

or at most three half-prisms" more dispersion than was before obtained by "ten whole ones," does so at the cost of *all definition*, and will be certainly allowed at Greenwich, as well as everywhere else, to be a mistaken step in modern spectroscopy before another anniversary of the Royal Astronomical Society takes place.

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#### Greenwich as a Meteorological Observatory

IN NATURE (vol. xv, p. 421) there appeared a brief abstract of the presidential address of Mr. H. S. Eaton to the Meteorological Society of London on February 21. The increase of temperature at Greenwich in recent years is stated to be in reality due to local causes and not to secular variation, to which it has, as he thinks, been erroneously assigned. The effect of the growth of the population of London from 900,000 at the commencement of the century to 3,500,000 at the present time, and the still greater increase in the comparative consumption of coal, Mr. Eaton considers to be manifested by the rise in the average temperature of the air at the Royal Observatory, and for this reason it is concluded that Greenwich is not a suitable place for a meteorological observatory of the first order.

If the view enunciated by Mr. Eaton be correct, it is evident that the temperature of Greenwich during recent years has been in excess of that of surrounding districts. Is this view borne out by observation? Taking the figures for a number of places in the south-east of England whose mean temperatures have been calculated for the same thirteen years ending 1869, and adding the usual correction for height above the sea, we obtain the following results as their mean winter, mean summer, and mean annual temperatures; Greenwich, 40°·4, 63°·1, and 51°·1; Camden Town, London, 40°·4, 63°·3, and 51°·1; Royston, 40°·5, 62°·3, and 50°·8; Colchester, 39°·4, 62°·8, and 50°·6;  $\frac{1}{2}$ Worthing, 41°·1, 61°·2, and 50°·7; Osborne, 42°·0, 62°·5, and 51°·8; Aldershot, 40°·9, 62°·6, and 51°·2; and Oxford, 40°·6, 61°·3, and 50°·4. A simple inspection of these figures is sufficient to show that the consumption of fuel and the vast population of London cannot be said to have had an appreciable influence on the temperature as recorded at the Royal Observatory, and that if the Greenwich observations show a rise of temperature during recent years, the whole of the south-east of England has shared in that rise. This result deduced from observations is such as might have been expected when the position of the thermometers at Greenwich and the mode of escape of the artificially heated air by chimneys into the free atmosphere is taken into consideration. It follows, therefore, that, so far at least as regards the temperature observations, the conclusion drawn as to the future of our great national Observatory as a contributor to the higher meteorological researches is not supported by the facts of observation.

ALEXANDER BUCHAN

#### Atmospheric Currents

I AM glad to have obtained from such exponents as Capt. Digby Murray and Mr. Murphy a clear statement of the old orthodox creed respecting the movements of the atmosphere.

The former, it is true, finds a difficulty in accepting Maury's belief that the currents cut one another in "curdles" in the equatorial calms, but none in adopting the same as regards the tropical calms, and his view may therefore, as I suppose, be taken as a modification of that which is graphically represented on Plate I. in the "Physical Geography of the Sea."

The question at issue between Capt. Digby Murray and myself amounts to this: Are rapid polar and equatorial upper currents observed over the region of tropical calms? Mr. Murphy's theoretical question appears to me to involve the inquiry—Is the force of the trades derived from the earth's rotation?

In tracing the course of the air particles along the route which he describes, the late Commodore Maury observes that this course is determined in certain particulars by "some reason which does not appear to have been very satisfactorily explained by philosophers." The latter do not as yet seem to have got rid of all the difficulties with which his theory is beset, which rather grow with its development.

I would beg the philosophers to look closely at the actual course of the atmospheric currents as shown by synoptic charts, not by charts of prevailing winds and mean pressures, which represent conditions never found at any one time in nature. The distinction between the "great currents" and the "temporary currents" is important enough, but it amounts to little more

than that between mean winds and actual winds; and to explain the mean winds on one principle and the actual winds on the opposite involves a fallacy.

Again and again we see a more or less irregular belt of high pressures, having central calms, extending across the North Atlantic. From the southern edge of this belt we may follow a particle of air in its course to and from the equatorial district of low pressure, also an irregular belt in the middle of which calms exist. The movement originates in the defect of pressure near the equator at the level occupied by the particle. Its velocity is governed by the steepness of the gradient; and its direction, in relation to the surfaces traversed, is affected by the increasing velocity of rotation of those surfaces. In the Doldrums it arrives at a district at which the gradient becomes zero and the horizontal movement has consequently disappeared; but a vertical movement has now been acquired from the difference in the tension of the particles above and beneath, a difference derived from solar heat. When the particle has arrived at a position in which this difference disappears the vertical movement vanishes, and a new horizontal movement commences owing to the defect of pressures on the polar side at the level then reached, and the direction of this movement is also affected in relation to the surfaces by their decreasing velocity of rotation. Where does the new movement terminate? Obviously in some district between the equator and the pole where horizontal pressures on all sides of the particle at its then level are equal, but where a vertical movement has been acquired, the particles near the earth's surface starting on their journey towards the equator. The *onus disputandi* lies with those who deny that such a district is presented by either of the belts of tropical calms.

We now look at the polar side of the calm belt of Cancer, and for this purpose we may take almost any, *e.g.*, of Capt. Hoffmeyer's charts. We see in the majority of cases an aggregate of cyclonic circulations around local barometric minima, interfering and imperfect, and commonly becoming more so as they are propagated towards the pole. But we see no "polar depression" distinct from these, on which, as represented in the chart, we can lay the finger and say, "This is the result of centrifugal force; those are due to steam power." Within these systems an upward movement of the air occurs, owing to vapour condensation and the liberation of heat. Consequently towards these the particles of air near the earth's surface at the poleward edge of the tropical calms begin to travel, the earth's rotation deflecting their course in relation to the surfaces traversed. And from these, at a certain elevation, the particles return to the tropical calms for the same reason as that which determines the upper currents over the trades.

From the phenomena observed in the northern hemisphere I argue, *mutatis mutandis*, to those of the southern, and I expect the argument to be admitted by one who, like Mr. Murphy, attributes much less influence than I do to the work of water-vapour, and who even thinks that the mean movements of our atmosphere would be unaffected by the removal of all the water of our globe.

On some occasions pressure is high over all the North Atlantic on the polar side of the tropic, the anticyclones apparently extending nearly to the pole. In these cases we have no surface counter-trades over that district, yet the north-east trades continue to blow on the southward of the tropic as usual.

I repeat that all movements of the atmosphere originate in differences of pressure derived directly and indirectly from solar heat, and not in the force of the earth's rotation. And I must add that it seems to me very strange that any one while regarding the trades and their upper-currents as simply the *effects* of pressure differences in the lower latitudes, should maintain that the south-west and north-west winds of the temperate zones are simply the *causes* of the pressure differences in the higher latitudes. It would be just as logical to regard the south-west and north-west winds as due to pressure distribution, and the trades as the compensation for their eastward movement. W. CLEMENT LEY

March 10

#### Electrical Phenomenon

LAST night I noticed a powerful development of electricity in a curious manner. I had thrown a piece of common, thick, white, unglazed paper upon a low fire which was tolerably full of ashes. When it was charred so as to be black and brittle, I happened to take it up and break bits off. To my astonishment they stuck firmly to my fingers. I broke off two pieces each an inch long, and resting them on the tips of my two fore-fingers,