A 20th Century Ecology: Peer Review in the Heyday of Print Journals

There was in the nineteenth century a huge spike in publications, including many unrefereed magazines that would allow their authors to raise controversial issues unchecked. Among the many new publications, though, were academic journals that helped to establish new academic sub-disciplines, in and out of the sciences. Important as the nineteenth-century was for the development of new scientific and academic specialties, science was not yet “big science,” a term coined by Alvin Weinberg in 1961 to describe the multi-billion dollar network of corporate- and government-funded research that pumps out massive amounts of information (Nye, 1996, p. viii). Nineteenth-century research was not yet “big” in this sense and the process of formally peer-reviewing research prior to publication was still developing.

Even at the beginning of the twentieth century, most research was not subject to peer review (narrowly defined as the standardized practices conducted by members of an editorial board of reviewers). In 1905, for example, Alfred Einstein published *Annus Mirabilis* which was not sent out to peer reviewers, although the journal editors, Max Planck and Wilhelm Wein, were both Nobel laureates and hence members of a very elite group of peers. It was not until 1940 that the *Journal of the American Medical Association* formally introduced peer review (narrowly defined), even though medicine had been one of the first disciplines to use loosely some form of peer review.

Things changed in industrialized nations after World War II, when the military and corporations started to fund research projects on an unprecedented scale: Big science was born, shifting the norms for academic culture in general, scientific or not. More specialized literacies were developed along with new journals, which excluded the general reader, and peer review became a tightly controlled phenomenon (relative to previous centuries). Still operative in twentieth-century rhetorical ecologies for peer review, though, were some of the social and economic selective pressures mentioned earlier, even if they were invisible or taken for granted (perhaps, for instance, being an English-speaking, middle-class white male). In addition to social and economic constraints on peer review were technological ones. With the advent of new...
technologies, such as the Xerox photocopier in 1959, peer review could be facilitated more cheaply on a large scale.

As the selective pressures from one area wane (conventional print-based peer review practices), others increase (such as various ratings games to assess the worth of an article, an individual, a journal, or an institution; such as new voices—new kinds of reviewers, perhaps from outside the discipline). All of this is consistent with the Darwinian metaphor—a given rhetorical ecology is not going to have one single, permanent selective pressure, but will develop new ones in order to adapt to other innovations, threats, and changes. Changes in archiving practices, for example, are reshaping digital ecologies and the way peer review functions.

But in what ways might the archiving practices of a librarian be considered a selective force in a rhetorical ecology? Erving Goffman (1961), whose study of interaction in classification systems, offers some answers. A change in a classification system, such as the way a research article is archived, potentially changes who has access to the research, which in turn potentially affects future changes in the classification system. Added to this dynamic, classified/classifier loop are issues of time, economics, and status of an institution or government. When one selective pressure is released, another may take its place. In Memory Practices in the Sciences, Geoffrey Bowker (2005) argues that, because an archive is always selective and therefore partly exclusionary, it is powerful, and an archive is most powerful when its exclusionary principles are invisible.

A Late 20th and Early 21st Century Ecology: Peer Review in the Digital Age

Librarians have long been responsible for managing established archival practices and for making economic decisions that may be invisible to the typical scholar. Now, librarians are making economic decisions that may change which archival practices are dominant in the next few decades. What happens when a librarian cannot justify the expense of a particular journal? What happens if librarians maintain subscriptions to for-profit mega-publishers that have bought many smaller journals—and what are the criteria with which the mega-publishers make their acquisition decisions? What survives and what perishes in this ecology? When for-profit mega-publishers buy out smaller journals, are their criteria for selection based primarily on profit or scholarly merit? Jean-Claude Guédon interrogates this in the Proceedings for Creating the Digital Future: Association of Research Libraries - 138th Annual Meeting. He says, “In the last
50 years, publishers have managed to transform scholarly journals—traditionally, a secondary, unpromising publishing venture at best—into big business. How they have managed to create extremely high profit rates is a story that has not yet been clearly told” (Guédon, 2001). What happens if the expense of scientific journals squeezes out other journals in the humanities and social sciences? How do these economic and archiving decisions shape the disciplinary balance within a larger academic ecology? Note that some scientific databases such as the Web of Science, the heart of the Web of Knowledge platform, are so expensive that librarians have greatly reduced options for databases in the humanities. Arguably, it is resistance to this expense, coupled with the availability of new technology, that has helped launch the search for alternative archiving practices such as open access.

Among those to embrace some open access archiving practices is the National Institute of Health (NIH), which now requires researchers to post data to an open access database for any research that is NIH-funded. This new archiving practice is significant. The NIH open access database has the potential to redefine (by degree) authorship, agency, and even science itself. These public open access databases may shift the center of science from claims made in peer-reviewed journal publications to evidence in communal databases, a potentially positive change in an otherwise publish-or-perish research ecology, where some researchers are tempted to publish findings prematurely. In both cases, the evidence is vetted, but in the case of open access databases, the vetting is ongoing by the marketplace of researchers.

