

The forum: Peer review as the enforcement of disciplinary orthodoxy

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*****Note: Figures may be missing from this format of the document**

Recently Omar Swartz (1997) solicited further discussion regarding Blair, Brown, and Baxter's article "Disciplining the Feminine" that appeared in the *Quarterly Journal of Speech* three years ago. I remember reading Blair, Brown, and Baxter's article with exultation. Finally, well-established scholars openly discussed the unstated ideological foundations-in this case, the "male paradigm" (Blair, Brown, and Baxter, 1994, p. 389-395)-underlying two hallowed institutions: the standards of scholarly achievement and the practice of peer review. The authors used two artifacts to show how disciplinary boundaries are established and maintained: Hickson et al.'s (1992) report on research productivity of female scholars in communication and the reviewers' comments regarding an earlier version of Blair, Brown, and Baxter's article. Although Blair, Brown, and Baxter have called attention to two scholarly practices (measurements of scholarly productivity and peer review), I concentrate on peer review because it serves as the primary mechanism for authorizing what counts as legitimate research. Scholarly research that has passed the gauntlet of peer review, therefore, appears in publications perhaps less to convey new information than to declare that such research carries the seal of approval from academic gatekeepers (Crane, 1972, p. 122). Usually the values of information and certification do not conflict. Problems arise, however, when innovative research is significant because it violates disciplinary norms and expectations.

Blair, Brown, and Baxter's comments on peer review might be dismissed as a complaint from disgruntled authors. Quite the contrary: their analysis indicates that the reviews of their work point to systemic problems in the evaluation of scholarly research. From the standpoint of self-interest, reviewers may be predisposed to evaluate scholarship in their specialties negatively. Since "the reviewers who are best equipped to evaluate a study are also likely to be competitors of the authors" (Relman and Angell, 1989, p. 828), scholarship that may threaten a reviewer's status could receive an unduly harsh evaluation. Furthermore, extant scholars in a specialty benefit from having fewer competitors in their area, a situation that makes reviewers reluctant to add to the ranks of published authors (Bach et al., 1996). What Blair, Brown, and Baxter describe goes beyond sheer selfishness or fear of competition. Far from idiosyncratic reactions to the particular research project, the reviewers' comments articulate ideological commitments firmly rooted in androcentric scientism. By daring to expose the ideological underpinnings of how communication research by women is evaluated, Blair, Brown, and Baxter went beyond challenging specific research. Their apostasy was far more radical because it suggested that what counted as acceptable research bore some connection to gendered expectations of appropriateness. This assault on utterly neutral and neutered scientific standards of scholarly merit was met with vigorous reassertions that the field of communication is progressive, enlightened, and essentially unassailable in its treatment of women and scholarship. The defensiveness of the reviewers, while not excusable, is understandable, as Mahoney (1982) notes in discussing criticisms of peer review: "When tacit authority structures are first identified and scrutinized, the initial response is rarely one of welcome" (p. 37).

CHATTER AMID SILENCE

Despite the hope that their "essay will evoke additional discussion" (p. 403) of the ideological underpinnings of peer review and of standards for disciplinary advancement, Blair, Brown, and Baxter's (1994) effort was met with silence in scholarly journals. With the burgeoning of electronic communication, however, discussions that

have been silenced or muted in scholarly journals emerge in other forms. While the communication journals greeted Blair, Brown, and Baxter's critique of scholarly standards with utter silence, discourse flourished on CRTNET, the Communication Research and Theory listserver. This relegation of discussion to an electronic forum might exemplify the democratizing effect of electronic communication (Shank and Cunningham, 1996). Since electronic discussions permit far more rapid feedback and multiply the interactions among participants compared to print journals, ideas are less likely to be filtered out by intellectual gatekeepers. Proponents of electronic scholarly journals contend that "electronic media are less hierarchical and, therefore, more threatening to some of our colleagues" (Cherry, 1996), presumably those who want scholarship to appear in more permanent, declarative (rather than discursive) form.

