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.

Ellinor H. Behre
Louisiana State University

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.

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.

Arthur N. Bragg
University of Oklahoma

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

[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.

light. In his experiments he used a mercury arc surrounded with black glass, which transmitted mainly the 366 lines of mercury. When this radiation fell into the eye it caused fluorescence of the materials of the eye, with the result that the observer saw a violet haze, which, being in the eye, was not useful for seeing anything. The effect corresponds exactly with the last sentence above quoted from Beebe.

The color of the eye fluorescence is somewhat uncertain. Wood spoke of it as a "violet" haze. W. de Groot[1] arranged an experiment in which various people looked at ultra-violet lines, and presented the results thus: "For 3650, 3345 and 3261 the description which the persons gave of the color was remarkable. They described it as a clear blue whereas the Hg line 4047 and Zn line 4057 were described as violet. It seemed to them as if the succession in the spectrum was reversed. To myself the color appeared more greyish, although with a hue distinctly bluer than that of the recognized 'violet' lines."

It must be remembered that Dr. Beebe was observing the phenomenon on a grander scale than has been produced in the laboratory. The entire scene which he saw through the quartz window of the bathysphere was lighted with the shorter wave-lengths of the daylight spectrum.

To work out the effects quantitatively will require more exact data than are available at present on the absorption coefficients of sea water for visible and near-ultra-violet light and on the visibility curve of the eye extended into the ultra-violet region of the spectrum.

On the basis of the foregoing explanation one is led to wonder about the fluorescence of the eyes of fish. The fluorescence would be troublesome for undersea daylight vision at these depths, and its absence from the eyes of creatures in such an environment would appear to be a favorable adaptation.

E. O. HULBURT

NAVAL RESEARCH LABORATORY,
WASHINGTON, D. C.

UNUSUAL SKY APPEARANCE

A CORRESPONDENT from Vienna, Va., writes that on either January 22 or 23, about 8 o'clock in the evening, she saw a light flashing in the southwest something like lightning. It would flare up several times, then die down. As she watched it, it became very vivid till it seemed to come from a great blazing light, almost a ball of fire. All this time it was moving around the horizon from the southwest until it had almost reached the starting point. She thought it perhaps more vivid when in the north, and that it seemed to be dying away in the southeast. It appeared to be very low, just showing above the foothills.

I myself was driving along Wisconsin Avenue in Washington on the evening in question, with my wife, and we were startled by what was probably the same appearance. It resembled what is called "heat lightning," only that it seemed to be very near indeed and not associated with any noise. The night, as I recall it, was very cold and dry, and I believe on the turn between two contrasting types of weather.

I would appreciate it if any of your readers will suggest to me an explanation.

C. G. ABBOT

SMITHSONIAN INSTITUTION

BOOKS AND LITERATURE

THE MICROPHYSIOLOGY OF NERVE

The Microphysiology of Nerve. By GENICHI KATO. 139 pp., 1934. The Maruzen Company, Ltd., Tokyo, Japan.

IN this concisely written monograph, Professor Kato has presented the results of a series of experiments utilizing his technique for isolating single nerve or muscle fibers in the Japanese toad. Using preparations in which either a single nerve or muscle fiber or both have been dissected free, Kato and his co-workers have abundantly demonstrated that the nerve impulse completely recovers after passing through a narcotized region. The magnitude of the conducted response of a single muscle fiber stimulated through a single nerve fiber is always the same at any strength of stimulus above threshold. Graded, non-conducted muscle fiber contractions localized at the site of small stimulating electrodes are obtainable only with weak stimuli and are unaccompanied by action potentials. Kato compares these responses to peculiar localized contractions occurring as a result of stimulation of a completely narcotized region of a muscle. Both of these types of contraction are found only under restricted conditions as a result of artificial stimuli and are entirely different from the conducted contractions in which there is no deviation from the all-or-none principle.

In observing spinal reflexes, Kato has shown that ipsilateral afferent stimuli are inhibitory to a crossed-extensor reflex (frog) at certain moderate current strengths, while with greatly increased strength of stimulation of the same nerve trunk the effect is summation with the crossed stimulation. This summation is a function of fibers which originate from free nerve endings in the epidermis, whereas the inhibitory effects

[1] *Nature*, September 29, 1934.