Author’s Choice

Science has recently been the target of several pot shots from other members of the publishing world. The criticism started, so far as we know, when an alert science writer of the Washington Post and Times Herald discovered that “Strontium-90 in Man III,” the article on page 1249, was in the Science office but had not yet been published. Because the concentration of strontium-90 is a matter of general concern, because the authors had chosen to have their report published in Science instead of giving prior release to the newspapers, and because the interval between receipt of the article and its appearance was longer than the reporter thought justified, he concluded that Science was either negligently slow in bringing important information to the public or was willfully withholding that information.

Under the title “Strontium-90 in U. S. Children, the Report That the AEC is Withholding,” The Nation then went off in two directions. It censured the Atomic Energy Commission and Science for “sitting on” an article of vital public concern, and then argued that the data are so limited that the conclusions lack significance and do not give anything like a true picture of the strontium-90 situation. The New York Times then joined the attack with a summary of The Nation article.

The critics claim that the report should have been given to the general press instead of being sent to Science, a journal that The Nation describes as a “respected but highly technical publication of limited circulation.” (We accept this description, but with amusement point out that Science has twice the circulation of The Nation.)

The claim raises a question that merits clearer formulation than the critics have supplied: Under what circumstances should the traditional customs of scientific publication be followed, and under what circumstances is it preferable to give a scientific report to the public press prior to its appearance in a scientific journal? The research worker has a choice. If he presents his material in an open meeting or gives it directly to the press, newspapers can report it immediately. The material reaches the public quickly—if at all—but relatively unscreened and rarely in sufficient detail to enable other scientists to form their own judgments about the adequacy of the conclusions.

If the report is published in a scientific journal, it does not reach the public as quickly, but when it does, it has survived critical scientific review, has frequently been made clearer as a result of suggestions from the editor or referee, and is published in sufficient detail to enable scientific colleagues to appraise data and methods as well as conclusions.

Custom dictates that the choice be made by the scientist rather than by the institution that supported the work or the editor to whom the account of it is submitted. Both alternatives have their proper uses, but there is not yet agreement on the conditions under which each is preferable. Until agreement is reached on the criteria for each, we will honor the choice an author makes when he sends an article to Science. We will continue to have articles critically reviewed and will publish the accepted ones in sufficient detail to enable other scientists to gain more information than they can normally get from newspaper accounts. Note, however, that when an author wishes to reach the public more quickly, there is an alternative channel open to him.

We think it desirable that both scientists and journalists have a clear understanding of the nature of these options.—D.W.
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Meetings

Facies Model Conference

A discussion concerning sedimentary rocks was held at the Illinois State Geological Survey on 4–5 Nov. 1958, for the purpose of pooling the knowledge and experience of the group concerning three topics: the existence and number of sedimentary associations; the possibility of establishing a model for each association that would emphasize the areal distribution of lithologic units within it; and the exploration of the spatial and sequential relations between the associations.

The participants included Albert V. Carozzi (University of Illinois); Francis J. Pettijohn (Johns Hopkins University); E. P. Potter (Illinois State Geological Survey); John Rodgers (Yale University); W. W. Rubey (U.S. Geological Survey); Raymond Siever (Harvard University); L. L. Sloss (Northwestern University); and E. L. Winterer (University of California, Los Angeles). Under the direction of a chairman, informal discussion was substituted for formal papers.

The first action of the group was to agree that recurring patterns of sedimentation give rise to a relatively small number of fundamental sedimentary associations rather than to an indefinite number of independent and unique patterns in space and time. This concept makes possible the broad application of a relatively small number of general principles to the identification and interpretation of the majority of sedimentary deposits.

The sedimentary association was defined as a collection of commonly associated sedimentary attributes. In the multidimensional space defined by the basic sedimentary attributes, a sedimentary association is simply a clustering of points. Subsequent discussion of the various sedimentary associations repeatedly emphasized the fact that no single variable or attribute is sufficient to define one of these clusters or associations, just as no single characteristic can be used by anthropologists to define a race or by psychologists to define personality. Although gradations between associations were recognized and emphasized at the very beginning, it was decided not to consider them until the basic associations had been defined.

The factors most often mentioned in the definition of a sedimentary association were gross geometry (thickness and areal extent); continuity and shape of lithologic units; rock types (maturity of the clastics and character of carbonates), sedimentary structures, and fauna (types and abundances). Five major associations were outlined.

Before discussion of facies models was initiated, the relations between the various associations were discussed briefly but were not exhaustively explored. Discussion made it immediately apparent, however, that not all transitions between the associations are possible and that some occur much more frequently than others.

A facies model was defined as the distribution pattern or arrangement of lithologic units within any given association. In the early stages of geological exploration, the function of the model is to improve prediction of the distribution of lithologic types. Successful prediction, it was agreed, is the measure by which a geologist’s understanding of a sedimentary association should be judged. Facies models were discussed under sandstone and carbonate subtypes.