Positive as the NIH move might be, publication in free, open access repositories may allow researchers to bypass conventionally peer-reviewed (and, in some cases, obscenely expensive) journals. Resistance to subscription to expensive, for-profit journals and databases may be intensified in universities in third-world countries where academic librarians and faculty have limited funds for library acquisitions. If restricted to conventionally peer-reviewed, print-based journals (often expensive), not only are third-world researchers likely to miss out on a great deal of scholarship, but also other scholars are deprived of their research perspectives. Open access repositories counterbalance the economic constraints in such ecologies.

However, when new archiving practices develop, there are ripple effects throughout the system. Theoretically, open access journals are peer reviewed, just like subscription-based print
journals. But, if open access journals are available for free (to the user, even if not to the library or university), then users may have less incentive to subscribe to particular academic journals, which in turn may make it more difficult for journals to sustain themselves economically. Throughout the twentieth century, the activity of peer review was coupled with the material presence of a journal and its board of reviewers. Other selective pressures were there in the research ecology, too, but peer review of articles in subscription-based journals was the dominant form of academic “communal witnessing.” Are there new forms of “communal witnessing” evolving along with new developments in online archiving? Or can “peer review” be sustained as is for a long time to come?

Any mode of publishing has economic consequences (some of which are indirect and largely invisible,) and it is still an open question to what degree open access will affect peer review in the long run. UK journalist Richard Poynder (2006) argues that open access is not a financial threat to peer review, something that Stevan Harnad (1982) claims as well. While Peter Suber (2007) agrees that open access will not undermine peer review, he concedes that defunding peer review may be a legitimate concern.

As suggested earlier, new technologies are making possible new forms of communal witnessing, and what the implications are for peer review is not entirely clear. One critic of conventional peer review writes:

“I think it is worth considering that abandoning [conventional] peer review has revolutionized scholarly communication in physics and some related disciplines. Today, the real innovations get posted to arXiv, there is a public discussion among peers (go figure) through counter papers also submitted to arXiv, and then the articles get sent to the peer reviewed journals as an afterthought, after all the real intellectual work happened on arXiv and through email. There isn’t much problem with goofballs, since everyone knows everyone in the community and there is a gatekeeper to screen the complete nutjobs”

While this respondent may have overestimated the degree to which a list monitor distinguishes between the “nut jobs” and other virtual witnesses, the point remains that there is some vetting, even in informal communication networks, and that embedded in these networks are still other selective pressures, including, perhaps, some mastery of subject matter. These
virtual witnesses collectively function in something similar to what Robert Boyle (as cited in Kronick, 2001, p. 38) dubbed an “invisible college,” or a group of individuals sharing a research interest but whose affiliation is not formally recognized.

Apart from the communal witnessing made possible with new technologies are time pressures. Time is a selective pressure affecting when something is reviewed- and where. When reviewers in one journal reject an article, in most cases it is printed somewhere at a later date, even if not in a journal as high in status (Daniel, 1993, p. 47). Paradoxically, peer review practices are blamed for being both too fast (from a quality perspective) and too slow (from a dissemination perspective). Time pressures might lead both researchers and reviewers to be expedient, as suggested by Zoë Corbyn (2009) in the article “Publish or Perish Factors in Spiraling Retractions: Retractions Up Tenfold.” Even if peer review doesn’t always catch hastily or dishonestly produced manuscripts, peer review remains a time-consuming, sometimes onerous process.

Time spent peer reviewing has triggered new alternatives: It is partly because peer review is slow that physicists have turned to pre-print publications, for example through arXiv at Cornell. Time also affects who can review. Harnad argues that time pressures make expert reviewers scarce, but that is all the more reason to keep a system in place for cultivating expert reviewers’ services. As he says,

“Those who think spontaneous, self-appointed vetting can replace the systematic selectivity and answerability of peer review should first take on board the ineluctable fact of referee scarcity, reluctance and tardiness, even when importuned by a reputable editor, with at least the prospect that their efforts, though unpaid, will be heeded” (Harnad, 2006)

Tied to constraints on time are value and the reward system in various rhetorical ecologies. Throughout the late twentieth century, peer reviewers offered their services with little reward other than the status associated with being a reviewer for a relevant journal. It remains to be seen what will motivate experts in a field to participate formally in open access review practices.

Whether or not new funding mechanisms and reward systems are established to protect the most familiar elements of scholarly peer review, peer review still functions within a dynamic
ecology, one that is shaped by economics, archiving practices, access, time, and politics, sometimes in a nested hierarchy (peer review of grant proposals often precedes peer review of research articles, the quality of peer-reviewed articles shapes the prestige of journals, individuals are peer-reviewed by the journals in which they publish, and institutions are ranked by the publication records of their faculty). One thing that is clear, though, is that peer review in the digital age exists in several forms, including “traditional” peer review of subscription-only print journals, “traditional” peer review in print journals with open access options, “traditional” peer review in open access-only journals; pre-and post-publication peer review; and dynamical peer review.

