Mr. Mellard Reade states that "Blown-sand of sand-dunes is not distinguishably more worn than the sand of the shore from which it is derived." I do not know what particular dunes are referred to, but I must say that my experience is quite the reverse of this. Blown-sands of deserts and dunes procured from many parts of the world have never yet failed to provide me with characteristically-rounded grains in great abundance.

Of course much will depend on the particular spot from whence samples are procured. Grains freshly blown up from the shore on to the surfaces of dunes would not become appreciably rounded until they had travelled some distance inland, and had been whirled about in hollows and depressions for some length of time. The places to find rounded grains of blown-sand would be, therefore, in such depressions some distance from the shore, and I feel sure that anyone collecting samples from such spots will confirm my opinion. It must be clear that the action of the wind *in time*, by hurling the grains one against the other, would produce (in the case of quartz) sphericity through abrasion, and numerous sands prove this.

A fact that does not appear to be known in connection with grains of blown-sands is that many of the grains exhibit the *mastoid* markings so frequently seen on flint pebbles, and these markings clearly show with what force the grains have collided. I have never found these markings on wave-borne sand grains, simply because in the denser medium—water—the grains do not collide with sufficient force to enable them to become developed. Some years ago, at St. Agnes, in Cornwall, I found a deposit of white quartzose sand (probably Pliocene), the larger grains of which were covered with these markings, and these alone, I considered, pointed to the Eolian character of the deposit.

Before we can base any conclusion—as to the *locating agent* of a particular deposit—upon the rotundity of certain sand-grains contained therein, we must satisfy ourselves that such grains were not already rounded and polished in the parent rock from which they were derived.

In reference to Mr. Pittman's letter on "Flexible Sandstone," it does not appear to have been noticed that nearly thirty years ago Dr. Wetherell published an opinion that the flexibility was due to the grains being "arranged in definite groups separated from one another by intervening cavities."

CECIL CARUS-WILSON.

BOURNEMOUTH, *July* 11, 1892.

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## SUBTERRANEAN EROSION OF THE GLACIAL DRIFT, A PROBABLE CAUSE OF SUBMERGED PEAT AND FOREST-BEDS.

SIR,—In December last a paper under this title was read before the Geological Society by Mr. William Shone, F.G.S., and more recently a *resumé* of it was given to the Chester Natural Science Society. The author described a section at Upton, near Chester, cut by two streamlets through Boulder-clay resting on a considerable thickness of sand. The clay sloped towards the sides of the streams,

and Mr. Shone stated that the percolation of water along the sand, towards the streamlets, had caused a subsidence of the clay to the amount of thirty feet. Not having seen the section I can give no definite opinion upon it, but in the paper referred to Mr. Shone endeavours to explain the subsidence of the Peat and Forest-beds at Ince, on the south shore of the Mersey, and on the west coast of England, as having been caused by the subterranean erosion or denudation of the underlying beds.

Mr. Shone gives the section of the Peat and Forest-beds from Ellesmere Port to Ince Ferry from my recently published "Geology of the Country around Liverpool," and assumes that the four basin-like depressions along the Manchester Ship Canal were caused by subterranean erosion and not by the deposition of silt and the growth of peat between ridges of sandstone. I do not, however, see that this theory can be satisfactorily applied to the post-Glacial beds referred to, for all the conditions are very different to those at Upton. It does not seem to be a logical conclusion to assume that because subterranean erosion occurs at Upton in consequence of a bed of sand underlying the Boulder-clay that it also occurs at Ince, in consequence of beds of grey silt and stiff clay underlying the Peat and Forest-beds. Mr. Shone refers to a bed of sand between the Boulder-clay and the post-Glacial beds at Ince; but it is quite a local deposit and changes to a grey clay within about 100 yards, and there is no such sand at Stanlow and Ellesmere, where the same amount of subsidence is shown. It does not seem possible that the beds of stiff clay could have been eroded beneath the surface under an area of several square miles of country, not only about Ince, but in other similar areas near Liverpool.

