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# Literate writing and collegial citing

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## Literate writing and collegial citing

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In the present editorial, the Editor-in-Chief of the *Journal* explains the processes by which manuscripts are handled when they are submitted for publication. Various categories of problem papers are described, and it is emphasized that the outcome of the review process is not always predictable. Metrics for measuring paper quality and journal quality are reviewed and discussed. Arguments are given to the effect that the quality of a paper and its chances of being selected for publication will be considerably improved if the authors adopt a philosophy of literate writing and collegial citing. The detailed meaning of these phrases is discussed with accompanying examples, including the case of a paper by J. J. Waterston, the publication of which was delayed for 47 years until the paper was rediscovered by Rayleigh. © 2000 Acoustical Society of America.

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### INTRODUCTION

Although the quality of the papers published in this journal is, by most accounts, regarded as high, it would be even higher if all of the papers exemplified literate writing and collegial citing. Many of the papers do indeed have these qualities, but there are a sufficient number that lack them to prompt this editorial. What is here meant by the two terms, literate writing and collegiate citing, is a priori ambiguous, so an explanation must first be given of the sense in which they are used here.

The history of this journal and of the professional society that sponsors it dictates that its communications be in the English language and, moreover, in the American version of that language. It is a rich language which has proven to be considerably adaptable for the concise expression of complicated ideas. It is not an especially precise language, however, and many individual words have widely divergent meanings. Such is the case with *literate* and *collegial*. Here the term *literate* is understood to mean<sup>1</sup> *showing or marked by an acquaintance with the fundamentals or background of a particular field*. The term *collegial* is understood to mean *of or relating to a group of individuals belonging to the same profession or having similar objectives*. The field of interest here is acoustics, the profession is acoustics or the aggregate of related professions, and the objectives are those stated in the mission statement of the Acoustical Society—to increase and diffuse the knowledge of acoustics and to promote its practical applications.

Thus, in what follows, should the writer refer to someone as an *illiterate author*, such a person is not being identified as someone unable to read and write, but as one whose writing displays either no acquaintance with the literature of acoustics in general or no acquaintance with the prior litera-

ture of the subject treated in that author's paper.

The purposes of this editorial are as follows: (1) to explain why literate writing and collegial citing are essential to a high-quality journal article, (2) to persuade editors and reviewers that the absence of literate writing and collegial citing should be valid and important criticisms of a manuscript, (3) to persuade prospective authors to write their papers so that they do not encounter such criticisms, (4) to suggest to disappointed authors that one of the reasons, although possibly not communicated to them by the editor or the reviewers, for their papers not being published was that they evinced such criticisms, and, of course, (5) to achieve an improvement in the quality of the *Journal*.

### I. THE SELECTION PROCESS

Before dealing with the principal subject matter of this editorial, it seems appropriate to review the process by which papers are selected for publication. The *Journal of the Acoustical Society of America* (JASA) currently has 30 associate editors who receive manuscripts directly from the authors. The right to submit articles is extended to everyone, and with a few exceptions all of the submitted papers are unsolicited. If no authors submitted papers to the *Journal*, it would cease to exist. The sustaining of the *Journal* as a high-quality and sizable publication requires (1) some efforts to encourage potential authors to do good research and to submit quality papers based on that research to this journal and (2) a careful selection as to which of the submitted articles should be published.

The associate editors are volunteers, and they serve without any financial compensation for the extensive amount of time that they have to devote to their editorial tasks. Over the 71 year history of the *Journal* an impressive list of dis-

tinguished acoustics researchers and professionals have served as associate editors. The list of current associate editors can be found on the back cover of this issue or in the "Information for Authors" section of the CD ROM. Principal tasks of the Editor-in-Chief are to select candidates for associate editorship, to persuade them that serving as associate editors is something they should do, and to propose such candidates to the Executive Council for appointment.

There is a strong analogy between the Editorial Board (the set of associate editors) and an academic department in a university. Just as professors have the responsibility for assigning grades in the courses they teach, so do the associate editors have the responsibility for deciding which manuscripts of those they receive are to be published. Just as professors have an academic freedom of deciding just how to teach their courses and of the detailed content of their courses, so do associate editors have the freedom of controlling the detailed process as to how they arrive at their publication decisions. Although literate writing and collegial citing may be regarded as important factors by the Editor-in-Chief, the individual associate editors have the right to regard them as minor or even inconsequential in their decisions as to whether papers are to be selected for publication.

Invariably, the associate editors use reviewers to advise them on the decision to publish. Reviewers are also unpaid volunteers and, moreover, they must remain anonymous; the only recognition they receive is to have their names included in a very lengthy list<sup>2</sup> of past reviewers that is published in the *Journal* once a year. The reviewers typically receive manuscripts unannounced (and often at very inconvenient times) from the editors with cover letters imploring their assistance. The reviewers are eminent people and busy people; they are also human beings, possibly with a variety of strong opinions and a modicum of irrepressible professional prejudices. Their knowledge of the applicable literature, although extensive, is not exhaustive.

There are certain by-products of the administrative structure and process described above that prospective authors should recognize at the outset:

- (1) Prompt handling and prompt reaching of decisions is in no way guaranteed. An author has no right to expect or demand such promptness. The process is, of course, not intentionally slow, but it is constrained because of its extensive reliance on unpaid volunteers. The overriding consideration is the quality of the *Journal*.
- (2) No submitted paper is guaranteed eventual acceptance. Although statistics are difficult to distill, it appears at this time that only about half of the submitted papers end up eventually being published. The process is not just a formality; many authors will not be happy with the outcome.
- (3) There is a substantial degree of *luck* involved in whether a paper becomes published. An author may, for example, submit two papers, one of which the author considers to be *great guns*,<sup>3</sup> the other to be mediocre. The mediocre paper may be published and the other may not be. The present writer hesitates to use the term *mistake* in this context; the process involves human beings and there is a fair amount of statistical fluctuations in its outcome.

