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The "unpublished" sources Steward mentions did not concern the errors but concerned cultural materials for which some form of publication existed in, or before, 1957, except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 The authors, by their own use of still unpublished Warrau data, confirm the importance of such material, since they sharply modify previous treatments [for example, *Handbook* (1948), vol. 3, pp. 869–881;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).

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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

throughout the country. It is also being sent to librarians throughout the country.

Darby, unfortunately, dodged the real problem by carefully avoiding the documented facts in the book, concentrating instead on character assassination and diatribe. This was hardly the treatment one would expect from a top scientist who was assigned to review a book obviously considered worthy of his talents. It was also extraordinary that the book was assigned to the chairman of a group that has enthusiastically supported the use of chemicals in foods, and that has not been divorced from the fortunes of the food and chemicals industries.

Darby charged that "most of the 'authorities' named in the book are the [organic] cult leaders, their gods, or a few true scientists whose work or expressions have been taken either out of context or out of time. . . ." Again, he gave no bill of particulars.

Would he classify as "cult" leaders such men as Malcolm Hargraves of the Mayo Clinic; Arnold J. Lehman, chief pharmacologist of the Food and Drug Administration; David E. Price, assistant surgeon general of the U.S. Public Health Service; Arthur A. Nelson of the Food and Drug Administration; the late Anton J. Carlson of the University of Chicago; David Rutstein, head of the department of preventive medicine, Harvard University; Francis E. Ray, head of the Cancer Research Laboratory, University of Florida; H. M. Sinclair, director of the Laboratory of Human Nutrition, University of Oxford; W. C. Hueper, head of environmental cancer research, National Health Institute—and many others of their caliber?

All of these scientists have warned against various aspects of the food-chemicals problem and are quoted in my book. Could they be accused of "blood-thirsty pen-pushing" and "muck-raking"?

The nature of Darby's "review" becomes apparent when he accuses me of taking "scientific facts" from certain publications which he implies are worthy of contempt. Does he think that merely because a scientific fact is reported in a lay publication such as *Time* (one of the magazines he named) it should be discredited? Readers of the review will note that the publications Darby listed are not cited for scientific content but primarily because of their colorful reactions to various substances used in foods.

In the same vein, Darby charges that the book "is an irresponsible bid for wide sales through sensationalism." This would indicate that he thinks anyone who doesn't agree with him is an opportunist and a scoundrel, and that those who don't share his convictions are incapable of acting in good faith.

For the record, I would like to point out that this book was written a year before it was published. At that time the cranberry and stilbestrol incidents hadn't yet exploded, and there seemed little likelihood of a large sale. I wrote the book without expectation of making money, but out of conviction about the harmful effects of these chemicals after a 3-year study of the problem, and Simon and Schuster made it clear that they were publishing it in the spirit of public service. (Every newspaperman is entitled to be bitten by one cause in his professional career; this was mine.)

In passing, I note that Christian Hamburger, whom Darby derides by innuendo, was only one of many physicians who warned against the dangers of the carcinogen stilbestrol in meat. Among the others are Hueper; Robert K. Enders, chairman of the department of zoology at Swarthmore College and an adviser to the Department of Agriculture and the Department of the Interior; and Carl G. Hartman, director of physiology and pharmacology for Ortho Research Foundation, a branch of Johnson and Johnson.

Could Darby be considered on the side of the consumer when he supports the use of a chemical like stilbestrol that, apart from its biological effects, fattens cattle by making the flesh absorb water?

Darby, of course, completely ignores the central theme of the book—the chronic effect of continued small doses of poison. From his so-called review no one would suspect that this problem exists. But even if he has convinced himself that it does not exist, the Food and Drug Administration still recognizes it and its threat to human beings.

Paul L. Day, scientific director of the Food and Drug Administration, recently stated that there are approximately 1500 chemicals used in foods and that some could be injurious to human beings. "The danger, where it exists, is usually not one of acute toxicity . . . but long-term toxicity, far more subtle and therefore more to be feared," he stated. "DDT, for example, causes no obvious symptoms when ingested in small amounts, but may collect in the liver and do irreversible damage."

Nor is it possible to discount the warning of Hueper that "it is . . . a well established fact that an appreciable and growing number of chemicals, of which a few are known to enter the human food supply, are capable of causing and do cause cancers in man under proper conditions of exposure."

That is what *The Poisons in Your*

Food is about. I ask the reader only to examine the evidence and make up his own mind about where the truth lies.

If, as Darby implies, the problem of poisons in foods doesn't exist, perhaps he could answer these questions: Why did the American Meat Institute recently ask Congress for $15 million for government research to find chemicals whose residues in meat would be less toxic. Why has President Eisenhower demanded a review of the entire food-chemicals subject? Why has Representative King of Utah introduced legislation to establish a committee to learn the effects of food chemicals on consumers? Why has Secretary Flemming been ordered to move behind the scenes to avoid any more cranberry or stilbestrol incidents that cause the public to ask embarrassing questions? Why is the Food and Drug Administration so disturbed about pesticide and antibiotic residues in milk?

I defy anyone of sound mind and good conscience to study the voluminous tables of permissible pesticide residues on fruits, vegetables, and meat and then say that a poison-in-foods problem does not exist.

