

Letters to the Editor.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The British Association.

THE Association is to consider whether it will once more adventure into the outer regions of the Empire. That such transgress is desirable I am satisfied and so stated most definitely in the lecture I gave in 1915, at the Royal Institution, on our Australian excursion. In the interest of the younger scientific generation and of our Empire, it is of the utmost importance that we should roam over the world and discover its amenities—but the effort must be wholehearted, whenever it be made. The one failure of our Australian expedition was the insufficient support of the younger men.

It is a question whether, at the present time, when the cost of travel is so high, it will be desirable to attempt a new expedition—the chance that it will be well supported by the young men is not great. The Society of Chemical Industry could carry only a very small party last year to Canada. Therefore, the choice of a region that shall not be too distant is desirable, if the decision be to travel.

Properly speaking, the Association should go further westward, to British Columbia, north of the C.P.R., to visualise its potentialities and gain some idea of its conditions. To recommence the cycle at a middle point such as Toronto seems undesirable, at present. Montreal is the natural and would be the proper point of redeparture and discovery. It has also the advantage that it is the centre of the only region on the American continent where freedom still prevails and men are thought to be capable of taking care of themselves. It is the duty of science to protest and erect some barrier against the advancing wave of spurious puritanism which so affects Americans and now so threatens the freedom of mankind. The recent all but successful attempt to ban Darwinism in every shape and form is sufficient proof of what may happen.

HENRY E. ARMSTRONG.

Bohr and Langmuir Atoms.

CHEMISTS feel a difficulty in explaining molecular combination in terms of electrical attraction between the apparently revolving electrons which seem to compose the peripheral parts of an atom; and they naturally prefer a more static arrangement. Indeed, it is not easy to explain the stability of molecules in terms of any kind of purely electrical attraction between the atoms composing them: and yet, ever since Faraday, there has been an instinctive feeling that electrical attraction and chemical affinity are one and the same.

The facts of spectroscopy seem to insist on a system of revolving electric charges, while the facts of chemical combination seem to demand forces which can be treated statically; so it has been suggested that internal electrons are responsible for the radiation, while external electrons control the chemical forces. But the stability of chemical compounds can scarcely depend on loosely held external electrons, which, moreover, ought to be revolving just as much though not so fast as the inner ones.

May not a reconciliation be found by abandoning the idea of electrical attraction between atoms as the

major chemical force, and substituting for it the interlacement of the magnetic fields which inevitably accompany rapidly revolving electric charges. The orbital motion of the electrons responsible for chemical affinity, so far from constituting a difficulty, gives us a clue; for in every magnet electrons are rapidly revolving, and yet magnetic force is static. The clinging together of nails or needles near a magnet is all due to revolving electrons. Working with magnetised steel spheres, tetrahedra, and other shapes, some one with the ingenuity of Dr. Langmuir or Prof. Bragg might succeed in building up structures or models of adequate chemical significance.

The difficulty about substituting a magnetic field for an electric one, as accounting for the facts of chemical affinity, is no doubt the double polarity. But, on the other hand, this inevitable feature gives greater scope as well as greater complexity, and may ultimately be found to be an advantage; in fact, I am beginning to think that the constitution of bodies cannot be explained without it. The phenomena which long ago suggested "normal- and contra-valence" would fall into line. The stability of chemical combination would be all that could be desired, and the electrons in each atom would be peacefully engaged in giving their spectroscopic evidence (so well interpreted by the genius of Prof. Bohr), unharassed in their movements and perturbations by having to associate themselves with any electric field other than that of their own nucleus. Their magnetic linkages would be a sort of unconscious extra.

The undoubted phenomenon of ionisation would have to be developed independently, along with other known facts about gross positive and negative electric charges, but in the formation of stable chemical molecules we should not have to appeal to ionic charge. Moreover, certain molecular groupings, held together by magnetic forces, might be found readily susceptible to ionisation, especially when subject to bombardment, or when packed close together in a liquid.

I do not suppose that magnetic attraction as the equivalent of chemical affinity is any new idea, but I suggest that it has been inadequately developed, and that it seems capable of effecting a reconciliation between the extraordinarily ingenious schemes—apparently opposed, and yet both containing elements of truth—of which the names at the head of this letter may be regarded as principal types.

OLIVER LODGE.

The Acoustics of Enclosed Spaces.

SINCE writing the letter published in NATURE of August 19, p. 247, my attention has been directed to a paper on "Sound Proof Partitions" by Prof. R. F. Watson (University of Illinois Bulletin for March 1922). The paper contains a valuable experimental investigation on one aspect of the subject, but much remains to be done.

I take this opportunity of correcting an error which seriously affects the numerical results I gave for the sound transmitted through walls. In applying the optical equations, I forgot for the moment that the intensity of reflection in the case of sound does not only depend on the refractive index but also on the relative densities of the two bodies concerned. Even if the refractive indices were equal, so that the sound would proceed in the same direction, there would still be a powerful reflection if the densities were very unequal. In the equation I gave, $1-\mu^2$ should be replaced by $a-\mu^2a^{-1}$, where a is the ratio of the densities. When sound passes from air to a solid body the second term is in general negligible, and

the transmitted intensity depends almost entirely on the ratio of the densities. If we take the case of a partition of wood having a density half that of water, calculation then shows that at normal incidence about 4 per cent. of the sound is transmitted if the thickness be 1 cm. Reducing the thickness to 2 mm., the intensity of the transmitted sound increases to 50 per cent., and rises to 80 per cent. if the thickness is only 1 mm.