Electronic communication seems to be developing into a refuge for discussions that have been squelched in traditional print media. Scientist Gilbert Ling, a harsh critic of the peer review process, found that his objections to peer review were ignored by former National Institutes of Health director Donald Frederickson. Since the public hearings in which Ling first testified ended in 1976, Ling (n.d.) has taken his case to the Internet, producing a large set of hypertext-linked documents that indict peer review. During the past year, when Social Text published a bogus article by Alan Sokal, Stanley Fish wrote an article in the New York Times excoriating Sokal for abusing the faith of the journal's editors. The newspaper did not allow Sokal to reply to Fish's criticisms. Finding the print medium unfriendly to his message, Sokal took his case to the internet (Brookey, 1996).

While computer-mediated scholarly discussions might democratize discursive participation, such communication has yet to make inroads on the monopoly that traditional print journals have on certifying what counts as legitimate scholarship. Unfortunately the computer-mediated discussion of Blair, Brown, and Baxter has become an alternative to dialogue in the traditional print media. Perhaps when computer-mediated communication is more fully integrated into academic life, it will act as a springboard for conversations in scholarly journals. As things stand, electronic communication substitutes for journal articles when the two media should be mutually enriching. The computer-mediated discussions have not made their way to the journals, the primary organs for certifying scholarship as acceptable within a scholarly discipline.

THE SILENT TREATMENT AS PUNISHMENT

One of the most traditional, effective, and yet rarely discussed disciplinary practices in academe is the "silent treatment." I employ this label intentionally to recall its use as a punishment for wayward children who violate familial protocols. To a child seeking attention from a parent or other authority figure, the most severe punishment would be not to have the authority figure recognize the child's behavior as worthy of notice. In academic circles, the silent treatment can be almost as devastating.

The worst fate an author can suffer is to be ignored. W. E. B. Du Bois eloquently expressed his fear of obscurity in the afterthought to *The Souls of Black Folk*: "Hear my cry, O God the Reader; vouchsafe that this my book fall not still-born into the world wilderness" (1903/1995, p. 278). As Diana Crane (1972) explained in *Invisible Colleges*, academic influence can be traced and quantified by unraveling the trails of source citations. An author's work earns a place as central in a discipline the more that other authors refer to it. Conversely, the lower the quantity of citations a work generates, the lower its quality is judged because the author's peers have deemed it unworthy of acknowledgment. By not citing an article, other members of academic disciplines not only relegate the article to obscurity but also protect the discipline from potentially subversive literature. Scholarship that threatens hallowed traditions of a discipline is discredited not by rebuttal but by disappearance.

The [non-]reaction generated by Blair, Brown, and Baxter's article is disappointing but hardly surprising. Swartz (1997) attributes the lack of attention to distaste for critics who dared to engage in "a disciplinary critique that involved specific material practices" (p. 253). He continues by objecting to the marginalization of "writers who embrace a Marxist epistemology" (Swartz, 1997, p. 255). Unlike Swartz, I believe the threat Blair,

Brown, and Baxter's essay posed to the field stems less from the feminism they espoused than from their choice of institutional traditions to critique: peer review and the criteria for determining scholarly productivity.

I fear, however, that any discussion of Blair, Brown, and Baxter's efforts must acknowledge that these scholars have been disciplined quite successfully for their transgressive article. It is disappointing that Professor Swartz has had to beg for extensions of and replies to Blair, Brown, and Baxter's article. It is odd that the responses to an article published nationally should appear in a regional journal, regardless of how prestigious that regional journal may be. It is ironic that a field that prides itself on self-reflection and analysis of virtually any form of communication, a field that touts and celebrates diversity, should be reticent to critique its own practices.

The silent treatment, while less obvious than direct rebuttal, is more efficient in its expulsion of purported deviance. Even if critics vilify a scholarly work, at least the opus is deemed worthy of notice. The silent treatment, however, does not permit acknowledgment of the ignored author as a peer. Instead, the victim of the silent treatment has status only as a non-entity unworthy of attention by more conformist scholars.

