The Warm Stratum in the Atmosphere.

WHILE not presuming to offer an explanation of the isothermal or relatively warm stratum in the high atmosphere, which the recent letters in NATURE have made known to others than meteorologists, I desire to point out that it is probably a universal phenomenon, existing at some height all around the globe. This inversion of temperature was first discovered by M. Teisserenc de Bort with the *ballons-sondes* sent up from his observatory at Trappes, near Paris, in 1901, and almost simultaneously by Prof. Assmann from similar German observations. Since then almost all the balloons which have risen more than 40,000 feet above Central Europe (that is, near latitude 50°) have penetrated this stratum, without, however, determining its upper limit. Teisserenc de Bort early showed that its height above the earth, to the extent of 8000 feet, varied directly with the barometric pressure at the ground. Mr. Dines (NATURE, p. 390) gives the average height of the isothermal layer above England as 35,000 feet, with extremes of nearly 50 per cent. of the mean. Observations conducted last March by our indefatigable French colleague, Teisserenc de Bort, in Sweden, just within the Arctic circle, showed that the minimum temperature occurred at nearly the same height as at Trappes, namely, 36,000 feet, although Prof. Hergesell, who made use of *ballons-sondes* over the Arctic Ocean, near latitude 75° N., during the summer of 1906, concluded that the isothermal stratum there sank as low as 23,000 feet.

During the past three years the writer has dispatched seventy-seven *ballons-sondes* from St. Louis, U.S.A., latitude 38° N., and most of those which rose higher than 43,000 feet entered the inverted stratum of temperature. This was found to be somewhat lower in summer, but the following marked inversions were noted last autumn:—October 8, the minimum temperature of −90° F. occurred at 47,600 feet, whereas at the maximum altitude of 54,100 feet the temperature had risen to −72°; October 10, the lowest temperature of −80° was found at 39,700 feet, while −69° was recorded at 42,200 feet, showing a descent of nearly 8000 feet in the temperature-inversion within two days. The expedition sent out jointly by M. Teisserenc de Bort and the writer, on the former's steam yacht *Otaria*, to sound the atmosphere over the tropical Atlantic during the summer of 1906, launched *ballons-sondes* both north and south of the equator within the tropics, and although some of these balloons rose to nearly 50,000 feet, they gave no indication of an isothermal stratum. In fact, the paradoxical fact was established that in summer it is colder eight miles above the thermal equator than it is in winter at the same height in north temperate regions. This results from the more rapid decrease of temperature in the tropics and the absence of the numerous temporary inversions which, as Mr. Dines has pointed out, are common in our regions below 10,000 feet. If, therefore, as seems probable, the isothermal or relatively warm stratum does exist in the tropical and equatorial regions, it must lie at a height exceeding 50,000 feet, from which height, as the data quoted show, it gradually descends towards the Pole, at least in the northern hemisphere.

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The Nature of X-Rays.

PROF. BRAGG in a recent letter (NATURE, April 16) credits me with the admission that the experiments I made on the intensity of secondary (scattered) X-rays are not so contrary to the neutral pair theory as I at first supposed. Will you permit me to correct this by saying that all the evidence I have obtained has verified the ether pulse theory in a more striking way than I ever anticipated, and I cannot think of a single experimental result obtained in researches on secondary X-rays which gives any support to his theory?

Prof. Bragg refuses, on the plea of want of knowledge of the constitution of the atom, to accept as conclusive the results of experiments I recently made. It appears to me, however, that such absence of knowledge is insufficient to conceal the disproof of his theory, and that the evidence (though by itself not sufficient to establish any theory) is quite sufficient to distinguish between the ether pulse theory and that proposed by Prof. Bragg.

The supposed difficulty in accounting on the ether pulse theory for the change in the ratio of intensities, to which he refers, is not one affecting the theory at all. Sufficient experiments have not yet been made to lead to a final choice between several possible causes as producing the bulk of this effect. The result itself is in harmony with the results of other experiments.

As Prof. Bragg is apparently not convinced, I venture to recommend the consideration of the following evidence obtained in investigating secondary X-rays, for I can only think that the study of this evidence would at least lead him to confine the application of his hypothesis to the explanation of phenomena which at any rate do not furnish so striking a disproof.

The evidence may be briefly summarised as follows:—

(1) The partial polarisation of a primary beam of X-rays.

(2) The identity in penetrating power of secondary (scattered) rays from light atoms, and of the primary producing them, though the scattered constitute only a fraction of the incident rays.

(3) The equality in the proportion of rays of different penetrating power which are scattered.

(4) The fairly complete polarisation of the rays scattered in a direction perpendicular to that of propagation of the primary.

(5) The distribution of the secondary (scattered) rays.

(6) The order of magnitude of the energy of scattered radiation.

(7) The homogeneity of a second type of secondary X-radiation from many substances.

(8) The fact of this homogeneous radiation being characteristic of the element emitting it, and independent of the penetrating power of the primary radiation producing it.

(9) The fact that for large ranges in the penetrating power of the primary these homogeneous secondary rays from some substances are proportional to the ionisation produced by the complex primary in air.

These are points that occur to me while writing; there are probably others.

The first five results (though not explicitly stated) were contained in the theory as given by Prof. J. J. Thomson ("Conduction of Electricity through Gases") shortly after the publication of the second experimental result, and before the others were experimentally observed. The sixth is in harmony with the calculation given by Prof. Thomson if we accept his theory of the number of electrons in the atom. Results (7), (8), and (9), obtained in joint-work with Mr. C. A. Sadler, can be explained on the ether pulse theory.

Prof. Bragg has given an explanation (based on what seem to me doubtful assumptions as to the behaviour of a neutral pair on collision with light atoms) of the fourth result. An explanation on his theory of the other facts necessitates in some cases very improbable assumptions; in others it appears to me to lead to absolute impossibilities. In no case can I find the slightest support for the neutral pair theory.

Regarding the nature of γ rays, or even of very penetrating X-rays, the direct evidence is much less conclusive, the corresponding phenomena being in reality more complex, for reasons which are beginning to be understood. For that reason I do not wish at present to discuss them, preferring to deal with what is to me a certainty, and waiting for the results of further experimental work to throw light on the more complex. Prof. Bragg commences at the other end with a hypothesis which gives an easy explanation of what on the pulse theory is somewhat obscure, but when an attempt is made to apply this to the simpler phenomena it is found inadequate, not only as a complete theory, but even as a supplementary one.

CHARLES G. BARKLA.
Liverpool, April 27.