writer's attention was called by Merriam to a very interesting paper by E. H. Barbour and C. B. Schultz[2] in which they describe a new genus and species, *Proantilocapra platycornea,* Barbour and Schultz, from the Lower Pliocene in Cherry County, Nebraska. However, no comparison or reference to the closely related genus *Sphenophalos nevadanus* Merriam was made. *Proantilocapra,* as figured, shows morphologic characters close to those of *Sphenophalos.*

The horn-cores as to size, attitude in relation to orbit and frontal and in cross-section are much like those of *Sphenophalos.* The latter differ in being bifurcate, a character common to *Sphenophalos* from the Great Basin Province.

The occurrence of the new genus *Proantilocapra* in the Lower Pliocene of Nebraska adds important data in solving this problem.

In the original description and occurrence of *Sphenophalos,* Merriam discussed the systematic position of the genus and recognized, in the then available material, characters that indicate a close relationship to the pronghorn antelopes.

Specimens of the same species collected later in other Great Basin localities of middle and earlier Pliocene studied by Furlong[3] confirmed this view.

A more detailed report on the new genus *Proantilocapra* by Barbour and Schultz and other representatives of the same species will be welcome.

E. L. FURLONG

CALIFORNIA INSTITUTE OF TECHNOLOGY

CONCERNING REASONING

THE twenty-four canaries fly freely in the laboratory. One of them, Billie Burke, alit on the top of the window shade, slipped, slid down between the window and the shade, came out at the bottom, parachuted to the concrete floor. That accident the canaries have many times each day, but Billie was unlucky, caught her wing and broke it. There have been three injured wings in the seven years, but this wing healed badly and the bird will never fly.

While she was convalescing I put her water and food near her on the floor. The others have their food on the top of the zinc-top table. She disliked eating down there alone, and the first realization I had of the bad healing was seeing her pathetically inadequate efforts to spring to the top of the table. This spring was with her legs. She did not use her wings at all. The spring carried her in the beginning to a height of three or four inches, later to a height of seven or eight.

Presently it occurred to me to build her a spiral staircase of sticks around the one table leg, the sticks two inches apart. This I did at night. All the next day she paid no attention to the sticks. The other canaries paid no attention either, at least did not regard the staircase as a way to the top of the table, though they might perch on a stick or hop up two or three. So the second morning I decided to put out Billie's water but not her food. I watched all day for something to happen. Nothing did. Toward evening I tied leaves of lettuce to the ends of the sticks. Still nothing happened. The other canaries ate on the top of the table where they were used to eating, and Billie stayed on the floor and did not eat. Nevertheless, the third morning when I arrived at the laboratory she was on the top of the table, and from then on has lived most of her life on the top of the table.

But in a week or two I began to realize that even this was not satisfactory. Birds like to sleep high, and nights when they were all getting ready you could see Billie cock her head to watch the others. Therefore, what I did now was find a small tree, stripped it of all but a few of its upper branches, mounted the tree on the end of the table, tacked a staircase of sticks around the trunk, and in thirty seconds she was perched at the top! I had no experimental intention, of course. I was only wanting to get her up there, and there she was. It had taken her two days to see how to get to the top of the table, and thirty seconds to see how to get to the top of the tree. Yet the ladder round the tree in general appearance was certainly sufficiently different from the ladder round the leg of the table. How she originally got to the top of the table I do not know. It may have been by a succession of blunders, or it may have been blunder plus recognition, as is the more likely, considering that there were nine steps. But assuredly it would seem that she recognized the essential characteristic of her first experience, and used it instantly to a new end.

GUSTAV ECKSTEIN

UNIVERSITY OF CINCINNATI

SPECIAL ARTICLES

GRAVEL CUSPS ON THE CALIFORNIA COAST RELATED TO TIDES

WHILE living in Santa Monica during the winter of 1933–34, the writer became interested in the triangular masses of gravel and cobbles, called "cusps," which were found on the beach at the mouth of Santa Monica Canyon. It was observable that these cusps were subjected to great variation in size and number.

[2] E. H. Barbour and C. B. Schultz, *Amer. Mus. Nov.,* No. 734, pp. 1–4, August 3, 1934.

[3] E. L. Furlong, *Carnegie Inst. Wash. Publ.* 418, pp. 27–36, 1931.

