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EDITORIAL: ON EDITING AND BEING AN EDITOR

Editorship comes with many responsibilities, which, from my perspective, frequently are not enacted by journal editors including in the field of science education. When Ken Tobin and I thought about founding a new journal, it was not only to provide the structure for a new kind of scholarship, a new kind of scholarly community with a different set of values concerning writing and publishing research, but also to approach journal editing and journal review processes in new ways. In this editorial, I articulate some of the problems in the current review processes in our field generally, responsibilities that many journal editors do not enact as part of their role in the development of a discipline. When we decided to bring this new journal into being, we explicitly discussed the need to create a new culture of editing and peer review in science education – which we, as others, reviewed and critiqued in a special issue of *Research in Science Education* on the same topic (Roth and Tobin, 2002). I have also written about the vagaries of peer review in funding organizations and articulated theoretical models that allow us to understand how in collective decision making outcomes are achieved – i.e., the evaluation of a research proposal – that are not always fair (or correspond) to the value of the study and its author (Roth, 2002b, 2004). I do not reiterate the points we made collectively or those issues I raised specifically in existing publications, but rather I elaborate on the issue of the potential dangers of peer review and the responsibilities that editors have, responsibilities that we (Ken and I) have vowed to assume when conceiving of this journal.

VAGARIES OF PEER REVIEW

Having been co-, consulting, and associate editor for a number of journals within and outside the field of education, I have been in a position to see who actually wrote reviews and developed a better appreciation for the match between the content of a paper and thereby the competencies of a reviewer. Although the process is called "peer review," there sometimes are tremendous gaps between the levels of experience and expertise of senior-level scholars and junior-level reviewers. Yet many editors indiscriminately add up the scores or recommendations that the reviewers made. This leads to substantial problems concerning a fair and valid assessment of a particular

Cultural Studies of Science Education (2006)
DOI: 10.1007/s11422-006-9024-y

piece of scholarship. As a consequence, peer review becomes a chancy process. Here some examples.

In one instance, a paper had employed a questionnaire including something around 20 items. From a stylistic perspective, the paper was well written. It was not surprising, then, that the two junior-level reviewers, apparently not versed in statistics, recommended it for publication with minor changes based on the fact that it was well written; neither reviewer addressed the issues concerning statistics. The rather well known scholar had analyzed the instrument as a whole, which did not exhibit statistically significant differences between the two populations to be compared. He had then analyzed each item separately at the conventional level of testing for Type I errors (false positives), which, in the social sciences, is $\alpha = 0.05$. Out of the 20 tests, three were statistically significant. The whole point of the paper hinged on the statistical significance of one of these tests. In addition, the items at reliability values between 0.60 and 0.80, values well below acceptable levels in psychology; these values mean that the items did not measure well the construct that they were claimed to measure.

Now, any somewhat statistically versed person knows that when you conduct more than one test, the error rate for the experiment as a whole increases. Thus, with 20 tests conducted, the possibility for a Type I error – i.e., making the statement that there is a difference when in fact there is no difference (false positive) – increases from 5 percent to 65 percent. Because the whole point of the paper hinged on one of the items in particular, there was a substantial risk, therefore, that the paper was based on a *false positive*. To keep the overall (experimentwise) error rate at an accepted and acceptable level, the tests would have had to be conducted at $\alpha < 0.003$; had the author done this, none of his tests would have reached statistical significance, and the whole point of the paper would have been moot. Taken together with the fact that the items did not measure well the intended constructs in the first place, I had serious concerns with the usefulness of the research.

Both reviewers – perhaps because of a lack of statistical expertise – had overlooked this fact and recommended the paper for publication with minor changes. I suggested to the editor that the paper should not be accepted based on the strong possibility that it was based on the result of a test to be a false positive and on an unreliable instrument. If the editor simply "counted beans" (i.e., votes), which frequently happens as I know from my experience in the system, the paper would have been accepted with minor changes. "One 'accept with minor revisions' plus one 'accept with minor revisions' equals 'accept with minor revisions'."

The converse also happens. Reviewers reject a paper that – when read by true experts and therefore true peers – evidently should have been accepted

for publication. In one instance this happened to a paper, which in a very eloquent way substantiated the different interpretations two theories led to when he analyzed episodes from a science class he had taught. The paper was well grounded theoretically and cutting edge from the perspective of cognitive and learning science research; it clearly was relevant to science education, because it evaluated the pros and cons of two theories for knowing and learning science in the context of a particular lesson. The author had done so with materials from his own classroom, which he evidently knew very well.

