above and slightly behind the house. The solar rays falling on the objects in the court-yard were transmitted through the shutter holes. There being no other light in the room, and the rays being strongly scattered by the rough whitewashed wall, the rays were sufficiently powerful to produce an image on the retina of an observer in whatever part of the room he might be; the room became, as it were, the box of a large camera.

On intercepting the rays with a smooth oval looking-glass, they were not, of course, scattered, and no image was visible on the glass, but the image could be reflected from the looking-glass to any part of the wall which contained the shutters through which the rays passed. The appearance produced when a servant was made to stand in the required position, was singular. A full-length (inverted) coloured figure appeared in an oval frame of bright white light, much larger, of course, than the looking-glass. The white light was produced by the glare from the gravelled yard, shadows on which were reproduced.

A dog-cart and horse were imaged on the wall most clearly, the chesnut colour of the horse being very distinct. The whole phenomenon was always producible at any time when the sun was in the proper position above the house.

Are not mirages of one class, *i.e.* the appearance of inverted images in clouds, produced in a similar way? The rays from a figure might pass through an opening in one cloud to the face of another otherwise unilluminated, and be thence scattered. I believe I have seen this explanation given somewhere, but I cannot remember where.

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ON TEMPERATURE CYCLES*

SINCE the discovery of an eleven years' period in the phenomena of solar spots, several corresponding periods (it is now well known) have been demonstrated in terrestrial phenomena, more especially in those of magnetism, auroras, cyclones, and rainfall. With regard to weather changes, it has been thought by Dove, that the tracking of a cycle in these could not, theoretically, be made an object of research; and that while some indications of a periodicity might appear, a great part of the complicated changes named must be, from the nature of the case, quite unperiodical. The series of observations by Dove on the subject led him to the conclusion (1) that divergences from the normal, especially those of temperature, are not local, but spread over large surfaces; but (2) that negative divergences, in one region of the earth, are compensated by positive in another; and conversely. That the compensation is perfect, and that the quantity of heat annually given by the sun is constant, has been affirmed also by Maury and others.

The data on which this conclusion is based are limited. They appeared quite insufficient to a German physicist, Dr. W. Köppen, who has recently been led to undertake a wider investigation of the subject. He has communicated to the Austrian Society for Meteorology a preliminary notice of his inquiries and results (*Zeitschrift*, Aug. and Sept. 1873), which will be found of considerable value.

We may first note here his materials and method. He furnishes a long list of places from which observations (more or less extensive) have been had; and in his first table he gives the divergences of temperature of individual years (1820—71) from the average temperature, and for the following regions: India, Tropical America, Temperate South America, South Africa, Australia, China, and Japan, Mediterranean region, Southern United States, Western U.S., Western Central Europe, Austria, South Russia, South-West Siberia, East Siberia, Central part of U.S., Atlantic States, British Islands, North Germany and Netherlands, North-West Russia, North-East Russia and Ural, North-West America, North East America, Iceland, Northern part of Europe. [The particular towns, &c., are given, and the author's purpose partly is, that the list may be supplemented by other series of observations (which he has not been able to see), being sent to the Central Physical Observatory at St. Petersburg, where he has chiefly been prosecuting this research.] The periods of observation ranged from three to thirty years; the average was taken from several years' observations. In many observation-series, the yearly average had to be calculated for the first time. Series of six years' length were the shortest admitted, and such short series only by way of completing the longer. The original sources of Prof. Dove's material were consulted.

A second table shows the divergences of temperature in various regions for the years 1768—1819. By way of condensing, a third table is given, in which the material from 1820—71 is arranged in five series, one of which represents the tropics, and the four others four successive ex-tropical zones. The zones are not bounded by determinate parallels of latitude, but it was sought to combine approximately equal material of observation and earth surface.

On comparison of the curves of Table III. with the sun-spot curves (according to Wolf), a striking correspondence at once appears, as far as the year 1854. In the tropics, the maximum of heat occurs $\frac{1}{2}$—$1\frac{1}{2}$ years *before* the spot-minimum; in the ex-tropical zones, on the other hand, it occurs *after* the minimum; in some cases (in the forties, *e.g.*) as much as three years after. The regularity and extent of the variations diminish from the tropics to the poles.

