On the other hand, it is known that the activity of the aurora is related to magnetic disturbance, and data exhibiting this relationship have been published in reports of the Expedition ("Terrestrial Magnetism" and "Observations on the Aurora"). In these circumstances, Dr. Chree kindly lent his original figures for hourly character number (representing magnetic disturbance), and these were used for making a comparison with the figures for potential gradient during winds up to 5 miles per hour, for the period March 1911 to February 1912.

For the purpose of this comparison, the potential gradients corresponding to Dr. Chree's magnetic character numbers (0=slight and 2=great magnetic disturbance) have been meaned for all hours during which the wind was not greater than 5 m.p.h. Generally speaking, the mean gradients corresponding to these character numbers vary during the course of the day similarly to the curves shown on p. 315 of "Meteorology," vol. i. The mean value for the year corresponding to

Character number 0 is 89 volts/metre.

" " 1 is 90 "

" " 2 is 96 "

Taking, however, means for each hour of the day, and allowing equal weight to each hour, the corresponding numbers are 88, 89, and 91. The evidence seems, therefore, to be against any direct connexion between magnetic disturbance and potential gradient at this station.

Figures have been derived for the mean value of the potential gradient at each hour of the day, for four periods of three months each, and these figures also do not encourage much hope in the reality of such a connexion. The mean values for the four quarters (commencing for convenience in February, and equal weight being allowed to the mean value for each hour of the day) are:—

Quarter.	Magnetic Character Number.		
	0	1	2
Feb. to Apr.	82 (82)* volts/metre	81 (83)*	88 (88)* volts/metre
May to July	77 (76) "	77 (78)	78 (78) "
Aug. to Oct.	93 (93) "	93 (94)	93 (96) "
Nov. to Jan.	106 (111) "	98 (99)	107 (107) "

* The figures in brackets represent the corresponding means when each observation during the quarter is given equal weight.

C. S. WRIGHT.

Wey Lodge, Portmore Park,
Weybridge.

A Substitute for the McLeod Gauge.

THE letter in NATURE of August 23, p. 276, fails to justify an attack on the McLeod gauge. The theory that water is absorbed by glass is a theory only, of which there is no proof, and there is no proof that the McLeod gauge reads incorrectly or even that it is inconsistent. Any variation can be observed only by comparison with another method of measuring low pressure, which is just as likely to vary or be inconsistent as the McLeod gauge. Animal emanation, the glass-blower's breath for example, is very difficult to dislodge from the surface of glass and might easily cause a disturbance in the electric spark phenomena.

A final pressure of 0.001 mm. is mentioned; this means a pressure which will support a column of mercury 0.001 mm. high. The McLeod gauge is the only practical means of determining this measurement directly.

In a former letter we were told that the McLeod

gauge may have special uses (such as the calibration of other gauges) "under rigidly controlled conditions." This can only mean clean glass, pure mercury, and an efficient drying medium, but it goes to prove my assertion that "there is no substitute for the McLeod gauge, in its perfect measurement of low pressures, expressed in height of mercury column."

HENRY A. FLEUSS.

47 Albert Road,
Caversham, Reading,
September 4.

MR. FLEUSS's letter answers itself. But may we trespass on your space for the last time in order to correct a misunderstanding which others than Mr. Fleuss have shared, and for which our original letter was doubtless responsible?

We never intended to deny that the McLeod gauge, used rightly, is a thoroughly trustworthy instrument and almost indispensable for the calibration of all other gauges, except possibly the Knudsen. But we deny that, as it is often used, it is trustworthy or convenient. It is still, or was recently, an almost universal practice to attach a McLeod to every pumping system and to take readings on it a few minutes after the apparatus has been exposed to the air, and long before the condensed water is removed from the glass. McLeod gauges used in such circumstances are, we repeat, "cumbrous and misleading devices," "mere relics of the past." They are responsible for the ridiculous underestimating of pressures which is still common in the papers of serious investigators. It is for gauges used in such circumstances that we advocate the Pirani gauge as a substitute. Its increasing use shows that we are not alone in our opinion.

N. R. CAMPBELL.

B. P. DUDDING.

J. W. RYDE.

Chimæras Dire: Transplantation of Heads of Insects.

I AM sorry that Prof. Przibram (NATURE, September 6, p. 347) should think that I treated Dr. Finkler's statements with unbecoming levity. The object of my letter was simply to direct attention to Dr. Blunck and Dr. Speyer's criticisms of Dr. Finkler's work and not to express agreement with one side or the other. Now that Prof. Przibram has undertaken to reply to these criticisms, the accuracy of my brief summary is perhaps no longer a matter of great importance, although I fail to see that it is in any way seriously misleading. It is true that I ought to have written "thorax" instead of "wing-cases," and also that Dr. Finkler has not yet told us with what he fed his inter-specific monsters; but I still think that the sentence "a male head led a female body into unwonted perversities" is not an unfair version of what Dr. Finkler has said. To those of us who have only a scanty knowledge of the subject it seems a less extraordinary thing that "the male head kept its usual reaction towards the female . . . when transplanted to a body of opposite sex" than that the body should acquire a new reaction as a result of the operation.

The complaint that Dr. Finkler had refused to submit his specimens for investigation by others was not mine, but was made, as I stated, by his German critics. The specimens which Mr. Boulenger brought to London have been repeatedly exhibited here. I was told that they were not to be submitted to dissection or section-cutting, but I am now informed that no such condition was imposed by Dr. Finkler in handing over the specimens.

W. T. CALMAN.

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