LETTERS TO THE EDITOR.

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The Constitution of Atoms and Molecules.

Dr. van den Broek's letter (Nature, May 7, p. 241) contains one or two misapprehensions of the views put forward in my paper (*Phil. Mag.*, April, 1914), and I shall accordingly endeavour to make my meaning clearer. The paper does not purport to show that Dr. van den Broek's hypothesis is incorrect—in fact, in my own belief, it is fundamentally correct, though not necessarily in complete detail—but only to show that it is incompatible with the present form of Bohr's theory. Any atomic theory has two main things to explain in connection with optics—the X-ray spectra investigated by Moseley and the ordinary light spectra of atoms. The fact that coplanar rings are mathematically impossible is conclusive against them, whether on Bohr's theory or the present dynamical one. This must be admitted, in the face of any other evidence which appears to support them. There can be rings of electrons in an atom provided that they are not coplanar, but they must be of the same order of radius. There is only one case in which coplanar rings are possible—the case in which bound electrons *do not repel* each other, which is considered in detail in a paper to be published shortly, but such a supposition is in complete contrast to the present form of Bohr's theory.

As my letter to Nature pointed out, we do not require an inner ring in order to explain X-rays having lengths of the order 10^{-8}. They can come from an ordinary ring of atomic size if the nucleus is of strength 10 or more, and the Balmer lines can be considered as an X-ray spectrum of hydrogen. X-rays can even come from the confines of a structural nucleus. Many physicists have not yet realised that the size of the wave-length given by a ring bears no fundamental relation to its radius alone. The *angular velocity* of the ring is the important deciding factor. If we suppose that the frequency of a line is the frequency of the vibration of the ring about its steady rotation, dynamics shows that it is of the same order as the frequency of rotation, w. If C is the velocity of light, the wave-length is of order C/w, and a ring of *any* radius can give any wave-length if it rotates with the proper angular velocity. So also can any portion of a structural nucleus, and, coplanar rings being impossible, the X-rays can come from the nucleus. The wave-lengths on Bohr's theory are also determined by the order C/w, and not in any fundamental way by the radius, as may be seen by an examination of Bohr's mathematics.

Although it is the only published attempt, Bohr's theory does not constitute the only one which can be suggested to deal with Moseley's results. The writer has obtained, for example, a simpler explanation of them by more ordinary dynamics, which will shortly be published, by attaching a definite structure to the nucleus—a structure which can explain a great deal more in connection with such phenomena as the velocity of emitted α particles. In this method, the meaning of N is essentially the same as in Dr. van den Broek's hypothesis. The difference is in detail only. It is not possible to dispute Moseley's contention that there is a fundamental number which changes by steps of 1 in passing from one element to another in the table, nor that it is an "atomic number" related to the charge on the nucleus. But there is an assumption—perhaps correct—made in identifying it with the *exact* place occupied by an element in the table as we now know it, and Bohr's theory is incompatible with this assumption. For the paper showed that if the atomic number of lithium, for example, is 3, it must (1) have no valency on Bohr's theory, and (2) it must have all its electrons in one ring, or moving in a manner prohibiting any two of them from forming a ring. The radii of the orbits of the two inner electrons cannot be more nearly equal than in the ratio 12 to 1.

Again, as in another paper (*Monthly Notices of R.A.S.*, April, 1914), no approach to the ordinary helium spectrum can be obtained from Bohr's theory if the atomic number of helium is 2. These are only illustrations of much more decisive results. They have related, in the work published already, to the supposition that the laws of force between bound electrons are those used by Bohr. But they are equally valid for other laws of force. The one case in which the coplanar rings can exist—when bound electrons experience no force from each other—is the only avenue towards the extension of the theory. But it has difficulties, and, in particular, it gives no place to Moseley's constant b, which is then zero in all atoms. The K radiation then leads to the conclusion that the atomic number usually differs by 1 from the place of the element in the table. Dr. van den Broek lays stress on the fact that $N-b$ changes from one element to another, and not N. But we must repeat, quite definitely, that b is zero in the only modification of Bohr's theory which can have more than one coplanar ring. By this statement, however, we do not imply that b has no existence in fact. Its different values for K and L radiation demonstrate that it is real. The theory would demand an identity of these radiations even if they came from different rings, when such rings can exist. A reconciliation with experiment can only be obtained by putting the electrons as a constituent part of the nucleus itself, or by supposing that X-radiation comes from the confines of the atom—the K type from a neutral atom, and the L type perhaps from an atom which has lost an electron. But this latter alternative is quite at variance with Dr. van den Broek's hypothesis, when calculations are performed, and the first has no relation to Bohr's theory.

The strongest argument in favour of Dr. van den Broek is the recent generalisation of the periodic table put forward by Soddy and Fajans, against which mathematical considerations cannot be raised; in fact, they tend to support it. This generalisation, however, in no case demands a strict identity between the nucleus charge, and the place in the table. The other phenomena depending on the atomic number could depend equally well, within the order of accuracy, on a number which differed from it by 1 or 2. In conclusion, so far as the table is concerned, Dr. van den Broek may be completely correct, but, if so, Bohr's theory cannot be modified to take account of X-ray spectra. The periodic table, however, is not a sufficient test. Astrophysical spectra demand, as proved in many papers in the *Monthly Notices*, the existence of simple "elements" the spectra of which can be calculated, which not only agree with actual spectra, but also have actually led to the discovery of several lines which the formulæ predicted. The atomic weight of one of these, with 6 electrons, is 2·94, as calculated theoretically. By an application of their interference method to a line in nebulæ, MM. Bourget, Buisson, and Fabry (*Comptes rendus*, April 6, 1914) have verified this value for the mass of the atom which emits the line. They have also made preliminary experiments on another element, and found results which support the theoretical value of the atomic weight, 1·31. Very simple elements can exist therefore in which the atomic number differs from the number of electrons, and Dr. van den Broek's hypothesis cannot be a complete principle, although perhaps satisfactory for the stable terrestrial elements. Nevertheless, if it is satisfactory in this range, Bohr's theory is not. J. W. NICHOLSON.