Yet two of the reviewers rejected it and the third recommended major changes. When I checked the backgrounds of the three reviewers, not a single one had experiences and background relevant and appropriate to the paper; and all were junior. That is, these reviewers had evaluated a paper that they did not have the appropriate competence in evaluating. As the associate editor, I therefore overruled the recommendations and suggested publication with minor change. Rather than accepting my recommendation, the editor actually questioned it despite his lack of experience and competence in the particular field. Only after my repeated protests did he give in and suggested – as a way out of the impasse – to have another reviewer look at it. I strongly recommended using somebody in the field who actually grasped the attendant issues. The editor agreed. The review recommended, "Accept as is." Here, too, the point is not that I was right but that simple vote counting would not have rendered an appropriate judgment, and would have been inconsistent in terms of the judgment of those who were really the expert in the area of the article.

The vagaries of the peer review process are clearly evidenced in the following example from my own experience. Together with my graduate students, I had written an extensive analysis of textbooks and scientific research articles and we developed and proposed a theoretical frame for conducting such analyses. We submitted the article to one of the leading journals in the field of science education. Both reviewers of the article rejected it on a variety of grounds, including the fact that it was dealing with the issues from an anthropological perspective. As a consequence, the editor rejected it. Just at about the time we received the rejection letter, there was a changeover in editorship. Because I was not convinced that the article had received a fair deal, I changed its title and resubmitted the *identical* article to the new editor. This time we received two "accept as is," and the article was published without much delay. "How can it be," I asked at the time, "that the same article receives two 'reject' recommendations in one round and then two 'accept as is' recommendations in another round of the same journal?" Surely, there are problems and contradictions in the system

and somebody within it has to bring about a change. Clearly, seasoned and competent editors immediately would have seen the potential of the article. There is not much change in that journal, though the editors have repeatedly changed since. Some of the reasons are quite apparent to me now.

TOWARD THE *JUDICIOUS* EVALUATION OF REVIEWER COMMENTS

One of the sources for the vagaries is the vote counting procedure, which pretends to guarantee objectivity. But this is nothing more than a smoke screen. In a case that recently came to my attention, a reviewer had torn apart (i.e., harshly critiqued) the article of a new scholar, whose doctoral dissertation was related to inscriptions, an area that I have worked in for nearly fifteen years and therefore know very well. The young scholar grounded his work in some of my articles, including one in *Review of Educational Research*, the journal with the most citations and highest impact ratings in all of education. There already is an extensive literature on the topic of inscriptions in the social studies of science. The review began with a dictionary definition of inscription and then suggested rejection of the article on this ground alone:

> I begin with two dictionary definitions. These are from dictionary.com but similar to entries found in any standard English dictionary. It is quite clear in the abstract that the author is using "inscription" to mean "representation." The author is not being original here but following a path set by others (who are cited). However, the fact that some people have gotten their incorrect use of English published does not make such usage correct. I am aware that languages grow and change as part of natural processes; but if someone wishes to deliberately induce a change, well it needs to make sense. Trading representation for inscription does not make sense. In this manuscript, as well as others I have seen, nothing but confusion is gained by making this trade. On this point alone, the manuscript is not publishable.

The reviewer then goes on, provides all items he had found in a dictionary, suggests that our young scholar did not know the history of science, and so on. It was clear that he was someone from a different paradigm, who rejected the article because of paradigmatic reasons than because of the content of the paper itself. Although the young scholar had thoroughly grounded his work in the pertinent literature, the reviewer suggested that should he have cited an entirely different literature supporting a very different paradigm. Ironically, the reviewer suggested that our young scholar knew nothing of the nature of science, while he himself attempted to argue across paradigms, a futile effort, as we know ever since the publication of *The Structure of*

Scientific Revolutions (Kuhn, 1970). The hatefulness of the review is clearly evident in his comments: "The author refers to Latour and Woolgar (1979) and Latour (1987) as if no further research on the practices of scientists has been done. The author, thus, would be well advised to read something like . . ." followed by a recommendation to read a book that takes a "very different perspective." In closing, the reviewer commented, "In sum, the use of 'inscription' is to make a rigmarole. The author's understanding of the history of science is faulty." Again, a perceptive editor ought to have weeded this review chaff from the review grain and provided our young scholar-author with appropriate recommendations for how to proceed with his article.

