of the body capable of communicating the highest temperature, but by this, plus the minor radiators or convections of the cooler bodies? The words I have put in italics distinctly imply such an assumption.

He seems to forget that I did, in the first place, observe and record the temperature of the surrounding medium. It was the 19° C. which served as my starting-point. As no additional radiations were introduced beyond those of the flames to be experimented upon, and the blackened bulb of my thermometer was surrounded by polished reflecting metal surfaces on all sides, except that exposed to the flames, all the subsequent increments of heat were unquestionably due to the radiators from those flames, whether they came directly from the flames themselves or were received and reflected from the back and sides of the polished chamber. Fully admitting the desirability of a continuous record of the heat thus communicated to the surroundings of the thermometer during the experiments, I nevertheless firmly maintain that, rude as it was, my apparatus (I refer, not to the thermometer, but to its adjuncts) was far superior to Capt. Ericsson's. Mine was liable to a small source of error from a possible accidental irregularity of radiation by the thermometer bulb, but his was specially devised to ensure a large amount of such irregularity, continually increasing with the progress of the experiments. It is not a little surprising that so careful and luxurious an experimentalist as Capt. Ericsson should have overlooked the fact, that the very precautions which he so elaborately introduced to secure equal radiation from his bulb are precisely adapted to produce the contrary result.

The arrangements by which his thermometer is "enclosed in an exterior vessel charged with water kept at a constant temperature" of 60° by communication with a capacious cistern, directly violate the conditions demanded by the Newtonian law of radiation, of which Capt. Ericsson is so able a champion; for as the experiment proceeds with an increasing number of flames, and consequent rising of the thermometer, this constant temperature of the water jacket goes on steadily augmenting the difference between the temperature of the bulb and that of its surroundings, and consequently secures just what it is intended to prevent, viz. a variable radiation. What is required to secure a constant degree of radiation from the bulb is not the constant temperature of the surroundings, but a temperature steadily increasing at the same rate as that of the bulb, in order that the *differential* and not the *absolute* temperature of the surrounding medium, &c., should remain constant. This was rudely obtained in my simple apparatus, as both the thermometer and its surroundings were simultaneously influenced by the same radiations.

Capt. Ericsson takes great pains to controvert my "assumption that the intensity of a gas flame is proportional to the gas consumed." This is unnecessary, inasmuch as I never made any such assumption, but have, on the contrary, endeavoured to prove that such cannot possibly be the case, by showing what becomes of the radiations from the interior of a large solid flame. If he will read chaps. 7 and 8 of "The Fuel of the Sun," he will see how and why this has been done, and learn the true bearings of the experiments under discussion upon this subject.

W. MATTIEU WILLIAMS

P.S.—The present is a suitable opportunity for asking a question which doubtless the philological readers of NATURE can easily answer. Many writers use the words "diathermancy," "diathermanous," "athermanous," &c., rather than "diathermacy," "diathermous," &c. Why is this? We do not say "thermanal" or "thermanometer," &c. Why, then, should we depart from the analogy of ancient usage in constructing the more modern compounds of the same root?

Pollen-eaters

MR. HART's note in NATURE, vol. vii. p. 161, is interesting to those who have paid attention to the subject of fertilisation by insect agency, and would be still more so if he could furnish the names of the species of both plants and *Syrphidae* that have come under his observation.

May I take this opportunity of calling the attention of the readers of NATURE to a suggestion which I made some months since in the *Journal of Botany*, and which has at present met with no response? I believe no greater service could be rendered to this department of physiological botany than a series of observations on the species of insects which frequent and assist in the fertilisation of our wild flowers. I know of no such list even

with respect to our commonest flowers. Here is a wide field for observation during the next season.

London, Jan. 7

ALFRED W. BENNETT

P.S.—At the time of writing the above, I had not seen Dr. Buchanan White's article in the January number of the "Journal of Botany," on "The Influence of Insect-agency in the Distribution of Plants," an admirable introduction to the series of papers I had in my mind.

Welwitschia

If you will kindly permit me, I wish to make an addition to your notice of my paper on "Welwitschia," read at the Linnean Society on the 19th ult. That paper was completed and put in Dr. Hooker's hands about three months ago; and the reading of it was delayed until I had seen Strasburger's recently published memoir on *Coniferae* and *Gnetaceae*. After perusing that valuable work, I added a small appendix to my paper, and it is to the omission of the remarks contained therein that I wish to direct attention.

