Letters

(Continued from page 263)

(italics theirs). The question of "interesting" data is one requiring a value judgment which will vary from one researcher to another. Personally, I can think of no datum which is quite as interesting as the length of time an animal can survive under conditions of stress.

The final argument which can be raised against the use of the survival-time technique is that some other measure should be found which will yield "the same or more valuable information." Certainly, if another end point were known which would have permitted us to specify the length of time the animals would live, it would have been used. In fact, all biologists would be studying this end point, for it would be a basic clue to the understanding of life itself. Unfortunately, such an end point is, as yet, unknown.

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Reference

1. The following experimental studies also contain additional references on the same topic: R. I. Dorfman, R. A. Shipley, E. Ross, S. Schiller, B. N. Horwitt, *Endocrinology* **38**, 189 (1946); R. I. Dorfman, R. A. Shipley, S. Schiller, B. N. Horwitt, *ibid.* **38**, 165 (1946); "Shock syndrome," *Ann. N.Y. Acad. Sci.* (1952); H. Selye, *Stress* (Acta, Montreal, 1950); H. Selye and V. Schenker, *Proc. Soc. Exptl. Biol. Med.* **39**, 518 (1938); M. Zarrow, *ibid.* **50**, 135 (1942).

Glaciation and Scientific Terminology

In the interest of public education in science and appreciation of science and scientists, I wish to protest the use of terms with long-established meanings to express concepts totally different. I refer to the use, by Brier and Kline, of the term *glaciation* to mean "formation of crystals . . . in supercooled clouds" [*Science* **130**, 717 (1959)]. For at least three generations *glaciation* has denoted "alternation of the earth's solid surface through erosion and deposition by glacier ice" [R. F. Flint, *Glacial Geology and The Pleistocene Epoch* (Wiley, New York, 1947)].

Millions of our lay public are familiar with this meaning. The study of glaciers and Pleistocene history is vitally significant in archeology and oceanography as well as in meteorology and geology. Communication among these interested groups, and with our supporting public, is jeopardized by the inadvertent introduction of multiple, diverse definitions of common and essential terms. As many geology teachers are discovering, the mischief already done by thoughtless or unwise use of terms is making effective teaching and efficient learning increasingly difficult. The progress and welfare of the newer and less developed scientific disciplines, as well as of the older ones, require care and discretion in using and developing our terminology.

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A preliminary search of the meteorological history of the term *glaciation* indicates that the particular usage criticized by Holmes may be a relatively recent revival of this derivative from the Latin word clearly denoting *freezing*. The term is fairly well established (perhaps not irrevocably) in the cloud physicists' vocabulary with reference to the sometimes striking and massive transformation of natural clouds to ice crystals [see, for example, F. H. Ludlam and P. M. Saunders, "Shower formation in large cumulus clouds," *Tellus* **8**, 424 (1956)].

If it is assumed that a term identifying this particular cloud phenomenon is a necessary or logical consequence of the expanding field of cloud physics, usage in this sense seems entirely consistent with a historically proper connotation. Without meaning to disagree with the general thesis of Holmes' communication, we offer the opinion that this example seems to be one where the distinction in the two meanings should be evident from the context in which the term is likely to appear.

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High-Altitude Observation Techniques

Those of us who work in the upper-atmosphere research field were pleased to find R. C. Staley's article "High-altitude observation techniques" in *Science* [**130**, 845 (1959)], since we feel that *Science* should endeavor to cover as broad a spectrum of scientific knowledge as possible. We were disappointed to find a number of errors in the article and feel that it would have been of greater value had it been written from a more authoritative and well-balanced viewpoint.

F. E. Roach, who was responsible for the exciting discovery that the diffuse light from the upper atmosphere appears in large patches in the night sky, called "airglow cells," is associated with the Central Radio Propagation Laboratory of the National Bureau of Standards and not with the Colorado High Altitude Observatory as stated by Staley. One of the coauthors of the article in which the phenomenon was first reported, E. Tandberg-Hanssen, was associated with the High Altitude Observatory during his stay in America. The existence of airglow cells was deduced from data taken under the National Bureau of Standards' IGY airglow program from the Bureau's Fritz Peak Observatory in the mountains above Boulder.

On page 847 Staley implied that the spectrum of atomic oxygen and sodium, the N_2 ion, molecular oxygen and nitrogen, and the OH radical extended to 10μ in the infrared. The long-wave-length edge of this emission is caused by the OH radical, whose spectrum ends at 4.4μ (the 9-8 vibrational transition). Only thermal emission by polyatomic molecules located lower in the atmosphere exists beyond that wavelength. Also, on page 847, Staley refers to the night airglow emission as originating at altitudes as high as 1000 km. This number came from a paper which has since been refuted. This shows the fallacy of taking data out of context from a table without having a thorough knowledge of the field. The airglow experts now agree that the airglow 6300 emission originates in the F region at heights not greater than 275 km.

It is now generally accepted that the very-low-frequency emissions known as "whistlers" are definitely lightning-originated; this is not just an "apparent" fact, as implied by Staley. On page 845, paragraph 1, a statement is made which implies that soft radiation was detected by balloons up to heights of 50 km. As correctly stated in the third paragraph, balloons can only reach heights of about 42 km. In the next to the last sentence on page 845 it is stated that atmospheric pressures can be found from radio studies of meteor trails. Atmospheric densities can be determined in this manner, but not pressures. Beynon's name is misspelled both in the text and in the reference. At the bottom of page 846 it is stated that ion production at a given height is extremely sensitive to temperature changes. The ion-production rate is only moderately dependent on the temperature because of the density change with variation of temperature. A number of statements in the article are controversial ones which have been taken out of context without proper qualification.

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