The process should not be judged by how it handled any one paper, but by its overall results. Such may be occasionally disturbing to individual authors, but it is a price the scientific community has to pay for using the peer review system. To paraphrase Winston Churchill's famous statement<sup>4</sup> concerning democracy—*No one pretends that [the JASA system] is perfect or all-wise. Indeed, it has been said that [it] is the worst [system], except all those other [systems] that have been tried from time to time.* (Nevertheless, the possibility that the outcomes may be too capricious is worrisome to the present writer—many authors will not wish to submit a paper to a journal with a sustained reputation for capricious handling of manifestly high-quality manuscripts. One objective of the present editorial is to suggest to authors a methodology by which the capriciousness can be minimized.)

- (4) The editors are under no obligation to explain in detail why a paper is not selected for publication. The process is not intended for the continuing education of researchers and prospective journal-article writers; it exists primarily for the purpose of deciding which papers are to be published. One example of when publication is not warranted is when the editor, after an extensive search, is unable to find and recruit a reviewer with sufficient background and competence (and whose advice and opinion the editor trusts) to give an adequate review of the paper. (The present writer, like many of the associate editors, is reluctant to use the term "reject.") A paper that is not published may have considerable merit, and it may end up being published in another journal that is of equal or higher eminence than JASA. The associate editor has to make a decision, and the prolongation of that decision may be deemed inappropriate. There is no a priori reason to give the author the benefit of the doubt. (There is a certain analogy here with denying tenure to a professor at a university; many colossal mistakes have been made over the years, but the universities typically have to make a decision, often in the presence of political and economic forces, and there are often strong reasons for not stalling the decision. The tenure candidate is rarely given the benefit of the doubt, but that does not mean the end of the world for that candidate. The university, in guaranteeing life-time employment to a candidate for whom it has insufficient basis for a confident decision, risks far more than does the candidate, who would have an additional year to find alternate employment. Similarly, an author must realize that it is the Acoustical Society of America which bears the greater risk when a questionable paper is published.)

## II. LITERATE WRITING AND ALLIES

The need for literate writing is well-accepted by workers in the humanities, but it is less appreciated in some of the areas that pertain to acoustics. (Such is, for example, especially so for the engineering sciences.) Lofty and eloquent arguments in favor of literate writing can be found in Mary-Claire van Leunen's *A Handbook for Scholars*,<sup>5</sup> in Barzun

and Graff's *The Modern Researcher*,<sup>6</sup> and in Mortimer Adler's *How to Read a Book*.<sup>7</sup> Van Leunen, for example, gives the following sentences:

*Scholarly writing is distinguished from all other kinds by its punctilious acknowledgment of sources. This acknowledgment is not just an empty form. ... Citation can also strengthen your rhetoric. When you must stand alone in an opinion, so be it. But when you have allies, call them to your side by citing them.*

For the present author, the most telling argument in favor of literate writing is what might be regarded as an elaboration of the latter three sentences in the above quotation. When one reads a journal article, one normally desires strong assurance that the author is well acquainted with the subject and, moreover, that the ideas being proffered have not been formulated in a vacuum, without full understanding of the related ideas of one's contemporaries and predecessors. If done with sufficient skill, literate writing will go a long way in providing such assurance.

A classic case where a stronger familiarity with the literature and literate writing that *called one's allies to one's side* might have made a big difference is the case<sup>8</sup> of John Jacob Waterston (1811–83). The *Journal* involved is not JASA, but the general circumstances could just as easily have occurred within modern times with JASA being the desired publication. The story also illustrates the point that not every paper “rejected” by a journal is either not new, not significant, or not correct. Waterston's paper was definitely new, it was significant, and, as eventually turned out to be the case, it was substantially correct. That Waterston's paper submitted in 1845 was not published at the time is of considerable embarrassment to the Royal Society of London, but Waterston himself has to share part of the blame.

Waterston's paper was partly literate in the sense that it did cite a substantial literature, although the citing was incomplete and capricious. The basic idea was speculative, and for this reason a reviewer stated that “this paper is nothing but nonsense.” The manuscript lay untouched in the Archives of the Society (presumably some musty room in the building that housed the Royal Society's administrative offices) until circa 1892. Rayleigh learned of its existence when he came across a little-known 1858 paper by Waterston which alluded to it. When Rayleigh went to the Archives to look at the manuscript, he confirmed that it was the first paper to correctly conjecture on what we now call the equipartition of energy (energy  $kT/2$  per translational degree of freedom, regardless of the nature of the molecule). Rayleigh, being the great scientist that he was, recognized the paper for its intrinsic worth, and had the paper published in the *Philosophical Transactions of the Royal Society* in 1892, about 47 years after the original submission. Rayleigh wrote a short introduction<sup>9</sup> which appeared just before the 1892 printing. In that introduction, Rayleigh gave the following relevant sentences:

*One circumstance which may have told unfavourably upon the reception of Waterston's paper is that he mentions no predecessors. Had he put forward his investigation as a development of the theory of D. Ber-*

*noulli, a referee might have hesitated to call it nonsense. It is probable that Waterston was unacquainted with Bernoulli's work, and doubtful whether at that time he knew that Herapath had to some extent foreshadowed similar views.*

In short, Waterston may have lost because he was either (1) unfamiliar with all of the relevant literature, (2) had failed to appreciate how that literature tied in with his own ideas, or (3) failed to cite and intelligently discuss that literature. Waterston's paper might have been published, although probably only after a requested revision, if Waterston had had the 1845 counterpart of a Lord Rayleigh as a reviewer, but he wasn't so lucky. (Rayleigh was born in 1842.)