Other selective pressures are emerging in the digital age, some of which could function independently of peer review narrowly defined. Some of these selective pressures include various metrics for quantifying impact, such as citation analysis and the ISI impact factor (IF) and a range of alternative impact measures, such as Google PageRank (PR), H-index, Y-factor, and so on. These measures all quantify something about the published research, often by counting the number of times the article has been cited. Such quantitative measures do not distinguish between articles cited for notoriety and articles cited for merit, between articles cited because they raise a question and articles cited because they address or answer the question. That is, these quantified measures do not assess purpose, audience or other rhetorical factors. Interestingly, we have observed scientists who lean heavily on quantitative evidence for their research but who question the usefulness or fairness of quantitative evidence such as impact metrics when used to judge their own productivity. The editor of Nature addresses the desire for objective measures of assessment and the inevitability of qualitative ones, here critiquing the abuse of impact factors: “How to judge the performance of researchers? Whether one is assessing individuals or their institutions, everyone knows that citation measures, while alluring, are overly simplistic… Most researchers prefer peer assessment.” He goes on to plea for qualitative assessment ("Ratings Games, 2005")

III: Poorly Understood Studies of Variation
However much peer review has been celebrated, it is a process that has undergone considerable scrutiny, even before the advent of open access publishing and new rating games. Since mid-century, the impulse for critique has come from both ends of the epistemological spectrum: At one end, positivist-leaning empiricists have found contemporary peer review practices biased, unreliable, and unfair- and have called for reform. At the other end of the spectrum, postmodernists have called into question a naïve faith in objectivity, agency, situation, and authorship, so much so that the most skeptical postmodernists resist any generalized research claim, peer-reviewed or not. What does seem to be recognized is variation in peer review practices, but what is little understood are the rhetorical reasons for this variation- value-laden judgments about the significance of a topic and the ethos of the researcher and his/her institution, all shaped by the readers’ experience and expectations in different rhetorical ecologies.¹⁰

Beginning with Robert Merton’s mid-century work, a body of sociologists has studied the role of peer review in the control and distribution of knowledge. Merton (1973) noted that scientific journals play a dual role in scientific communities, both by certifying new knowledge (particularly via peer review) and by providing a forum for competition (for establishing priority and recognition of research, via peer review, acceptance rates, and rankings).¹¹ In the 1970s, Merton and Harriet Zuckerman (1971) started examining disciplinary variation in rejection rates. What could explain the variability? More recent sociologists of science have argued that the more cutting edge or novel a claim is, the more disagreement there is in peer review- or the more variability there is. Other scholars have looked at decision structure: When the prevailing structure of decision-making is “innocent until found guilty,” there are high acceptance rates; in contrast, when the prevailing structure of decision-making is “guilty until found innocent,” there are low acceptance rates. What all of these studies have in common is recognition of some variability: Peer review is not a mere reflection of reality or of sound evidence.

Throughout the next few decades, a number of disciplines started investigating variability in their own peer review practices- initially in psychology, later in law, medicine, physics, and chemistry. Ongoing studies show some level of dissatisfaction with peer review. In 1986, the American Council of Learned Societies called for reform of peer review (even though peer review was believed to be worth keeping). In the same year, the first International Congress on Peer Review and Biomedical Publications was held. Conference organizer and deputy editor of
the *Journal of the American Medical Association (JAMA)*, Drummond Rennie, has commented that:

“...There seems to be no study too fragmented, no hypothesis too trivial, no literature too biased or too egotistical, no design too warped, no methodology too bungled, no presentation of results too inaccurate, too obscure, and too contradictory, no analysis too self-serving, no argument too circular, no conclusions too trifling or too unjustified, and no grammar and syntax too offensive for a paper to end up in print” (Rennie, 2002, p. 2760).

One reason some researchers express dissatisfaction with peer review is the influence of extra-textual qualities affecting judgment, such as nationality, gender, and the status of an author’s institution - in short, ethos (Peters & Ceci, 1982; Zuckerman & Merton, 1971). Related to judgments about the author and the author’s associations (not the research) is what Merton called the “Matthew Effect”: the rich get richer and the poor get poorer. By this he means that a senior researcher may get a disproportionate amount of recognition, and the recognition snowballs. Conversely, junior researchers are under-recognized, and the silence grows. Merton’s principle applies not only to the researchers, but to the researchers’ institutions and other variables in the research ecology.

In recent decades, peer review variability has become well documented, even if the reasons for it are poorly understood. In 2006, the prestigious journal Nature devoted an issue to the “peer-review debate,” where John Moore lamented how poorly understood this valuable but flawed process is: “Even science writers and journalists who should act as important links between scientists and the public sometimes seems not to appreciate what peer review means” (Moore, 2006).

Not surprisingly, in my own study (see footnote 12), almost half of the surveyed engineering faculty describe dissatisfaction with the quality of peer review. In my study, nearly 30% of the editors reported less than 30% agreement on the first round of review. This lack of consensus might raise eyebrows, and, indeed, some studies reporting limited correlations with pairs of referees have raised questions about reliability, fairness, and validity of review.
practices (Daniel, 1993). However, these findings are disappointing primarily when the expectation of the investigators is that peer review could be an absolutely objective process.

