

4. On the Danger of Hasty Generalisation in Geology. By Alexander Bryson, Esq.

5. On the Deflection of the Plummets caused by the Sun's and Moon's Attraction. By Edward Sang.

In this paper it was shown that the attraction of the sun causes a deflection of the plummet, having its maximum about the 240th part of a second, and proportional to the size of twice the sun's zenith distance ; the deflection is at its maximum when the sun is 45° above or below the horizon, and occurs in the vertical plane passing through the attracting body.

The deflection due to the moon has its maximum about the 60th part of a second, and follows the same law ; it is toward or from the attracting body according as the zenith distance is less or more than 90° .

Upon the cross-level of a transit instrument, the joint effect is to cause a semi-diurnal oscillation small at the quarters and rising to the 24th part of a second at new and full moon ; while the influence upon meridian observations is sufficient to cause a disagreement between the greatest inclination of the moon's orbit, as observed at St Petersburg and Madras, amounting to the 50th of a second.

The general conclusion drawn was, that we cannot determine the positions of the heavenly bodies true to the 100th part of a second without having made allowance for this source of disturbance.

6. Note on Gravity and Cohesion. By Professor William Thomson.

The view, founded on Boscovich's theory, commonly taken of cohesion, whether of solids or of liquids, is, that it results from a force of attraction between the particles of matter, which increases much more rapidly than according to the inverse square of the distance, when the distance is diminished below some very small limit. This view might, indeed, seem inevitable, unless the idea of "attraction" is to be discarded altogether ; because the law of attraction at sensible distances—the Newtonian law—demonstrated by its discoverer for distances not incomparably smaller than the earth's dimensions, and verified by Maskelyne and Cavendish in a manner rendering it impossible for any naturalist to reasonably doubt its applicability

to the mutual action between particles a few hundred yards or a few inches asunder, seems to give only very small, scarcely appreciable, forces between bodies of such masses as those we experiment on in our laboratories, everywhere placed as close as possible to one another,—that is to say, in contact, and does not seem to provide for any considerable increase of attraction when the area of contact is increased, whether by pressing the bodies together, or by shaping them to fit over a large area.

But if we take into account the heterogeneous distribution of density essential to any molecular theory of matter, we readily see that it alone is sufficient to intensify the force of gravitation between two bodies placed extremely close to one another, or between two parts of one body, and therefore that cohesion may be accounted for without assuming any other force than that of gravitation, or any other law than the Newtonian. To prove this, let two homogeneous cubes be placed with one side of each in perfect contact with one side of the other; and let one-third of the matter of each cube be condensed into a very great number, i , of square bars perpendicular to the common face of the two; and let the other two-thirds of the matter be removed for the present. The mass of each bar will be $\frac{1}{3i}$ of the whole mass originally given in each cube.

Let us farther suppose that the two groups of bars are placed so that each bar of one group has an end in complete contact with an end of a bar of the other. The attraction between each two such conterminous bars, however small their masses are, may be increased without limit, by diminishing the area of its section, and keeping its mass constant. But the whole mutual attraction between the two groups exceeds i times the attraction between each of the conterminous pairs, and may therefore be made to have any value, however great, merely by condensing each bar in its transverse section, and keeping their number and the mass of each constant.

We may now suppose another third of the whole mass to be condensed into bars parallel to another side of the cube, and the remaining third into bars parallel to the remaining side. If, then, either of these cubes be placed with any side in contact with any side of the other, and allowed to take the relative position to which it will obviously tend—that in which the bars perpendicular to the

common side of the two cubes come together end to end, there will be produced, by pure gravitation, a force of attraction between them which may be of any amount, however great, and which will be greater, the greater the ratio of the whole space unoccupied within the boundary of either cube, to the space occupied by the matter of the bars.

This illustration has been chosen merely for the sake of definiteness and simplicity ; but it is clear that any arrangement, however complex, of woven fibrous structure, provided only the ratio of the unoccupied to the occupied space is sufficiently great, will lead to the same general conclusion. Farther, it is clear that the same result would be produced by any sufficiently intense heterogeneousness of structure whatever, provided only some appreciable proportion of the whole mass is so condensed in a continuous space in the interior that it is possible, from any point of this space as centre, to describe a spherical surface which shall contain a very much greater amount of matter than the proportion of the whole matter of the body which would correspond to its volume. Except in imposing this condition, the theory now suggested interferes with no molecular hypothesis hitherto propounded, continuous or atomic, finite atoms, or centres of force, static or kinetic.

Physical science abounds with evidence that there is an ultimate very intense heterogeneousness in the constitution of matter. All that is valid of the unfortunately so-called "atomic" theory of chemistry seems to be an assumption of such heterogeneousness in explaining the combination of substances. This alone, it is true, does not explain the law of definite combining proportions ; but neither does the hypothesis of infinitely strong finite pieces of matter ; and whatever is assumed to be the structural character of a chemical compound, a dynamical law of affinity between the two substances, according to the proportions of them lying or moving beside one another, must be added to do what some writers seem to suppose done by their "atomic theory."

It is satisfactory to find that, so far as cohesion is concerned, no other force than that of gravitation need be assumed.

The following Donations to the Library were announced :—
On Binocular Vision and the Stereoscope : a Lecture by William B. Carpenter, M.D., &c. 12mo.—*From the Author.*

- Description of a New Species of *Clerodendron* from Old Calabar, which flowered in 1861 in the Royal Botanic Garden of Edinburgh. By John Hutton Balfour, A.M., M.D., &c. 8vo. —*From the Author.*
- Abstract of the Proceedings of the Geological Society of London, Nos. 78, 79, 80.—*From the Society.*
- Man and his Helpmate. By William Thomas Thomson. Folio. *From the Author.*
- Monthly Return of the Births, Deaths, and Marriages Registered in the Eight Principal Towns of Scotland. March 1862. 8vo. —*From the Registrar-General.*
- Monthly Notices of the Royal Astronomical Society. Vol. XXII. No. 5. 8vo.—*From the Society.*
- Journal of the Chemical Society. No. LX. 8vo.—*From the Society.*

Monday, 28th April 1862.

PROFESSOR CHRISTISON, V.P., in the Chair.

The following Communications were read :—

1. Experimental Inquiry into the Laws of the Conduction of Heat in Bars, and into the Conducting Power of Wrought Iron. By Principal Forbes.

The experiments described in this paper were all made in 1850 and 1851, upon a plan which was fully explained by the author in letters to Mr Airy and Professor Kelland in the former year. Some notice of them appeared in the British Association Reports for 1851 and 1852, and the apparatus was supplied by a grant from the Association.

In previous inquiries into the thermal condition of a long conducting bar heated at one end, two assumptions have always been made: *First*, that the flux of heat across any transverse section of the bar is proportional throughout to the rapidity of the decrement of temperature reckoned along the axis of the bar (or to $\frac{dv}{dx}$, where v represents the temperature, above that of surrounding space, of any