It is further noticeable that as the interval from maximum to minimum of the spots is always greater than that from minimum to maximum, a corresponding inequality occurs in the temperature changes.

On these results Dr. Köppen remarks that, while there is evidently some connection between the two kinds of phenomena, the sun-spots do not act directly by darkening a part of the solar disc; for, as the temperature of the earth's surface is a function of the solar radiation, the change in the former must follow that in the latter; but the opposite occurs, as we have seen, in the tropics. It is probable that the temperature of the sun's surface is (from some unknown cause), at its highest one or two years before the minimum of the sun-spots. That the spots (if we suppose them to be solid bodies) take so long to melt that their minimum only occurs after the maximum temperature of the earth's surface, is not remarkable, considering their size.

If we consider the period 1800—71, we find a section of about 40 years, with marked periodic variation, 1815—54, and two periods, before and after, showing great disturbances, (say) 1792—1815, and 1854—66. Whether in 1865 we have again entered (as the curve would seem to indicate) on a time of distinct periodic variation, will doubtless appear in the next ten years.

The observations before 1800, again, show such anomalies in the temperature, that we should almost doubt the existence of connection with the sun-spots were it not for the convincing evidence, of the years 1815—54. We find all possible cases, from complete indifference of the temperature in contemporaneous change of the sun-spots (1750—71), and a short correspondence of both (1772—77), to a well-marked and regular variation of temperature (1777—90), which stands to the sun-spot curve, in exactly the opposite relation to that found in 1816—54. True, the observations here are only from a small fraction of the earth (West Europe and the New England States); but the continuance of the same curve shows the normal variation in 1816—54 quite distinctly. The estimation of the spots previous to 1826 is somewhat arbitrary, but an error such as that the maximum is put in the place of the minimum cannot be supposed. And lastly, if it be urged that the turning points

* Abstract of paper by Dr. W. Koppen in the Austrian *Zeitschrift fur Meteorologie.*

of the temperature curve (1779 maximum and 1785 minimum) are precisely where, according to the mean length of the sun-spot period of 11·1 years, they must be; that there may, perhaps, be an 11 years period in the temperature independent of the sun-spot period, and that, in the present case, a displacement which the spot period has experienced is not shared by the temperature period; we have to remember that the correspondence of the temperature changes in 1815—54, does not merely extend to the average length of the periods, but that all peculiarities and disturbances in the sun-spot curve are, in these 30 or 40 years, reflected in the temperature curve. Further observation is needed to explain this phenomenon. Possibly (the author suggests), we have here the interference of a number of quite independent periodical actions; and (without laying stress on the fact, in default of causal evidence), he notices that the greatest negative anomalies occur, for a considerable time, in a series which progresses by multiples of 9, and in such a manner that an interval of 27 alternates with one of 18 years. Thus—

$$\begin{matrix} 1740 & = & 1767 & = & 1785 & = & 1812 & = & 1830 & = & 1857 \\ & +27 & & +18 & & +27 & & +18 & & +27 & \end{matrix}$$

The first four agree; there is merely the quite isolated cold year 1794 intermediate. Going further, we find divergence; for the table shows a strong negative anomaly about 1836; but we have, again, the well-authenticated negative anomaly of 1856—57 conforming to the rule. Renou has assigned, for the return of the cold winter of south-western Europe, a period of 41 years; the author asks whether the time 27 + 18 = 45 years does not better agree with the phenomenon. On this view, the first winter, reckoning back from 1740 is 1695, and this is recorded as having been one of excessive cold. Between these two occurs one winter of extraordinary cold, 1709, but it is quite isolated, the neighbouring years having been warm. If we go still further back, the periodicity cannot be ascertained with any certainty. If the rule is correct, and its validity between 1740 and 1857 not a mere accident, *i.e.* the expression of quite other laws, we have to look for a very cold year in 1875 (being 1857 + 18).

Dr. Köppen proposes, in a future communication, to treat of hydro-meteors, and to examine the influence of periodic weather changes (at several years' interval) on some phenomena of organic nature.

