Letters

Authors, Editors, and Referees

The complaint by I. H. Page about editorial practices (Letters, 12 Mar., p. 1241) seems justified from an author's standpoint, but the situation is not as hopeless as one might think. For some of his indictments, appropriate suggestions to the editor could do much to get questionable procedures modified. Some of his criticisms are, I believe, unjustified and really point to the need for a change in authors' attitudes. And one, I regret to say, accurately describes an unfair practice by some editors.

If an author is writing for a particular journal, then he should know the preferred citation format of that journal and list his references accordingly. The difference between methods in the amount of effort and preparation time required is usually insignificant. But if an author has prepared his manuscript according to an established style sheet, and the editor still requests changes, then the author has a right to object. Without authors and their papers, professional journals would cease to exist. Editors might sing a different tune if insistence on format changes meant the loss of good papers. It is for this reason that the journal I edit (and certain other federal journals, too) does not issue a style sheet to authors. We accept manuscripts in any format. If that format doesn't match our style, we assume the burden of changing it. With a pot of paste, a pair of scissors, and the services of modern copying machines, an editorial clerk can do a remarkable job on a manuscript. This service makes it much easier for the author-reviewer-editor team to agree on necessary editorial and subject-matter changes.

I disagree with Page's statement that an editor has no right to request the withdrawal of some bibliographic references. Recently I made a survey of a randomly selected group of manuscripts submitted for publication by the U.S. Department of Agriculture. This survey revealed unnecessary citations in the following categories: (i) Citations dating back to the 18th century, (ii) citations of only peripheral concern to the subject, (iii) citations supporting universally accepted knowledge, (iv) citations inserted mainly to show the depth of the author's library research, and (v) citations designed to foster good relations between the author and his co-workers or supervisors.

Criticism of the use of *et al.* might be justified if research papers had the charm and appeal of the Rodgers and Hammerstein musicals in the comparison Page offers. But some papers, together with their multiple authors, go properly into limbo simply because they contribute little to scientific knowledge. However, I agree that co-authors deserve to be cited in the reference section; in the text, *et al.* does have a place.

Page's suggestion that a "uniform, simple system of bibliographic reference" be adopted is most logical but unlikely to be realized. (How long have we been waiting for uniformity with respect to measurements or other more important tools and procedures?) The style established by the U.S. Government Printing Office, which is generally followed by federal journals, seems to be a step in the right direction.

I heartily agree with the criticism leveled at editors who pass along to authors the "scurrilous personal diatribes, thinly veiled as scientific criticism" that come from reviewers. An editor should know enough about the discipline his journal represents to weed out such comments. But even more important is the need for the scientific community to establish reasonable guidelines for reviewing and to train review committees in the art of constructive criticism. Such a program could contribute much to the smoother functioning of the author-reviewer partnership and, consequently, to the strengthening of research management.

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. . . Suppose you are a licensed engineer, holding the Member grade in the American Society of Mechanical Engineers, with a record of perhaps half a column in *Who's Who in Engineering* and a paragraph in *Who's Who in the East*, and you believe that you can contribute an idea worth considering by geologists, relating to the source of the forces producing continental drift. You submit a paper. The editor writes back that the reviewer has rejected it because you have no knowledge of basic engineering principles—"Everyone knows that the pressure in rock layers is the same in all directions." You reply that the reviewer does not qualify to pass on your paper, and supply an exact argument based on elastoplasticity. Does the editor go back to the reviewer for a check? He does not, presumably because it would never do to question the reviewer's competence. . . .

I wrote a paper on an engineer's view of the problem of the Old Stone Tower at Newport. I had found that the peculiar sizes and arrangements of the windows in the 2-foot-thick walls were due to a desire either to see and recognize signals by fire at night, or to permit signaling to a ship making port at night, utilizing wall light from a hearth, direct firelight, and increasing and decreasing direct firelight. One editor rejected the paper because, "as everyone knows," more than a hundred years ago lighthouses did no more than show the light of the fire around the whole horizon. Another reviewer for another journal, head of the department of anthropology in a large university, rejected on the grounds that "if anyone wished to see better he would use larger windows." Not in winter and not when exposed to Indian attack! . . . An engineering journal published the paper with alacrity. . . .