Vote counting hardly constitutes good decision-making, phronesis, and therefore is inappropriate for editors who, because of their tremendously important mediating role in the production and reproduction of their field, ought to make deliberate decisions and wise choices. Judicious judgments are especially called for in a culture that too often has reproduced itself by enculturating new (especially new male) scholars who sleighten the work of others. That is, in some science education journals, there is a culture whereby reviewers appear to perceive it as their duty to enact harsh gatekeeper roles. Critiques often are harsh, even personal to such an extent that new scholars in particular are turned off from participating in the scholarly community after a rejection or two. The double-blind peer review process lends itself to such harshness, which I sometimes refer to as "B-52 mentality," meaning that you enact damage to real living people from such a distance that you are not aware of the suffering your attacks bring about. This does not have to be so, and one can easily envision a culture where reviewers frame what they have to say about an article with respect, that is, in such a way that they could have said it to the person in a face-to-face meeting. (I personally sign my reviews, and if my signature does not show up on the materials that the editor provides to authors, it is because they removed it.) As editor, I believe to have the double duty to make sure what the authors get to read is useful without euphemizing any existing problems. As an editor, I am therefore striving to make ours a kinder science education culture, a culture where individuals recognize and concretely realize a collective responsibility to kindness. By kindness I do not mean to say that "anything goes" but that editors enact solidarity with respect to the experience of those whose work is, for one reason or another, inappropriate for publication in a particular journal. Editors *are* in positions that they can change the culture of reviewing by working with reviewers to ascertain they recognize their role and responsibility both to specific authors and the culture as a whole.

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EDITORIAL POWER

Elsewhere I provided theoretically founded, sociological analyses of the processes that lead to the stabilization of the status quo in a field such as science education (Roth, 2005) and the tremendous power wielded by editors, whose gatekeeper role in the peer review process has long-range effects on tenure and promotion, and therefore the production and reproduction of the field in terms of its members (Roth, 2002). (Here, *power* is understood as a middle term rather than as a determinate (causal) factor of editorial processes.) I do not need to reiterate these analyses here. But I want to make salient that editorial decisions are not to be taken lightly, as they do not just affect manuscripts but real people, who have dreams, hopes, and desires. We cannot act in the way warlords, generals, and presidents do, who, in the name of their cause, kill thousands and yet are able to sleep easily, because all those killed are merely (and euphemistically) "collateral damage." When some journals in science education and other fields reject 80 and even up to 95 percent of the manuscripts submitted, we cannot treat those repeatedly rejected and "drop-outs" as collateral damage. Peer reviewers and editors do have an educative function and they have ethico-moral responsibilities to their discipline and its culture. There are collective responsibilities to produce and reproduce solidarity toward the scholarly community, which implies each and everyone in the community, including editors.

EDITORIAL RESPONSIBILITY

An action has at least three elements: performance, intention, and effect (Ricœur, 1992). Each act therefore is complete only when we know what effect it has had, the nature of which we establish from the action (including suffering) of *the other*, the transitive complement of the acting subject. (The concept of *agency* requires further development, as it is one-sided emphasizing individual intentionality at the expense of collective sensibility and responsibility.) Because our actions have effects, and in fact are only completed through the effect they have on another (Bakhtin, 1993), human beings, imbued with agency, inherently are responsible. More so, because the effect our action has on another, which is exhibited in his or her reaction, we are responsible for this response as well. That is, we are not only responsible for our own actions but also for the actions of the other (Levinas, 1998).

In communicating the results of a manuscript evaluation, an editor faces tremendous responsibilities not only toward *this* author but also to the

scholarly community as a whole. An editor's job, therefore, cannot not lie in vote counting and, if the case, constructing the sum total of all reviewers' comments. The editor has to select, judiciously, from the reviews, disregard those that are inappropriate, add where they are limited, and so on. An editor is responsible to the community, as the articles that appear in a journal define the resources readers may use not only in citing existing work, but also in designing their own future studies.