Another reason some researchers express dissatisfaction with peer review is variability about what constitutes an important research question. There may be stable criteria for evaluating technical accuracy, but there is less consensus about what is important and by what criteria we would judge something to be important. As one engineer said in my study, “I've had reviews come back where one reviewer thought that the paper was great and another that that it was only fair. Perhaps it is the nature of my research relating engineering to policy, decision making and regulatory compliance,” suggesting that the technical data are not questioned as much as the implications or applications of engineering. Another respondent observed, “There are a lot of 'junk' papers out there though (usually correct, but not very useful).” How tolerant that respondent is of work outside his or her area, I do not know, but another professor suggests that reviewers are heavily biased by the kind of research they do: “A paper can contribute in different ways to the technical field. Some reviewers are heavily biased against some efforts in the area he/she is not familiar or not in his favor. For example, good quality experimental research is valuable, time consuming. Some more theoretical researchers would not consider experimental work as valuable as paper and pen analytical work. The author should make the significance and contribution clear.” Still another respondent commented, “Some reviewers are heavily biased against some efforts in the area he/she is not familiar or not in his favor.”

The engineers I surveyed observe variation in the value of a research topic; likewise, the editors I surveyed identify “the topic” as one of the central issues in determining acceptance/rejection of a peer reviewed manuscript. When asked what constitutes the primary reason for outright rejection, 40% of the respondents indicated “research is not closely related to issues covered by the journal,” and 30% said “research is not novel enough (even if the science is solid).” Only 13% of the respondents said the primary reason for rejection was “claim is insufficiently supported by the data.”

Another reason some researchers express dissatisfaction with peer review is variability about what constitutes an important research question. There may be stable criteria for evaluating technical accuracy, but there is less consensus about what is important and by what criteria we would judge something to be important.
This suggests that peer review may function more to assess questions of value (importance, relevance) than to assess technical accuracy. If so, the main filters for accuracy may be elsewhere in the research ecology: A researcher is unlikely to secure a tenured position without some evidence of competent technical skill; a researcher is unlikely to provide evidence of competent technical skill without having passed certain courses or scoring well on aptitude and admissions tests; a researcher might not perform well on certain tests if lacking certain educational opportunities, and so on. What peer review (narrowly understood as the review process before print publication) gauges may have less to do with the validity of a knowledge claim than the value of a knowledge claim. That is, peer-review (narrowly defined) is highly value laden. The review of validity may rest with a more open court, past and future, in the research ecology.

If a primary reason for variation in peer review is the value of the topic, lesser reasons include the researcher’s ethos as understood by the reviewer, whose judgment is shaped by expectations and experience. That a researcher’s ethos is not independent of reviewer’s expectations (and prejudices) is suggested by this engineer’s complaint: “I believe a couple of factors do affect the reviewer’s opinion. One is the name of the author’s institution. Another is the English writing skill. In later case, even if the technical contents are good, the reviewer complains like hell.” Indeed, a few of the other respondents in my survey of engineering faculty criticized non-native speakers of English in the open comments.

Some of the variation in reviewers’ experience and expectations can be understood as developmental, as a learning curve, for reviewers live in historical real time and are inevitably inexperienced when first engaged in peer review. From this perspective, we should expect a wide expert-to-novice range of evaluations. As Godoy (2004) suggests in a study of peer reviewing in engineering, novice reviewers may be more likely to be exclusively negative and more likely to focus on “lower order issues.” Indeed, over a third of the respondents (35%) in my survey reported that they were novices when they first reviewed another manuscript for publication: They were still in graduate school. With a few exceptions, the rest of the respondents indicated they were assistant professors, still relatively inexperienced, when they first reviewed a manuscript. Not surprisingly, half of the 18 surveyed engineering faculty in my study said they changed their criteria for evaluating manuscripts over time. Five faculty (28%) said they address the same range of issues but their focus or emphasis has shifted with experience.
The point here is that faculty typically do engage in rhetorical activity- and change their practices in some predictable ways- but they seldom acknowledge that activity as rhetorical, whether the activity is positive or not. Even those who write about peer review sometimes describe it in contradictory ways. For example, I note that Donald O’Shea’s (2004) advice in “Reviewing Papers” ultimately sounds like advice any writing teacher would offer new teaching assistants (TAs), encouraging reviewers to attend most of all to the content of the paper and least of all to editing- to resist copyediting, even though judicious feedback about exposition might be helpful, but this advice follow pages of rule-bound advice.

Even if researchers tend to have unrealistic and a-rhetorical expectations for peer review, many journal editors do not. Charles Kiesler (1991) makes a case for qualitative decision making in an article entitled “Confusion Between Reviewer Reliability and Wise Editorial and Funding Decisions.” Arguably, what Kiesler means by “wise editorial decisions” is what I could call rhetorical insight. Richard Horton, editor of the prestigious medical journal The Lancet, begrudgingly acknowledges what I would call a rhetorical reality: “The mistake, of course, is to have thought that peer review was any more than a crude means of discovering the acceptability- not the validity of a new finding.” While acknowledging the rhetorical reality, Horton’s tone is that of a jaded positivist: “We portray peer review to the public as a quasi-sacred process that helps to make science our most objective truth teller. But we know that the system of peer review is biased, unjust, unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish, and frequently wrong” (Horton, 2000, pp. 148-149)

Open access advocate and editor Stevan Harnad (2006) still very much defends peer review but Harnad describes it in rhetorical terms: “Peer review is dynamic and answerable: Dynamic, because it is not just an ‘admit/eject’ decision by a gate-keeper or an ‘A/B/C/D mark by a schoolmarm, but an interactive process of analysis, criticism, and revision that may take several rounds… like all exercises in human judgment, even expert judgment, peer review is fallible, and sometimes makes errors of both omission and commission…”
My own survey of 38 editors suggests that editors, usually researchers themselves, have markedly more rhetorical views of peer review and publication than do the researchers who contribute to their journals.

