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