They were not very conspicuous till after a large storm around New Year's Day, but from then on they began alternating between wide-spread development and almost complete extinction. Almost daily observations of their changes were obtained for several months in the later winter and spring.

Effect of Tide and Waves

Examination of the accumulated data showed a definite two-weeks cycle of cusp formation and destruction. Surprisingly enough, the occasional storms proved to have only a minor effect on the cusps and it appeared that the main factor in their change was the tides. During the neap tides, when the range between high and low water was least and when there was only one principal tide a day, the cusps became well developed both in size and in number. During the spring tides, when the range was large and there were two main tides a day, the cusps were either cut away or more commonly buried by sand. In general, the beach was eroded during the cusp-forming period and was built up with sand during the cuspless period. The same general relations were found at other beaches along the coast in the same vicinity.

In order to evaluate the effect of the tide and of the waves, a graph was prepared (Fig. 1), which shows with arbitrary values: (1) the size of the waves (determined roughly each day); (2) the range of the tide during the preceding 24 hours and 52 minutes, and (3) the area covered by cusps. It appeared that large waves tended to eliminate the cusps in the same way that large tidal ranges produced this result. Accordingly, during some neap-tide periods the cusps did not develop to any great extent because of the off-setting effect of the waves. In order to show the relationship directly, the curve for the waves and the tide range is given in inverted form. Since the tides were thought to have approximately twice the influence of the waves, a curve was drawn combining the two influences in that proportion, and it will be noted that this curve follows quite closely to the curve of cusp area.

Causes of These Influences

The influence of the tide on the shores is presumably chiefly through the longshore currents which it produces. So far as could be told, these currents run from northwest to southeast (or west to east) during the flood tide and are not observable during the ebb tide. The winds cause currents in the same direction much of the time, so that the greatest currents are developed during spring tides when the west wind is particularly strong. Under these conditions the sand is driven along the coast and builds up the beaches covering up the gravel cusps. Evidently during periods of slack currents the waves come in directly onto the coast and cut away the sand, re-exposing the cusps. Johnson,[1] quoting Palmer, called attention to the fact that cusps are formed by waves "driven directly on the beach" and are destroyed by oblique waves. Somewhat the same principle is illustrated at Santa Monica.

Need of More Observations

It would be interesting to know whether the tidal cycles are important along other shores, either in connection with cusps or in the building up and cutting away of the beaches during neap and spring tides, respectively. So far as the writer knows, these cycles have never been reported in the literature. This may be because of failure to make enough observations or because conditions along the coast of Southern California are particularly favorable to the development of a tidal cycle, due to scarcity of storms and due to the diurnal tides which are found along the Pacific coasts, in contrast to the Atlantic, and make for greater differences between neap and spring tides when the diurnal tides correspond with the neap tides. Furthermore, it should be admitted that since the observations recorded here were not very protracted, it is conceivable that the tidal cycle which is suggested is purely coincidental. It is to be hoped that some scientists vacationing at the beach will have

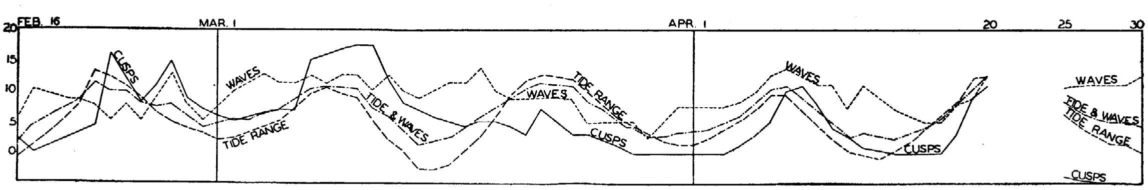

Fig. 1. Illustrating the relation between the area of cusps and the tidal range and wave size at the same times. The curves for wave size and tidal range are given in inverted form so as to correspond more with the cusps. The observations were made daily during most of this period. The curve given as "Tide and Waves" is a combination of these two factors weighting the tide range as twice that of the waves.

[1] D. W. Johnson, "Shorelines and Shore Development," p. 457, New York, 1919.

the opportunity to gather more information on the subject.

