Fundamental nutritive requirements may be considered, in a sense, to be satisfied, from one source or other, so long as life continues—if not from the food, then necessarily from the body.

It is sometimes only through the accumulation of discordant experimental results during the course of years that the effects of failure adequately to recognize the principle to which we call attention become apparent.

In spite of the simplicity and obviousness of the foregoing expressions, the experimenter in the field of nutrition will realize that the point of view is exacting and that its full observance would require very much more knowledge of the details of nutrition than is now possessed by any one.

The experimenter can only strive toward finality of results by planning his rations in consideration of the most that is known as to nutritive values of foodstuffs and nutritive requirements of animals—which, in a few words, and in most relations, signifies that in nutritional investigation rations should be complete, perfect and sufficient, in all characteristics except the single one upon which evidence is sought.

Information which would be most helpful, in relation to the whole subject of measures of nutritive effects and requirements, is detailed knowledge of specific nutritive deficiencies in relation to the utilization of food, and as to the extent of the protection, and the time element in the protection, of the animal, from food nutrient deficiencies, which may be afforded by drafts upon its own nutritive reserves.

E. B. FORBES

PENNSYLVANIA STATE COLLEGE

THE LAW OF EFFECT

THORNDIKE[1] has just come out with an unusually striking demonstration of the law of effect, the principle that in learning a "satisfying after-effect strengthens directly the connection producing it," and Ogden[2] has hurried forward to say that after all a dynamical account of such relationships is preferable and that the retroaction of satisfaction simply means that a total temporal integration is most firmly established when it has completed itself. The time may come when the scientific world can do without the concept of cause-and-effect, or may remake it so that a cause can be subsequent to its effect. However, I do not believe that we are yet forced to any such novel view in theoretical psychology. There are at least four possibilities from which to choose:

(1) Success stamps in the preceding action retroactively—which is what the law of effect seems casually to mean, though it may be interpreted as (3) below.

(2) Success is the consummation of a process that is stamped in as a whole, so that the first part of the process actually is affected by a later part—which is, I think, nearly what Ogden means.

(3) Organization of a content, being potentially learning for ultimate reproduction, leaves a trace which persists to be affected by subsequent events. I believe that this view is really Thorndike's.

(4) "Retroactive facilitation" is actually the absence of subsequent inhibition: all mental organization would lead to memory but for the subsequent destruction of the traces, and success provides conditions for minimal destruction. This view is derived from the experiment of Jenkins and Dallenbach.[3]

The difficulty with the first two views is that, simply conceived, they imply the reversibility of time, the dependence of the present upon the future. The temporal Gestalt has, it seems to me, clear value as a scientific concept, but not in the form of (2). With such sensible and plausible alternatives as (3) and (4), why should we refuse, as Ogden does, to discuss the interrelation and mutual effects of the parts of the total integration?

EDWIN G. BORING

HARVARD UNIVERSITY

IS THE SPELLING AMOEBA SACROSANCT?

I HAVE received two or three blue-pencilled copies of a statement made in SCIENCE of February 10 of the current year (page 170) to the effect that: "Generic names are sacred and their spelling may not be changed to suit the whims of writers. Amoeba can not become ameba." Inasmuch as I am one of the illiterate who have dared to use the spelling "Ameba" in a recent book, presumably to the corruption of the youth of the land, my curiosity has naturally been aroused, and I have followed up the matter a bit bibliographically. I find that the original spelling was *Amiba,* a name given by Bary de St. Vincent in 1822. Ehrenberg admits this in a paper in 1830, although he impiously changed the spelling to Amoeba and uses this form of spelling in his well-known monograph of 1838. Surely Ehrenberg had no more right in 1830 to lay profane hands on what is "sacred" than we have to-day, so the *oe* form should have no better standing than the *e* form among zoological ecclesiastics! But then why use the term at all? Taxonomists have agreed, I believe, in accordance with the "International Code of Zoological Nomenclature" to accept the generic nomenclature set forth in the tenth edition of Linnaeus' "Systema

[1] E. L. Thorndike, SCIENCE, 77: 173–175 (February 10, 1933).

[2] R. M. Ogden, SCIENCE, 77: 240 (March 3, 1933).

[3] J. G. Jenkins and K. M. Dallenbach, *Amer. Jour. Psychol.,* 25: 605–612 (1925). *Cf.* W. S. Hunter, "Foundations of Experimental Psychology," 599–605 (1929).