Comments and Communications

Editorial Policy

EDITORS of scientific journals have two reasonable ways of dealing with bad work. The best way is to reject it. The second way is to publish it and be ready also, to publish a reader's criticism of it. Too often, at present, neither policy is followed; instead, the bad work is published without criticism, because critical letters are against the policy of the journal.

Some scientists tolerate this situation for the sake of peace; they are not compelled, they say, to take notice of bad work. At the same time, their own work will be delayed in publication, and will lie in strange company in the journal's pages when it finally appears. I do not agree that we should let bad work go uncriticized; fear of criticism is a valuable check on bad work. But if an editor will not allow correspondence, it is all the more his duty to keep his standards high.

However, there are legends in our midst to the effect that some great man was neglected during his lifetime because his theories were so strange that the journals would not publish them. Perhaps a very small proportion of these legends may be true, at least in a modified form. But the belief in them is real, and that seems to be why some editors are too timid to reject bad work—they are afraid that thirty years later it may turn out to have been good after all. It is clear that if one abandons one's judgment to that extent there is nowhere to stop. In short, I can see no sensible alternative to the two policies which I first suggested.

I would like to illustrate my case with an example. No doubt many readers may be able to supply their own, but the following may also provide chemists and botanists with a chuckle.

The main theme of this article (Cooper, H. P., *Soil Science,* 69, 7 [1950]) could be expressed as follows: "The more difficult it is for the ion of a metal to be reduced to the metallic state, the more freely that ion will be absorbed by plants." (This rule actually holds for the sequence K, Ca, Mg, Fe, but breaks down, for example, with Na.) A second theme could be expressed, "A major action of light falling on a plant is to decompose molecules into their elements; for example, light of wavelength 4300–4400 angstroms has just the right amount of energy to decompose magnesium chloride into free magnesium and chlorine."

I have reworded both these themes to make them readable. The second theme appears thus in the article: "The radiant energy absorbed by chlorophyll is approximately the same as the decomposition voltage of certain nutrient salts [the accompanying table lists $FeCl_2$, $CrCl_2$, $ZnCl_2$, $MnCl_2$, $AlCl_3$, and $MgCl_2$]; therefore, it seems logical to assume that absorption of radiant energy by plants may result in reducing certain nutrient compounds to the elemental state."

Both theories imply that the elements exist as such—potassium, iron, magnesium, chlorine—inside the plant, at least temporarily. In reply to a statement that this precipitation of potassium is absurd, the author merely says (p. 29) that "many nutrients are solids in the elemental state and cannot be evolved like those that are gases" (referring here to oxygen in particular). This does not help us much; if the metallic potassium does not evolve like a gas, it ought to be demonstrated all the more easily as it lies glistening within the cell.

The paper includes a table (p. 10) showing that iodide and sulfide are equally strong reducing agents, and that acetate is a stronger oxidizing agent than chlorate.

The editor of the journal introduced this article with an explanatory note saying that, of many referees, all but two were against publication.

It seems only too clear that the reason for publishing this kind of article is the fear that later ages may consider it sensible. We come back then to the choice suggested at the start. Bad work should not be published, but if it is, then the same journal should open correspondence columns in which critics can give it the treatment it deserves.

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Agenda for S.U.N. Commission (I.U.P.A.P.), July 1951

AT THE next meeting (Copenhagen, July 1951) of the International Union of Pure and Applied Physics, the Commission on Symbols, Units and Nomenclature will discuss and may adopt resolutions recommending the universal use of certain units and symbols of interest to many American physicists, including certain symbols and nomenclature for nuclei, and units for electricity and magnetism. Universal agreement on units and symbols is certainly desirable although admittedly very difficult of attainment. It is a policy of the S.U.N. Commission to recommend a usage only when there is overwhelming support for it. The commission therefore invites discussion of questions on its agenda and in particular the questions presented here. Comments and discussion may be sent directly to Professor J. de Boer, Secretary of the S.U.N. Commission,[1] University of Amsterdam, Holland, or to the writer of this notice for transmittal to the commission.

NUCLEI

1. *Symbols for nuclei.* It has been proposed that the

[1] A. Perard, Director of the International Bureau of Weights and Measures. Sèvres, France, is president of the S.U.N. Commission. Other members are: J. de Boer (Amsterdam), E. Griffiths (Teddington), H. Konig (Berne), E. Perucca (Turin), and F. G. Brickwedde (Washington).