

our standard of reference for air temperature until a further reduction of the photographs shall enable us to extend the period and so further improve them. The mean temperature of Greenwich, according to Table 77, is $49^{\circ}72$.

The mean temperatures of Table 52 are also deduced from the photographs, without correction for omitted days; they serve to show the climatic variations in different years: if desired, the effect of the omitted days could be readily determined. Here the mean temperature comes out $49^{\circ}69$.

The table of mean air-temperatures for the period 1847-1873, deduced from eye-observations (Table 125), to which particular attention is drawn, p. 526, is naturally of secondary importance, and really owes its introduction into the volume partly to accident. It was thought desirable in preparing for press the deductions from the photographic records, to add, in the same volume, but in a separate section, a collection of results of the observations of the earth-thermometers for the period 1847-1873, and a table showing the mean air-temperature during the same period seemed a proper accompaniment to these results. Now the photographs had been discussed only for the period 1849-1868, and, as there were already existing mean temperatures for the longer period, 1847-73, deduced from eye observations (by use of Mr. Glaisher's corrections), these temperatures were taken for comparison with the earth-thermometer results. The expression "accurate mean temperatures" (last line of extract, p. 526) was never intended to indicate that Table 125 should supersede Tables 52 and 77, but simply to explain that the air temperatures were *mean* temperatures as distinguished from the *noon* temperatures of the remaining tables of the section. Further it is indicated in a sentence not quoted, but forming portion of the paragraph from which the extract already referred to is taken, that the eye observation series was adopted for this section because photographic results were not available for the whole period. It may be here explained that employment, for reduction of the eye-observations, of the corrections for diurnal range, &c., given by the photographic records, produces values in harmony with the photographic values, and corrections so derived are now used in the reduction of eye-observations for immediate use. But Table 125, as it stood, fulfilled the object for which it was inserted, that object being rather to exhibit *variations* of temperature. No argument founded on the absolute temperatures shown by the earth-thermometers has much significance, because we are unable to test their index errors. In fact, the true value of the whole collection of tables in the earth-temperature section consists, not on their exact indication of absolute temperature, but on the information which they afford in regard to comparative changes of temperature, the retardation of temperature, and generally the propagation downwards of surface-waves of temperature.

In the last paragraph of the notice attention is directed to what is called the "somewhat rough method" adopted in reducing the barometric observations to 32° . This erroneous idea may have, perhaps, been encouraged by the circumstance that in the Introduction no mention appears to have been made of the fact that the temperature of the underground apartment in which the photographic barometer is placed is almost uniform. Considering this in connection with the construction of the apparatus, in which (as fully explained in the Introduction) the register depends on the height of the mercury in the lower tube of a syphon barometer, and is influenced by the expansion of some four inches only of the mercurial column, it will be understood that the effect of temperature (change of temperature) in a period of twenty-four hours (the extent of one sheet) is not perceptible.

In the third and fourth lines of the paragraph, on p. 526, commencing "From this table," it would seem that the word "excess" should be "defect."

WILLIAM ELLIS

Royal Observatory, Greenwich, October 7

Saturn's Dusky Ring

I WRITE to call the attention of observers to the present appearance of the inner dusky ring of Saturn. Although the ring is not very open, only permitting that portion near the ends to be seen on the nights of the 9th and 11th of this month, using a portion of a large reflector sufficient only to show Enceladus clearly, it was most prominent, and not to be overlooked. It had the appearance of being covered with bright points, such as a rough dusk paper touched lightly with chalk would give; that part in front of the ball being dark, and showing as a fine dark line across, equal in width and shade to the shadow beneath, so that the narrow part of the whole ring appeared on the face of

the planet as if bounded by two fine parallel dark lines. The wide and the narrow division at the ends of the ring were very plain.

Not having seen this dusky ring before, I can only go by the published accounts, but it is so much brighter than they would lead one to suppose that it is very difficult to believe that it is not changing very rapidly. The observation of one to whom the ring is familiar would settle this point.

A. A. COMMON

Ealing, October 13

Suicide of the Scorpion

DOUBTS having been expressed at various times, even by learned naturalists, as to the reality of the suicide or self-destruction of the scorpion by means of its own poison, and these doubts having been again stated in NATURE, vol. xx. p. 553, by Mr. R. F. Hutchinson, of Peshawar, as the result of his own observations, I think it may be useful to give an articulate account of the phenomenon as it has been related to me by an eye-witness, which removes all possible doubt as to its occurrence in certain circumstances.