What I desired to emphasise in my previous letter is that the diminution in the sound transmitted with increasing thickness is not necessarily due to any absorption, but is explained by the effect of the reflection at the second surface which, when the thickness is small compared with the wave-length, neutralises the reflection at the first surface. This does not appear to be sufficiently appreciated, and some of the conclusions drawn in Prof. Watson's paper require correction accordingly. The effect of the second surface is also of importance when total reflection ought to take place according to the usual formulæ at the first surface. With a thickness less than a wave-length, part of the sound is transmitted. This case has been treated by Lord Rayleigh ("Collected Works," vol. vi. p. 71).

ARTHUR SCHUSTER.

The Annelids of Iceland and the Faroes.

THIS is a subject about which very little has hitherto been known. In discussing the part which white ants play in the economy of nature Prof. Henry Drummond compared them with earthworms. He referred to Darwin, and said that in "Vegetable Mould" a reference was made to the existence of earthworms in Iceland. I cannot find any such allusion. It is true that a few worms have been recorded for Iceland and one for the Faroes. I am fortunately in the position to add somewhat to our knowledge. My son, Dr. J. Newton Friend, having recently returned from an expedition in those islands, I have had the privilege of examining his collection of annelids. The following are the results:

The common earthworm (*Lumbricus terrestris*, L.) flourishes in Iceland. I examined twenty-three specimens, twenty of which were perfectly adult. Not one of them differed in any particular from the type as found in England. I hoped to find spermatophores, but in this matter disappointment was experienced. The specimens were collected near Reykjavik at the beginning of August, and were just in the right condition for laying their cocoons.

The red earthworm (*L. rubellus*, Hoffm.) was also found. Though adult it was smaller than our English specimens usually are, and the dorsal surface was of a darker brown colour. I have often found similar specimens, however, in the British Isles, so that they are in no sense to be looked upon as a variety.

The purple worm (*L. purpureus*, Eisen=*L. castaneus*, Sav.) has already been recorded, alike for Iceland and the Faroes. Thus each of the three common species of European Lumbrici is now known to be a denizen of Iceland. To these may be added two species of the genus *Dendrobaena*. One of these (*D. rubida*, Sav.) is best known by the variety usually named the gilt-tail, a denizen of ripe manure and decaying leaves. The other (*D. octoedra*, Sav.), although widely distributed geographically, is not a common species in Great Britain. Up till the present, then, five species only of the Lumbricidae have been recorded.

The shores of Iceland, if they could be worked as Claparede worked the Hebrides, would doubtless yield a variety of forms, especially the red-blooded pot-worms or pachydrilids. None of these, so far

as I am aware, has been up till the present placed on record, but *Clitellio arenarius*, O. F. M., well known on our English coasts, is reported for the coasts of Iceland.

I can find no records for the Faroes except the purple worm already noted. It is, therefore, with peculiar pleasure that I am able to add two new members to the list. These are both enchytraeids, and were collected in peaty earth near a stream some two miles inland from Thorshaven. One of these was a pachydrilid (*Lumbricillus lineatus*, O. F. M.). I have written fully on the synonymy in the *Irish Naturalist*, and my conclusions are supported by the more recent investigations of the American helminthologist, Welch.

The other enchytraeid is of the white-blooded kind (*Mesenchytraeus oligosetosus*, Friend). It was found some time ago among gleanings made in Jersey, and described by me in the *Zoologist*. I have more recently found a striking variety of this worm, or an allied species, near Birmingham. The Faroes material agreed with the Jersey. The worm is about a third of an inch in length, and belongs to the group which has enlarged setae on the segments which contain the spermathecae. It may also be noted that I found one of the commoner opalines parasitic in the Faroese enchytraeids. Our list therefore stands thus: *Mesenchytraeus oligosetosus*, Faroe Is.; *Lumbricillus lineatus*, Faroes; *Clitellio arenarius*, Iceland; *Dendrobaena rubida*, Iceland; *Dendrobaena octoedra*, Iceland; *Lumbricus purpureus*, Faroes and Iceland; *Lumbricus rubellus*, Iceland; *Lumbricus terrestris*, Iceland.

Addendum (Aug. 16).—A further investigation with pocket lens has resulted in some interesting additions to the foregoing list:

Achaeta minima Southern, only 1 mm. in length, but agreeing exactly in all particulars with the material from Ireland. The intestine contained peaty soil with a number of diatoms.

Marionina (*Chamaedrillus*) *sphagnetorum* (Vejd.).—Very slender, but true to type. White (colourless) blood. I stated my reasons in these columns some time ago for transferring this species to the genus *Chamaedrillus*.

Dorylaimus obtusicaudatus Bastian. A fine female nematode, about 2½ mm. long. All from the Faroes.

HILDERIC FRIEND.

Cathay, Solihull.

On the Reality of Nerve-Energy.

THE expression "nerve-energy" is widely used both by non-technical writers and by medical and physiological authors as well.

What the former mean by it is of no particular moment; but in medical and physiological literature it should connote something quite definite, if indeed the existence of nerve-energy is admitted at all. There seems a doubt whether its existence is to be admitted in a formal sense, for although certain physiologists use the expression nerve-energy as a convenient term, the thing itself is not discussed in their text-books, nor does it find a place in the indexes.

If nerve-energy has no place in the scheme of things vital as conceived by modern physiologists, then the term ought not to be used by them just because it is occasionally a very useful one. When they do use it, it means no more than "innervation."

The subject is full of difficulties, one of which is our having to reckon with the use of the still vaguer