FRANCIS P. SHEPARD
UNIVERSITY OF ILLINOIS

DEHYDRATION AND INFILTRATION

RECENTLY, in a series of papers too numerous to be cited here, there has been much discussion regarding the composition of fixing fluids designed to secure specific effects, mainly with plant tissues. The writer has done a great deal of similar experimentation, but the results have invariably been rather erratic and never wholly satisfactory. It was finally concluded that the trouble was caused by the fluids used following fixation and preceding infiltration with paraffin or balsam, and not to the fixing fluids themselves nor to the physiological condition of the tissues.

The tissues of all living organisms contain water and possess the capacity for absorbing water. The water contained in, say, vacuoles presumably does not react towards dehydrating fluids exactly as does the water in cell walls or in nuclei. The nature of the combination between water and the particular structure containing it varies according to the latter and may conceivably be altered during fixation.

Most technicians appear to take it for granted that the main if not the sole criterion of successful fixation is the apparent lack of changes during dehydration and infiltration. The writer, on the contrary, has become decidedly of the opinion that no blame can be placed upon the fixing fluid if disaster results during the post-fixation stages. In other words, "successful" dehydration and infiltration is not dependent upon "successful" fixation: the two processes are mutually exclusive.

The fluids commonly employed for dehydration are powerful desiccators. From the observation that the most satisfactory cases of infiltration resulted when only the free and not the combined water was removed from the tissues, it was apparent that it is possible to produce dehydration without causing desiccation. To state the case in another way: good infiltration occurs when the water is replaced, but the water-absorbing capacity of the tissue is not destroyed. The use of a fluid immiscible with water following, and in combination with, a dehydrating fluid which in itself produces desiccation only makes matters worse. Tissues become excessively hardened, plasmolysis occurs, and in the case of embedded material "cracking" results during sectioning on the microtome.

It seemed that what was required was a fluid miscible in all proportions with water, ethyl alcohol (in order to care for fixing fluids containing alcohol), paraffin and balsam and which would replace all free water yet cause no alteration in the water-absorbing capacity of the tissues. Practically all fluids in common use are automatically eliminated from consideration, as they fail to conform to one or more of these specifications. Normal butyl alcohol comes nearest to conforming, but the anhydrous product is miscible with water to the extent of not more than 8 per cent. by volume; experience has already demonstrated that it was very unsatisfactory with many plant tissues, the reason being that desiccation had taken place. Dioxane, first suggested by Graupner and Weissberger[1] for use on animal tissues, and tertiary butyl alcohol[2] now appear to be the most promising reagents. Each is miscible in all proportions with water, ethyl alcohol, paraffin and balsam or xylol-balsam, as well as with most of the fluids in common laboratory use (except that dioxane will not mix with pure glycerin unless 10 per cent. water is added). An absolutely anhydrous product must, of course, be used to insure perfect dehydration.

Sufficient experience with both dioxane and tertiary butyl alcohol has already been obtained to indicate that, when properly used, these fluids: (1) Eliminate the use of ethyl alchohol and all fluids commonly employed to precede infiltration with paraffin or balsam. (2) Give perfect preservation of the fixation image. Plastids, mitochondria and similar cell constituents are preserved with remarkable fidelity, whereas such things are generally dissolved or otherwise rendered invisible by the usual combination of absolute alcohol and clearing fluid. No plasmolysis nor shrinkage results. (3) Remove all free water but leave the water-absorbing capacity quite unaltered, which makes it possible to soften hard woody tissues by simply exposing one cut end of the piece of embedded material to the action of water for a short time. (4) Produce no hardening whatever, thus permitting perfect microtoming. (5) The most delicate materials, such as *Volvox,* fern prothallia, moss protonema, filamentous freshwater and marine algae, and fungal mycelia, intended for whole mounts, can be transferred from water through either of the fluids directly into xylol-balsam diluted with that fluid and this evaporated down to mounting consistency with no shrinkage, collapse or hardening. Stains are perfectly preserved.

In the experimental work, a variety of fixing fluids

[1] *Zool. Anzeiger,* 96: 204–206, 1931. The writer, however, is indebted to Miss Enid A. Larsen, of the School of Biology, Stanford University, for information concerning, and the experimentation with, dioxane.

[2] The dioxane was obtained from the Eastman Kodak Company (#2144 1,4-dioxane), the tertiary butyl alcohol (a Shell Oil Company product) from the California Botanical Materials Company of Palo Alto, in whose laboratories it is being extensively used. The writer is not aware that tertiary butyl alcohol has previously been used for the purposes noted above.