Undulations in clay deposits.

A ditch about two feet deep, and running nearly east and west, on the grounds of this college, presents a profile as if the clay (which is of unknown depth) had been shaped into undulations, with crests from eight to fifteen feet apart, and then covered unconformably by the sandy soil, which over the crests is about two or three inches deep, and in the troughs about two feet at most. The 'strike' of the crests is nearly north and south. This peculiar formation has been observed over a large area of country in this vicinity. A surface peculiarity is the occurrence, at intervals of one or two hundred yards on the prairies, of low mounds a foot or two high, usually covered with dewberry briers. West of this place, in Milam and Williamson counties, the nearly level prairies are mammillary, with slight elevations eight or ten feet apart, presenting the appearance of old tobacco or potato hills on a gigantic scale. These appearances, visible from the cars, excite the curiosity of all who observe them; and a plausible theory of their cause might not only gratify this, but lead to some very important discoveries in dynamical geology. For these reasons I desire to present this problem to your geological readers; and, if it has already been solved, my apology for ignorance of the solution must be that I am not a geologist. H. H. D.

Agricultural and mechanical college of Texas,
College Station.

'A singular optical phenomenon.'

Having made the phenomena of binocular vision a special study for many years, I was greatly interested in the letter of 'F. J. S.' in *Science*, No. 57. But I confess I do not quite understand it. I can but think that the phenomenon he describes is only an example of '*phantom image*,' produced by binocular combination of similar figures of a regular patterned field — in this case, the squares of the coarse screen. But in that case the image ought not to be inverted nor enlarged. As to the inversion: if, as I suppose, your correspondent imagines it inverted only because it moves with the head, he is probably mistaken. There is no optical law by which an inverted image could be formed under the conditions described. The movement of the image is simple parallactic motion. The point of sight being the centre of parallactic rotation, if the image be nearer than the object, the motion will be in the same direction as that of the head of the observer; but, if the image be farther off than the object (a far more difficult case), the motion of the image will be opposite that of the head.

There still remains, however, the enlargement of the image. This is incomprehensible to me. It ought to be diminished in exact proportion to its nearer distance. Neither can I at all understand what is said about the phenomenon as seen by a near-sighted person. I should be glad if your correspondent would repeat and describe more accurately the phenomenon; for there are no phenomena more illusive, and requiring more practice to understand, than those of binocular vision. If I am right as to the nature of the image, it ought not to be seen with *one eye only*.

The subject of phantom images is fully explained in my little volume on 'Sight,' pp. 107–119.

JOSEPH LECONTE.

Berkeley, Cal., March 18, 1884.

The possible origin of some osar.

The writer does not profess to have an extensive acquaintance with these problematic structures; but, a few examples having been discovered in his researches in Dakota, the subject of their origin has been thrust upon him.

From several considerations, which need not be given here, it seems extremely improbable that the quaternary glaciers in that region bore, either on their surface or in their depths, any considerable amount of *débris*, at least nothing coarser than dust upon their surface, and perhaps gravel in their lower portions. How, then, can steep meandering ridges nearly continuous for miles, ten to thirty feet in height, running nearly at right angles with a great moraine, and much more stony than the surrounding surface and the general mass of the till, be explained?

The following hypothesis is offered for criticism. Given a sub-glacial stream, or a super-glacial one, which, near the edge of the ice-sheet, has cut an ice-cañon through to the ground moraine: the presence of the ice-cliffs on either side would tend to force a plastic body like the till toward the stream, and cause it to rise underneath the stream, like the 'creeps' frequently occurring in coal-mines or deep cañons. Now, if the streams have only a velocity sufficient to wash out and carry off the finer material, the bowlders and gravel will be left in excess, and in ridges along the line of the stream. Of course, stratified beds of sand and gravel would form a considerable portion of hills produced in this way, just as in the case of those formed according to the usually accepted theories. Similar breaks and lateral repetitions of the ridges might result according to either of the theories.

It seems, moreover, not improbable that some of the re-entrant spurs of terminal moraines may have begun in this way; the cañon developing into a notch, and giving rise to lateral flowage of the ice, as well as increeping of the till. J. E. TODD.

Tabor, Io.

Osteology of the cormorant.

In late numbers of *Science*, several communications have appeared from Dr. Shufeldt (ii. 640, 822; iii. 143) and Mr. Jeffries (ii. 739; iii. 59) on the 'osteology of the cormorant,' and especially on so-called 'occipital style;' and complaint is made by Mr. Jeffries (iii. 59) that Dr. Shufeldt 'does not mention the nature of the bone' in question. Neither gentleman seems to have been thoroughly acquainted with the literature of the subject; and inasmuch as both are members of a committee of the American ornithologists' union, appointed to investigate the anatomy and physiology of the birds, they may be thankful for a reference to a special paper on the anatomy and functions of the bone in dispute. It is to a memoir by William Yarrell that I refer. Yarrell designated the 'occipital style' of Shufeldt as the 'xiphoid bone,' and in 1828 communicated to *The zoölogical journal* an article (iv. 234–237, art. xxviii.) 'on the use of the xiphoid bone and its muscles in the corvorant (Pelecanus carbo Linn.),' which is accompanied by two figures on plate vii. (figs. 5 and 6) illustrating the skull, with the xiphoid bone, and the muscles in relation with it and the lower jaw. The development of the peculiar bone is correlated with the weakness of the lower jaw; but for further information those interested must refer to *The zoölogical journal*, where they will likewise find references to the views of other authors.

Lest *Science* or myself should be charged with making or overlooking a typographical error, I beg to add that 'corvorant' is the substitute for cormorant, adopted by Yarrell, probably from a false or confused idea as to the etymology and history of the word. THEO. GILL.