In the description of the male flower, Strasburger and I almost completely agree. It possesses two outer parts of the perianth, two inner parts, six stamens, which I believe to arise by branching from two primordial stamens, although Strasburger does not agree with me in this, and two carpels. The formula of the flower may be expressed thus:—

In the female flower I had very great difficulty in coming to a conclusion as to the value of the two outer parts, but the inner I concluded was a covering of the nucleus, an ovular integument, and not carpellary. There were only two ways of deciding what was the morphological significance of the two outer parts, either by comparison with the male flower, or by comparing them with the parts in the flowers of *Ephedra* and *Gnetum*. I applied to Dr. Hooker for specimens of these genera, and he has kindly promised to procure them for me. As Strasburger's material for the examination of *Ephedra* and *Gnetum* was imperfect, it is still of importance to examine both in detail. Being, therefore, obliged to fall back on comparison with the male flower (the study of the development alone not being sufficient for the purpose), I described the two outer parts as forming a perianth, although I could not feel certain that I was correct in so doing, and could not explain the occurrence of the short stalk under them, no such stalk existing in the male. On looking at Strasburger's figures of *Ephedra*, I at once saw that I had been in error in describing the outer parts as forming a perianth, and in the appendix stated that they were *carpellary*.

The formula would therefore be:—

The carpels, therefore, exist in both flowers; but whereas in the male they are anterior and posterior, in the female they are lateral. Kindly make this correction, because I do not think that after Strasburger's magnificent work, the *Gymnospermous* theory is for a moment tenable.

Should any correspondent be able to obtain specimens of *Ephedra* and *Gnetum* for me, I would be greatly obliged, as I am desirous of completing my paper on "Welwitschia" by a description of its embryogeny, as well as that of the other two genera. Specimens which have been put in absolute alcohol are by far the best for examination, but that, I fear, could only be obtained abroad with great difficulty.

Dublin, Dec. 27, 1872

W. R. MCNAB

Gauges for Ocean Rainfall

IN reply to Mr. Miller's letter on ocean rainfall, in NATURE, vol. vii. p. 123, I beg to acquaint your correspondent that I have endeavoured to meet the difficulties he mentions, by designing two forms of rain-gauge for use on board ship. One is of a cylindrical form, and composed of a collector and receiver, detachable from each other, and is suspended on gimbals in a frame or vena. The rainfall may be estimated either by a glass scale at the sides, or by emptying the contents into a graduated glass measure.

A description of this instrument as above designed appeared in the *Journal of the Scottish Meteorological Society* for January 1870, and was illustrated by diagrams.

The other form consists of the cylinder as above, divided into col-

lector and receiver, detachable from each other, but it is poised on a pivot projecting from the floor below, into a conical cavity in the bottom of the receiver. It is also enclosed in a square box, from which, in each case, the cylinder is removeable entire for emptying the contents, and the rainfall admits of being estimated in the same way by scales or glass vessels.

A full-sized model of this instrument has been made, and was exhibited at the annual meeting of the Scottish Meteorological Society in July last, and a notice of it appeared in the account of the proceedings of the meeting in the Edinburgh papers of July 4, 1872. It has likewise been exhibited at the Meteorological Office, Victoria Street, London, and its construction has been approved of by several naval officers, and others specially interested in rainfall.

I may add that some gauges are being constructed, with the view of being used on board such steamers as would permit of their being placed under the superintendence of interested and scientific officers.

I hope by-and-by to be enabled to present to the readers of NATURE some results of the observations made by these gauges, which may lead to an introduction of such instruments as part of a ship's equipment, and so to put them in possession of some trustworthy observations of the rainfall at sea.

W. J. BLACK

Star Shower in 1838

I AM not sure that the following extract from my note-book may not have been printed by the British Association; but even in that case it may be thought suitable for reproduction at the present juncture.

"1838, Dec. 7.—A great number of falling stars were observed between 6^h and 7^h. In about half-an-hour 40 were counted, sometimes by one, sometimes two, sometimes three observers—two at a medium. They were of all magnitudes up to the first: the larger dissolved into a train of light, but left no train behind them: the S. and W. quarters were chiefly observed, but their prevalence seemed to be universal: they all fell in nearly a vertical direction, but those in the N.W. and S.E. quarters inclined towards the S.W. The colour of the more conspicuous ones seemed to verge towards orange. Their courses were of no great length. There was at the same time a pale auroral light along the N. horizon from N.W. to N.E., apparently equally extended on each side of the true meridian. The Meteors were not watched after 7^h, but about 11^h upon looking out again I saw one, the only one in several minutes, in the S.W.; but it had no longer a vertical direction, its course pointing now to the N.W.

"For account of this phenomenon as observed by Mr. Mavery at Gosport, see 'Proceedings of the Meteorological Society during the session 1838-1839,' p. 9."