**IV: Implications for Practice**

Just as journal editors typically see a rhetorical landscape that their contributors fail to see, we teachers of writing see a rhetorical landscape that our students are often reluctant to acknowledge, particularly when it comes to evaluating non-fiction sources and understanding the complexities of peer review. We can, perhaps, help our students to better navigate new rhetorical ecologies not so much by teaching them in radically new ways, but rather by putting radically new emphasis on some old practices. Simply put, I call for more rhetorical analysis of those sources that our students often assume are factual.

Arguably, we excel at teaching our students to analyze non-fiction that is blatantly rhetorical: nonfiction with political, social, or cultural agendas. We are less inclined to ask our students to analyze texts that are primarily informative or factual. By not doing more rhetorical analysis of sources that seem to be fact-based, we may be leaving our students vulnerable where they most need to be reading actively, rhetorically, selectively.

So what can we expect- do we think a typical college freshman will understand what we have just described as the rhetorical ecology of peer review? In a word, no. However, we believe we can work toward a nuanced understanding of sources, formally peer reviewed or not, with scaffolding something like this (offered with all the usual disclaimers about stages- this is meant as a guide or heuristic, not as a lock-step, rigid sequence):

**Stage 1: From Absolutism to Qualified Skepticism.**

For students who are still fairly black/white thinkers, we might foster a healthy skepticism of factual sources by asking students to assess them by standards of authority, objectivity, quality, coverage and corroboration, currency, and relevance. Numerous universities have useful library websites that elaborate on these criteria. 14
To make the point that even peer-reviewed articles can be wrong, it might be useful to cite these pieces (fully cited in Works Cited):

a) “Publish or Perish Factors in Spiralling Retractions: Retractions up Ten-fold”

b) “Ghostwriting Is Called Rife in Medical Journals”

c) “Sokal Affair”

For students accustomed to assuming that most published information is true simply because it is published, this is an important start. It matters to question the validity, accuracy, and reliability of information-based sources, even peer-reviewed sources. Focusing on these factors alone, though, can reinforce very positivist assumptions, and we encourage looking ahead to Stage 2, a more social view of peer-reviewed sources.

**Stage 2: From Isolated Source to Rhetorical Situation**

Although Edbauer Rice objected to Bitzer’s term “rhetorical situation” because it is static, Bitzer (1968) nonetheless uses the term to shift the focus from an isolated text to a text embedded in a social dynamic, one with a sender and receiver whose expectations are constrained by the situation. It is the concept of the rhetorical situation that Margaret Kantz (1990) invokes in her article “Helping Students Use Textual Sources Persuasively,” where she encourages students to learn to read sources “as telling not the eternal truth but a truth to a particular audience” in order to solve some problems. Students, Kantz suggests, should look closely at the author’s motives for producing the document by asking “why?”, “how?” and “so what?” about the author’s initial question. In addition to echoing Kantz’s advice [and you-name-it versions of rhetorical reading], I suggest two other strategies to put a human face on peer review practices:

a) Explicitly link questions during student peer review of work-in-progress to discussions of peer-reviewed sources. Heighten students’ awareness of similarities and differences between their peer review activity and the activity of various professional peer reviewers.

b) Show students a peer-review flow chart for a given journal, such as the one in Appendix 2. Who are these reviewers? What do they know and expect? For example, would the editor-in-chief of a journal about bioengineering share the same assumptions
about the role of nature in engineering as the editor-in-chief for a journal about civil engineering? (Probably not.) What about other sources- do newspapers have editors? Do they have boards of reviewers? Do textbooks? Does Wikipedia? Who judges a given document with what motivation? With this activity, then, we might discuss how mediated most information is. Data rarely speak for themselves, unmediated by (sometimes interested) parties. Is this a problem? When would it be? When is it not the case? All of these questions point to the importance of considering suitability, not just accuracy of sources.

Stage 3: From Rhetorical Situation to Rhetorical Ecology

In “Made Not Only in Words: Composition in a New Key,” Kathleen Yancey (2004) enjoined us to think about the implications of this digital age for the teaching of writing. As she, Charlie Lowe (2004), Sarah Sutton, and Denise Landry-Hyde (2007) and others have suggested, writing teachers could do more to cultivate a critical awareness of authorship, of the boundaries of the classroom, of access and copyright, and of rhetorical invention. We add to this list rhetorical reading of all sources, but particularly of factual sources, in their dynamic, shifting rhetorical ecologies.

Edbauer Rice argues, and we maintain as well, Bitzer’s term “rhetorical situation” fails to describe the dynamic, shifting, looping dimension of literate activity and the emergence of knowledge accepted as factual. To consider changes in the digital culture and its implications for readers, writers, and information, students might read and discuss articles debating the merits of Wikipedia, blogs, and so forth. Consider, for example, “The Hive,” (Poe, 2006) Marshall Poe’s analysis of Wikipedia in The Atlantic Monthly.