DEMYSTIFICATION OF PEER REVIEW

Although a great deal of discussion has been devoted to how scholars and practitioners are socialized into their fields (e.g., Ziman, 1968), much less scrutiny has been devoted to the exclusionary practices academics employ to maintain the status quo in their disciplines. One exclusionary practice that remains somewhat mysterious is peer review, the mechanism considered essential for establishing what counts as legitimate research (Ziman, 1968, pp. 111-116). Blair, Brown, and Baxter's critique of peer review helps to demystify the methods a scholarly discipline uses for self-maintenance. As long as institutions, groups, or individuals stifle discussion about a practice, the practice itself remains naturalized, "the way things are." This refusal to subject practices to scrutiny underlies the familiar situation of bureaucratic inertia, where inefficient or downright harmful practices continue simply because "it's always been done that way." Kafka's fiction, especially works such as *The Trial* and *The Castle*, depicts the ultimate reductio of mystified bureaucratic practice. No actions make sense to people outside the bureaucratic structure because the practices themselves never have been questioned. If challenges to a practice are not permitted, then the practice itself becomes unjustifiable because even its supporters eventually may lose the capacity to defend institutionalized normality.

By bringing peer review into a forum that permits public scrutiny, Blair, Brown, and Baxter remind readers that disciplinary norms are contingent choices. Bach et al. (1996) point out that scholars should avoid naturalizing disciplinary practices, reiterating the reminder "that our current norms are, at least in part, matters of collective choice, not necessity" (p. 415). If reviewers understand that they articulate scholarly customs and preferences, they can remain the custodians of disciplinary traditions without suffering the myopia (and patronizing attitude) attendant to believing they voice timeless truths. As Adrienne Rich (1979) observes in the context of patriarchal practices, silence encourages unthinking continuance of the status quo. Silence in the face of objectionable academic practices therefore makes those who are silent complicitous with the oppressors. It is not surprising that peer review has escaped the scholarly spotlight. Relman and Angell (1989) note that since "peer review has traditionally been regarded as a confidential transaction, it has received little outside attention and even less systematic study" (p. 828). Gradually, the conspiracy of silence surrounding peer review might be changing.

Blair, Brown, and Baxter are not the first scholars to conduct an extensive case study of flaws in peer review. Biomedical researchers and psychologists, for example, have devoted much attention to peer review. In 1975, Congressional hearings were conducted on the merits and drawbacks of peer review as a means of selecting grant recipients in the sciences. In 1989, the American Medical Association convened the First International Congress on Peer Review (Relman and Angell, 1989). In 1993, Harcum and Rosen published a book-length study that harshly criticized a series of reviewers' comments on the authors' manuscripts submitted to journals in psychology. A quick search of the biomedical and psychological literature on the subject will reveal dozens of articles published in major journals during the past decade. These studies and others point to growing concerns about how well the peer review system operates.

PEER REVIEW AS A FILTER

Blair, Brown, and Baxter's concern about the reviews their scholarship received raises a larger issue about peer review per se. If peer review were to function optimally, it would act as a filter for scholarship. Methodologically sound research on significant intellectual issues would be published, while inferior work would be filtered out of scholarly journals. Peer review has earned plaudits for its ability to siphon out research that does not meet the minimum standards for acceptable scholarship. Indeed, one of the primary lessons scholars have drawn from the notorious cold fusion debacle is that peer review plays an indispensable role in quality control of research (Gross, 1995; Taubes, 1993). Supposedly, any circumvention of the peer review process drastically increases the chances that shoddy research will be accepted in a field because it has not been vetted by qualified experts before public dissemination. While cold fusion provides an example that could vindicate peer review, it assumes that reviewers can and do prevent poor research from gaining public notice. That supposition is not always warranted.

