## LETTERS TO THE EDITOR

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### The Origin of the Aurora Spectrum.

IN your issue of June 16, Prof. Schuster calls attention to the fact that the wave-length of the aurora line nearly coincides with the wave-length of the bright green line in the spectrum of krypton. Prof. Ramsay and Dr. Travers give the wave-length of this line as 5566·3. I find it to be 5570·40 (Rowland's scale), which brings the line close to the mean of the best determinations of the aurora line. According to Scheiner ("Die Spectralanalyse der Gestirne") the best measures, when reduced to Rowland's scale, are:—

| | | | |
|---|---|---|---|
| Ångström ... | 5568 | Huggins ... | 5572 |
| Vogel ... ... | 5572 | Copeland ... | 5573 |
| Vijkander ... | 5573 | Gyllenskiöld | 5569 |
| Lemström ... | 5570 | | |

Mean 5571·0.

To this must be added Campbell's determination at Mount Hamilton: 5571·6 (see translation of Scheiner's "Sp. d. Gest." by E. Frost, p. 326).

Considering the difficulty of measuring the aurora line, I think the difference is not too large to be compatible with the identity of the lines. Satisfactory evidence might be gained, if the other krypton lines could be observed in the spectrum of the aurora. I subjoin the mean of four different determinations of the yellow and green krypton lines. They were photographed on orthochromatic plates, together with lines of mercury, sodium and argon, which served as standards.

| | Mean error. | Ramsay and Travers. |
|---|---|---|
| 5562·35 ... | 0·03 ... | 5557·3 |
| 5570·40 ... | 0·03 ... | 5566·3 |
| 5871·10 ... | 0·03 ... | 5866·65 |

Hannover, Technische Hochschule, November 2. C. RUNGE.

### The Boring at Funafuti.

FURTHER information has been received this week from Prof. David, of Sydney, as to the progress of the boring at Funafuti. On September 6 it had reached a depth of 987 feet, passing through a hard dolomite-like coral rock, apparently similar to that mentioned previously as occurring below about 700 feet. Boring in the bed of the lagoon from the deck of H.M.S. *Porpoise* had been continued; the one mentioned in your last number was carried through sand, composed of fragments of calcareous organisms in which broken pieces of coral became commoner in descending, to a depth of 144 feet in the bed-rock of the lagoon, or in all 245 feet below sea-level. There progress was stopped by hard coral rock, which could not be pierced, because the great length of unsupported pipe (about 120 feet) made driving impossible, and the loose stuff above prevented them from applying another device. Captain Sturdee, though unable to stay much longer at the island, contrived to move the *Porpoise* about 90 feet nearer to the centre of the lagoon, where another boring was made at about the same depth. This was carried through 80 feet of sand, as before, which was then succeeded by a rather hard coral gravel; the lumps varying up to the size of a man's fist. It was pierced to a depth of 33 feet, when the time limit was reached, and the work was necessarily abandoned. The results, however, are most interesting, and our friends in Sydney may be congratulated on the success of boring so far in a depth of a hundred feet of water. When letters left the island the main bore was still progressing, though the supply of diamonds was nearly exhausted, so that there seems every hope that it will be carried below a thousand feet. But what has been already accomplished will be an immense addition to our knowledge of atolls.

T. G. BONNEY,
Vice-Chairman of the Coral Reef
Committee of the Royal Society.

### Asymmetry and Vitalism.

IT seems to me that Prof. Japp has not understood the purpose of my reference to "the formation of hematite nodules and flints in chalk." I instanced this simply as showing that segregation slowly takes place notwithstanding great restraints, such as that which a chalk-stratum offers; and my argument was that if segregations of hematite and flint take place in long periods notwithstanding such great restraints, it may reasonably be inferred that segregations of such slightly-different molecules as those of dextro- and laevo-protein would gradually take place under the slight restraints offered by a colloidal substance like protein. Unless due time is given, nothing can be expected.

Prof. Japp thinks I do not "quite realise to what extent enantiomorphous molecules are alike." He says that the two classes of molecules differ only as right and left hands differ. That seems to me a sufficient difference to determine segregation. There must be different *attitudes* in relation to incident forces. Can it be held that differences of attitude have no effects? The members of a mixed mass of molecules differing in their attitudes could not react in absolutely the same manner upon incident forces; and it may be inferred that their differential reactions will produce differential motions.

But Prof. Japp's fundamental fact seems to me to furnish an answer to his criticism. The basis of his argument is that these groups of right-handed and left-handed molecules severally produce rotation of a polarised ray in different directions. If they thus act differently upon the ray when they form an aggregate, they must act differently upon it when existing individually. Though in a mixed aggregate their respective actions on the ray cancel one another, yet each molecule of either kind will be acted upon and will react differently from each molecule of the other kind, and their reactions will *not* cancel one another. Hence there will be initiated those differences in their behaviour which cause segregation. If in a state of nature they are under some conditions subject to polarised rays, the implication seems to be that this result will take place. HERBERT SPENCER.

Brighton, October 29.

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I DO not understand how Mr. Herbert Spencer can imagine that the action of plane-polarised light (a form of energy which is merely polar—not asymmetric) can possibly effect the separation of enantimorphs. As I pointed out in my former letter, nothing short of an asymmetric influence could do this.

Speaking of enantiomorphous molecules, Mr. Spencer says: "There must be different *attitudes* in relation to incident forces. Can it be held that differences of attitude have no effects?"

There will undoubtedly be differences in the effects; but, owing to a peculiarity in the behaviour of enantiomorphs under the influence of symmetric forces, these differences will not be apparent in the final result. Thus, if we subject dextro-tartaric acid and laevo-tartaric acid separately to the action of heat, both will decompose at the same temperature and at the same rate, and will yield the same products in the same relative amounts. There is a "difference of attitude," and there will be a difference in the "effects," so far as in the one case some of the right-handed acid becomes left-handed, whilst in the other, some of the left-handed acid becomes right-handed; and in both cases, by similar inverse changes, stopping short, however, half-way, some mesotartaric acid is formed. But in both cases the final result is the same: namely, the establishing of an equilibrium represented by an optically inactive mixture of racemic acid with a little mesotartaric acid. There is a difference in the two *changes*—a difference which our knowledge of the opposite asymmetry of the two initial compounds enables us to read into the processes, thus saving Mr. Spencer's general proposition; but there is no difference in the *results*. But it is quite evident, from what Mr. Spencer has written about the separation of enantiomorphs, that it is in the results that he expects to find a difference; and here he will be disappointed, so long as symmetric influences only are brought to bear on enantiomorphs. Under such influences, just as in the foregoing case, *there occur two changes of inverse character, conditioned by, and exactly balancing, the inverse character of the two enantiomorphs, so that the final result is the same for both enantiomorphs.* This is what Mr. Spencer overlooks. He does not perceive that a uniform force acting upon two enantiomorphs may be modified by them so as to act in two opposite asymmetric modes. He must interpret his third "abstract proposition" and its corollary