Ultimately, I encourage students to develop their own roles as selective forces in the digital age, to heighten their own critical judgment when other complicated vetting processes are shifting, and to have a keen awareness of the value and limitations of formally-peer-reviewed literature and of rhetorical explanations for variation. I encourage students to judge not only the source (How responsibly did the authors support their claim?) but its suitability for the student’s
needs (How responsibly can this source be used for my purpose in this situation?) When might a newspaper be as useful as a peer-reviewed technical article? What are the limits of highly accurate, but narrowly focused articles? When are alternative perspectives valuable and under what conditions?

Conclusion

I want to foster a greater appreciation of peer review as an important selective pressure without denying its variability, much of which can be explained rhetorically. Viewed rhetorically, peer review is a nearly broken system that cannot be trusted to consistently, impartially, and accurately certify “new knowledge.” Viewed rhetorically, peer review is a socially-saturated human activity in which some disagreement is inevitable. Viewed as a rhetorical ecology, peer review is understood as dynamic, shifting, multi-layered set of activities that permeate a discipline with many pre-and post-publication dimensions. While an open-ended system of critique is vital to research, that critique never was limited only to the assessment of a given journal’s editorial board. Even traditional, print-based peer review activity will be more responsible if reviewers understand the potential and limitations of their judgment. The challenge for us as writing teachers, then, is to provide students with sufficient scaffolding to risk making more criteria-based judgments, even if no judgment can be guaranteed for all times.

In closing, consider the words of several journal editors. As Daniel tells us, the editor of Science once asked, “But why peer review? Why not an objective, all-knowing, all-wise genius to serve as an editor?” And he then replied, “Such mortals do not exist. It is essential to divide the task of evaluation and to bring expertise to bear on the various papers that are submitted.”

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Concluding that “The general experience of many editors is that review leads to improvement of nearly every manuscript” (as cited in Daniel, 1993, p. 75). A similar comment comes from David Lazarus (1982), former editor-in-chief of the American Physical Society, which publishes the Physical Review, Physical Review Letters, and Review of Modern Physics:

“I am neither surprised nor dismayed at [research findings about disagreements among peer reviewers]… In only about 10-15% of cases do referees [in physics] agree on
acceptance or rejection the first time around— and this with the authors’ and institutional identities known!... But so what?... Good science is not and never has been ‘objective,’ except to those who have never practiced it” (Lazarus, 1982, p. 219).

In other words, Lazarus is amazed that anyone would not recognize the rhetorical nature of peer review. We will do more to protect peer review— to ensure that it evolves in ways that serve knowledge-making well— if we help the public understand that it is a human activity saturated with socio-historic variables— in short, that it’s rhetorical and will continue to change in new research ecologies.

**Works Cited**


*About the author*

What does writing have to do with discovery? How do scientists and engineers, among others, use language to discover, justify, communicate, and document their claims? Martha D. Patton, an associate professor of English at the University of Missouri, explores these questions, both in her teaching and in her research. She is deeply involved in interdisciplinary collaborations, particularly with faculty in STEM disciplines. Other interests include women and gender studies, second language learning, genre studies, and the teaching of writing.
Notes

1 Harvard announced its support of open access (OPEN ACCESS) publication in February 2008. But what is OPEN ACCESS? Catriona J. MacCallum, senior editor at PLoS Biology, writes “as open access grows in prominence, so too has confusion about what open access means, particularly with regard to unrestricted use of content—which true open access allows.” http://biology.plosjournals.org/perlserv/?request=get-document&doi=10.1371/journal.pbio.0050285. According to Wikipedia, OPEN ACCESS is “free, immediate, permanent, full-text, online access, for any user, web-wide, to digital scientific and scholarly material, primarily research articles published in peer-reviewed journals.” http://en.wikipedia.org/wiki/Open_access That is, OPEN ACCESS is a mode of publishing, not a kind of software. Not to be confused with open access is open source, a kind of software. Again, as Wikipedia says: “Open source software (OSS) began as a marketing campaign for free software. [1] OSS can be defined as computer software for which the human-readable source code is made available under a copyright license (or arrangement such as the public domain) that meets the Open Source Definition.” http://en.wikipedia.org/wiki/Open_source_software

2 UK journalist Poynder says “The argument that open access threatens peer review is most often made by scientific publishers. They do so, argue open access advocates, not out of any genuine concern . . .their real motives, critics add, is simply to protect the substantial profits they make from scientific publishing.” http://poynder.blogspot.com/2006/10/open-access-death-knell-for. Also, consider this from a Wikipedia entry: “Opposition to open access has largely been from commercial publishers, whose business model depends upon providing access to research only to those who will pay for subscriptions” http://en.wikipedia.org/wiki/Open_access. I use the term “mega-publisher” to refer to for-profit publishing conglomerates such as Elsevier, Wiley, and Thomson that have bought and consolidated smaller presses. The following quote from Information Today focuses on Elsevier: “In a development dubbed ‘one of the unlikeliest mergers of the year’ by analysts, Anglo-Dutch publisher Reed Elsevier (owner of LEXIS-NEXIS) and Dutch rival Wolters Kluwer have agreed to a $32 billion merger. The proposed merger would create the world's largest scientific publishing and information group, with combined sales that last year would have reached $8 billion.” http://www.highbeam.com/doc/1G1-19952075.html

3 In Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science, Charles Bazerman calls attention to the role of communal validation in the development of scientific knowledge. First, Bazerman says, the scientific report was delivered orally to a literal community of witnesses, the assembled body of members of the Royal Society; subsequently, a written report was made to a virtual community of peers who read the report. “The report of the experiment is little more than a news report that such an event took place and was witnessed by the assembled body. The validity of events rests on the communal witness and not the story told.” (75) While Bazerman is accounting for the origin of the experimental article, the process of communal validation applies to most forms of research and scholarship, not just scientific discoveries.

