Letters

The Incorrect Use of "Base"

The ions of calcium, magnesium, and potassium, especially when they are held as exchangeable ions on colloids, are often referred to collectively as "bases" by workers in soils and some related subjects. This absurd mistake is made by men of some repute and extends to recent textbooks, which must thereby confuse the next generation of students. The offenders may excuse themselves by saying that they want to distinguish calcium and magnesium ions (which produce trivial acidity on interaction with water) from aluminum and ferric ions (which produce much acidity), and that *base* is the only word they can think of for the former.

It is apparently useless in dealing with such people to point to the work of Brønsted of over 30 years ago, so perhaps we should make it easier for them to reform. In fact, no short term exists for "rare-earth cations of charge one or two other than beryllium." "Nonhydrolyzing cations" (which comes closer to the intention) is also too long. The word *alkalon* has been suggested, by analogy with *lanthanon*, which could replace the clumsy "elements of the rare earths." Whether or not *alkalon* is acceptable, it is important that a short alternative term be invented, in order to put an end to the present misuse. Editors might then be bold enough to refuse to print the word *base* when it is used, as it commonly is, to mean "very weak acid."

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Blood Typing of Aged Material

Madeleine Smith's article on "Blood groups of the ancient dead" [*Science* **131**, 699 (1960)], published under the heading "Current problems in research" and summarizing the work done to date in blood typing of aged bone or tissue, gives the impression that paleoserology presently provides a useful tool for research into the history and genetics of ancient populations. Unfortunately, this is not the case, at least at present.

Smith, in summarizing the developments in technique and reports of typings since the beginning work of Boyd in 1933, fails to include in her bibliography the paper by F. P. Thieme and C. M. Otten entitled "The unreliability of blood typing aged bone" [*Am. J. Phys. Anthropol.* **15**, No. 3 (1957)],

which is crucial to the subject under discussion. To summarize briefly the results reported in that paper, tests were conducted on aged samples of bone from individuals of known blood type, on stains made from blood of known type, aged and then dried; and on antigens of known blood type that had been subjected to the action of bacterial enzymes. Forty-seven percent of the 19 bone samples buried over 2 years and tested gave incorrect results. Results were incorrect from 37 percent of blood samples aged three weeks, then dried on paper and tested; from 53 percent of samples aged two months; and from 100 percent of blood samples polluted with 0.5 gram of soil and aged 2 weeks before the dried stains were tested. Each of the known ABO types later tested as another type in at least one case. Furthermore, the established effect which bacterial enzymes have on blood-group antigens was confirmed. In the presence of certain enzymes one antigen may be changed so as to behave in the inhibition test like another type.

A variety of factors may combine or act singly to cause the observed unreliability of conclusions about the blood type of aged material. The inhibition test itself is not a direct and reliable method and gives frequent nonspecific reactions in tests of ABO or other antigens which are not fresh. The frequent unreliability of tests also results from the presence of adventitious antigenic elements indistinguishable from blood-group substances, or from the influence of bacterial enzymes in transforming or destroying the specificity of blood-group substances. Under such conditions, the positive reactions obtained with traditional techniques should be regarded as far from reliable.

Possibly the future will bring direct and reliable tests of the antigens, as indicated by findings suggestive of group-specific features. When this is accomplished, it may then be possible to get reliable evidence which can be used in studying the blood types of ancient populations.

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I must offer Thieme my regrets that in my review I do not quote his 1957 paper. My bibliography was not exhaustive; however, I believe that I did discuss the major points mentioned in his paper and gave some idea of what progress had been made in their study.

I note that on the basis of the results quoted in his paper, Thieme considers my views too optimistic. I would suggest, with all courtesy, that those re-

sults are open to some criticism. Thieme has himself attacked the techniques used but does not seem to make due allowance for technical error in estimating the accuracy of his own results. There is much to question in the methods he describes; for instance, I have discussed elsewhere the inadvisability of using AB serum as a diluent in inhibition tests on bone, and in my own laboratory have found that results obtained with this method are never clear or reliable. Perhaps it is permissible to suggest, also, that percentages based on a series of eight A's, three B's, one AB, and seven O's cannot be used to finalize an argument. Thieme's paper is a significant contribution to the subject, but it would be disappointing if, at a stage in research when all workers are agreed as to the technical inadequacies, further study should be abandoned and a pessimistic attitude taken on such scanty evidence.

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Not Cooking with Gas

After reading in your editorial [*Science* **132**, 113 (15 July 1960)] that it is impossible to cook potatoes by boiling at 11,000 feet, "even boiling overnight," I drove up to Climax, Colorado, a town of about 2500 which flourishes at 11,320 feet altitude. There I made a door-to-door survey, asking housewives how long it takes to boil potatoes in Climax. All of them said the same: *1 hour*; with a pressure sauce pan, 10 minutes.

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This was Darwin's statement, not ours [*see* A Naturalist's Voyage in the Beagle, *Publ. 104 (Everyman's Library, reprinted 1930, Dent, London; Dutton, New York), pp. 310-311*]. *Darwin's relevant comments are that the elevation "was probably not under 11,000 feet, and the vegetation in consequence exceedingly scanty. The root of a small scrubby plant served as fuel, but it made a miserable fire, and the wind was piercingly cold . . . the potatoes, after remaining some hours in the boiling water were nearly as hard as ever. The pot was left on the fire all night, and the next morning it was boiled, but yet the potatoes were not cooked." These conditions are far from comparable to those obtaining in modern kitchens in Climax, Colorado, but at any rate, Darwin was right in principle.*— Ed.