Darby must know that there is a problem and that it is a serious one. It will not be solved, nor will it cease to exist, through being ignored or through attempts to smear and discredit those who attempt to inform the public about the risk it is being subjected to. The problem is only intensified and people become disenchanted when men of influence try to suppress and distort the truth.

WILLIAM F. LONGGOOD
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I appreciate the opportunity to comment on Longgood's letter in which he discusses my recent review of his book, *The Poisons in Your Food*. In order to determine whether my "remarks went far beyond the accepted bounds of a review," and to judge Longgood's concern for the recency of the review, I took the occasion to refer to the second edition (1957) of Webster's *New International Dictionary of the English Language*, published by the C. C. Merriam Company. This defines a book review as "a critical account of a book, usually a recent book." I believe the review to which Longgood takes exception fits the definition.

WILLIAM J. DARBY
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It is by no means unusual for Science *to review a book within a month of its publication. Occasionally, in fact, we obtain galley proofs of a book and publish a review at the same time the book is published.*—ED.

Meetings

Preserving Our Science Archives

An important conference on science manuscripts was recently held at the Powell auditorium of the Cosmos Club in Washington, D.C. This 2-day general discussion was made possible by a grant from the National Science Foundation to the History of Science Society. The organizing committee was under the chairmanship of Nathan Reingold of the Library of Congress and included Herman R. Friis of the Society of American Archivists; Philip M. Hamer, National Historical Publications Commission; Robert P. Multhauf, Smithsonian Institution; and André C. Simonpietri, National Research Council. With this broad backing, the growing problem of handling scientific archives is receiving needed attention.

In order to understand adequately the main factor, science, in our current culture, it is necessary that there be appropriate preservation, cataloging, analyzing, and reviewing of original documentary material in all of the sciences. It is becoming more important all the time to learn how our scientific ideas are generated, transmitted, tested, and applied. This information is by no means fully available in formal scientific communications. It is to be found in laboratory daybooks; in personal, organizational, and editorial correspondence; and in notebooks, manuscript drafts, organization reports, personal memoirs and diaries, records of interviews, autobiographical notes, sound recordings, pictures, and movies. This great mine of scientific information is often totally neglected. The material is either cleared out by impatient secretaries or administrators, thrown away by unthinking relatives, or destroyed deliberately by overmodest, disillusioned, or frustrated scientists themselves or, sometimes, by jealous pupils or successors.

Of course, much of this scientific documentary material is not worth saving. The pertinent question—What shall we save in our scientific archives?—was well explored by a panel at the conference. This group included A. Hunter Dupree, University of California (Berkeley); Harry Alpert, University of Oregon; Kendall Birr, State University of New York; Hugh Odishaw, U.S. National Committee for the IGY; Ralph Gabriel, American University; Nathan Reingold, Library of Congress; and Luther Evans, Brookings Institution. In the discussion it was emphasized that selection of material for preservation, for comment, or for publication may contribute to myth-making or orthodoxy but is necessary to avoid being overwhelmed by bulk.

All of the speakers stressed the importance of the informal records of scientists and the need to educate both scientists and university librarians and archivists about the desirability of systematically preserving such records.

The historian and archivist of science must be a discriminating artist as well as a sound judge of what is scientifically significant. The role of the archivist in scientific documentation was discussed in a paper by Wayne C. Grover, Archivist of the United States, which was read by Deputy Archivist R. H. Bahmer. The broad scope of the problem of maintaining science archives was indicated by Henry Guerlac of Cornell University, president of the History of Science Society. Richard Shryock, librarian of the American Philosophical Society, showed the value of such archives for historical as well as scientific purposes.

Case studies of research experience in science achives were reported by Whitfield Bell, associate editor of *The Papers of Benjamin Franklin*; by Richard Hewlett, historian of the Atomic Energy Commission; and by Saul Benison of Columbia University. Donald Fleming of Harvard (in a paper read by Henry Guerlac), Karl F. Heumann of the National Research Council, and Oliver W. Holmes of the National Archives discussed proposed research in the problems of science archives and the possible solutions. Bentley Glass of Johns Hopkins University commented on the difficult matter of stimulating individual scientists to take responsibility for the management of their own scientific records and correspondence.

Every scientist has an obligation, as a member of society and as a scientist, to keep accurate records of his scientific work. Such records are usually conveniently kept in his laboratory daybooks. The notebooks may also include abstracts of his reference reading or of conversations, reports, or discussions. These records are supplemented by his correspondence files. All these comprise his personal scientific archives. If the laboratory in which he works is properly managed, this unpublished material becomes part of the laboratory library. As it accumulates it is periodically cataloged, analyzed, and reviewed. What is clearly worthless is discarded, but what appears to have continuing interest is kept. Gradually, what becomes historically significant may be transferred to the library of the institution concerned, whether university, agency, or commercial concern. Some of it may find its way to state historical societies. Wherever it may be deposited, it should be listed in the National Union Catalog of Manuscript Collections, which is being assembled by the Library of Congress. These scientific records may become the basis, like hospital records, for significant case histories, or, if analyzed and reviewed, for statistical treatment in