Mr. Shone's theory is, however, not original in connection with the district, for in 1854 the late Mr. John Cunningham, F.G.S., brought it before the British Association, and, so recently as 1887, in a paper read before the Liverpool Geological Society, and published in the Proceedings, on the "Stanlow, Ince, and Frodsham Marshes," I attributed the sinking of the land for about fifty yards along the edge of the Marshes to the influence of water from the river on a bed of sand underlying the grey clay and Peat and Forest-beds, but I afterwards found that the sand was not persistent, and that the slope of the land towards the Mersey was probably the original form of the ground. According to Mr. Shone's theory the surface of the land should fall rapidly along the edge of the Gowy and other streams, but I have seen no such subsidence.

Several instances have been described where the Peat and Forest-beds occurred on the Bunter Sandstone, many feet below the range of the tides. About Ince these beds rest on the rock in many places, and at various elevations. Along the shore on the north of the line of section the Peat and Forest-bed, with the trunks of trees, was seen resting on the Boulder-clay, and at the distance of a few yards on the rock.

The Boulder-clay rests on sand in cliff sections in many places around Liverpool, but I have never seen such an instance of subsidence caused by subterranean erosion as that described by

Mr. Shone. Possibly I may have overlooked some similar section, but I do not remember reading of any such subsidence in older formations. It is very remarkable that such an active agent has not been observed in the Tertiary formations of the South of England, where the beds of clay and sand are similar, and occur under the same conditions.

G. H. MORTON.

209, EDGE LANE, LIVERPOOL.
*July 16th*, 1892.

## "CONE-IN-CONE" STRUCTURE.

SIR,—Observing that the "Cone-in-Cone" controversy still goes on in the GEOLOGICAL MAGAZINE, I beg you will permit me to remark in this connection, that the question whether this puzzling formation occurs on both sides of slabs and nodular masses of calcareous rocks, clay-ironstone, etc., *i.e.* whether the apices of the layers of cones point upwards as well as downwards or not, was set at rest long since, at all events to *my* entire satisfaction [See GEOL. MAG. for January, 1887, p. 17]. It seems to me that Fig. 5 therein entirely upsets Mr. Jno. Young's theory of how this rock was formed.

Since I resided in U.S.A. my attention has repeatedly been called to double cone-in-cone (one layer over another, with the cones set in opposite directions) occurring in a certain bed of limestone in the Lower Productive Coal-measures of Western Pennsylvania, as well as in the Portage-beds of the Devonian series, upon which the place I write from is built; but as yet I have not had an opportunity of demonstrating that the said *double cone-in-cone* exists, by making a photograph of same *in situ*, which I mean to do as soon as possible, and send you a copy of. I may, however, say here, that this variety of cone-rock occurs both in flat irregular-shaped nodules or cakes, and also in beds, whenever or generally when the limestone-bed it runs in thins down to only a few inches. I do not imagine that the cone-in-cone *coal*, spoken of by Mr. Garwood in this month's GEOL MAG. (July, 1892, p. 334) can be of similar origin to that so often seen in clay-ironstones, limestones, etc. I think Mr. Garwood's cone-formation in coal is what miners sometimes call "cockscomb coal;" a structure commonly met with in the smokeless coal-beds of Glamorganshire, and more rarely in anthracite in Pembrokeshire. The "Hard mine" seam of N. Staffordshire sometimes exhibits a somewhat similar fracture, and I once detected cone-coal in the ordinary pit-coal (bituminous) of the "main" seam in Leicestershire. It runs in the semi-bituminous coals of Liege, Belgium. I look at it in coal as a kind of crystallization.

W. S. GRESLEY, F.G.S.

ERIE, PENNA., U.S.A.,
14*th July*, 1892.

## MISCELLANEOUS.

WE have much pleasure in announcing that the Queen has been pleased to approve of the following promotion in the Most Honourable Order of the Bath (Civil Division); to be K.C.B., PROFESSOR WILLIAM HENRY FLOWER, C.B., F.R.S., Director of the British Museum (Natural History), Cromwell Road, S.W.