### III. CREDIBILITY AND PAPER Q98

To further explain the pragmatic benefits of literate writing in the context of publication in JASA, the present writer here discusses a hypothetical modern paper that might have been submitted to JASA. This paper is for brevity referred to as paper Q98. The nomenclature is such that individual papers are labeled by their level of quality: Q01, Q02, ..., etc., with Q100 being a perfect paper. Paper Q98 is slightly less than perfect. It is not a speculative paper as was Waterston's, but it has other problems.

The hypothetical paper Q98 has two authors; neither was previously known to the editor or the reviewers, and neither had published before in JASA or any of the other better-known acoustics journals. The subject matter of the paper is applicable to architectural acoustics; the text is highly mathematical, and the level of the mathematics and the elegance with which the mathematics is written are impressive. Nine references are cited in the paper and appear in the bibliography at the end. Seven of the references are to books, the other two are to journal articles. Among the books, two are older general textbooks on acoustics, one is a vintage textbook on architectural acoustics, one is a handbook devoted to mathematical functions, one is a monograph on spectral analysis, one is an older monograph on wave propagation in general, and the other three are to vintage books on mathematical physics. The two cited papers appeared in JASA over 20 years ago. There are no acknowledgments at the end of the paper and consequently no evidence of institutional or external support of the reported research.

The subject matter of Q98 deals with a standard partial differential equation that appears in acoustics and in many other fields. Standard boundary conditions are imposed; the basic feature distinguishing the problem from what one would find in standard texts is that the spatial region within which the partial differential equation applies does not have a simple shape. Perhaps with a tacit recognition that the analysis applies to subjects other than acoustics, the authors refer to the symbol  $c$  as the wave speed, rather than as the speed of sound. The one cited JASA paper whose title suggests some substantial relation to the subject matter of Q98 is a relatively pedestrian paper resting on very simple mathematics, having substantial graphical display, and having an extensive discussion of practical applications. In paper Q98,



there is actually very little explicit discussion of the two cited JASA articles; the emphasis is strongly on the mathematical development.

Paper Q98 does not correspond directly to any area in which the associate editor who received it had done research, so two reviewers are selected whose research interests have some relevance to the subject matter.

The first question of concern to the reviewers is whether the mathematical development was correct. They note that the authors present some numerical results for simple cases which agreed favorably with numerical results obtained by another method, so the likelihood of the mathematics being in error seems slight. They scan the mathematical steps, looking for a clear-cut instance of mathematical incompetence, and find none.

The next question addressed is whether the overall idea is truly new. Who can say for sure on such a matter? The reviewers do not go to the local library and devote extensive time to an exhaustive literature search. Even if they were to do so, the local library would probably prove to be inadequate. To really be sure, they would have to read the abstracts and dig into the contents of a great number of mathematical publications, many of which would have to be secured on interlibrary loan. A mere scanning of titles would have been insufficient. Instead, the reviewers think of all the related works that they recall seeing, at one time or another, and ask themselves whether they had ever seen anything quite like what was in paper Q98. The answer is no.

The question of the significance of the work is another matter to be addressed, but here—who can really say for sure what is significant? The mathematics is somewhat intricate and the succession of steps does require a nontrivial amount of thought; the problem is beyond what one would assign as a homework problem in a graduate course.

Thus one has a paper which is most probably correct, arguably new, and arguably significant. Should it be published? The present writer, were he the editor, would say no for the principal reason that the paper does not exhibit literate writing. The editor and the reviewers had no *a priori* reason at the outset for believing that the authors were literate in acoustics or in the subject matter of the paper. The references were all 20 years old or older, and the selection seemed somewhat haphazard. There was no evidence that the authors had done much of a literature search or that they had assimilated an understanding of related acoustical literature in the course of writing the paper. The present writer would have been uncomfortable in giving the authors the benefit of the doubt that the contribution was new. Given that the proposed boundary value problem is of a standard type which occurs in many branches of mathematical physics, and given the voluminous literature on partial differential equations, it seems inconceivable that some competent mathematically oriented scientist should not have tackled a closely related problem. Although it is possible that no one tackled a problem identical to that addressed by the authors, a truly literate work would have mentioned papers that addressed similar problems and would have discussed how the content of the cited papers differed from that of the paper under consideration.

An author might counter that they had indeed searched the literature exhaustively and that they found no such paper worth mentioning. Journal space is at a premium and they felt that no papers should be cited unless they are truly relevant to the work being presented. The present writer might acquiesce to such an argument if the total number of citations were much larger (say, 20 or more), with the bulk of the citations being to papers published within the past ten years. Otherwise, the assertion that nothing they had found was worth citing would be viewed as a distortion of the truth or as an excuse to avoid work that the authors did not enjoy doing.

A final criticism of the paper is that the significance of the work was not persuasively argued in the text. Literate writing may have accomplished this, but such was lacking. If the authors indeed found nothing in the past 20 years pertaining to the subject that was worth citing, then why should anyone in the next 20 years, except possibly the authors themselves, find their paper to be worth citing.

