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 *Syrphidæ* 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 Coniferæ and Gnetaceæ. 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:—

$$Ca_2\ Co_2\ An_2^3\ Gn_2$$

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:—

$$Ca_0\ Co_0\ An_0\ Gn_2$$

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.

W. R. MCNAB

Dublin, Dec. 27, 1872

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 vexa. 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-