experimental result led in a manuscript submitted to the *American Journal of Botany* to the following statement, "The respiration was identical under aerobic and anaerobic conditions." In the experimental situation the anaerobic metabolism was almost certainly alcoholic fermentation, and if we examine the two equations we can see the errors in this statement:

$$C_6H_{12}O_6 + 6O_2 \rightarrow 6CO_2 + 6H_2O \qquad \Delta F = -704 \text{ kg. cal.}$$
$$C_6H_{12}O_6 \rightarrow 2C_2H_5OH + 2CO_2 \qquad \Delta F = -54 \text{ kg. cal.}$$

Since the rate of carbon dioxide production was equal in each case, 3 times as much glucose disappeared under anaerobic conditions, alcohol accumulated, and the rate of energy liberation was but 23 per cent. the aerobic rate. A common term, respiration, led the unwary into calling a difference in energy liberation of over 400 per cent. an identity!

There appears to be objection to the use of fermentation in reference to higher organisms because it has been used uncritically for the metabolism, regardless of its nature, occurring in certain micro-organisms. However, the term has a more precise meaning, as a type of metabolism without reference to the organism in which it occurs. That the later term is justified may be seen from historical considerations. Fermentation initially meant the effervescence of a gas, either in brewing or in a chemical reaction. Pasteur[3] recognized fermentation as a type of energy liberation occurring in certain micro-organisms, but he also recognized that ordinary oxygen consumption could occur in such organisms. Later he[4] broadened the term, fermentation, to include a type of metabolism regardless of the organism in which it was found. On page 267 (*loc. cit.*) he says, "We can even conceive that the fermentative character may belong to every organized form, to every animal and vegetable cell, on the sole condition that the chemico-vital acts of assimilation and excretion must be capable of taking place in that cell for a brief period . . . without the necessity . . . of atmospheric oxygen." On page 273 *et seq.* Pasteur says, "Our theory mentions that all cells become fermentative when their vital action is protracted in the absence of air, which are precisely the conditions that hold in the experiments on fruits immersed in carbon acid gas. The vital energy is not immediately suspended in their cells, . . . consequently, fermentation must result." There is thus excellent justification in the writings of Pasteur for fermentation as applied to higher plants and animals.

Respiration is a very old term in the medical literature, and it meant the inspiration and expiration of air. During the mid-nineteenth century the term was broadened to include the utilization of oxygen in cellular oxidations. This aspect has been well reviewed by Scheer.[5] Pfeffer[6] recognized that anaerobic metabolism occurred in higher plants, and he designated it by intramolecular respiration, though he clearly uses the term as equivalent with fermentation. The writer has been unable to locate the first use of anaerobic respiration, but the term has become widely adopted in our text-books of plant physiology. However, the author does not believe our texts are authorities to be blindly followed when their usage is less precise than that of many research workers in a particular field.

The writer believes we would do well to retain respiration for metabolism of type 1, above, and retain fermentation for type 3. If this is objectionable to some, intramolecular respiration is preferable to anaerobic respiration. Seifriz implies that those who disagree with him (and are unnamed) are not plant physiologists but physiological chemists. I do not see the relevance of Seifriz's classification of scientists to the subject under discussion. Further, Seifriz claims his views are those of plant physiologists, medical physiologists (are they to be denied the use of their term glycolysis?) and bacteriologists. At least one plant physiologist raises his voice in dissent.

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"PHOTOPERIODISM" VERSUS "PHOTOPERIODICITY"[1]

BIOLOGISTS who have had occasion to refer to the literature on the influence of the length of the daylight period on living organisms have doubtless noticed that botanists refer to this phenomenon as "photoperiodism," while most zoologists use the term "photoperiodicity." Assuming that such synonymy is useless and confusing and that there is still a possibility of eliminating it in a field as new as this, it may be well to consider the origin, use and aptness of the two terms in an effort to reach some conclusion as to which of the two terms is preferable.

The term "photoperiodism" was not used by Garner and Allard[2] in their 1920 paper in which they announced the discovery of the influence of the length of the daylight period on the growth and reproduction of plants, but was used in a brief paper[3] which appeared in SCIENCE in 1922. In both this paper and the paper entitled "Further Studies in Photo-

[3] Louis Pasteur, *Ann. de Chimie et de Physique*, 3rd S. 58: 323, 1860.

[4] Louis Pasteur, "Studies on Fermentation, The Diseases of Beer." Eng. ed. London, 1879.

[5] B. F. Scheer, *Ann. Sci.*, 4: 295, 1939.

[6] W. Pfeffer, *Landw. Jahr.*, I: 805, 1878.

[1] Contribution No. 87 from the Science Divisions of the University of Houston.

[2] W. W. Garner and H. A. Allard, *Jour. Agr. Res.*, 18: 553–606, 1920.