4 In Memory Practices in the Sciences, Geoffrey C. Bowker examines the way knowledge has been recorded in a collective memory. On page twenty, he credits James Walsh and Gerardo Ungson with describing five storage bins for organizational memory, one of which is “ecology.” A memory is not discrete; it has traces and functions within an environment. Similarly, in “Unframing Models of Public Distribution: From Rhetorical Situation to Rhetorical Ecologies,” Jenny Edbauer Rice argues that a rhetorical situation is not “a totality of discrete elements,” but is a dynamic flux of social processes where the “elements of rhetorical situation simply bleed.” She goes on to say that “rather than primarily speaking of rhetoric through the terministic lens of conglomerate elements, I look towards a framework of affective ecologies that recontextualizes theorizing public rhetoric’s (and rhetoric’s publicness)” as a circulating ecology of effects, enactments, and events by shifting the lines of focus from rhetorical situation to rhetorical ecologies. Like Biesecker, Phelps, and Warner, I want to add the dimensions of history and movement (back) into our visions/versions of rhetoric’s public situations . . .this ecological model allows us to more fully theorize rhetoric as a public(s) creation.”

5 One proponent of Darwin’s evolutionary theory was Ernst Haeckel, a scientist who coined the term “ecology” in 1866 and who speculated about knowledge in evolutionary terms. Others who applied either ecology or evolution to theories of knowledge include Karl Popper, who, according to Donald T. Campbell, attempted “to apply the whole of evolution to epistemology,” especially in Of Clouds and Clocks (419). Other evolutionary epistemologists include Ernst Mach, Henri Poincare, John Dewey, and several generations of pragmatists, most of whom believe
that knowledge evolves via some process of selection or in some court of reason such as peer review. Stephen Toulmin’s evolutionary account of argumentation straddles the rhetorical and the Darwinian, an account that would square with Edbauer’s phrase “rhetorical ecology.” In Human Understanding: The Collective Use and Evolution of Concepts, Toulmin argues that knowledge evolves via a dual process of innovation (which creates a pool of variants) and selection (including but not limited to peer review).

6 In Writing Selves/Writing Societies: Research from Activity Perspectives, editors David Russell and Charles Bazerman define writing as activity functioning within a dynamic, interactive socio-historic context, sedimented with layers of other activity. Paul Prior, too, in Writing/Disciplinarity: A Sociohistoric Account of Literate Activity in the Academy defines writing as activity functioning in a similar heterogenous, dynamic, socio-historic context.

7 Erving Goffman is a Canadian sociologist who published Asylums: Essays on the Social Situation of Mental Patients and Other Inmates in the same year that Michel Foucault wrote The History of Madness in the Classical Age (1961); both critiqued institutional classification systems. When comparing the works of Goffman and Foucault, Ian Hacking notes that both were both looking at classification systems in mental institutions, but their analyses are relevant to classification systems in academic institutions, for both involve “looping.” Hacking, too, explores classification in academic institutions and describes his project this way: “It was about interactions between classifications of people and the people classified. But also about the ways in which those who are classified, and who are altered by being classified, also change in ways that causes systems of classification to be modified in turn. I call this the looping effect of classifying human beings. Classifying changes people, but the changed people cause classifications themselves to be redrawn” (Hacking 2004: 279). This is similar to Pierre Bourdieu’s sense of habitus—Bourdieu, like Hacking, was influenced by Goffman.

8 On page 30 of their 2008 study of stakeholders in the digital age, Ross Housewright and Roger Schonfeld document the invisibility of the library. With faculty survey data to support the claim, they conclude that “an important lesson is that the library is in many ways falling off the radar screens of faculty.”

9 http://www.ethnography.com/2008/07/when-is-peer-review-the-gold-standard-and-when-is-it-only-tin/

10 Also fueling sociological inquiry into the nature of science was John Ziman’s 1965 book Public Knowledge: An Essay Concerning the Social Dimension of Science.

11 This disjunction is something already well established in technical communication, particularly by Dorothy Winsor in Writing Like An Engineer: A Rhetorical Education. Winsor’s research focuses on disjunctions between rhetorical practices and a-rhetorical expectations among engineers. Summer Smith Taylor and Martha D. Patton build on this observation in “Ten Engineers Reading : Disjunctions between Preference and Practice in Civil Engineering Faculty Responses.” There the disjunction observed is in faculty review of student work, not in faculty review of their peers’ work. Earlier, Cheryl Geisler observed similar discrepancies in the practices of philosophers, who taught one way (matter-of-factly, hiding the open areas of dispute) and reasoned and wrote in another (engaging in open-ended arguments). This disjunction can be observed, too, in some of the empirical critique of peer review where variability is overly-associated with imperfection.