Beveridge (1957), in his classic primer for scientific researchers, warns against premature publication. Two assumptions undergird Beveridge's advice. First, researchers should err on the side of caution, avoiding grandiose claims that might make findings seem more significant than they are. Second, the review process that is designed to weed out poor research sometimes fails, publicly displaying shoddy material for all to see. The best and the worst of the peer review process emerge simultaneously. On one hand, scholars should conduct their research carefully so it will meet the standards of the reviewers. On the other hand, the reviewers might not recognize and filter out poor research before it reaches the pages of scholarly journals.

In a controversial study that generated an entire book of commentary, Peters and Ceci (1982) resubmitted articles that had been published in mainstream psychological journals, changing only the names and institutional affiliations of the authors. Of the twelve resubmitted manuscripts, nine were not detected as having been published previously. Furthermore, Peters and Ceci (1982) found that reviewers were more favorably disposed to submissions from authors with high-prestige institutional affiliations. Setting aside questions of bias regarding institutional status, Peters and Ceci (1982) demonstrated that reviewers do not always live up to their charge of being discerning intellectual gatekeepers. Some commentators remarked that the results were not entirely surprising. Manuscript reviewing is time-consuming and usually carries no compensation. Reviewers often are chosen more because they are willing to do the job than because they are the leading researchers in an area of scholarship (Scarr, 1982). As a result, the peer review process and the quality of scholarship can suffer.

MARGINALIZATION OF INNOVATION

As Swartz (1997) observes, the insulting reviews Blair, Brown, and Baxter received demonstrate the disfavor that innovative thinking can incur. A common criticism leveled against peer review is that it rewards deference to authority but punishes challenges to accepted scholarly traditions. Critics of peer review blame the practice for stifling innovative thought by channeling research along already well-trodden paths approved by the senior researchers who serve as gatekeepers for publications and grants (Lovelock, 1991). An editor of two medical journals considered the "most disastrous" aspect of peer review to be "its built-in bias against highly innovative work" (Horrobin, 1982, p. 33). This conservatism generates a vicious circle. Authors, recognizing that deference to authority increases the likelihood of publication, rarely submit innovative work since they fear its rejection (Scarr, 1982). Paradoxically, the senior scholars least likely to suffer from taking intellectual risks are the most socialized into academe and therefore most likely to uphold and defend tradition. As Berger and Luckmann (1966) point out, one mark of a defined social organization—in this case, academics in a particular field of study—is its emphasis on traditionalism. The more one is socialized into an intellectual community, the less one may question its traditions. Those who seek to be socialized into that community, therefore, are expected to defer to the community's established authorities and practices.

Although their study did not investigate ideological issues, Harcum and Rosen (1993) concluded that problems in peer review were systemic because the process encourages employment of a heuristic that insulates authority

against challenges, even when those challenges have merit. Several elements of the heuristic guiding manuscript disposition bear special relevance to Blair, Brown, and Baxter's experience (Harcum and Rosen, 1993, p. 3):

- * "Familiar theories are superior."
- * "Traditional methods are better than novel ones."
- * "The evaluator is more knowledgeable than the author."

The first heuristic principle would, as Swartz (1997) indicates, predispose reviewers to reject manuscripts that espouse potentially subversive doctrines or methods. The second principle would marshal resistance against research that criticizes customary practices in the field. This heuristic probably explains the defensiveness of the reviewers in the face of challenges to how scholarly productivity should be measured. The final heuristic accounts for the imperious tone that Harcum and Rosen label the "Evaluator Delusion" (1993, pp. 7-8). Had the reviewers of Blair, Brown, and Baxter's work approached their task with the attitude that every manuscript has the potential to reveal some knowledge, the criticisms might have focused more on substantive theoretical and procedural issues.