Should a revision be encouraged? The writer would here again say no. The content of the first version of the manuscript strongly suggests that the work is not of sufficient significance to warrant publication. A well-written unbiased literate revision with an accurate discussion of the existing literature would possibly only confirm this. The authors' precipitous submission of the manuscript without a careful survey of the relevant recent literature and without a well-written discussion of how their work fits into the context of the literature has severely prejudiced their case, and the credibility that any revision subsequently submitted is without bias in its reporting of the related literature will be lacking. If literate writing is required to establish the credibility of the authors' understanding of the field and of their claims that the work is both new and significant, then the literate writing must be present in the initial submission, not in a revision that the authors were coerced into writing.

#### IV. CITATION METRICS AND THERMOMETERS

The assertion, implicitly stated toward the end of the preceding section, that the expected number of future citations of a publication should be taken into account in the assessment of the significance of the work, is intrinsically controversial and warrants some discussion. A characteristic of modern times is that those who must make economic decisions desire quantitative indicators of quality that are easily measured. For scholarly journals, principal indicators<sup>10</sup> used by librarians are the following:

- (1) *impact factor*—The number of citations in the current year to articles published in a specific journal in the immediately preceding two-year period divided by the total number of articles published in the same journal in the corresponding two-year period. For example, suppose<sup>11</sup> that in 1999, one finds, among all the articles in a very large collection of journals, that a certain number  $N_1$  of the citations in these "citing articles" are to articles which appeared in JASA during the years 1997 and 1998. Also, in the same two-year period (1997 and 1998)

JASA published N2 articles. The reported impact factor would be N1/N2. This number would be reported as the 1999 impact factor for JASA.

- (2) *five-year impact factor*—The number of citations in the current year to articles published in a specific journal in the immediately preceding five-year period divided by the total number of articles published in the same journal in the corresponding five-year period. The definition is analogous to the impact factor described above, only the average is carried out over a five-year period. In the example given above in the definition of the (two-year) impact factor, one would redefine N1 to be the number of 1999 citations to articles which appeared in JASA during 1994, 1995, 1996, 1997, and 1998. The number N2 would be redefined to be the total number of articles which appeared in JASA during the same five-year period.
- (3) *cited half-life*—The number of years, going back from the current year, that account for 50% of the total citations received by the cited journal in the current year. For example, suppose that in 1999 JASA received a total number of NT citations from all the articles published in all the journals in that year, of which a number N99 were to articles published in JASA in 1999, a number N98 were to articles published in 1998, etc. Suppose in addition that the sum of N99, N98, N97, N96, and N95 is (0.47)NT, while the sum of N99, N98, N97, N96, N95, and N94 is (0.54)(NT). Then the reported cited half-life for JASA in 1999 would be 5 years.

The data on which these calculated numbers are based is readily available (although not *freely* available) to librarians, so there is ample opportunity for creative librarians with access to large computers to come up with metrics that are custom-tailored to their institutions. One can envision, for example,<sup>12</sup> the following:

- (4) *cost-per-citation metric*—One starts with the database listing all of the articles published during, say, 1999 by all of the faculty in the library's university. From this database, one can build a more extensive database listing all of the articles that were cited in those faculty publications. Of these cited articles, let us say that NU2 were to articles published in JASA during 1997 and 1998, and that NU5 were to articles published in JASA during 1994, 1995, 1996, 1997, and 1998. The yearly subscription price to JASA is JDOLLARS. The estimated future cost of each faculty citation to JASA can be estimated to be either (2)(JDOLLARS)/(NU2) or (5)(JDOLLARS)/(NU5), where it might be supposed that both calculations yield approximately the same number. A cost-conscious librarian with a fixed budget could then choose the journals "most appropriate" to the institution based on one or both of these metrics. Journals would be prioritized by the smallness of their cost-per-citation metric. If JASA should, for example, be number 23 in this ranking, but the total subscription costs of the journals ranked 1 through 22 exceed the library's annual budget, then the library would not subscribe to JASA.

The JASA editors could play an analogous "game" and use the same database to calculate an *effective contributory impact factor* for any given paper published in JASA. Such a factor would take into account the three ways a paper can affect, positively or negatively, JASA's overall impact factor: (a) the publication of the article adds to the number of papers that are in JASA, (b) the paper may cite previous JASA publications, and (c) the paper itself may be cited in future publications. If the JASA impact factor is  $I_J$  and the effective contributory impact factor for any given paper is  $I_C$ , then the definition of a suitable  $I_C$  must be such that the average of all the  $I_C$ 's is  $I_J$ . A mathematical analysis leading to an appropriate definition is somewhat intricate, but could presumably be worked out in a short time by most of the readers of this editorial. In the interest of brevity, the analysis is omitted here. The result, which in retrospect should be in accord with one's intuition, is the following:

- (5) *effective contributory impact factor*—Suppose a given paper, say Q50, is published in 1997. Paper Q50 cites a number NREF of references that were published in JASA in 1995 and 1996. In the two years, 1998 and 1999, one finds that, in all of the papers published in all of the journals, that Q50 is cited a total of NCITED times. Then the appropriate value  $I_C$  for paper Q50 is

$$I_C = \frac{1}{2}(\text{NREF}) + \frac{1}{2}(\text{NCITED}).$$

If a given paper's  $I_C$  is less than  $I_J$ , then that paper can be regarded as having a negative influence on the *Journal's* impact factor; if it is higher, then it has a positive influence.

