plete and quantitatively sufficient. . . ." The question which Mitchell raises is, in reality, "which is the 'normal and most characteristic value' of a foodstuff—that determined by its full potentialities, when it is adequately supplemented, or by its limitations, when fed alone?" The difference is simply one of point of view. It is normal to use feeding stuffs as components of approximately complete rations; they are not commonly fed alone; and I have used the word "characteristic" to mean "representative."

Mitchell states that "the recent developments in the net energy conception, initiated and defended by the Pennsylvania group, have tended to complicate the problem of net energy determinations and perhaps even to discourage those who have hoped to put the conception to practical use in the rationing of farm animals."

There have been no recent developments in the net energy conception, so far as I know. It remains as at first proposed, and it is as unassailable as the law of conservation of energy. But there has been much new light cast upon the subject of energy metabolism, and a searching analysis of the problem of determining energy values, in studies published from this institute—which, however, should be discouraging only to those who adhere to the objective of determining net energy values *of individual feeding stuffs as constants.*

The idea of determining net energy values of rations, however, is worthy of consideration. This is a logical deduction from the work of this institute. I have made this deduction; have advocated the determination of such values, and have enumerated some of their apparent uses in the study of problems in the field of animal production.[3]

In regard to Mitchell's speculations as to the cause of specific dynamic action, the relation of the dynamic effects of nutrients to the combinations in which they are fed, etc., we do not care to comment, especially since the methods of determination of specific dynamic effects, and the measurements of these effects—in the literature—have been so unsatisfactory, in fact, so largely fallacious, in the light of findings of this institute during the past six years, especially as set forth in a very recent paper by Kriss, Forbes and Miller,[4] which places the problem of determining specific dynamic effects of nutrients in a new and vastly improved position.

The new point of view and procedure depend upon Rubner's idea[5,6] of a specific dynamic effect of body substance katabolized, from which follows the hypothesis (Forbes, Braman and Kriss,[6]) of a status of minimum heat production of life in which the energy requirement of the animal would be rendered available without waste of heat—that is, without energy expense of utilization; heat increments (dynamic effects) as usually determined at planes of nutrition below energy equilibrium being less than the true energy expense of utilization by the amount of the dynamic effect of body nutrients katabolized (Forbes, Braman and Kriss[7]); heat increments determined above maintenance, with the heat production of maintenance as the base value, therefore representing the true energy expense of nutrient utilization.

We are free to admit, however, that if—as we have concluded—net energy values of individual foodstuffs are not constants, because of the supplementing effects of food combination, in rations, and other conditions affecting the economy of food utilization, then it is conceivable that, for similar reasons, specific dynamic effects of individual nutrients likewise are not constants. We have unpublished results on conditions affecting specific dynamic action, and a second year's experiments on the subject are in progress.

The recent studies of this institute on specific dynamic effects and their determination afford an improved basis of understanding and procedure from which to investigate this question. In this connection I would propose that it would save confusion to limit the term "specific dynamic effect" to signify the dynamic effect of specific kinds of nutriment, and to use the equivalent term "heat increment" to signify other dynamic effects—that is, those which are not specific of particular kinds of nutriment.

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MORE EVIDENCE ON THE STRUCTURE OF CHROMATOPHORES

A RECENT communication by Herrick[1] regarding the discussion between Sumner and Mast as to the nature of the chromatophore leads me to enter the lists. Like Herrick, I am not concerned with the problem of terminology; I disagree with Herrick, however, on several points of structure and function. The evidence I wish to present in brief, below, is from two types of chromatophore differing from each other and from Herrick's material. Herrick used epidermal melanophore of frog tadpole; my observations were on melanophore of goldfish and chromatophore of squid.

First, Herrick comments that he has "seen no evidence to support the statement of Mast[2] that pigment granules move on definite paths through the cytoplasm." In melanophores of goldfish with Chambers'

[3] *Jour. Nutrition,* 8: 509–534.
[4] "Die Gesetze des Energieverbrauchs bei der Ernährung," Leipzig und Wien, 1902, S. 370.
[5] *Jour. Agr. Research,* 37: 285, 1928.
[6] *Jour. Agr. Research,* 40: 77, 1930.
[7] SCIENCE, March 16, 1934.
[1] SCIENCE, November 10, 1933.

micromanipulator,[3] I have been able to push the pigment granules entirely out of place; they slip back into the same position, however, when the needle is removed. Nor is this the result of purely mechanical pressure—it can be seen in living untouched cells, though less strikingly. Likewise, when the pigment granules are so pushed out of place I have been able to see definite intracellular channels, evidenced by differences in the organization of the cytoplasm, in the place where the granules have been.

