

November 11, 1907, quoted by Dr. Chree, stood alone, one would readily accept his explanation, but since that date many similar differences, though not quite so large, have been recorded. In fact, the noticeable point about the so-called isothermal layer is the very large differences of temperature that are found at the same time over places a few hundred miles apart, and over the same place within a period of twenty-four hours. Because we cannot explain the phenomena, are we, therefore, to doubt their existence? It is perfectly natural to do so; the question is simply one of the credibility of the evidence.

The evidence is of various kinds. If one of the instruments used in England be completely immersed in a bath of liquid by an observer A, the temperature of the bath being, say, between $+30^{\circ}$ C. and -50° C., a second observer B having the record and the instrument can ascertain within 1° C., or at the most 2° C., the temperature of the bath used by A. Why, then, cannot B equally well ascertain the temperature of the air through which the balloon has carried the instrument? Secondly, these instruments are carried up by a balloon travelling through air that has been in contact with the balloon; the balloon in general bursts, and they fall, moving now at a much greater speed, since in England we use no parachute. Two traces are made, the one showing the temperature during the ascent, the other during the descent, but it is not often possible to say which is which. As a rule, the two traces are quite distinct; mostly, one indicates a temperature of from 1° C. to 3° C. below the other throughout, but sometimes the traces cross and re-cross each other. However, the point is that the two traces are practically identical; any peculiarity of gradient shown on the one is reproduced at the same height on the other. Now I think it lies with those who imply that our instrumental records are untrustworthy to explain this. If the temperatures shown by these two traces are not the approximate temperatures of the air, what are they? Systematic errors could not be the same in the different circumstances of the ascent and descent. It is inconceivable that casual errors could always so combine as to give errors of the same magnitude in pairs time after time. It is even less likely than that a man, drawing coloured balls from a bag, should draw the same colour in every two consecutive draws, for not only is the general trace reproduced, but every peculiarity in it is also reproduced.

Thirdly, the results obtained on the Continent and in America agree perfectly with those obtained with different instruments and a different system in England. This alone is not a good argument against the possibility of large casual errors, since casual errors are eliminated in the means, but the two sets of observations are as yet not very numerous—about 100 in England—and they show the same general relation between the temperature and height of the isothermal column and the height of the barometer at the surface.

Dr. Chree, from the last paragraph of his letter, appears to think that the instrument makers supply the scale. This is not the case in England, and I do not think it is abroad. Almost every instrument sent up in England to the present time has been made here. The University of Manchester is responsible for the scales of those that it sends up, and I am responsible for the scales of the rest. These scales are verified before and after each ascent. The lag in our instruments is very small, since we depend on the expansion and contraction of a strip of very thin German silver, but I do not see that the lag affects the general question, since it will be largely eliminated if we take the mean of the ascent and descent.

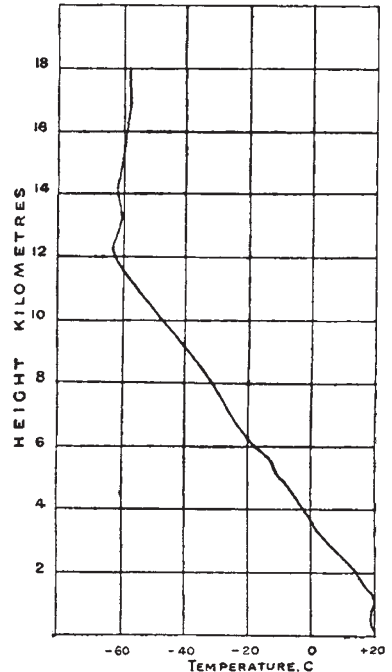
W. H. DINES.

Pyrton Hill, Watlington.

As one who subscribed to the "Confession of Monaco," may I be allowed to say that no definitions of the names stratosphere and isothermal layer were supplied at the conference as those present understood the terms? The meaning of a word has often divided the orthodox from the heterodox, and for the benefit of Dr. Chree, and also of "heretics in England," I will endeavour to make the matter clearer. Balloon ascents show that, apart from irregularities near the surface, the temperature of the air

decreases with height fairly regularly up to a certain point; above this point the regular decrease ceases, and for still greater heights the temperature changes are very small; sometimes there is a small increase, sometimes a small decrease, and sometimes the temperature remains almost constant up to the greatest height reached by the balloon. At any one place and time it thus appears that the atmosphere is divided into two layers, which differ markedly from one another in their vertical temperature distributions.

A diagram from an actual ascent made here on October 1 of last year shows the two characteristic temperature gradients. To the upper layer the names isothermal layer and stratosphere have been given; the latter name is due to M. Teisserenc de Bort, who surmises that the lower layer, or troposphere, is the part of the atmosphere concerned in the vertical circulation associated with cyclones and anticyclones, while the stratosphere lies above such movements. The name isothermal layer is not a fortunate one; certainly none of the orthodox who were assembled at Monaco would maintain that the upper layer is isothermal either in time or in a horizontal direction. Some less misleading term might have increased the number of the "elect." Both terms, however, are now in general use, and give definite names to a definite thing, which, as Huxley said, is the object of nomenclature.



The characteristic temperature gradient of the upper layer has been found over all parts of Europe, over the Atlantic, and over North America, but near the equator, if it exists at all, it is at a much higher altitude than in temperate latitudes. Its absence over the equator, and the fact that lower temperatures have been recorded there than in any other part of the atmosphere, seems to me to be a further proof, if such were needed, that the temperature gradient of the upper air recorded in other places is not the result of instrumental error.

CHARLES J. P. CAVE.

Ditcham Park, Petersfield, June 6.

The Sense of Proximity.

IN NATURE for March 11 there is an interesting account by Dr. McKendrick of some investigations by Kunz, of Mülhausen, and Prof. Griesbach, on the senses of the blind. Among other points that he refers to and discusses is the question of the ability of the blind to avoid obstacles and find their way about. This calls to my mind some observations and experiments which I made upon myself some eleven years ago with reference to my ability to find my way about with my eyes shut or in the dark. These I had intended to extend and amplify, but up to the present these further experiments have been crowded out by press of other work.

Many people have the feeling that if, for instance, they are in a room in the dark, they have some perception of their relation to objects in the room, and particularly can appreciate when they are near one of the walls. I can remember having had this feeling for many years, but never had the opportunity of putting it to scientific test