LETTERS TO THE EDITOR.

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The Structure of Magnetite and the Spinels.

THE structure of the spinel group of crystals is very interesting. These crystals are cubic, and possess the greatest possible number of symmetries. The composition is given by the formula, $R''R'''_2O_4$, where the divalent metal R'' may be Mg, Fe, Zn, or Mn, and the trivalent metal R''' may be Fe, Mn, Cr, or Al. Magnetite is $FeFe_2O_4$.

The structure is fundamentally the same as that of the diamond. Each carbon atom of the diamond is to be replaced by the divalent metal atom; the distance between two neighbours being 3·60 Å.U. in magnetite as against 1·53 Å.U. in diamond. The four oxygen atoms are arranged in a regular tetrahedron about the divalent atom. The lines joining the latter to the former are parallel to the four cube diagonals. Any two neighbouring tetrahedra point towards each other. If each perpendicular from a tetrahedron corner on the opposite face is produced it encounters another tetrahedron, passing first through the middle point of a face and then through the opposite corner. A trivalent atom lies on each such connecting line, half-way between the tetrahedra. The distance between a divalent and the nearest trivalent atom is 7·20 Å.U. Four trivalent atoms are associated with each tetrahedron, but each atom is shared by two tetrahedra. As in other cases already examined, the molecule has no separate existence. The size of the tetrahedron may not be the same in all members of the group of crystals. The divalent atom lies at the centre of a tetrahedron of oxygen atoms, and the trivalent at the centre of an octahedron.

W. H. BRAGG.

Leeds, July 12.

The Magnetic Storm of June 17, and Aurora.

PROF. BARNARD's interesting letter dated June 25, in NATURE of July 15, on what is termed "The Great Aurora of June 16, 1915," is at first sight rather puzzling to the non-astronomical reader. The large magnetic storm began about 1.50 a.m. on June 17. On June 16, it is true, there was a magnetic disturbance, but not such as to suggest a striking auroral display. The explanation presumably is that Prof. Barnard is referring to an astronomical day, commencing at Greenwich noon on June 16. This, at least, would explain his statement that at Wisconsin (about 90° W.) at 21h. 25m. "the sky was bright with dawn." This one would expect between 3 and 4 a.m. local time. If this is correct, then the first auroral appearance chronicled by Prof. Barnard was at 3.30 a.m. on June 17, Greenwich civil time, and the maximum brilliancy about 8.15 a.m. It was principally during these morning hours that the Kew magnetic curves had the rapid oscillatory character usually associated with aurora and earth currents. The newspaper reports quoted by Prof. Barnard seem to fit this explanation.

Passing to the Rev. A. L. Cortie's letter (p. 537), it really emphasises the difficulty of deciding whether individual sun-spots and magnetic storms are connected. There are often a number of spots visible at one time. A spot remains visible for a number of days, during which there may be several magnetic storms. If spots cause storms, the rule one spot one storm may not be observed. If I selected any given date, storm or no storm, the chances are Father Cortie could supply a spot. I think Father Cortie has not quite grasped my argument that quiet days show the twenty-seven-day period equally with disturbed days, and that one can scarcely associate them with limited areas or "anti-spots." If one associates them, as he now seems to do, with an undisturbed state of a whole solar hemisphere, why not equally associate storms with a generally disturbed state of a whole hemisphere? As a matter of fact, the average quiet day seems associated with a practically average state of solar spottedness. The 600 quiet days selected by the Astronomer Royal from 1890 to 1900 gave for Wolfer's provisional sun-spot frequency a mean value of 41·28, the mean from all days of the eleven years being 41·22. They showed the twenty-seven-day period very clearly.

C. CHREE.

Richmond, Surrey, July 17.

Surface Tension and Ferment Action.

IN NATURE of June 17 Messrs. E. F. and H. E. Armstrong criticise the conclusions drawn by Mr. Beard and myself in a paper published in the Proc. Roy. Soc. of June 1, under the title "Surface Tension and Ferment Action." We drew the conclusion that the action of invertase was inhibited by surface tension. According to Messrs. Armstrong the inhibition observed under the conditions of our experiments was due simply to a minute trace of alkali given off by the glass. They state in confirmation of their view that the action of the alkali given off by ordinary glass is so marked that it is impossible to obtain consistent results with invertase, unless hard glass vessels, test-tubes, and storage bottles are used. That is certainly not our experience. We failed to find any difference in the readings between two mixtures of cane-sugar and invertase, of which one was kept in contact with glass beads at medium temperatures, as long as the amount of invertase used was relatively large. Our experience in that respect is apparently in accordance with that of Sörensen, who states that the effect of the alkalinity of glass makes itself felt only in the case of invertase solutions which have been especially purified.

In our experiments an inhibition was noticed only when the amount of invertase was relatively small. Under these conditions an alteration in the hydrogen-ion concentration produced by the minute trace of alkali given off by glass may have had some share in producing an inhibition, but it does not account for certain features of the phenomenon, which we have been careful to emphasise in our paper. If the alkali from the glass was entirely responsible for the effect one would expect the inhibition to persist in its entirety after the glass beads have been removed. This was found not to be the case. Again, the weakening of an invertase solution, which had been allowed to stand in contact with glass beads at medium temperatures and which we ascribed to absorption of the ferment by glass, cannot be explained on the ground put forward by Messrs. Armstrong. Their view necessitates the assumption of so large an amount of alkali given off by glass to an invertase solution, that it should be detectable by such an indicator as phenolphthalein. This again was not the case.

The interruption of my work has unfortunately delayed the completion and publication of similar observations with diastase carried out by Mr. McCall and myself. It was found that the inhibition produced by extending the surface-glass water could be almost completely removed by coating the glass with a thin film of a surface-active substance, such as methyl alcohol, ethyl alcohol, amyl alcohol, ether. On the other hand, films of ligroin and xylol deposited on the glass failed to remove the inhibition.