of the softer materials, except along some of the tributary streams, where it slowly yields to undermining.

The terraces noted by Tolman have, in many places, been eroded into soft, unconsolidated materials, with the result that they are now very indistinct,[6] but nevertheless real. Several levels can be made out in some places. On the eastern, or tableland side of the valley, there are many faint terraces, the highest of which is several hundred feet above the highest similar feature yet found on the western side. This seems to indicate that during the life of the ancient lake, there was a slow, probably intermittent, uplift of the volcanic tableland, or a sinking of the valley floor.

Well-rounded pebbles, usually of dense, flow-banded rhyolite, but occasionally of obsidian, are scattered along the eastern terraces, and are regarded as additional evidence of wave action at these sites in the past. The rounding of the pebbles is farthest advanced on the lower terraces.

There are two lines of evidence regarding the time at which the lake existed. When light conditions are most favorable, terraces can be seen etched into some of the glacial moraines that project from the mouths of canyons in the Sierra Nevada. These moraines were formed during the Blackwelder's Tahoe age;[7] hence the valley floor was deeply covered with water during or subsequent to Tahoe (Iowan) time. Whether or not the same condition existed before the Tahoe epoch, the writer can not say. The closing stages of lacustral history are recorded by domes of lithoid and dendritic tufa[8] along the lowest terraces on the southeastern side of the valley. At one place, the tufa deposit crosses a short, dry wash, some 30 feet deep, evidently eroded during a temporary lowering of the lake level. The tufa in the bottom of the gully shows such slight evidence of corrasion that it is tempting to regard this level as having been finally abandoned by the lake only a few hundred years ago. Under the present climatic conditions, however, water very seldom flows in this wash, and the slight corrasion of the tufa may therefore represent all the time since the Tioga (Wisconsin) epoch.[9] For this reason, the closing stages in the history of Long Valley Lake are tentatively placed in, or shortly after, Tioga time, perhaps 10,000 or 20,000 years ago.

[6] One sharp, distinct terrace in unconsolidated material does exist on the southwestern side of the valley, but it is thought that this has been caused, or at least greatly accentuated, by recent movement along one of the bounding faults of the Sierra Nevada.

[7] Eliot Blackwelder, "Pleistocene Glaciation in the Sierra Nevada and Basin Ranges," *Bull. Geol. Soc. Amer.*, 42: 865–922, 1931.

[8] I. C. Russell, "Quaternary History of Mono Valley, California," U. S. Geol. Surv., 8th An. Rept., Pt. 1, pp. 310–315, 1886–87.

[9] Eliot Blackwelder, *loc. cit.*, p. 881.

Besides giving evidence concerning the approximate time at which the lake disappeared the tufa domes are of interest because they indicate that such deposits can be formed, at least locally, along the shores of a water body that has an outlet.

Although more details are desired, the existence and approximate age of Long Valley Lake are proved. At the highest stage it may have been about 17 miles in length and 8 miles across at the widest place. The greatest depth attained could not have been less than 250 feet.

EVANS B. MAYO

CORNELL UNIVERSITY

DO CHROMATOPHORE WALLS CAUSE MOVEMENT OF PIGMENT GRANULES?

IN a recent reply to Sumner by Mast,[1] there is expressed an opinion that " . . . a chromatophore is somewhat like a branched, heavy-walled rubber tube closed at the distal ends of the branches and at the other end joined to a heavy-walled rubber bulb filled with pigment granules suspended in a fluid, and that movement of the granules in the chromatophore is due to action in the heavy walls which surround the cavity containing the granules." A further statement is, " . . . the distribution of the granules in a chromatophore is due to action in the wall which surrounds the cavity containing the granules . . ." With this conception of the method of bringing about changes in the amount of pigment exposed to view in chromatophores, reasons are given why one should or should not use certain terms relative to these changes.

I have not been particularly concerned in this controversy over terminology; but if the use of terms is to be determined on the basis of the functioning of the chromatophore or parts of these cells, I believe there should first be fairly common agreement among investigators as to what actually takes place during "distribution" and "congregation" of the granules of pigment.

In my study of chromatophores, I have been impressed by the unusual thinness of the walls. Although the terms "thick" and "thin" are only relative and the actual measurement of the thickness of the walls of chromatophores has not been done, as far as I know, I see no reason for calling the walls thick. Furthermore, I have found no reason for believing that the walls of the chromatophore cause the pigment granules to move. In my study of the epidermal melanophores of frog tadpoles in living, uninjured tissue, in which observations were made continuously over periods of hours and with magnifications up to 1,000 diameters, the employment of an ocular micrometer did not reveal the slightest change in diameter

[1] SCIENCE, March 16, 1934.

of any part of the pigment cell during the distribution or congregation of the pigment. The movement of the pigment as a whole or of the individual granules, whether going into or out of the branches of the cell, did not suggest to me that any part of the melanophore was acting in the capacity of a rubber bulb.

