the case of a yearling the cost f.o.b. Gardiner, Montana, is approximately $55, the animal weighing 1,000 pounds crated; while for a full grown buffalo, which weighs about 2,000 pounds crated, the cost is approximately $70. All animals are shipped express collect. Should the charges in connection with shipment amount to less than the sum remitted, a refund of the balance will be made. Requests should include information as to the number, sex and approximate age of the animals desired.

THE *Journal* of the American Medical Association reports that at a meeting of a class at the Prussian Academy of Sciences in Berlin, a preliminary report was presented by Dr. Richard Walzer, of Berlin, on a recently discovered manuscript by Galen. The manuscript, which is entitled "On Medical Empiricism," furnishes a complete Arabian translation by Hubais of the work that is known in the Occident only through a few scanty fragments. "On Medical Empiricism" is by no means a polemic against empirical medicine but rather a manual in which the eminent Greco-Roman physician for didactic reasons permits the advocates of unsupported theories to present their views, the mode of presentation being that first the dogmatizing physician attacks the empiricist and then the empiricist is given ample opportunity to defend his position. This manuscript, along with the "Subfiguratio Empirica," will be an important source of information with regard to the empirical medicine of antiquity, which, in the main, has been known only through the polemical attacks of its opponents. The manuscript contains, furthermore, some new material on the history of philosophy, especially on skepticism, and a new fragment of the writings of Democritus and of the Cynic philosopher Diogenes. Likewise the antecedents of Hellenistic empirical medicine will be clarified by Walzer's discovery.

DISCUSSION

THE LAW OF MAXIMUM NORMAL NUTRITIVE VALUE

EVIDENCE has been accumulating for many years, but especially during the past quarter century, which is in harmony with the conclusions of the writer and his associates (1) that, in a critical sense, foodstuffs can not be evaluated individually; (2) that net energy values of individual foodstuffs are fundamentally variable, and hence are not practicable standard measures of reference, and (3) that the most nearly logical, single, conventional measure of whole nutritive value is the net energy of the nutritively complete ration.

The time, therefore, seems propitious for the formulation of a principle underlying these ideas—which may be called "The Law of Maximum Normal Nutritive Value" and which may be stated as follows:

> An individual foodstuff expresses its normal and most characteristic nutritive value, for a given kind of animal, under specified conditions governing nutritive requirement, only as it is a part of a ration which is qualitatively complete and quantitatively sufficient, for the conditions existing—except as it may express the same value by virtue of the capacity of the animal temporarily to protect itself from food nutrient deficiency by drafts upon the nutritive reserves of its own body, or as it may express even higher apparent value, under certain pathological conditions, or during undernutrition, by virtue of the protective or body-sparing capacities of nutrients.

It is true that a foodstuff may seem to have a greater value when added to a markedly deficient ration than when added to a more nearly perfect one, if the entire supplementing effect of the combination is, illogically, credited to the supplementary food; but this is only because the supplement is thus credited with more than its own value, since, logically, each component of the more nearly perfect ration should be credited with greater value than it has in the less nearly perfect ration. Manifestly, there is no scientific method of apportioning the nutritive value of a diet among its components.

From the critical point of view of the principle stated foodstuffs can not be compared with each other, as to nutritive value, except with reference to a constituent, or a quality, or a capacity which they have in common—which, obviously, constitutes an incomplete and therefore imperfect basis of comparison.

This position may seem extreme, but is finally inescapable. The experimenter has only to choose between a voluntary adoption of this logically correct point of view, and a forced acceptance of the same attitude, after having been driven from one fundamentally untenable position to another until no choice remains.

A reason for the frequent failure of investigators adequately to observe this principle, in nutritional studies, is apparently the fact that, under many conditions, especially in short-time periods of experimental observation, nutritive deficiencies are, for the time being, completely masked by drafts upon the reserves of the animal body. Experimenters, therefore, often overlook the fact that under some other conditions the same deficiencies would unquestionably affect apparent nutritive values.

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).