Letters

Animal Research

As a member of the AAAS I object on moral grounds to those editorial policies which led to the publishing of the article "Effects of differential infantile handling upon weight gain and mortality in the rat and mouse" [*Science* **130**, 629 (1959)] by V. H. Denenberg and G. G. Karas. Their experiments consisted of handling young rodents for various lengths of time and then starving the animals to death. The purpose of this procedure was to determine the stress effects of infantile handling on rats and mice.

In all experiments involving pain or death to animals, (i) the information to be obtained should be worth (in terms of human benefit or real increase in fundamental scientific knowledge) the suffering caused, and (ii) the same or more valuable information should not be obtainable, with a reasonable amount of extra effort, by more humane procedures. These two criteria are practically platitudes.

Since I am not a psychologist I shall not comment on the worth of the experiment. The second criterion given above was not observed and, indeed, the experiment was not only immoral but imprecise. Certainly competent biologists could have suggested tests (anatomical, physiological, biochemical, and so on) for estimating the effect of the stress on the animals that would have provided more precise and interesting data than the one crude fact—how long it took for the animals to starve to death. This more exact information would not have required killing the animals in so cruel and extended a fashion. Note well, it took over 9 days for some of the rats to die. If Denenberg and Karas were not capable of performing these more demanding tests, then they were obligated to ask capable biologists and chemists to assist them, and, of course, to share the authorship of their article.

Very often there is strong pressure exerted on scholars to publish. The editors of *Science* have a moral obligation to see that they do not encourage needlessly painful experimentation by their publishing policies.

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The task of selecting an end point, or dependent variable, in research is always a critical one. At times it is a relatively simple matter since, for some problems, an appropriate end point has been established by theory and empirical findings. At other times it is difficult, for a variety of reasons, for an experimenter to arrive at a decision as to which variable or variables to measure. In selecting an end point it is generally agreed by investigators that certain criteria must be met. These criteria include *relevancy, sensitivity,* and *precision.* It is obvious that the measured variable must be relevant to the problem under investigation; it must be precise in the sense that it is repeatable; and it must be sensitive enough to detect differences induced by the experimental treatments, if such differences exist. Any variable which satisfies these demands is scientifically valid and may be used to study subhuman organisms. When research is conducted on human beings, other criteria in addition to those listed above must be met.

An end point which has been widely used with animals as a measure of resistance to stress has been survival time after the introduction of some stressor agent. One such stressor has been starvation, either by itself or in combination with other agents. Selye discusses various classes of stimuli including starvation, which act as stressors. The reader is referred to Selye and the other cited literature for further information and references (*1*). Thus, the biological literature definitely establishes that survival time following starvation is a *relevant* dependent variable.

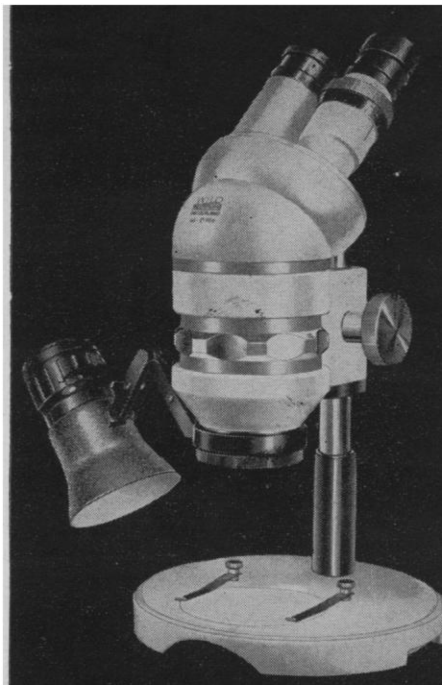
The precision of the survival-time end point has been attested to by the fact that different researchers, working independently, have been able to reproduce one another's results. As was indicated in our paper, Karas and I obtained the same general findings as did two other pairs of researchers. In addition, a partial replication of our data has established the repeatability of the phenomenon. Thus, the survival time measure is a *precise* end point.

The statistical analysis of our data found differences among the various groups well beyond the usually accepted level of significance. These statistical findings have held up in the partial replication. Thus, survival time is a *sensitive* end point.

Since the three criteria specified above have been met, it follows that our measure is scientifically valid and can be used to assess the effects of the experimental treatments.

It may still be argued that the technique is "crude" and that it does not provide "interesting data." With reference to the question of crudeness, it is pertinent to quote here a statement by Selye and Schenker (*1*), who exposed rats to a cold stress under starvation conditions and recorded survival time. In discussing the merits of their technique Selye and Schenker state that "the test is *specific* because it is based upon the preservation of life . . ."

(*Continued on page 266*)



SCIENCE is published weekly by the AAAS, 1515 Massachusetts Ave., NW, Washington 5, D.C. Second-class postage paid at Washington, D.C., and additional mailing office. Annual subscriptions: $8.50; foreign postage, $1.50; Canadian postage, 75¢.

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(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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