In the next place, I have observed that, untouched, the rate of movement of these granules varies as the distance from the central pigment mass. Under stimulation with the needle, the rate is definitely correlated with the distance from the point of application of the needle and the state of aggregation of the parent granule mass. I have seen no jerkiness or variableness in rate of movement that could not be explained as necessitated by the position of the granule in the stream. Nor did I ever see one granule lingering and then overtaking others.

On the other hand, however, living squid chromatophores in tissue cultures[4] will often pulsate without changes in the position of the pigment, which may at such a time be highly diffused in clumps, or scattered, leaving absolutely clear and entirely homogeneous unchannelled spaces in the chromatophore. At other times, when the chromatophore pulsates, the pigment occupies not nearly the whole area of the visible sac-like chromatophore. In this material, then, there is no evidence of definite paths in the cytoplasm nor of regular rate of movement of the granules.

To my mind this situation proves to be just another of those cases in which we tend to attempt to bring under one head a number of phenomena which have similar appearance but entirely different structural or functional nature. The work of Parker and his students, and others, seems to indicate that this is true of the control of the chromatophores: my impression is that investigators may well agree that it is also the case as regards their nature and activity.

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IS THERE A DIGESTIVE CANAL IN CILIATES?

COSMOVICI[1] recently reported seeing a coiled canal running from the cytostome to the cytopyge in *Colpidium colpoda*. Hall and Alvey[2] failed to confirm this observation. Recently I noticed a peculiar thing which tends to confirm Cosmovici's results.

[3] Reported before the Louisiana Academy of Sciences, Shreveport, La., March, 1932.
[4] Reported before the Louisiana Academy of Sciences, Ruston, La., March, 1933.
[1] C. R. Soc. Biol., Vol. 106, pp. 745–749, 1931.
[2] Trans. Am. Micros. Soc., Vol. 52, pp. 26–32, 1933.

While feeding carmine to Protozoa I saw an individual of *C. striatum* which had long strings of carmine in its cytoplasm. The appearance could easily have been caused by the animal's having taken carmine into a digestive canal, such as that described by Cosmovici. This individual entirely lacked typical food vacuoles, although others in the preparation were forming them readily. Another specimen from the same culture possessed both carmine strings and food vacuoles. These two were the only individuals seen to have these carmine strings, despite repeated attempts to find others.

Hall and Alvey criticize Cosmovici's interpretation of his results by pointing out that the canal seen by the latter may well have resulted from the conditions of his experiments and thus not be a normal structure. This is in accord with my own view; I can not yet believe that a digestive canal occurs in normal Protozoa. Nevertheless, the limited observations reported here could not easily be explained by the same type of criticism. It would appear, therefore, that the question of a digestive canal in Protozoa is not yet settled.

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THE BLUE LIGHT IN THE SEA

IN SCIENCE of November 30, 1934, Dr. Beebe wrote a preliminary statement of the results of his descents into the sea in the bathysphere during the summer of 1934. In the course of his investigations of the undersea illumination he made the following interesting observations:

> The day of the first dive was an exceedingly brilliant one, and the surface of the sea very calm. In consequence, light was still visible to the eye at 1900 feet, 200 feet farther than on any previous dive to this depth. At 2000 feet not the slightest hint of illumination was observable.
>
> A problem of color not yet explained is that from 200 feet down, through the spectroscope, the blue is gradually replaced by violet, until at a depth of 400 feet the latter color is dominant. Yet to the eye, at no time of the descent is there any trace of violet or lavender, only the strongest of blues, appearing brilliant long after it has lost all power for actually seeing anything in the bathysphere.

It seems that the blue fluorescence of the eye when subjected to ultra-violet and violet light may be the explanation of the fact that to Beebe the light appeared a blue color, whereas in the spectroscope only violet light was seen. Professor R. W. Wood in public lectures some years ago demonstrated in a very striking manner the "violet haze," as he called it, which was seen by the eye stimulated with ultra-violet