A creative editor may seek to estimate what the  $I_C$  of a submitted manuscript would be should that manuscript be published; the numbers NREF and  $I_J$  are known at the outset, it being a reasonable assumption that  $I_J$  does not change much over a short period of time. The remaining number, NCITED, can be estimated from the first author's track record. In this manner, one arrives at the following definition:

- (6) *projected effective contributory impact factor*—Suppose a paper Q51 is submitted in 2000 and has NREF references to papers published in JASA in 1998 and 1999. The database shows that the first author has published, say, three papers in the ten year period ending with 1997. For these papers, the average number of citations by others in publications that appeared in the two immediately following years is (NCITED)<sub>TR</sub>, where the subscript "TR" abbreviates track record. Then the projected effective contributory impact factor is

$$I_{C,\text{proj}} = \frac{1}{2}(\text{NREF}) + \frac{1}{2}(\text{NCITED})_{\text{TR}}.$$

If the author has no track record, then the number might be calculated with NCITED<sub>TR</sub> set to zero.

A reader may justifiably criticize any reliance on this number, one reason being that a manuscript that an author *submits* is not necessarily representative of the author's papers that eventually end up in print. The present writer would

counter that the definition is here made with “tongue in cheek,” and one can make whatever use of it one wishes. One intriguing observation is that an author need only cite a number greater than  $2I_J$  of references which were published in JASA during the past two years to achieve an increase in JASA’s impact factor. As discussed further below, an author who does this is one who is practicing *collegial citing*.

To individuals who read and use JASA, as contrasted with librarians and possibly with editors, the metrics described above are somewhat irrelevant. If an article pertains strongly to one’s research, then one should read that article, regardless of where it is published and regardless of how many times it has been cited. On the other hand, it is only human that authors would like for their work to be read and appreciated by others. It is difficult for an author to gauge how much his or her work is being read and to what depth the readers are reading a paper. The only practical measurement, however imperfect, that exists at present is how often (and in what manner) the work is cited in subsequent literature. Most scholars would strongly decry the idea that a paper that is cited twice as often as another paper is substantially better or more significant than the other paper. They would, however, sense that there is something wrong with a paper that is never cited over a ten-year period.

The idea of taking number of citations as a metric of paper quality or of journal quality is analogous to using body temperature as a measure of one’s being ill. If someone runs a temperature of  $105^\circ\text{F}$  (or  $41^\circ\text{C}$ ), then all would agree that that person is ill. The person is not necessarily more ill than a person who runs a temperature of  $102^\circ\text{F}$ , and one would not seek to cure the illness by plunging the person into a cold water bath and waiting for the body temperature to drop to  $98.6^\circ\text{F}$ . Nevertheless, the fact of the illness cannot be dismissed—regardless of how one interprets the detailed significance of the numerical value of the body temperature.

That a relatively low impact factor may be of concern to some editors is exemplified by a recent editorial<sup>13</sup> in the *Journal of Applied Mechanics* (JAM), which is published by the American Society of Mechanical Engineers (ASME). There the editor laments that the impact factor for JAM is lower than that of some peer journals and lower than that of two of the other journals published by the ASME. The editor continues with the statement: “*It would thus appear that there are quite a few papers appearing in the Journal which fail to be cited in the near term and it is clear that, if such a trend continues, the Journal will suffer.*” (With considerable trepidation, especially since JAM is a journal he greatly respects, the present writer suggests that the opening sentence in the introduction of the current editorial would apply equally as well to JAM.)

The dominant question remaining is whether JASA should knowingly publish papers that are unlikely to ever be cited, or which at best will be seldom cited. If the editor and the reviewers are convinced that a paper is truly “great guns,” then that paper should be published, no matter what. Perhaps someday a Lord Rayleigh will come along, discover the paper, bring it to the attention of the scientific community, and it will “blossom forth.” At that time, the Acoustical Society can congratulate itself on having had, in contrast

to the Royal Society of 1845, editors and reviewers with great perception and foresight. On the other hand, if it is certain that the work, even if by some fluke it should be extensively cited, could never be of anything but minor consequence, the present writer would argue that the paper should not be published.

## V. THE EGOIST AND PAPER Q99

An *egoist* is the antithesis of a colleague, and in many cases the author of a manuscript comes across as an egoist. To illustrate the point and to persuade potential authors to write as colleagues rather than egoists, the writer here describes another hypothetical paper, this one labeled as Q99, as it is possibly of slightly better quality than Q98, but still not perfect.

Paper Q99 is authored by a person who has been writing papers for some time on a somewhat specialized topic, some of which have appeared in JASA. The current paper is, to some extent, a continuation of that research. The manuscript has 12 references, of which six are to the author’s previous papers. Of the remaining six, two are to journal articles authored more than 20 years ago, the remaining four are to generic textbooks.

The editor who receives the manuscript has a difficulty in identifying an appropriate reviewer. Ideally, such a reviewer should have some familiarity with the author’s previous papers, but the list of citations gives no clue as to who such a person might be; possibly no such person exists. The editor goes to *The Citation Index of the SCI*<sup>14</sup> to find who has been citing the author’s work in the past and finds that there are very few citations—other than those given in subsequent publications written by the same author. None of the names of the citers are familiar to the editor, so the editor consequently sends the paper to potential reviewer A, who has a reputation for knowing just about all there is to know about this general area of acoustics, but who has no knowledge in depth about the specific topic of the submitted paper. Reviewer A declines, stating that he or she is really very busy. This process—of the editor asking, and of the reviewer declining—goes through several iterations, until eventually a good citizen is found who agrees to review the paper.