While residing many years ago during the summer months at the baths of Lucca, in Italy, in a somewhat damp locality, my informant, together with the rest of the family, was much annoyed by the frequent intrusion of small black scorpions into the house, and their being secreted among the bedclothes, in shoes, and in other articles of dress. It thus became necessary to be constantly on the watch for these troublesome creatures, and to take means for their removal and destruction. Having been informed by the natives of the place that the scorpion would destroy itself if exposed to a sudden light, my informant and her friends soon became adepts in catching the scorpions and disposing of them in the manner suggested. This consisted in confining the animal under an inverted drinking-glass or tumbler, below which a card was inserted when the capture was made, and then, waiting till dark, suddenly bringing the light of a candle near to the glass in which the animal was confined. No sooner was this done than the scorpion invariably showed signs of great excitement, running round and round the interior of the tumbler with reckless velocity for a number of times. This state having lasted for a minute or more, the animal suddenly became quiet, and turning its tail or the hinder part of its body over its back, brought its recurved sting down upon the middle of the head, and piercing it forcibly, in a few seconds became quite motionless, and, in fact, quite dead. This observation was repeated very frequently; in truth, it was adopted as the best plan of getting rid of the animals, and the young people were in the habit of handling the scorpions with impunity immediately after they were so killed, and of preserving many of them as curiosities.

In this narrative the following circumstances are worthy of attention:—(1) the effect of light in producing the excitement amounting to despair which causes the animal to commit self-destruction; (2) the suddenness of the operation of the poison, which is probably inserted by the puncture of the head into the upper cerebral ganglion; and (3) the completeness of the fatal symptoms at once induced.

I am aware that the phenomena now described have been observed by others, and they appear to have been familiarly known to the inhabitants of the district in which the animals are found. Sufficient confirmation of the facts is also to be found in the narratives of G. Biddie and "M. L." contained in NATURE, vol. xi. pp. 29, 47, and it will be observed that the circumstances leading the animal to self-destruction in these instances were somewhat similar to those narrated by my informant. It is abundantly clear, therefore, that the view taken by Mr. Hutchinson, viz., that the "popular idea regarding scorpionic suicide is a delusion based on an impossibility" is wholly untenable; and indeed, the recurved direction of the sting, which he refers to as creating the impossibility of the animal destroying itself, actually facilitates the operation of inflicting the wound. I suppose Mr. Hutchinson, arguing from the analogy of bees or wasps, imagined that the sting would be bent forwards upon the body, whereas the wound of the scorpion is invariably inflicted by a recurvation of the tail over the back of the animal.

ALLEN THOMSON

London, October 11

Climatic Effects of the Present Eccentricity

I ASK for an explanation of the following difficulty:—

Dr. Croll says, in his "Climate and Time" (p. 65), that "the

temperature of a place, other things being equal, is proportional to the heat received from the sun."

His reviewer in the *Quarterly* for July last says: "The mean January temperature of England may be taken at 39° F., which is equivalent to 278° F. of absolute temperature" (meaning, above the temperature of space taken at -239° F.), "and if we calculate what would be the mean temperature of the same month when the sun was distant 97,500,000 instead of 91,000,000 of miles as it is now, we find it comes out 242° F., which is equivalent to 3° F. of our thermometer, or 29° of frost."

If we use the same method to find to what extent the present value of the eccentricity ought, even now, to affect temperatures on the earth's surface, we arrive at a result apparently so contrary to experience that I think "there must be a mistake somewhere." I ask your readers to tell me where.

Let S be the temperature of space. Choose two places in equal north and south latitude; and let U , U' be their July temperatures respectively, A , A' , their January temperatures, i.e., at aphelion and perihelion; e the eccentricity. Then we have, according to the principle used by Mr. Croll and his reviewer—

$$\frac{S + A'}{S + U} = \left(\frac{1 + e}{1 - e}\right)^2.$$

With the present value of the eccentricity, viz., 0.0168, this gives—

$$A' = 0.0695 S + 1.0695 U,$$

giving to S the usually accepted value, -239° F.

$$A' = 16.61 + 1.0695 U.$$

$$\therefore A' - U = 16.61 + 0.0695 U \quad \dots (1)$$

That is to say, the January temperature of the place in south latitude, ought to exceed the July temperature of the place in equal north latitude by more than 17° F.

In like manner we find the relation between U' and A to be—

$$U' - A = -15.53 - 0.07 A \quad \dots (2)$$

That is to say, the July temperature of the place in south latitude ought to be more than 16° lower than the January temperature of the place in north latitude.

Now it may be replied that geographical and meteorological causes may completely mask these differences. The mean June temperature of the northern hemisphere is known to be higher, instead of lower, than the mean December temperature of the southern hemisphere, and it is considered that this is sufficiently accounted for by the excess of land there. If this explanation be true, the effect of the excess of land must be capable of increasing the mean temperature not only by the number of degrees by which the northern hemisphere exceeds the southern, but by this amount *plus* 17° F.

Subtracting (2) from (1)—

$$(A' - U') - (U - A) = 32.14 + 0.07 A + 0.069 U.$$

This shows that, so long as A' is greater than U' and U greater than A , this difference is greater than 32°. That is, the difference between the excess of summer temperature over winter in the southern hemisphere exceeds the like excess in the same latitude north by more than 32°. Is there any indication of an excess of annual variation in anything like this extent in the southern hemisphere?

