

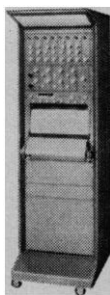
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Letters

Abbreviated Reference Citations

May I make a plea for considerably more sparing use of abbreviations when citing references to scientific periodicals? I believe this practice originated at a time when periodicals were far fewer in number, and when references were commonly placed in footnotes, its purpose being presumably to prevent the footnotes' occupying too much space.

Nowadays, few are familiar with the titles of the majority of periodicals, and references are usually listed at the end of each article. The amount of space saved by abbreviation of titles is negligible, and the obscurity thereby introduced may well be considerable for some readers, especially when such heights of esotericism are reached as *B.A.N. for Bulletin of the Astronomical Institutes of the Netherlands* or *Röfo for Fortschritte auf dem Gebiete der Röntgenstrahlen*. Sometimes, as in abstract journals, where condensation is of great importance, the practice of abbreviating titles may be tolerated, but for the most part it gains nothing and merely sets the reader a more or less difficult conundrum whenever he wishes to trace a reference so cited.

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Native Peoples of South America

Leeds' review of *Native Peoples of South America* [*Science* **131**, 94 (8 Jan. 1960)] calls for comment regarding responsible reviewing rather than reply to particular points.

First, I must mention that what are absurdly called "unnecessary errors" refer mostly to works published after the manuscript was in press and to works *not yet published*. For example, Leeds knows that his own research among the Yauru had not yet been started in December 1957, when I worked on a final revision of the manuscript, for at that time he asked me for advice regarding where to work in the field. I received Moore's study of Inca property and law for review about the time *Native Peoples* was published. Murra's work on the Inca is, so far as I know, still unpublished.

An author is obligated, of course, to keep reasonably abreast of the published literature. At the same time, it is rather pointless for the reviewer to carp about omission of published and unpublished research done after the book was written. If the reviewer believes that the research makes an impor-

tant difference in scientific understanding, rather than in descriptive minutiae, he should spell out his point, preferably in an article of his own or in "Notes and Comments."

Second, Leeds seems not to have read the book carefully, for he makes the incredible assertion that it merely follows the culture area divisions of the *Handbook of South American Indians* [(Bureau of American Ethnology, 1948), vols. 1-6], which I edited. The *Handbook* is based upon four culture areas; *Native Peoples*, upon eight cultural types and 11 subtypes, which correspond only partly to areas. The book explains repeatedly that this typology is evolutionary in being based upon structure and developmental process rather than upon the traditional descriptive and historical taxonomy used in the *Handbook*.

While anthropologists will not be misled by the carelessness of this review, the general reader might conclude that *Native Peoples* has little new to offer.

JULIAN H. STEWARD

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Steward's reaction surprised me, as I still feel my review was favorable. I pointed out that the book constitutes a "useful compendium . . . for professionals and students alike," and has originality respecting historical reconstructions from linguistic affiliations, American Indian acculturation, and trans-Pacific diffusion. Further, the review listed all eight culture types Steward mentions.

However, to me, responsible book reviewing, as Steward's own reviews suggest [for example, *Am. Anthropologist* **62**, 144 (1960)], means more than mere repetition of contents. The reviewer must make a critique according to his view of the truth. Hence, I called the present classification a "refinement" of the *Handbook* classification because I felt it was already largely suggested there. Hence, too, I made criticisms of two kinds. The first concerned explanatory principles. Similar phenomena suggest using common explanatory principles. If common principles are inapplicable, this, too, must be shown. I felt the authors failed to do either regarding warfare, for example, thus possibly missing aboriginal evolutionary regularities. The second concerned the "unnecessary errors" which referred to *facts*, for which I gave sources, all published in, or before, 1957, 2 years before the book's publication, including one on the Yauru [Le Besnerais, *J. soc. américanistes* (1954)]. "Unnecessary" implies that *standard* interpretations of data exist. Unless these are reinterpreted with relevant rationales and new data, as was

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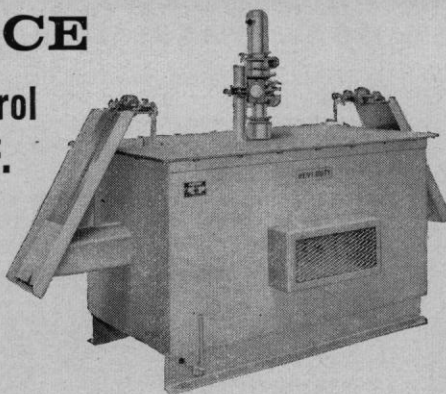
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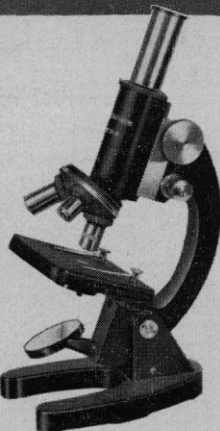
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Letters

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not the case here, one must conclude that "unnecessary" errors were made.

The "unpublished" sources Steward mentions did not concern the errors but concerned cultural materials for which some form of publication existed in, or before, 1957, except for my Yaruro materials (Steward did indeed suggest investigating this interesting group, and I have not forgotten this). Some sources appear to me, despite the authors' belief that South American interpretations "will not be greatly affected by current research" (p. vi), to be changing current views. Thus, Moore [thesis, Columbia University (1957); *Dissertation Abstr.* 17 (Apr. 1957); Columbia Univ. Press, (1958)] and Murra [thesis, University of Chicago (1956); *Dissertation Abstr.* 16, 90 (1956)] have modified our conception of the Inca's state and economy and hence, perhaps, of their evolutionary status. Similarly, Wilbert's comment and Le Besnerais' material further confirm Hohenthal's exclusion of northern South American "marginals" from the "Marginal" category [thesis, University of California, Berkeley (1951), abstracted in *Kroeber Anthropol. Soc. Papers* 16 (1957)], a category the authors largely retain (chaps. 13 and 14, especially pp. 374, 454). The authors, by their own use of still unpublished Warrau data, confirm the importance of such material, since they sharply modify previous treatments [for example, *Handbook* (1948), vol. 3, pp. 869-881; Steward and Faron, *Native Peoples of South America* (1959), p. 245], creating still unsolved classificatory problems requiring speculation (p. 443).

ANTHONY LEEDS

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Food Additives

In a recent issue of *Science* [131, 979 (1 Apr. 1960)], William J. Darby made comment about my recent book, *The Poisons in Your Food*. His remarks went far beyond the accepted bounds of a review.

I understand that it usually takes several months before a book is reviewed in *Science*. In this case, however, Darby's attack appeared only one month after my book was published by Simon and Schuster. The timing was especially fortunate for those who favor the wholesale addition of chemicals to foods. Only a few days after Darby's "review" appeared, the Manufacturing Chemists' Association was able to send reprints of it to newspaper editors