Although general agreement was not reached concerning what should be included in a facies model, both basin architecture and the relationship of transport direction to depositional strike played prominent roles in the discussion. The mechanism of quartz and carbonate...
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Ref: Fluorometric Determination of Adrenalin and Noradrenalin in Aqueous Solution
SIDNEY ROSTON, Anal. Chem. Vol. 30, Pg. 1363

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Forthcoming Events

June
5-7. American Gastroenterological Assoc., and American Gastroscopic Soc., annual, Atlantic City, N.J. (H. M. Pollard, University Hospital, Ann Arbor, Mich.)
6. American Acad. of Tuberculosis Physicians, Atlantic City, N.J. (O. S. Levin, P.O. Box 7011, Denver 6, Colo.)
6. International Cardiovascular Soc. (North American Chapter), Atlantic City, N.J. (P. T. DeCamp, 3503 Prytania St., New Orleans, La.)
6-7. American Diabetes Assoc., Atlantic City, N.J. (E. Paul Sheridan, 1 E. 45 St., New York 17.)
6-20. and 27. Recent Advances in Medical Technology, symp., Staten Island, N.Y. (N. Colosi, Wagner College, Staten Island, N.Y.)
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8-9. Isotope Effects in Chemistry and Biology, conf., Lemont, Ill. (Miss B. Litt, Isotope Effects Conference, Argonne Natl. Lab., P.O. Box 299, Lemont.)

8-12. American Medical Assoc., Atlantic City, N.J. (F. J. L. Blasingame, 535 N. Dearborn St., Chicago 10, Ill.)

8-12. Association for Research in Ophthalmology, Inc., Atlantic City, N.J. (L. V. Johnson, 10515 Carnegie Ave., Cleveland 6, Ohio.)


11-14. American Electroencephalographic Soc., Atlantic City, N.J. (J. K. Merlis, University Hospital, Baltimore 1, Md.)


(See issue of 17 April for comprehensive list)

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Letters

Women Scientists

The editorial “Science for the misses” [Science 129, 749 (1959)] leads me to believe that your readers will be interested in some data which I have assembled (with the help of Barbara Drew Atwood). Graduates of seven women’s colleges who are included in American Men of Science were counted, and the numbers were expressed as percentages of total living graduates of the respective colleges. The results follow (the first percentage is for the physical sciences; the second, for the biological): Mt. Holyoke, 0.46, 0.75; Bryn Mawr, 0.48, 0.57; Goucher, 0.40, 0.53; Vassar, 0.34, 0.32; Wellesley, 0.24, 0.24; Smith, 0.14, 0.25; and Radcliffe, 0.14, 0.13. The total is 532/87,012, or 0.61 percent.

Less than 1 percent of the 87,012 alumnae who were living in 1956 are in American Men of Science. Is this an indication of lack of opportunity for women scientists, of less innate scientific ability in women, or of women’s greater interest in home, children, and cultural activities other than scientific?

I believe that both men and women can be grouped into three categories: (i) those who must be scientists at any cost; (ii) those who are not interested and who would never be scientists; (iii) a group intermediate in size—those who, under the stimulus of economic necessity, prefer science to any other field. Most men in both categories (i) and (iii) become scientists. Women in group (i) persist in their study, but most women in group (iii) work as assistants, and so on, until marriage, children, or economic improvement release them.

I sometimes wonder, after many years of teaching college science, if it is wise to urge or to tempt persons, men or women, in group (iii) to become scientists. To give all possible aid and encouragement to those in group (i) might, in the long run, accomplish more.

Anna R. Whiting
University of Pennsylvania, Philadelphia

Supercooled or Subcooled?

Brahm’s article, “How does a raindrop grow?” [Science 129, 123 (1959)], is an excellent survey of our knowledge on this subject. I would, however, like to raise a question about the use of the word subcooled to indicate cooling of water below 0°C. To the cloud physicist and other scientists, subcooled and supercooled are generally regarded as interchangeable. It seems, however, a little unwise and completely unnecessary for scientists to use two words, which, it would seem from their structure, ought to have opposite meanings, to indicate
the same thing. The prefix sub ordinarily is accepted as meaning "less than," and super, as meaning "more than" (for example, subhuman and superhuman). Inasmuch as the word being modified by the prefix is cooled and not temperature, it appears that the word supercooled is preferable to subcooled for indicating excessive cooling.

In reaching this conclusion I examined two standard sources [Webster's New Collegiate Dictionary (Merriam-Webster, 1958) and the U.S. Weather Bureau Weather Glossary (1945)]. Both listed supercooled ("to cool below the freezing point without solidification"); neither listed subcooled.

In view of the above considerations and in view of the fact that so many scientific articles are now read by non-scientists and by foreign scientists, I would like to suggest that serious consideration be given to avoiding the ambiguity that might arise from use of the word subcooled (and, similarly, undercooled) in scientific writing.

