abundant as to constitute a rock-forming mineral, whilst as an accessory it occurs also in the glaucophane-schist.

The author further describes a peculiar epidote, containing iron, from the glaucophane-schist, and also a peculiar garnet, occurring in rhombic dodecahedra about the size of a pea, which includes many other minerals, but no glaucophane. The garnet is of a deep yellow colour, and is anisotropic, a circumstance probably due to strain from the interposition of other minerals.

## CORRESPONDENCE.

### NORWEGIAN "RHOMBEN-PORPHYR" FROM THE CROMER BOULDER-DRIFT.

SIR,—In 1884 I collected some erratics from the cliff-sections near East Runton, amongst which was a specimen which proves to be exactly similar to the well-known "Rhomben-Porphyr" of Southern Norway and elsewhere. It will be interesting, perhaps, to put on record the occurrence of this uncommon and local rock. A small piece of this specimen has been sent to the Mineral Department, British Museum (Natural History).

540, KING'S ROAD, LONDON, S.W. CHAS. D. SHERBORNE.

### THE GLACIAL DEPOSITS OF SUDBURY.

SIR,—As one of those who believe that sea-ice was the main agent in the formation of the East Anglian Drifts, allow me to enter a protest against the conclusions drawn by Mr. J. E. Marr in his paper on the Sudbury sections in the June Number of this MAGAZINE.

He entirely omits to consider the action of coast-ice on a sinking shore, though he must be well aware that this agency has been prominently referred to as concerned in the formation of Boulder-clay.

He asks the question, "Why are not the incoherent Tertiary beds, on which the contorted Glacial deposits rest, themselves disturbed?" and he thinks that this fact is incapable of explanation except by the theory which invokes the passage of land-ice over the East of England. I perfectly agree with him on the point that the incoherent Tertiary beds could only escape contortion by being frozen hard so as to behave like the harder rocks of other districts; but is it, I would ask, only on an actual land surface that such sands could be frozen into a solid mass? I am writing in the country away from books of reference, but think I am correct in saying that the sand on the shores of Siberia is frozen into a perfectly hard and solid mass for some distance below the water, and I think the fact is mentioned in Nordenskiöld's "Voyage of the Vega."

Mr. Marr dismisses the agency of icebergs because he thinks the deposits could not be frozen "over large areas at the bottom of the muddy sea in which the icebergs were drifted;" this is probably true of those parts of such seas in which large and massive icebergs

would ground, but it is not true of the shallower parts near the shore on which the coast-ice acts and on to which floe-ice and pack-ice is often driven with immense force,—agencies which seem to be quite as capable of carrying with them the masses of partially frozen materials and of pushing them over a floor of solid frozen sand as Land- or Glacier-ice could be.

Mr. Marr refers to a recently-described case where a glacier *traversing a narrow valley* seems to have overfolded certain deposits of stratified sand and clay; thus comparing what may happen in a narrow valley with the phenomena of a district of which he himself says "not only do the contortions occur in the drifts which occupy the valley bottoms, but they are also found in the accumulations which lie on the summits of ridges." Are we to suppose that so able a geologist as Mr. Marr thinks an ice-sheet over-riding a ridge will act in the same way as a glacier pushing itself through a narrow valley?

The sections round Sudbury are exceedingly interesting, and Mr. Marr deserves our thanks for calling further attention to them and for recording new aspects of the changing pit-faces, but in his charge to the jury he has not put all the possible alternatives, and consequently his summing-up is biassed in favour of one explanation.

June 6, 1887. A. J. Jukes-Browne.

## THE CAUSES OF GLACIATION.

Sir,—I ask leave for a few remarks on the question of the causes of glaciation, as there are some points connected with it on which I think sufficient stress has not hitherto been laid.

The total amount of direct solar heat received at any place is admittedly nearly constant whatever be the eccentricity of the earth's orbit. The amount indirectly received through the medium of air-currents, clouds, and ocean-currents may vary; but if the variations of this indirect heat are ascribed to the raising or lowering of the temperature, the causes of this raising or lowering must be sought for in the distribution of the direct heat. We come, therefore, to the question, What distribution of direct heat over the various seasons (the total amount being unaltered) is most favourable to glaciation?

In the first place, then, it seems clear that the Glacial period could not have been produced by the freezing of water *in situ*. A snow-cap or ice-cap reaching an elevation of hundreds or thousands of feet over the sea-level could only have resulted from falls of snow. The former question is therefore resolved into the following, What distribution of direct heat is best calculated to increase the annual snow-fall?

In answering this question, two principles must be borne in mind. First, that snow will not fall, or at least will not lie, if the temperature is much above freezing-point. In such cases either rain would take the place of snow, or else the snow would melt at once. Second, that very little snow falls when the temperature is very low. Great cold preserves the snow that has fallen, but it seems necessary for a