The efforts of Blair, Brown, and Baxter suggest that academics apply the same criterion of opacity to peer review as to scholarly research. One obligation researchers assume is to make their findings opaque to other researchers. In other words, the methods and reasons that led to the conclusions should be revealed fully to permit testing and contestation by other scholars. Blair, Brown, and Baxter have revealed how peer review functions more as a proclamation of authority and a dogmatic assertion of disciplinary bounds. The obligation of opacity requires researchers to justify why and how they reached their conclusions. Reviewers expect authors to justify and document their claims, so why not extend the same standards to include the reviewers themselves (DeBakey, 1982)? Holding reviewers to the same scholarly expectations as authors perhaps would encourage more substantive and temperate reviews. Peer review, as a practice that circumscribes what counts as "legitimate" research, can serve as a forum for declarations of deviance/conformity moreso than revelations of how those judgments were reached. The quotes from referees that appeared in Blair, Brown, and Baxter's article clearly demonstrate that the reviewers recognized their role as spokespersons for the field of communication. Since the authors' critique of purported scholarly achievement was deemed apostate, the reviewers understandably (but not excusably) became defensive in the face of what they must have considered an attack on the field's criteria for determining merit.

LEGITIMIZING THE ILLEGITIMATE

With all the concern about stifling innovation, it is easy to overlook the other problem that peer review should avoid: embracing trendy scholarship at any cost. This eagerness to accept and endorse whatever "flattered the editors' ideological preconceptions" (Sokal, 1996b, p. 62) apparently accounts for the results of physicist Alan Sokal's notorious ruse on the editors of *Social Text*. Sokal (1996a) submitted an article to *Social Text* that couched blatant factual errors and sheer nonsense in the jargon of popular postmodern and poststructural theorists. The editors of *Social Text* published the article, not realizing that it was Sokal's attempt to see whether "a leading North American journal of cultural studies" (Sokal, 1996b, p. 62) would publish something just because it was saturated with fashionable lingo.

One could respond to the Sokal incident by reaffirming the positivistic insistence that the truth value of research remain independent from its conformity to intellectual authority figures. Weinberg (1996) does just that, separating the intellectual hegemony of a discipline's practitioners-i.e., their unambiguous criteria for truth and falsity from social hegemony, the reliance on authority and other factors unrelated to the research itself. Although this bifurcation would exclude from "legitimate" scholarship the type of insulting reviews Blair, Brown, and Baxter received, it fails to account for actual intellectual practice. The criteria for what counts as valuable research extend beyond considerations of truth value, especially when the research is speculative. Peer

review, in fact, serves as one means of establishing the standards for research that will be authorized as worthy of scholarly attention. Certification of research as having run the gauntlet of peer review involves a "cognitive-rhetorical" (Mahoney, 1982, p. 36) process that is supposed to combine evaluation of accuracy with judgments of how well the writer presents and argues the position.

Regardless of how one might view the moral side of Sokal's editorial experiment, the incident illustrates the danger of bowing too far in the direction of innovation for innovation's sake. Scholars have become progressively more aware of the stifling effect that peer review has on innovative thought. Research, however, does not count as significant or ground-breaking simply because it involves methods or objects of study that defy scholarly customs. In our eagerness to encourage creative scholarship, let us try to avoid infatuation with novelty.

CONCLUSION

Effective peer review would maintain rigorous standards of scholarly excellence to keep poor research out of public forums. At the same time, reviewers must be open to innovative methods, unconventional topics, and a range of stylistic options. Perhaps one way to balance the demand for rigor with the need for creativity would be to articulate more explicitly the obligations reviewers, authors, and editors assume vis-a-vis each other in the publication process (Baue, 1993). Every field faces the challenge to apply stringent criteria for judging the merits of scholarship while simultaneously maximizing the opportunities for innovation. Maintaining the delicate balance, what Kuhn (1977) called "the essential tension," between disciplinary conventions and creativity remains a central challenge for scholars in all disciplines.