12 For the Thomson survey results, see Testa www.im.microbios.org. To build on these results and existing research in other fields and to get some idea of contemporary peer review practices in engineering, we conducted two surveys, and the results of those two surveys will be spliced in with a discussion of other studies of peer review. In our study of peer review in engineering, we focused on both faculty and editor perspectives. First, to get a faculty perspective, we emailed engineering colleagues at our institution and invited them to participate in an IRB-approved, web-based survey of their own practices. Eighteen individuals responded to the ten-question survey, and many offered comments in the open-ended text boxes that accompanied each question. Even if the sample was small and statistically insignificant, respondents provided a range of responses about their experiences reviewing and being reviewed. Some of these individuals agreed to participate in follow up interviews. Secondly, to look at the editor’s perspective of peer review, we emailed scores of editors of engineering journals and invited them to participate in another IRB-approved, web-based survey of peer review practices associated with their journal. Thirty-eight editors responded and, again, many added comments in open-ended text boxes. If anything surprised us in the results of our own survey data, it was that our technically-savvy colleagues in engineering were not
necessarily aware of or affected by some changes in digital culture. Given the digital revolution, given changes in publication practices in physics, chemistry, medicine, and the life sciences, and given the technical orientation of engineers, we were somewhat surprised that a number of the engineering editors we surveyed were unfamiliar with open access: 23%. This disjunction between expectation and practice is reinforced by a research culture dominated by the values of “big science,” a research culture that does little to reward the teaching of and research about writing, a research culture that does not know what to make of “soft” disciplines or inquiries into social practices.

13 Stevan Harnad still very much defends peer review to protect readers from the “raw sludge that first makes its way to the editor’s desk”—see his comment on Poynder’s blog http://poynder.blogspot.com/2006/10/open-access-death-knell-for-peer-review

14 See the University of California-Berkeley: http://www.lib.berkeley.edu/instruct/guides/evaluation.html the University of Louisville, http://louisville.edu/library/infoliteracy/critical-evaluation-of-resources.html which has adapted with permission the library site from the University of Oregon, http://libweb.uoregon.edu/guides/findarticles/credibility.html]
Appendix 1

Chronology of some recent changes in digital culture

1991 – World Wide Web (Note: “Although the basic applications and guidelines that make the Internet possible had existed for almost a decade, the network did not gain a public face until the 1990s.” [http://en.wikipedia.org/wiki/Internet#History](http://en.wikipedia.org/wiki/Internet#History) )
1991 – arXiv.org – first free online archive, pre-print physics repository at Cornell
1994 – Open Access self-archiving first formally proposed by Stevan Harnad
1997 – Medline – made available as PubMed (most comprehensive index to medical literature)
1998 – American Scientist Open Access Forum – leading to formation of one of first Open Access medical journals
1999 - Open Archives Initiative – protocol for meta-database harvesting
1999 - *British Medical Journal* experiments with open peer review
2000 – BioMed Central – a for-profit Open Access publisher (over 170 journals)
2001 – Creative Commons
2001 – Budapest Open Access Initiative Conference – hosted by OSI in December
2002 – Budapest Open Access Initiative Statement – signed by many scholars in February (establishes community standards, not copyright law, to enforce attribution)
2003 – PloS (Public Library of Science) launched – triggered explosion of Open Access journals
2003 – Bethesda Statement on Open Access Publishing
2003 – Berlin Declaration of Open Access to Knowledge in Science & Humanities (Oct)
2006 – 5 of 7 Research Councils UK adopted Open Access mandates
2006 – *Nature* experiments with open peer review (not very successfully)
2006 – *Philica* UK – features anonymous post-review
2006 – Lund Univ Library – hosts Directory of Open Access journals – lists 2800 +
2008 – Harvard Open Access repository announced; *Harvard Crimson* story (Feb)
2008 – NIH requires all peer-reviewed work arising from NIH funds to be included in PubMed Central (May)

Definition of rating metrics used as a proxy for importance of a document

<table>
<thead>
<tr>
<th>Metric</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Citation Analysis</strong></td>
<td>metric used to determine impact of an article or journal based on frequency and pattern of citations</td>
</tr>
<tr>
<td><strong>Google PR (PageRank)</strong></td>
<td>metric to determine impact of hyperlinked sets of documents</td>
</tr>
<tr>
<td><strong>H-index</strong></td>
<td>measure of actual productivity and apparent impact</td>
</tr>
<tr>
<td><strong>Impact Factor (ISI)</strong></td>
<td>the number of citations the articles in a journal receive in a given year divided by the number of articles published</td>
</tr>
<tr>
<td><strong>Y-factor</strong></td>
<td>combination of both the ISI IF and the weighted PageRank</td>
</tr>
</tbody>
</table>
Appendix 2 “Path to Publication” slide (William Likos, “Universal Recipe”)

[Diagram of the Path to Publication process]

The Path to Publication

- Editor-in-Chief
- Ombudsman
- Editor #1
- Editor #2
- Editor #3
- Editor #4

Editorial Board (45 Members)

- EBM
- EBM
- EBM
- EBM
- EBM
- EBM
- EBM
- EBM
- EBM

Reviewer #1
Reviewer #2
Reviewer #3

1) Accept
2) Revise
3) Decline

Time Line: 0.5 – 3 years

Author

revised paper

2/3 majority