HERBERT S. APPLEMAN

Air Weather Service,
Scott Air Force Base, Illinois

The practice, in meteorology, of using interchangeably the words subcooled and supercooled (and also undercooled) when referring to liquid water which has been cooled to temperatures colder than 0°C is unfortunate indeed. To this extent I agree heartily with Appleman. However, I cannot agree that it would be preferable to restrict ourselves to the term supercooled. My reason for preferring subcooled and undercooled is etymological. The point of reference which is implied in the use of all such words (for example, superheated, supersaturated, subsaturated) is that of the equilibrium condition. In this context the prefix sub denotes under, below, beneath, whereas super denotes over, above; therefore it seems preferable to use the terms subcooled and superheated when referring to a phase which has been cooled below or heated above its equilibrium temperature. On etymological grounds the term undercooled is even more desirable than subcooled because it is usually regarded as undesirable to mix words of Latin-Greek and Anglo-Saxon roots.

I consider it unfortunate that most desk-size dictionaries list supercooled but not subcooled. However, I have learned from one of the compilers that the 1959 edition of the Weather Glossary will cite subcooled as preferable to supercooled. I also find many other scientists who feel that it is desirable to make this change in nomenclature [for example, see Johnson, Physical Meteorology, p. 240; Mac-Donald, Advances in Geophysics, p. 245].

Roscov R. Brahaim, Jr.
Department of Meteorology,
University of Chicago

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Letters

Teaching and Research

Impressed by what Caplow and McGee bring out in their book The Academic Market Place [see Science 129, 357 (1959)], Victor G. Fourman de- protests the emphasis on teaching ability and the concomitant stress on publication in the academic evaluation of college professors. With this aspect of Fourman’s argument I most heartily agree. Unless he be frankly engaged as a research professor, no member of a college or university faculty should be advanced on the basis of publications alone.

However, in all the literature I have seen on this troublesome subject there is little or no mention of what seems to me the really critical thing in the whole question of teaching versus research—the one valid reason why department heads, deans, and presidents may be justified in demanding that a faculty man publish. Unless a college teacher is actively engaged in grappling with the unknown somewhere on the forefront of knowledge, he will not bring into the classroom the point of view, the frame of mind, the mode of attack, the general air of the investigator, and these qualities are just what is essential if a teacher is to show, in the presence of the student, by various forms of example, how to go about dealing with the problems in his subject.

These remarks are directed mainly at the problem of college teaching—teaching in the undergraduate world. Graduate work deserving of the name is concerned with educating the student in the ways of original investigation, and to put a noninvestigator in charge of such work is indeed asking the blind to lead the blind. But even here the investigator should be a good teacher, not necessarily in the way that his colleagues in the undergraduate field are good teachers—and in fact there is often a difference—but a good teacher nevertheless.

Now it is publication that is nearly always emphasized in this picture and, unfortunately, not always research; this is one vice of which Fourman justly complains. Quality of publication should of course take first place in any individual evaluation, for the prime value of publication itself, in this context, is the evidence it affords that the author is really an investigator. Over and above all the cant and hypocrisy that have, regrettably, invested much discussion of the matter, the valid case is after all rather simple: A man can hardly go very far in sound research without finding something new, and when he does he owes it to his fellow scholars to make known the results of his work.

And there is also the negative side of the picture. If a teacher does no more than read and absorb the literature on his subject (this he must do as minimal preparation) it is highly likely that in the course of a few years he will go stale in his own thinking.

And finally, all this must probably be qualified by the truism that in a broad field like college teaching all kinds of genius are needed. Many years of association with many kinds of teachers have brought me to realize that there probably are some people who can stimulate students in certain desirable ways without doing any kind of research. But for the reasons given above, in view of the essential fact that the main thing college can do for a student is to show him how to learn and how to think, such teachers should be the exception and not the rule. Men and women who can do a good job of both teaching and research are probably not as rare as many would have us believe.

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I should like to express my strong approval of the article on “Government sponsorship of scientific research” by L. V. Berkner [Science 129, 817 (1959)].

Like many members of the scientific community I have had grave doubts about the wisdom of setting up a federal department of science headed by an officer of cabinet rank. Increasingly, however, I have become convinced that such a department is practically a necessity, if science is to play the role that it must play in any vigorous society today. Berkner’s article provides the most powerful argument that I have seen in favor of such action, and to me the argument seems practically unanswerable.

As regards the scope of such a department I should go along with Berkner’s argument almost entirely except that I should like to see the National Science Foundation included in the proposed department. It is true that its inclusion would modify the structure, and expand the responsibilities, of the department, as envisaged by Berkner. I believe, on the other hand, that the National Science Foundation would probably flourish more vigorously and obtain more adequate support if it were a part of a federal department of science. The foundation has hitherto been almost a stepchild of the government. Its functions are of enormous importance; it should be the government agency with prime responsibility for the promotion of fundamental scientific research in this coun-