The trouble with some reviewers is that they read into an article what they think is there, particularly if they tend to disapprove of what they think is said. The trouble with others is that

they do not wish to admit that they are passing upon a question they are not at all qualified to pass on. . . . In any case, I think that a reviewer who rejects on erroneous grounds should be required to meet the objections of the writer without hiding behind the skirts of the editor. The present procedure protects the reviewer from tarnishing his advanced-degree rating, but does nothing for science itself.

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. . . It would be nice if editors would refrain from stamping their serial numbers and dates of receipt and otherwise heedlessly marking up submitted manuscripts until they have been accepted for publication. Sometimes a rejected manuscript is filled with such extraneous matter, including editorial corrections of doubtful value. The manuscript rejected by Journal A might be very acceptable to Journal B, but all the doodlings require the investigator to have his overtaxed secretary retype it.

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Methinks Page doth protest too much about the little detail of inclusive pagination in references. It is hard on a reader with limited reference facilities not to be told whether he is being referred to a one-page abstract or a hundred-page paper; most reproducing and library services require inclusive pagination in order to provide copies. Indeed, the number of illustrations should also be included, especially if they are in addition to the stated pagination.

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. . . While it is undoubtedly true that, as Page says, alphabetic arrangement of references by authors' names helps one in seeking one's own name in the papers of others, it has another and possibly more important function than the ready satisfaction of vanity, namely, to make it easy to find a reference some time after one has read the paper. I have often been exasperated by the necessity to reread an article in order to find a particular reference because the references were listed in the order in which they had been cited in the article. This is especially notable in review articles containing many references. . . .

Page questions whether referees should remain anonymous. I am certain that they should not. When I was editor of a journal I required all referees to submit signed reviews, which were forwarded to the authors. Not a single referee refused to sign his review, nor did any author take offense at referees' remarks. More important, the reviews became more scholarly and therefore more helpful to the authors and to the editor. . . .

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Stone Age Intellect

In "Callanish, a Scottish Stonehenge" (8 Jan., p. 127) Gerald S. Hawkins writes: "Thus, Stonehenge may well have been a device of such precision and complexity of design as to indicate a level of intellect far surpassing that which we have hitherto been willing to ascribe to Stone Age man."

The "Stone Age" is the period (covering most of man's life on earth) which precedes the period in which man made significant use of metal for tools. It is divided into Old Stone Age, Middle Stone Age, and New Stone Age. Anthropologists are agreed in ascribing to man of the latter part of the Old Stone Age, the Middle, and the New Stone Age the term *Homo sapiens*, the term applied to contemporary man, to signify that he is the same *Homo* in brain power (and physical development, also). Men of the New Stone Age in Europe discovered and exploited the domestication and cultivation of grains, the domestication of animals, the making of pottery and textiles, village life, and so on. (Some techniques, such as pottery, may reach even further back into the Stone Age.) Men of the American continent, who were stoneagers up to the time of the Spanish conquest, developed a complicated social organization, monumental architecture, an intricate calendar.

In general, the inventions associated with the New Stone Age, to use V. Gordon Childe's word, "revolutionized" man's economy and way of life. I do not see that, viewed in the perspective of such Stone Age accomplishments, the Stonehenge-Callanish complexes "indicate a level of intellect far surpassing that which we have hitherto been willing to ascribe to Stone Age man."

Hawkins seems to consider the British monuments scientific. For instance, he sees the Callanish people as "not as scientifically advanced" as those of Stonehenge. Since the concept of "science" suggests theory as well as observation, I wonder if all scientists would agree that the British monuments, no matter how accurately positioned, are in fact scientific achievements. Their complex arrangement can be explained entirely on the basis of astronomical observations made over a period of time; no theory worthy of the scientific method need be involved, and to postulate one is going beyond the concrete archeological evidence. One could as easily attribute to these early Britons a magical understanding of the observed phenomena as a scientific one. As for "precision," this is not in itself a sign of "surpassing" intellect.

Hawkins is surely correct in believing that archeologists will want to relate the sites he discusses to other sites. Let us hope that future studies and discoveries will give us some insight into whether the British monuments and their astronomical observations were an isolated development, or whether there is some link with other cultures of the period 2000–1500 B.C., for instance, with the more advanced Bronze Age cultures of Egypt, the Aegean, and the Near East (an area of great astronomical preoccupation).

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I heartily agree with Schwartz that there should be more astronomical studies of megalithic monuments around the Mediterranean as well as in the British Isles. The Stonehenge and Callanish discoveries indicate that astroarcheology may have hitherto been somewhat neglected, and yet it can throw much light on the fascinating problem of the intelligence of ancient man.

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