The good citizen reviewer has not read any of the author’s previous papers. Moreover, being human, the reviewer is not willing to diligently read all of those papers in preparation for a thorough review of paper Q99. The reviewer may not even look at those previous papers, possibly because the paper’s author does not write with exceptional clarity and possibly because some of the cited papers are difficult to retrieve. The disposition of the paper at this point is capricious. One possibility is that the reviewer simply goes through the manuscript and looks for obvious errors and makes notes as to suggestions that would improve the paper. The novelty of the paper is taken as a given; the author’s previous papers were all taken as novel; this is different from any previous paper by the author; and it seems certain that no one but the author would have addressed the present problem. That the paper is significant seems evident, as all the previous papers in this sequence were adjudged by other reviewers to have been significant, so this one must be also.



The good citizen reviewer sends back a long list of suggested cosmetic improvements; this list is transmitted to the author; the author submits a revision with the suggested improvements taken into account; the paper is accepted and published.

An alternate scenario is that the reviewer recommends the paper not be published because the case for the work being significant is too weak. Although this is a continuing work that has resulted in a number of previous publications by the author, there is no indication at all that anyone has been reading those papers or is carrying on related research. If there were any such person, then why didn't the author cite them? Another disturbing feature is that the author is blasting on ahead without looking around in the scientific community to see if anyone is doing work which might impact the present author's work. Perhaps it is time to call a halt to this chain of noncollegial publishing. No one other than the author would miss the next few installments.

It is not clear that the reviewer or the editor could ever convince the author that he or she has been operating as an egoist rather than as a colleague, but that is the basic problem. If the author had written all the papers in the sequence with a concerted effort to discuss the relationship of the current work with work that was being carried out by others, then the cited persons might have taken notice of the author's research. There might have been a dialog in the literature, with a synergism of work carried out by different groups. The small price that the author would have had to pay is that he or she would have to read some papers written by persons other than himself or herself; these papers would have to have been understood in some detail and then assimilated in the writing of the subsequent papers—and they would have to be cited. In brief, the author would have to assiduously cultivate the art of *collegial citing*.

A cynical reviewer might harbor the suspicion that, in actuality, the author was incapable of doing the reading and assimilation that was required to produce the literate writing and collegial citing that the papers were so strongly lacking. Perhaps at some time in the distant past, some thesis advisor had carefully laid out the relevant background and pointed the author in a certain direction. Momentum, persistence, and a certain luck in the assignment of reviewers for the author's submitted manuscripts had resulted in a healthy list of refereed publications. If one's ego is sufficiently great, or if one does not care whether anyone reads one's papers, or if there is no pressure to secure external funding for one's research, then one blithely carries on.

The present writer's view is that, were every paper in the *Journal* to have been written by an egoist (rather than a collegial) author, then the *Journal* would be in serious trouble. Possibly, most egoist authors are capable of reform; they may only need a loud "wake-up call." If so, then the discussion in this editorial might help.

## VI. THE ALLEGED INTERLOPER AND PAPER Q99.5

The use of the word *interloper* is here intended to be provocative, but the adjective *alleged* is intended to soften the provocation. The standard dictionary definition of an interloper, as being one who thrusts himself or herself into any

position or affair, which others consider as pertaining solely to themselves, is much stronger than the sense which the present writer intends. Unfortunately, the English language provides no single word that succinctly conveys the image of someone who enters briefly into a group endeavor for no apparent reason and who has no intention of joining that group.

The term *interloper* is here intended to imply a hypothetical person, who may be entirely nonexistent, and who submits a paper to JASA with the following attitude:

*I never read JASA, and I certainly have no intention of ploughing through its pages to find something worth citing. You may have a nice Society and have nice semi-annual meetings, but I have no intention of entering into the affairs of your Society or ever going to any of your meetings. Probably the other authors of papers in JASA would welcome me as a colleague, but I really don't care whether they do or not; I already have a fine set of colleagues, and they are all I need. Typically, I publish all of my papers in other journals, but just this once I am condescending to submit a paper to JASA. Consider yourself fortunate that I have done so. There are of course no references to JASA in this paper, but I can't imagine there would be anything previously published in JASA that would be relevant to what I have done. You may have some difficulty in finding a suitable reviewer among the membership of your organization, but if you go outside that group, you should be able to find such a person, although this paper is so much obviously better than what you usually publish that you should not need much of a review. The other authors of papers in JASA have my permission to cite this paper as much as they wish, but don't expect me to reciprocate.*

The present writer agonized considerably as to whether the above paragraph should be included in this editorial. One risks being considered paranoid, for, quite possibly, no author of a submitted manuscript has such a blatantly arrogant attitude. However, some authors do incur the risk of being perceived by editors, reviewers, and JASA readers as *alleged interlopers* and they can avoid this risk by incorporating *collegial citing* in their manuscripts at the outset. Of course, if they really are interlopers, then they won't want to do this.

Paper Q99.5 is received by an editor from an author of whom the editor has no prior knowledge. The paper appears to be truly concerned with acoustics, and it is quite possible that there have been papers in JASA at one time or another that may have been related to the topic of the paper. The topic is nevertheless not a mainstream of contemporary acoustics, so the title or author of a relevant JASA paper does not come immediately to the editor's mind. The present paper has no references to JASA at all; the plurality of the references are to papers published in one particular journal, here referred to as *Journal X*. There are also some references to papers published in *Journal Y*, and some to papers published in *Journal Z*. Many of the references are to papers previously published by the author, although the number of



these is not so overwhelming that one would tag the author as an egoist.

One question the editor might ask himself or herself is why is the author submitting this paper to JASA—why not to *Journal X* instead? The author's cover letter gives no clue; it may even be the case that the format of the manuscript suggests that the author has made no special attempt to follow the instructions that are spelled out in JASA's *Information for Contributors*.<sup>15</sup> The editor has a vague suspicion that the paper was first submitted to *Journal X* and was rejected, but the cover letter does not say this was so, and the *Journal* currently does not have a policy that requires authors to disclose such information. Also, in all handling of such papers up until the present, the editor typically disregards the fact that no JASA papers are cited.