King's College (University of London).

Temperature-Difference between the Up and Down Traces of Sounding-Balloon Diagrams.

IN his paper on the daily temperature change at great heights (January issue of the Quart. Journal of the Roy. Met. Soc.), Mr. Dines deals with the double traces shown by the diagrams of registering-balloons. He ascribes the difference for a great deal to the heating effect of the balloons, as the instruments swim in the wake of dead but heated air that follows the ascending balloon.

He rejects as a possible cause any thermal lagging of the instrument, because the double trace is most apparent in the isothermal layer, and also because it mostly occurs by day and not by night.

Receiving this number of the Quart. Journal, it just happened that I had made a synopsis of this kind of temperature-difference for the Batavian ascents, which throws another light on this question.

At Batavia the balloons are of a larger type than those used in England; also the string between balloon and instrument is much longer, measuring 30 m. and more. Moreover, it has been observed in numerous cases that up to the greatest heights the whole system of balloon-parachute-instrument often swings strongly. Accordingly any heating effect by the air in the wake of the balloon seems most improbable.

The instruments are of the pattern usual on the continent and made by Bosch (Strassburg); they are provided with clockwork. When possible the heights have been calculated separately for the ascent and the descent; thus, when the downward temperatures were found to be lower than the upward, the corresponding heights became lower, and accordingly the difference of temperature for the same calculated height in the ascent and the descent was increased. In half of the thirty ascents which up to the present have been made, the balloon was liberated 1–1½ hours before, and in the other half 1–1½ hours after sunrise. Thus, in the first cases only the latter part of the descent took place at an hour that solar radiation begins to be active.

The mean differences found are:—

Temperature Higher in the Ascent than in the Descent.

Height in km.	Before sunrise °C.	After sunrise °C.	Number of cases Before	Number of cases After
1	0·0	0·4	16	14
2	0·1	0·4	18	14
3	0·4	0·9	18	16
4	0·2	1·4	18	16
5	0·2	1·5	17	15
6	0·7	2·4	16	15
7	1·0	3·0	17	12
8	1·2	3·3	15	13
9	1·3	3·2	15	12
10	2·2	4·4	15	11
11	2·7	4·6	15	11
12	3·0	4·3	15	11
13	3·4	5·0	15	10
14	3·8	3·6	13	9
15	3·3	2·9	12	9
16	3·5	2·5	6	5

The prominent fact, demonstrated by this table, is that up to 13 km. the differences before sunrise are much smaller than those after sunrise.

The synopsis teaches, that before sunrise negative values occur in all heights, especially below 7 km. In one case up to the stratosphere the difference was negative in all heights.

On the contrary, in another ascent it went up to 9·2°. After sunrise no ascent, with negative values only, occurred, and in one case the differences amounted to 11·2°.

For the stratosphere, only in eight cases a set of these differences was obtained, its height being so great in these low latitudes that only part of the balloons reach its layers. Only in one of these cases (after sunrise) the descent-temperatures in the stratosphere exceeded those of the ascent, and in another case (before sunrise) higher temperatures alternated with lower.

In the five other cases (before sunrise) the sign of the differences in and below the stratosphere were contrary. It must be borne in mind that scarcely any isothermal state prevails in the tropical stratosphere, but that the temperature increases with the height (*cf.* my letter in NATURE of March 5, p. 5).

However, in the above-mentioned case of alternating positive and negative values, isothermal condition was met with up to 23 km.

The reversal in sign of the difference, which accompanies the reversal of the temperature gradient, strongly points to a thermal lagging of the instrument. Its heavy parts, and the basket also, will lag strongly and will influence the thermograph. In the ascent the lesser the ventilation the greater the heating. Thus the influence will increase with the height, as the ventilation decreases. In the descent the ventilation in most cases was greater than in the ascent, and accordingly the negative lagging less. After sunrise the thermal lagging of the basket will be enhanced in the ascent by sun-radiation, which easily explains the fact that the differences are larger after than before sunrise.

Perhaps the English instruments, being smaller than the German, have a smaller thermal lag than the latter. Thus Mr. Dines's explanation may be applicable to the facts observed in England, and mine to those met with in Java. From them I think the following lessons may be learnt, which applies to most Continental ascents made in a similar way and with the same pattern of instruments:—

(1) The temperatures of ascent and descent should be averaged.

(2) When descent or ascent is available only, a mean correction, to be derived from a large number of corresponding cases, should be applied.

(3) The temperatures and heights taken from the publication of the International Committee, in which, in most of the cases, ascents only are given, are affected by a systematic error.

W. VAN BEMMELEN.

Batavia, March, 1914.

Cellular Structure of Emulsions.

THE same arrangement that is shown by Fig. 2, in NATURE of May 7 (p. 240), may be seen in an emulsion of Oriental finely powdered coffee suspended in milk and water. I have supposed that it is connected with a strange phenomenon which I reported in NATURE about forty years ago. Sooty rain-water, after standing for some hours, will develop clear planes of water, as much as 10 cm. long and only 1 or 2 mm. wide. These planes are most readily seen by candle light when vertical, but may develop at any inclina-