[3] W. W. Garner and H. A. Allard, SCIENCE, 55: 582–583, 1922.

periodism"[4] which appeared in 1923 the authors stated that they were "indebted to Mr. O. F. Cook of the Bureau of Plant Industry for suggesting the term photoperiodism, which seems to meet all requirements as to both aptness and simplicity." Following Garner and Allard's lead, other botanists working in this field adopted the term and have used it exclusively in both this country and Great Britain ever since.

The first experimental report on the influence of the photoperiod on animals was that of Marcovitch[5] on plant lice in 1923. However, comprehensive studies of this phenomenon in animals did not begin until 1930, when the first of Bissonnette's numerous papers appeared. Bissonnette was the first to use the term "photoperiodicity." In a letter to the writer he described the origin of the term as follows:

> This term arose when Ross Harrison, editor of the *Journal of Experimental Zoology,* objected to my use of the term "photoperiodism" after Garner and Allard in a paper[6] back about 1930. He said he saw that I had coined a new word "photoperiodism" in place of "photoperiodicity," and I let it go at "photoperiodicity," which now has come to include any periodic or rhythmic process controlled by photoperiods. It is not only reproduction controlled photoperiodically, but includes pelt cycles, plumage cycles in birds and migrations also. So I guess I am responsible for its appearance in biological literature.

Zoologists have in general, but not exclusively, followed Bissonnette in his use of the term photoperiodicity, rather than Rowan,[7] who in 1926 took over Garner and Allard's term. As examples of the exception, Baker in 1936[8] and Rollo in 1941[9] both use the term "photoperiodism."

There seems to be some doubt in Bissonnette's mind as to the strict synonymy of the two terms, as indicated by the above quotation and the following one[10]: "Garner and Allard . . . coined the word 'photoperiodism' for the response of plants to changes in relative lengths of day and night by beginning to bloom, or their exhibition of 'sexual' photoperiodicity." It seems clear, however, from the use of the term by Garner and Allard and other botanists and from the dictionary and encyclopedia definitions, that "photoperiodism" is not a term applied solely to the reproductive aspects of the phenomenon.

As far as the intrinsic merits of the two words are concerned, "photoperiodicity" is probably the better, because "periodicity" is a recognized and widely used term, whereas there is no such word as "periodism." However, it appears to the writer that the term "photoperiodism" should be used by biologists instead of the term "photoperiodicity" for the following reasons: (a) It was the first term proposed. (b) It is much more widely used than the term "photoperiodicity." Not only do some zoologists use it, but all botanists use the term, and to date much more work has been done on plants than on animals in this field. (c) Both the Encyclopedia Britannica and Webster's New International Dictionary list the word "photoperiodism" but fail to list "photoperiodicity," even as a synonym. (d) Although this is perhaps a minor matter, it is somewhat shorter and easier to pronounce than "photoperiodicity."

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[4] W. W. Garner and H. A. Allard, *Jour. Agr. Res.,* 23: 871–919, 1923.
[5] S. Marcovitch, SCIENCE, 58: 537, 1923.
[6] T. H. Bissonnette, *Jour. Exp. Zool.,* 58: 281–319, 1931.
[7] W. Rowan, *Proc. Boston Soc. Nat. Hist.,* 38: 147–189, 1926.
[8] F. C. Baker, *Canadian Entom.,* 67: 149–153, 1936.
[9] M. Rollo, *Bird Banding,* 12: 161–164, 1941.
[10] T. H. Bisonnette, *Quart. Rev. Biol.,* 11: 371–386, 1936.

SPONGE NAMES

IT is to be hoped that there will soon be renewed interest in physiological experimentation using Porifera. To-day the emphasis is on war-winning, but ultimately the lowly animals may again receive attention.

Unfortunately the names of Porifera require some notice. It is common practice for text-book writers to copy the mistakes made in earlier text-books until accuracy becomes an irritating intrusion.

Consider the commonest commercial or bath sponge genus. Linné in his "Systema Naturae" named it *Spongia,* and for exactly a century it was always called that. In 1859 a person named H. G. Bronn decided he didn't like Linné's name, and arbitrarily announced that it should be changed to *Euspongia.* Because Bronn was a professor in Heidelberg University, he was meekly followed in his utterly unwarranted act, and to this day most texts of zoology call it by his presumptuous name. Shall we pick out some American University and say that its professors may change scientific names at their whims? The correct name is still *Spongia.*

The second most common commercial or bath sponge has had a miserable time with names. F. E. Schulze in 1879 described it in general, and named it *Hippospongia,* and so it has been known. Yet it seems that Professor Schulze failed to set up a type specimen, and therefore in 1934 Maurice Burton designated a specimen as type of *Hippospongia.* But alas, Dr. Burton's specimen is of the other genus, that is to say, a *Spongia.* Now we begin to go around in dizzy circles. If Burton has the right to select the type specimen (and this is probable) then *Hippospongia* falls in synonymy.

A partial solution occurred to me. In 1936 I de-