REFERENCES

- Bach, T. E., Blair, C., Nothstine, W. L., and Pym, A. L. (1996). How to read "How to get published." *Communication Quarterly*, 44, 399-422.
- Baue, A. E. (1993). Peer and/or peerless review: Some vagaries of the editorial process. In R. E. Bulger, E. Heitman, and S.J. Reiser (Eds.), *The ethical dimensions of the biological sciences* (pp. 127-133). Cambridge: Cambridge University Press. (originally published 1985).
- Berger, P. L., and Luckmann, T. (1966). *The social construction of reality: A treatise in the sociology of knowledge*. Garden City, NY: Doubleday Anchor.
- Beveridge, W. I. B. (1957). *The art of scientific investigation* (rev. ed.). New York: Norton.
- Blair, C., Brown, J. R., and Baxter, L. A. (1994). Disciplining the feminine. *Quarterly Journal of Speech*, 80, 383-409.
- Brookey, R. A. (1996). Recontextualizing the genetic debate: A response to Condit. *Communication Studies*, 47, 152-156.
- Cherry, S. (1996, 24 June). Re: from EduPage . . . [On-line]. Available http://www.missouri.edu/~rhetnet/peer_review/0002.html.
- Crane, D. (1972). *Invisible colleges: Diffusion of knowledge in scientific communities*. Chicago: University of Chicago Press.
- DeBakey, L. (1982). Authorship and manuscript reviewing: The risk of bias. In Harnad (pp. 24-25). Du Bois, W. E. B. (1995). *The souls of black folk* (R. Kenan, Intro.). New York: Signet. (originally published 1903).
- Gross, A. G. (1995). Renewing Aristotelian theory: The cold fusion controversy as a test case. *Quarterly Journal of Speech*, 81, 48-62.
- Harcum, E. R., and Rosen, E. F. (1993). *The gatekeepers of psychology: Evolution of peer review by case history*. Westport, CT: Praeger.
- Harnad, S. (Ed.). (1982). *Peer commentary on peer review: A case study in scientific quality control*. Cambridge: Cambridge University Press.
- Hickson, M., III, Stacks, D. W., and Amsbary, J. H. (1992). Active prolific female scholars in communication: An analysis of research productivity. *Communication Quarterly*, 40, 350-356.
- Horrobin, D. E. (1982). Peer review: A philosophically faulty concept which is proving disastrous to science. In

Harnad (pp. 33-34).

Kuhn, T. S. (1977). *The essential tension: Selected studies in scientific tradition and change*. Chicago: University of Chicago Press.

Ling, G. (n.d.). Peer review system suppresses innovation and progress [On-line]. Available <http://www.gilbertling.org/lp21.htm>.

Lovelock, J. E. (1991). *Small science*. In J. Brockman (Ed.), *Doing science* (pp. 175-191). New York: Prentice Hall.

Mahoney, M.J. (1982). *Publication, politics, and scientific progress*. In Harnad (pp. 36-37). Peters, D. P., and Ceci, S. J. (1982). *Peer-review practices of psychological journals: The fate of published articles, submitted again*. In Harnad (pp. 3-11).

Relman, A. S., and Angell, M. (1989, 21 Sept.). *How good is peer review?* Letter. *New England Journal of Medicine*, 321, 827-829.

Rich, A. (1979). *On lies, secrets, and silence: Selected prose 1966-1978*. New York: Norton. Scarr, S. (1982).

Anosmic peer review: A rose by another name is evidently not a rose. In Harnad (pp. 53-54). Shank, G., and

Cunningham, D. (1996). *Mediated phosphor dots: Toward a post-Cartesian model of computer-mediated*

communication via the semiotic superhighway. In C. Ess (Ed. and Intro.), *Philosophical perspectives on*

computer-mediated communication (pp. 27-41). Albany: State University of New York Press. Sokal, A. D.

(1996a). *Transgressing the boundaries-toward a transformative hermeneutics of quantum gravity*. *Social Text*, 46/47, 217-252.

Sokal, A. (1996b, May/June). *A physicist experiments with cultural studies*. *Lingua Franca*, 62-64. Swartz, O.

(1997). *Disciplining the "Other": Engaging Blair, Brown, and Baxter*. *Southern Communication Journal*, 62,

253-256.

Taubes, G. (1993). *Bad science: The short life and weird times of cold fusion*. New York: Random House.

Weinberg, S. (1996, 8 August). *Sokal's hoax*. *New York Review of Books*, 11-14. Ziman, J. (1968). *Public*

knowledge: The social dimension of science. Cambridge: Cambridge University Press.