continuance of disturbance the oscillations of the magnets seem to be so locally modified that it becomes difficult to trace correspondence: some movements appear to correspond, and some not. A strongly-marked bend in the trace at one place may appear, as it were, stunted in that at the other place, or may not be perceptible at all. The disturbances appear to die out at pretty much the same time at both places. All this confirms very much what Mr. Whipple has already pointed out as regards Melbourne (NATURE, vol. xxii. p. 558).

M. Dechevrens, in some remarks which accompany the sheet of curves, notes that the disturbance of August 11–14 is the greatest experienced since the establishment of photographic registration at Zi-ka-wei in the year 1877, and he considers that the changes then observed (those of vertical force included, of which he gives no curves) are similar to such as would be produced by a powerful magnet placed in a certain defined position. It may perhaps be here pointed out that the results given by the Astronomer-Royal in his paper, "First Analysis of One Hundred and Seventy-seven Magnetic Storms" (*Phil. Trans.* for 1863) appear to give no support to a theory of this kind, and indeed seem conclusively to show that at Greenwich the observed disturbances cannot be accounted for in any such way.

It should be added that M. Dechevrens reports also that strong earth currents were experienced on August 11 and 12 on the submarine telegraph lines connecting Shanghai with Nagasaki and with Hong-Kong, as well as on the land lines in Japan, so much so that correspondence was frequently interrupted, but that no interruption appears to have been experienced on the occasion of the generally smaller magnetic disturbance of August 18. WILLIAM ELLIS

Royal Observatory, Greenwich, November 6

Meteor

A VERY large and brilliant meteor was observed here at 6h. 41m. p.m. G.M.T., on November 8. Its size was at least equal to one-fourth of that of the full moon, and it lit up the whole garden for about a second and a half. It was pear-shaped. The colour was white, and left behind it a pale red train. Its path was from a point half-way between α and δ Persei to 3° above Ursæ Majoris. The sky was rather hazy at the time.

Stonyhurst Observatory, November 9 S. J. PERRY

Condition of Jupiter

ON the evening of the 2nd I had a fine view of Jupiter with my 6-inch Cook's equatorial. The general appearance of the planet was remarkable for the bright colouring of the belts and of the red spot, a circumstance strongly noted by a gentleman who was observing with me, and who had not seen the planet for some time.

I could not however trace the usual white ring round the red spot. Below the red equatorial belt was a row of four or five small irregularly-shaped spots, nearly black in tint, and resembling sun-spots seen under a low power.

These dark spots seem now affecting Jupiter's surface in several parts, and are certainly not usual to it. About 9h. 26m. Satellite I. was occulted. I watched it gradually coming to contact, and at last it seemed to advance on the face of the planet, at least one-half of its diameter appearing to project thereon. It then faded out gradually.

September 3, 1879, at 9h. 8m., with the same instrument Satellite III. reappearing after occultation, was slightly (but certainly) projected on to the disk of the planet. It will be interesting to notice whether the present condition of Jupiter will be accompanied by more than ordinary displays of auroræ, of which symptoms have already appeared.

Guildown, November 6 J. RAND CAPRON

P.S.—Since writing the above accounts reach me of auroræ at Brighton on the 3rd and in the Orkneys on the 4th instant.

Vox Angelica

MANY of your readers may be acquainted with the nature of the Vox Angelica stop on a good organ. It consists of two ranks of pipes of small scale and delicate quality of tone, one of which is tuned slightly sharp, so that a wavy (hence called *Unda Maris*) sound is produced. Now it is possible to obtain very similar effects on an ordinary Estey American organ. Given the viola and violetta stops to be drawn out, wrap a band of india-rubber (an ordinary elastic band does very well) round the neck of the viola stop so that it cannot return completely home, on moderate pressure, and allowing a fraction of an inch to intervene between its true final position when inactive; beats will be heard of intensity depending upon the deviation from complete occlusion of this stop. The nearer the viola stop is to occlusion the more rapid the beats; but it is undesirable to obtain rapidity, as the lower notes are too prominently out of tune in this case. Anybody can, by experiment, determine the proper amount of deviation to be employed, and having done this the effect is remarkably good. On an Estey, the two stops mentioned are the only admissible ones for such an experiment, from consideration of overtones. No doubt some of your readers may adopt a more elaborate and convenient method of regulating the deviation than by elastic bands, after some experiments. It may seem a paradox to obtain beautiful concordant effects by the use of discordant vibrational relations, but it is undeniable that on a first-class organ the Voix Celeste, or Vox Angelica, or Unda Maris, is a most beautiful stop, and is capable of producing perfect *con sordini* effects. GEORGE RAYLEIGH VICARS

Woodville House, Rugby, November 3

Solids and Liquids at High Temperatures

SOME years ago I made an investigation much simpler but somewhat similar to that referred to by Prof. Carnelley in NATURE, vol. xxii. p. 435. An account of the experiments then made was communicated to the Royal Scottish Society of Arts, 1874–75. One of the results of that investigation was that while we do know something about the temperatures at which different forms of matter change from one state to another when a "*free surface*" is present, yet we are utterly ignorant of the temperature at which that change will take place when no "*free surface*" is present. It will be necessary here to explain that a "*free surface*" is any surface of the body under examination at which it is *free* to change its state. A surface of water, for instance, in contact with its own vapour is a "*free surface*" for the water passing into the gaseous state. The surface of a piece of ice in water, again, is a "*free surface*" at which the water may freeze or the ice may melt. And what are known as the freezing, melting, and boiling points of water are the temperatures at which these changes take place when such "*free surfaces*" are present. As to what the freezing, melting, and boiling points are when these "*free surfaces*" are absent, we have at present no knowledge whatever. All we know is that the freezing point is lower, and the "melting" and "boiling points" are higher, than when "*free surfaces*" are present.

The first of these points is too well known to be referred to here. The last point was illustrated in the paper referred to by an experiment in which water was heated in a metal vessel under atmospheric pressure to a temperature far above the "boiling point, when the water exploded and violently ejected itself from the vessel. The superheating of the water was accomplished by carefully excluding all "*free surfaces*" by bringing the water into as perfect contact with the metal of the vessel as possible.

Many experiments were also made to get direct and thermometric experimental illustration of the existence of ice at a temperature above the "melting point," but no satisfactory illustrations were got, on account of the great difficulty of getting quit of "*free surfaces.*" Of course so long as there existed a "*free surface*" at the surface of contact of the ice with the thermometer, the temperature at that part could not rise above the "melting point." It was however shown by indirect evidence that ice may exist at a temperature above the "freezing point" by referring to the well-known and beautiful experiment of passing a beam of light through a block of ice. When this is done with the aid of proper apparatus it is seen that the heat of the ray is absorbed by the ice, and that melting takes place at different points inside the block. Now the presumption is that the heat is absorbed at all points inside the block, but as the melting only takes place at certain points the heat absorbed where there is no melting must raise the temperature of the ice at those points above the "melting point," and the heat there absorbed by the ice will be conducted to the "*free surfaces,*" where it is spent in melting the ice.

Now though I was perfectly prepared to find that Prof. Carnelley had succeeded in heating the *inside* of a block of ice to a temperature above the "melting-point," I certainly did not expect so high a temperature as his experiments indicate to be