But observe the result at the equator. If the latitudes of the two places are continually diminished they will eventually be found both of them on the equator; in which case A' and A become identical, and likewise U' and U . Now the right-hand side of the equation being positive, the left-hand side must be so too. Hence $-(U - A)$, which was negative, in becoming $A' - U'$, which is positive, must pass through zero. This shows that one effect of the eccentricity is that it is not under the equator that the January and July temperatures are the same, but under some latitude north of the equator.

When the two places are both on the equator, or rather when only one place upon the equator is considered,

$$A - U = 16.07 + 0.03 (A + U) \text{ (nearly).}$$

If we put for $\frac{1}{2}(A + U)$ the mean temperature of the equator, or 80° F., this equation gives $A - U = 21°$ F. nearly.

That is to say, the January temperature of a place on the equator ought at the present time to be about 21° F. higher than the July temperature, if the temperature of space is so low as -239° F.

The temperatures themselves would be—

$$A = 90\frac{1}{2}, \quad U = 69\frac{1}{2}.$$

I would ask, therefore, whether there is any indication of so great a difference as the above at any station on or close to the equator.

If $A - U$ is not so great as 21° F., it must be owing to causes which diminish A or increase U . The place being on the equator, would not be reached by the north-east trade-winds; moreover, in July their extension towards the equator would be least. Consequently, they would have little effect to increase U by bringing warmth from the heated continents. In a similar way the south-east trades would be at their weakest in January, and have their least effect to diminish A by bringing cold air and water from the Southern Ocean. Meteorological causes would, therefore, seem to tend rather to exaggerate than to mask the difference in question, if the observations were taken in an insular position near the equator.

I believe there is admitted to be some uncertainty about the value used for the temperature of space. Herschel's investigation in his meteorology may not be thought satisfactory. But it is remarkable that Pouillet, following quite a different method, arrived at almost the same result. At any rate the temperature which the earth would assume, were the sun extinguished, must be very low. But is it so low as -239° F.? If it were, it appears that, if the principle used be correct, those results would follow which I have suggested; and I ask whether any observations bear upon the question? It is obvious that it touches Dr. Croll's celebrated theory somewhat closely.

O. FISHER

Harlton, Cambridge, October 4

Does Sargassum Vegetate in the Open Sea?

HAVING had many opportunities of observing patches of "living Sargassum in the open sea" from the deck of H.M.S. *Challenger* during her cruise in the North Atlantic in the early part of the year 1873, I venture to offer a few remarks in reply to the above inquiry of your correspondent in *NATURE*, vol. xx. p. 552. The track of our ship between Madeira, the Canary Islands, St. Thomas in the West Indies, Bermudas, and the Azores is almost equivalent, as a glance at the map will show, to a complete circumnavigation of the central part of the North Atlantic generally known as the Sargasso Sea. During this cruise *Sargassum bacciferum* was met with frequently so as to render the appearance of this seaweed a sight quite familiar to all on board the *Challenger*. It was first seen on March 2 in about lat. 22° 30' N., long. 42° W., halfway between the Canaries and the West Indies. Again on March 6, lat. 21° N., long. 49° W., quantities of gulf-weed drifted past the ship. On more than one occasion large patches of Sargassum were observed extending from the vicinity of the vessel to a great distance. The gulf-weed was also encountered between St. Thomas and the Bermudas group, and was last met by us between the latter islands and the Azores on June 18, lat. 35° N., long. 53° W.

As regards the exact form and appearance of this interesting alga, I cannot do better than quote from the graphic description given by Sir C. Wyville Thomson in the pages of "The Atlantic," vol. ii. pp. 9, 10:—

"They (the patches) consist of a single layer of feathery bunches of the weed (*Sargassum bacciferum*), not matted but floating nearly free of one another, only sufficiently entangled for the mass to keep together. Each tuft has a central brown thread-like branching stem studded with round air-vesicles on short stalks, most of those near the centre dead, and coated with a beautiful netted white polyzoan. After a time vesicles so encrusted break off, and where there is much gulf-weed the sea is studded with these little separate white balls. A short way from the centre, towards the ends of the branches, the serrated willow-like leaves of the plant begin; at first brown and rigid, but becoming farther on in the branch paler, more delicate, and more active in their vitality. The young fresh leaves and air-vesicles are usually ornamented with the stalked vases of a *Campanularia*. The general colour of the mass of weed is thus olive in all its shades, but the golden olive of the young and growing branches greatly predominates. The general effect of a number of such fields and patches of weed, in abrupt and yet most harmonious contrast with the lanes of intense indigo which separate them, is very pleasing."¹ On p. 339 of the same volume we find the following remark:—"Very few of the higher algæ live even occasionally on the surface of the sea; the notable exception is the gulf-weed

¹ "The Atlantic," by Sir C. Wyville Thomson. (London: Macmillan and Co., 1877.)