Thus the first task at hand is to find an appropriate reviewer. If the editor cannot think of anyone that he or she knows who is an ideal match for the subject matter of the paper, the next recourse is to examine the reference list and the manner in which the references are cited to discover some clue as to whom to ask to review the paper. What becomes evident from this examination is that the author is associated with a *collegium* that is different from any with which the editor is associated. (A *collegium* is "an association of individuals of the same class or rank formed to promote their common interest in some business pursuit or enterprise.") The editor does not personally know any of the cited authors, although a few of the names are vaguely familiar. Picking the right reviewer is now analogous to the party game of "pin the tail on the donkey." The editor picks someone (reviewer X) and hopes for the best.

What bothers the present writer about the above hypothetical scenario is that, once reviewer X is selected, JASA is operating fully as a surrogate for *Journal X*. Reviewer X has no special interest in the good of JASA or of the ASA, and the expectation is that he or she will review the paper just as if it had been submitted to *Journal X*. The reviewer may have no special acquaintance with what has previously been published in JASA and will not be bothered at all by the fact that no JASA articles are cited. Even if it were so that the paper had previously been submitted to *Journal X* and turned down, there is only a small chance that the current reviewer is the same person as the previous reviewer. Given the capricious nature with which the reviewer has been selected, the editor may have picked the least discerning of all the possible reviewers that an editor of *Journal X* might have picked—thus there is a reasonable chance that the paper will be selected for publication in JASA even though it would never have been selected for publication in *Journal X*.

How could JASA have possibly handled Paper Q99.5 any differently? This is a difficult question that warrants input from a variety of individuals. The present writer's tentative method of handling such a case, were he the associate editor receiving the manuscript, would be the following. First, the paper would never be considered as submitted unless or until the author had fully complied with what is spelled out in the *Information for Contributors*. Second, the editor would act as an initial or screening reviewer and send a "review" to the author with the following criticisms or

questions. One question would be that of why there are no JASA articles cited in the paper. Given that JASA is the world's largest and oldest journal devoted to acoustics, it is highly surprising that a paper on acoustics should not have cited any JASA articles. The present writer's estimate is that, over its lifetime, JASA has published of the order of 25 000 research papers. Does the author really intend to imply that none of these papers have any relevance to the submitted paper? If such is genuinely the case, then why is the author submitting the paper to JASA? The writer would further ask the author: who does he or she hope will read the paper and make use of its results in future research? The writer will moreover ask the author, in the answer to this question, to include some specific examples of persons who either publish occasionally in JASA, present papers at ASA meetings, or who either subscribe or frequently read articles in JASA. If no such person is identified, then the author would be asked why he or she would expect any such person to regard the work as significant. The author would be told that full consideration for publication in JASA will not take place until the editor receives a literate article that shows that the author is familiar with the relevant related work that has previously been published in JASA. Even if the author continues to assert that there is no such work, the editor will insist that this be demonstrated by literate writing with citations to whatever is most closely related, even though the relationship be slight. The author will also be asked to practice some collegial citing in the writing of the paper, so the author brings out a case for why any of the JASA collegium might be able to make use of the results of the author's research.

Some readers may object to the above handling and express the feeling that it is a bit harsh. The writer has some sympathy with this feeling, and is willing to consider articulate discussions of alternative handlings. Nevertheless, the reader should realize that there is nothing illegal in the proposed scenario. Moreover, the outcome has a possibility of improving the quality of the *Journal*. Although the citation metrics discussed in Section IV may seem somewhat crude (and even crass), they do have some relevance in gauging the quality of the *Journal*. Paper Q99.5, if not substantially rewritten to incorporate appropriate literate writing and collegial citing, is expected to have a negative influence on the JASA impact factor. It achieves this in a double fashion: first, it cites no JASA papers, and, second, it stimulates no future citations from the collegium of JASA authors. If the author complies with what the writer suggests (or insists upon) above, then both of the terms, NREF and NCITED, in the effective contributory impact factor will increase. Furthermore, the *alleged interloper* ceases to be such and becomes a *colleague*.

## VII. CONCLUDING REMARKS

The dominant theme of the present article is that, in the archival reporting of scientific research, a straightforward factual account of what one has done is simply not enough. The scientific literature is, almost by definition, accumulative; and this is especially so for most of acoustics. No paper

stands alone and no writer stands alone. There is a rich heritage from the past, and there are others who are thinking about similar problems.

Scientific societies such as the Acoustical Society of America were formed to bring people together who had a common interest in the progress of a certain branch of science. While some may perhaps think that *The Journal of the Acoustical Society of America* is something entirely separate from the Society that sponsors it, that is not the case. JASA is the Society's chief instrument for achieving the purpose of facilitating communication among researchers in acoustics.

Communication is a two-way process; one listens and one speaks; one reads and one writes. When one speaks, one should do so with a full cognizance of what the other conversationalist has just said. Similarly, when one writes, one should do so with a full cognizance of what has been written by others and with a full cognizance of the interests of others who one would want to read one's papers.

