

historical verification, he thinks only of an apparent shortening of the year, forgetting the month, which, as it has turned out, has proved the tell-tale. Least of all can he have imagined that the question could be answered, or even asked—What becomes of the lost velocity?

But he says expressly that it is probably the tidal influence of the earth that has brought the moon to face the earth constantly as it does. Mr. Tait, in the passage from which I have quoted, treats this suggestion as originally Helmholtz's, though, speaking under correction, I should have said it formed part of Laplace's explanation. Be this as it may, Kant published it when Laplace was five years old.

The essay was reprinted by Nicolovius in his collection of Kant's minor pieces. I do not know the date of this publication, but 1839 is the date of Rosenkranz and Schubert. Yet we have Arago in his latest work, and Herschel at least as late as 1851, affirming the invariable length of the day, not merely as approximately established by history, but (internal disturbance apart) as a direct result of dynamical principles. It is not "general publicity" that was wanting in Kant's case, but special publicity, the publicity of Transactions and Jahrbücher. It is to be hoped that those who work in the several departments of knowledge are now learning to know better what their neighbours are about.

Hadley, Middlesex, Jan. 21

C. J. MONRO

Pollen-eaters

I AM sorry that I am unable to give Mr. Bennett all the information that he desires, as I have never studied the classification of the Diptera, and do not know the species of the flies in question; nor do I like to trust my memory as to those of the flowers. The common dandelion is, however, I think, an especial favourite; and it is evident that in this and other Compositae the movements of any insect among the flowrets must scatter some of the pollen upon the stigmas, and some may even be carried on its legs and body in its wanderings from flower to flower, though the smooth body and cleanly habits of the fly must be rather an obstacle to this. But to some other species of flowers, on the pollen of which I have sometimes seen them feeding, and paid particular attention to this point, I think it very doubtful whether their visits can be anything but injurious.

W. E. HART

P. S.—Allow me to correct a misprint in my former note. By the substitution of an "N" for the initial "h," the expressive popular name of "hoverers," which I used in writing of the *Syrphida*, has been rendered quite meaningless. I should also be glad to know whether I am correct in the use of the word "mandibula," in the same note.

Meteors in South Pacific

WHILE some natives of these islands were preparing my boat for a journey during the night of October 27-28, they were considerably alarmed by the appearance of a very large meteor. As far as I could ascertain from them it became visible near to ζ Ceti, and rushed towards the south-east, leaving a bright train in its wake. One of the natives described it as being as large as a man's head; the others thought it was larger than that. These statements about size must be taken with caution, but from the excitement it caused I believe the meteor was a very large one. It was seen a little before local midnight.

I was travelling the same night from midnight to 3 A.M., and during that time I observed eleven meteors: two of which appeared near to Pollux, and disappeared near to α Hydæ. The other nine all appeared near to η Canis majoris and proceeded through the constellation Argo Navis. The last I saw, at 3 A.M., reached nearly to the Southern Cross, which was then just above the horizon.

Only one of the eleven meteors I saw was at all remarkable for size, and that was about as bright as Sirius, with which I compared it. This was the only one which left any perceptible train.

I should add that, although Samoa is in the Western hemisphere, our local time is that of the Eastern hemisphere; hence the dates given are twenty-four hours ahead of the true time of our geographical position.

S. J. WHITMÆ

Samoa, South Pacific, Oct. 30, 1872

Aurora Spectra.

MR. CAPRON'S notice of my observations with regard to the auroral spectra compels me to say a few words with regard to them which I should rather have deferred till I could confirm my suspicions by fresh experiments. The spectrum which appears to coincide with the aurora, is not the ordinary spectrum of oxygen obtained by the disruptive discharge, but is, I have little doubt, that described by Wällner (Phil. Mag. p. 420, vol. xxxvii.) It is not uncommon in ordinary *lumière* tubes, but I obtained it with a feeble discharge in tubes filled with electrolytic oxygen, and therefore put it down to that gas. It is now two years since I made these experiments. Circumstances compelled me to leave the research incomplete, and I have hitherto been unable to return to it; but greater experience in the difficulties of spectrum work has suggested sources of error which I did not then suspect, and I should not feel any surprise if the spectrum in question turned out to be that of some carbon compound from the india rubber connections. It certainly has a strong family likeness to these, and if it were so, would confirm Prof. Piazzi Smyth's coincidence with citron acetylene. I will endeavour shortly to decide this.

As to instrumental power, the greatest I have used on the aurora has been that of a 60° bisulphide prism; but this is sufficient to show in both lines a breadth distinctly greater than the slit. Unfortunately, however, as I think I have before stated, the auroral line appears equally nebulous on both sides, while that in the tube spectrum is shaded mainly towards the red. On the other hand it is fair to note that this ceases to be visible when the light is faint and the dispersive power not greater than that employed on the aurora.

North Shields, Jan. 18

HENRY R. PROCTER

On the Words "Diathermanous," "Diathermancy," etc.

IN reply to the question of Mr. W. M. Williams in the postscript to his letter (*NATURE*, vol. vii. p. 202) I beg leave to make a few observations. I presume that the author of the above terms thought that the idea of the permeability of a medium by radiant heat could be better expressed by derivatives of the verb $\thetaερμαίνω$ (I heat) than by those of the more elementary words $\thetaέρμος$ (hot) or $\thetaέρμη$ (heat). To the former of these classes "diathermanous," "diathermancy" (from $\thetaερμαντис$), and "diathermacy" (from $\thetaερμασία$), all belong, though they are not very regularly formed, as our English termination "cy" corresponds to the Latin *tia*, as "clemency" from *clementia*, or the Greek "τεια," as "policy," from *πολιτεία*. "Diathermous" belongs to the latter class.

According to precedents the substantive corresponding to "diathermous" might be "diathermy," as bigamous, bigamy, or "diathermeity," as diaphanous, diaphaneity, or "diathermosity," as porous, porosity, but not "diathermacy," in which the second letter "a" clearly points out its derivation from the verb.

In point of form the words of Latin origin, "transcalent," "transcalency," which have been used in the same sense, are quite unobjectionable, and have the great advantage of corresponding in form with "transparent," "transparency," "translucent," "translucency," so that the words expressing permeability by the rays of light and heat are of similar form, though perhaps the derivation of "calescence" from a neuter verb, when an active sense is wanted, may be an objection to their use.

Seeing, therefore, that "diathermous" has no very eligible substantive corresponding to it, I submit that the original words "diathermanous" and "diathermancy" are most eligible in sense and least objectionable in form of the words of Greek derivation. Some perhaps, in spite of the above objection, may prefer the Latin words.

W. D. L. L.

Dr. Sanderson's Experiments

MAY I ask Dr. Burdon Sanderson to kindly state in your columns one or two matters relating to the cheese employed by Dr. Bastian in the experiments at which he assisted. He justly remarks on the value of a knowledge of methods of demonstrating important facts—and I would therefore ask for the advantage of other readers as well as my own—some further information. I have already to thank Dr. Bastian for stating the specific gravity of his turnip-solution, in reply to my request.