LAVOISIER'S WORK IN THE FOUNDATION OF THE METRIC SYSTEM

SINCE the publication of the article on the Metric System, in NATURE, vol. viii. p. 386, my attention has been drawn to some recent information showing the important part taken by the celebrated Lavoisier in the scientific operations for establishing the basis of the metric system of weights and measures in France. Lavoisier's name has hitherto been little noticed amongst those of the men of science who were prominently engaged in this work; but it is now clearly proved that up to the period of his being guillotined on May 8, 1794, when he fell a victim to the revolutionary fury during the reign of terror, no one took a more active or serviceable part in the scientific labours for founding the Metric System than Lavoisier.

This information is contained in a "Notice historique sur le Système Metrique," by General Morin, lately published in the "Annales du Conservatoire des Arts et Métiers." It is derived from original documents left by Lavoisier, and now in the possession of the Académie des Sciences. These documents have since been submitted to my inspection by M. Dumas, and full details of them will soon be given to the world in the fifth volume of the works of Lavoisier, which M. Dumas is now completing.

Although Lavoisier's name does not appear in the list of the original Committee of Weights and Measures in France, yet it is shown that he was very actively engaged in making the arrangements for their meetings and in preparing the minutes of their proceedings, as appears from papers and letters in his own handwriting. It was through his personal agency that funds were provided at Paris for continuing the measurement of the arc of the meridian in Spain by Méchain. And more particularly, all the actual comparisons for determining the length and dilatation of the standard measures used by Méchain and Delambre for measuring the basis, and known as the *Règles de Borda,* were made, not by Borda, but by Lavoisier. The subsequent computations only were made by Borda. Lalande has expressly stated that the work of preparing them was executed by Lavoisier and Borda, but that the construction of the measures of platinum and brass, forming metallic thermometers, and of the comparing apparatus used, was carried out under Lavoisier's directions. The published report upon the construction and verification of these measures in 1792 is contained in the "Base du Système Metrique," vol. iii. p. 313. It was drawn up by Borda, but Lavoisier's name is not mentioned in it.

Another very important part of the work, the determination of the weight of a cubic decimetre of water, was carried out, in the first instance, chiefly by Lavoisier. This branch of the operation had been specially entrusted by the Committee to Lavoisier and Haüy. The necessary apparatus was constructed under Lavoisier's directions, and all the requisite measurements and weighings of the cylinder were made by Lavoisier and Haüy. Hitherto few details of the actual processes of this scientific determination have been given to the public, and the whole credit of determining the weight of a cubic decimetre of water, upon which the kilogram, the unit of metric weight, was based, has been attributed to Lefévre-Gineau, to whom, in conjunction with Fabbroni, the work was entrusted after Lavoisier's death. In point of fact, Lefèvre-Gineau appears to have repeated, in the winter of 1798-9, all the observations made by Lavoisier and Haüy five years before, using the same instruments and obtaining nearly similar results.

The facts are stated as follows by Bugge, the Danish member of the Commission, in the thirtieth of his letters describing his visit to Paris, and published in 1800:—

"The final results of the labours of this special commission, consisting of Lefevre-Gineau and Fabbroni, to whom Van Swinden and Trallès were afterwards joined), was that the true kilogram, the weight of a cubic decimetre of water at its maximum density, or at 4° C., was 18,827 French grains of the old French pound, *poids de marc.*

"By the laws of August 1, 1763, and April 7, 1795, the kilogram is determined to be 18841 grains of the old French pound, *poids de marc,* in accordance with the experiments of Lavoisier and Haüy. This determination was adopted by the Chief Office of Weights and Measures in France, and the Standards have been hitherto made for the Departments accordingly. So that there now exist two kinds of kilograms, the legal or provisional, and the scientific or true kilogram. The difference between them is fourteen old French grains."

The difference is partly attributable to Lavoisier's determination having been made at the temperature of melting ice, instead of that of the maximum density of water adopted for Lefevre-Gineau's determination. The unit of Metric weight, the Kilogramme des Archives, appears to have been based on the later observations of Lefevre-Gineau, and to have been legalised by the law of Dec. 9, 1799, after Bugge's letter was written.

H. W. CHISHOLM