T. W. WEBB

Salmonidæ of Great Britain

IN reply to the Rev. W. S. Symonds's questions (NATURE, Vol. vii. p. 162) regarding the occurrence of certain salmonoids in Welsh and non-glacial lakes, I beg to draw his attention to the sixth volume of the "Catalogue of Fishes," published by the trustees of the British Museum, which, I believe, contains the information for which he asks. I would with pleasure extract this information for him if I were not ignorant as regards the glacial or non glacial character of some of the lakes. The geographical distribution of the various kinds of Charr is given in detail on pp. 125-154, and that of the *Coregoni* on pp. 172-199. The group of Charr and that of *Coregoni* are by no means limited to lakes, many true charr, like *Salmo, fluviatilis*, *fontinalis*, &c., being more or less exclusively river-fish; and *Coregonus oxyrinchus* being common in salt water on the coasts of Holland at certain seasons of the year. In addition to Sir Philip Egerton's observation that he has taken *Salmo ferax* in Lake Bala, I may mention that the British Museum possesses an example from the Lake of Llanberis, presented by S. P. W. Ellis, Esq. (Catal. Fish, p. 93.)

British Museum, Jan. 6

ALBERT GÜNTHER

M. Figuier and the Origin of American Indians

ON page 484 of Figuier's work, "The Human Race," the author speaks of the Mohawk Indians of the Rio Colorado, and

on the opposite page reproduces M. Mollhausen's drawing of two Mojave Indians, as described in vol. lii. of Pacific R. R. Reports, by Messrs. Whipple, Ewbank, and Turner. As the Mohawk Indians of New York and the North-west are so totally distinct from the Colorado Mojaves, I thought it desirable to call attention to the error.

M. Figuier, I notice, in other portions of his work, finds the origin of the original peoples of America a difficult problem to solve, and I think contradicts himself. He states, on page 16, that, "unless we regard men as a solitary exception among all living beings, unless we withdraw them from the operation of the universal laws of nature, we must come to the conclusion that they do but form a certain number of races of one and the same species, and all descend from one primitive unique species." I do grant that it must have been a very unique species, whose descendants could have varied to the extent that man has. But it is not the question of variation of species that I wish to allude to, but the geography of the question. In speaking of what M. Figuier calls "the red race," pp. 404-406, he states—"The Indians cannot be accurately brought into connection with either the white, yellow, or brown race;" and again, "Probably the population which existed in the new world before the arrival of the Europeans was made up of several types different from those that are extant at present in the other regions of the globes, types having a great tendency to modify themselves, and which were obliterated whenever they came in contact with the races of Europe." But to re-ascend back to this primordial population would now be impossible." There is here a plain acknowledgment of a strictly autochthonic American people, modified since by contact with European races. This latter contact we believe, of course, to be purely imaginative; but if there was an autochthonous people in America, as the "primordial population" of Figuier is supposed to be, how then can "all (men) descend from one primitive unique species?" M. Figuier does not believe in the evolution of man from some pithecoïd creature; he claims to have "shown . . . that man is not derived . . . from any animal." How this stand can be taken, and still the unity of the race asserted to be true, we cannot understand: for surely it cannot be denied now, that man was once lower than the lowest savage, although different from modern savages; and, as in America, there have been found traces of man's presence, as old geologically as those found in Europe; as fossil men have been found in California; and drift implements in the river gravels of the Delaware Valley, on the opposite side of the Continent; and as these implements, in part, show that their fashioners were little, if any, in advance of the beings first worthy to be called men, how could they have descended from a stock in common with the European and Asiatic races? It must have been, indeed, a unique species, whose nearest relations spread over the whole continent of North America; or starting somewhere on the Pacific coast, finally reached the Atlantic, yet made no advance—learned nothing in a slow overland journey of three thousand miles. The "primordial population," of which M. Figuier speaks, we doubt not originated in America; its pithecoïd ancestry may have been European or Asiatic, but if so, the "old world" monkey was somewhat Americanised before it evolved that peculiar red-race which we call the Indians. If there ever was land communication between South America and the "old world" tropics, this pithecoïd man may have reached the shores of the Southern Continent, and lost the ape-like characters after his arrival. Either evolved thus, or created *de novo*, as M. Figuier claims, the American savage is purely an American institution, and upsets that unity which M. Figuier claims for every race, tongue and condition, savage and civilised, throughout the world.

CHARLES C. ABBOTT, M.D.

Trenton, New Jersey, U.S.A., Dec. 23, 1872

THE ZODIACAL LIGHT

FOR several nights lately the zodiacal light has been exceedingly bright and well-defined, and more particularly on the nights of November 24 and 27; on the evening of the 24th I found an explanation of what had often perplexed me before, viz. the existence of a faint,