Editors also have a responsibility in the sense that journals are major vehicles for shaping a field. In many journals, this shaping may be in a conservative direction, whereby existing genres, forms of research, discourses, research methods, and theories are reproduced leading to the continual reproduction and maintenance of the status quo. The community begins to ossify when new perspectives are systematically rejected as a part of a conservative review process and the unwillingness of editors to allow new forms of scholarship to emerge. We created this journal as a structure to break open the cycles of reproduction of our field and to provide possibilities for producing the discipline in new ways.

ENACTING RESPONSIBLE EDITORSHIP

Science education, perhaps more so than in other disciplines that study science and the production of scientific knowledge, appears to constitute a conservative culture. The field of science has changed little over the past several decades. I still remember the difficult debates and battles it took to get the science education community in general and the journals in particular to accept (good) qualitative research as a matter of course. In 1992, I was part of a large meeting at the NARST conference discussing the importance of considering and accepting different forms of research and genres; several of us got together to write an editorial articulating what we had come up with in the process (Kyle et al., 1992). The conservative trend in the discipline continues: the peer review process and editorship have contributed to reproducing the status quo and impeding changes to our field. Responsible editorship, that is, an editorship characterized by responsibility toward the generalized other in the field, implies opening our culture toward the future, toward its own development, toward greater solidarity, inherently a collective phenomenon. In my work as an editor who does his part in such a development, I have some guiding examples that mediate my actions.

There are a number of editors that stand out for me; they exemplify a different kind of editorship that are closer to what I want to do as an editor, though not exactly in the same form. During the past decade, Ken Tobin

repeatedly has told me how the former editor of the *European Journal of Science Education* Richard Kempa had worked and, in this style of working, taken a leadership role. When the regional editors or Richard identified an article as being sound, he accepted it *as is* on the grounds that tinkering and addressing all issues reviewers raise can only lead to homogenization and regression toward the mean or median (a term that shares etymological roots with *mediocre*). When editors ask authors to address all reviewers' comments, that is, when reviewers come to meddle too much with content and form of scholars' manuscripts, all articles come to look and read alike. There is little diversity in form or content, because of the pressure to conform to the same conception of what scholarship ought to look like. A received form of editorship, where the author – and especially a junior scholar – has to address nearly all the recommendations, can only lead to a regression to the median and a collective movement toward mediocrity (etymologically, median and mean have the same root). A community that follows the received model of the peer review and editorial process risks slipping into mediocrity.

Another example that stands out is provided by the different individuals that have edited *Semiotica*, a leading journal devoted to semiotic and linguistic issues. The editors make a decision about the potential of an article to contribute to the discipline, and then accept the article as is. The copy editor may recommend changes, but these are designed to make sure all the references are in place and to take out any obviously incorrect use of English and to query difficult to understand sentences.

Editors also have a special responsibility in choosing the appropriate manner and tone when they interact with authors who have submitted a manuscript for consideration. I know several editors who have interacted inappropriately with authors, to the point of writing very aggressive letters denigrating an author in their editorial summaries with feedback to the authors. Comments such as, "I do not want to ever see this paper again" and "I have told you often enough that I do not let you get away writing like this" are simply inappropriate in any editorial letter, let alone in letters from a senior editor to a very junior scholar.

CODA

When Ken and I thought about creating a new journal that focuses on the cultural nature of science, we also talked about enacting a different kind of editorship than that common in our discipline. We do think or believe that reviewers can provide important information and input, but the ultimate responsibility for making decision lies with the editor or, in our case, the

editors. The choice begins with finding appropriate reviewers rather than randomly chosen them, or reviewers chosen such that the article will be rejected. Thus, we do not pretend to have used an objective process if we know that an article is inappropriate or that it does not fit the mission of the journal. We let the author know immediately and recommend submitting the manuscript elsewhere. And we take seriously our responsibility of selecting judiciously even if a reviewer is very familiar with the topic or domain of a particular manuscript, as there always exists the possibility for misunderstanding, misinterpretation, excess, etc. We also intend to be proactive in challenging authors and ourselves to push the boundaries, to open up new literary forms and topics of inquiry rather than insisting on reproducing received forms of research and genres of writing. In the end, my hope is that by taking head-on our responsibilities as editors of *Cultural Studies of Science Education*, we constitute new resources for being who we are collectively and make possible new forms of being-with one another.

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