The present editorial argues that citing applicable prior work, giving credit where credit is due, and citing original sources of ideas and procedures used in the research is also simply not enough. The author has to explain, and often in some detail, just how the present paper fits into the grand scheme of things. The scientific literature of today is overwhelming and it is inevitable that it will become even more so in the future. A journal such as JASA plays an important role in the management of such literature. In its editorial process, it seeks a careful selection of what is being written on acoustics; it provides not a representative selection, but a quality selection of the current acoustics literature. With well-written articles, each of which exemplifies literate writing and collegial citing, the hope is that the readers of JASA will have access to a manageable source for following the dominant trends in acoustics research. The *Journal*, like the authors who publish within its pages, wants to be unique. It does not want to be regarded as just one of a vast proliferation of places where authors can store the accounting of their research results.

The phrases, *literate writing* and *collegial citing*, that figure prominently in the present editorial, are not just convenient catch phrases; they encapsulate a philosophy of archival research paper writing. Prolific writers who adopt this philosophy may perhaps find that their output is slowed down. Nevertheless, the prediction is that their impact will be considerably increased. The literate writing in the papers will increase the credibility that the authors know what they are writing about and that what is written is worth reading and worth contemplating. The outreach to other workers in the field through *collegial citing* will attract specific interest from those whose future work could benefit from the results reported in the author's papers. The author will find that his or her work is being cited and cited often, and the frustrations that one sometimes has, that one's work is neither read nor appreciated, will begin to disappear.

Writers of research articles should recognize, if they have not already done so, that good archival writing, going beyond the usage of good grammar and beyond the skillful selection of appropriate phrases, can be fun. It may not be as

much fun as doing the research itself, but there is a great potential satisfaction in the execution of a scholarly account of that research which places it fully and securely among the best literature of one's field. The realization and the relation of the fact that one is not alone is important to anyone; writing one's papers so that one tells others that they also are not alone is even more important. Just as Robinson Crusoe was elated when he found the footsteps in the sand, so will your colleagues be elated when they learn that there is someone out there, someone whom they respect, who has read and assimilated their work, and who moreover appreciates its significance and its place in the panorama of acoustics research.

<sup>1</sup>The definitions given are paraphrased from various definitions in *Webster's Third New International Dictionary of the English Language, Unabridged*, Philip Babcock Gove, Editor-in-Chief (Merriam-Webster, Springfield, Massachusetts, 1961).

<sup>2</sup>See, for example, D. W. Martin, "Appreciation to the 1997 reviewers of manuscripts submitted to the *Journal*," *J. Acoust. Soc. Am.* **104**, 2–7 (1998).

<sup>3</sup>The (quaint, unabashedly British and Victorian) term "great guns" is taken from a remark by J. C. Maxwell in a letter to a colleague, in which he stated that he had a paper in the works which he regarded as great guns. Given what Maxwell accomplished, it is here regarded as the highest accolade that an author might give to the author's own work.

<sup>4</sup>W. Churchill, from a speech made in the British House of Commons, 11 November 1947; cited in *The Concise Oxford Dictionary of Quotations* (Oxford University Press, 1981), 2nd ed., p. 71.

<sup>5</sup>M.-C. van Leunen, *A Handbook for Scholars* (Alfred A. Knopf, New York, 1978), esp. pp. 9–10.

<sup>6</sup>J. Barzun and H. F. Graff, *The Modern Researcher* (Harcourt, Brace, and Jovanovich, San Diego, 1985), 4th ed. (the first edition appeared in 1957), esp. pp. 357–376.

<sup>7</sup>M. J. Adler, *How to Read a Book* (Simon and Schuster, 1940), esp. pp. 127–129, 276–282.

<sup>8</sup>S. G. Brush, *The Kind of Motion We Call Heat* (North-Holland, Amsterdam, 1976), Book 1, Chap. 3, pp. 134–149.

<sup>9</sup>J. W. Strutt (Lord Rayleigh), "On the physics of media that are composed of free and perfectly elastic molecules in a state of motion," paper 191 in *Scientific Papers by Lord Rayleigh* (Dover Publications, New York, 1964), Vol. 3, pp. 558–561. This is a reprinting of an introduction to a memoir by Waterston with the same name: *Philos. Trans. R. Soc. London, Ser. A* **A183**, 1–5 (1892).

<sup>10</sup>Institute for Scientific Information (ISI), "ISI hypertext terminology and concept glossary," appearing on the World Wide Web at URL site <http://www.isinet.com/help/glossary.html> (version as read on 28 February 2000).

<sup>11</sup>E. Garfield, "The impact factor," *Current Contents* **25**, 3–7 (20 June 1994). Reprinted on the World Wide Web at URL site <http://www.isinet.com/hot/essays/7.html> (version as read on 28 February 2000).

<sup>12</sup>E. Garfield, "The application of citation indexing to journals management," *Current Contents* **33**, 3–5 (15 August 1994). Reprinted on the World Wide Web at URL site <http://www.isinet.com/hot/essays/9.html> (version as read on 28 February 2000). The hypothetical metric described in the text is inspired by Garfield's account of some activities carried out by Joshua Lederberg of Rockefeller University.

<sup>13</sup>L. Wheeler, "Special announcement from the technical editor," *Trans. ASME, J. Appl. Mech.* **66**, 1054 (1999).

<sup>14</sup>The *Citation Index of the SCI* is an alphabetical list by [first] author of all the references (cited items) found in footnotes and bibliographies of journals carried in the SCI [Science Citation Index]. The print version is published annually by the Institute of Scientific Information (Philadelphia) and typically extends over a large number of volumes (13 in 1998). The on-line version can be found on the World Wide Web at URL site <http://www.isinet.com/products/citation/wos.html>

<sup>15</sup>"Information for Contributors to The Journal of the Acoustical Society of America (JASA)," published in the front matter of the first issue of each volume of the *Journal*. The most recent appearance was *J. Acoust. Soc. Am.* **107**(1), ix–xiii (2000).