As the pigment moved out into the branches, some granules usually reached the extremities of the cell before others had little more than begun to move; and, as the process continued, the cell branches received more and more pigment. In this movement of the pigment, a few granules might move more rapidly than others for a time, only to be overtaken by those that were following. No one granule appeared to maintain a constant rate of movement. Similar movements were observed when the pigment moved out of the branches, some granules lingering for a time and then by more rapid movement overtaking others ahead. I have seen no evidence to support the statement by Mast[2] that pigment granules move on definite paths through the cytoplasm.

EARL H. HERRICK
LOUISIANA STATE NORMAL COLLEGE
NATCHITOCHES

PANCREATECTOMY—A WARNING[1]

THE nutritional condition of an animal when pancreatectomy is performed determines to a large extent the length of survival if no insulin is given. Chaikoff, Macleod, Markowitz and Simpson[2] have emphasized the longer survival of thin dogs than fat ones after this operation. The Allen treatment for human diabetes probably owes its success to the inanition of the patients.

In studies of the relation between other endocrine glands and the pancreas, animals frequently submit to one operation prior to the pancreatectomy. This preliminary operation often leads to a state of undernutrition which is conducive to long survival following removal of the pancreas. At least this is the interpretation of Ring and Hampel (unpublished) concerning the occasional longer survival of their cats, in which thyroidectomy or unilateral adrenalectomy preceded extirpation of the pancreas. Even the better results obtained by depancreatizing animals in two stages depends in part upon this nutritional factor.

Work on adrenalectomized diabetic cats has recently been reported by Long and Lukens.[3] Their results are strikingly similar to those which Ring and Hampel obtained on diabetic cats with Eck fistulae. One such animal survived for 17 days after the withdrawal of insulin. Near the end of this period the glycosuria disappeared and convulsions were a frequent occurrence unless food was kept constantly by the animal. These convulsions were promptly relieved by glucose ingestion. Animals in better nutritional condition survived a shorter time. The writer was unable to maintain the weight of the animals with Eck fistulae, so that he has never felt justified in drawing any conclusions from this work.

G. C. RING

THE TROPICAL RAT FLEA IN THE INTERIOR OF THE UNITED STATES

IN his book "Insects and Disease of Man," Fox (1925) states that the tropical rat flea, *Xenopsylla cheopis*, is not known to occur in the interior of the United States. We have been able to find no reports of this flea from this part of the country (Iowa), although rat flea data taken by members of the United States Public Health Service have shown that it commonly occurs on rats at our seaports.

In February of this year we took one specimen which we identified as *Xenopsylla cheopis* from a rat shot on the dumping ground of the city of Ames. During July we shot six more rats on the grounds and took from them 259 fleas. Examination of these showed that 257 of them were *Xenopsylla cheopis* and 2 were *Ceratophyllus fasciatus*. We are indebted to Dr. H. E. Ewing, of the Bureau of Entomology, United States Department of Agriculture, to whom we sent a male and a female flea, for confirming our determination of *Xenopsylla cheopis*.

Thus it appears that the tropical rat flea is well established in the interior of the United States. Whether it will persist here is problematic. Rothschild (1910) states that although the insect is practically cosmopolitan, it can not apparently flourish in temperate and cold climates, but Fox and Sullivan (1925) found it as far north as Boston every month of the year from December 1, 1922, to November 10, 1923.

As others have pointed out, the presence of the flea host in itself does not constitute a menace to a community, for bubonic plague has not made its appearance along the Atlantic seaboard where *Xenopsylla cheopis* occurs on rats in considerable numbers. If, however, bubonic plague should be introduced into the Middle West through shipping on the Mississippi River, or otherwise, there now exists the possibility of its becoming endemic among rats, for its vector seems to be well established.

ROBERT L. ROUDABUSH
ELERY R. BECKER
IOWA STATE COLLEGE

[2] SCIENCE, November 10, 1933.

[1] From the Laboratories of Physiology in the Harvard Medical School.

[2] I. L. Chaikoff, J. J. R. Macleod, J. Markowitz and W. W. Simpson, *Amer. Jour. Physiol.*, 74: 41, 1925.

[3] C. N. H. Long and F. D. W. Lukens, SCIENCE, 79: 569, 1934.