He suggests that such winds would be even more persistent in the hypothetical southern continent. In that connexion we must remember that in spite of the northern cold summer monsoon the Mackenzie coniferous forest does extend more than 150 miles north of the Arctic circle, and indeed north of the southern limit of the maximum "tides" in the Mackenzie (by tide we here mean the rise of five or six feet in the eastern Beaufort Sea caused occasionally by westerly gales. The tide, proper, is less than one foot).

No such extremes as the occasional Arctic +95° F. are necessary for the prosperity of conifers. Mr. Elihu Stewart, the Forestry Commissioner of Canada, put on record in the publications of his department (in 1907 or 1908) that he had seen trees 100 feet high more than 100 miles north of the Arctic circle in the Mackenzie delta. Trees above 75 feet in height abound forty or fifty miles farther north. I do not know of any systematic temperature observations taken in the Mackenzie delta at the approximate northern limit of the conifers, but I suppose that 70° F. in the shade is there exceedingly rare. I should judge then that any hypothetical conditions in the Antarctic considered adequate to produce maximum temperatures of 75° F. (even though rarely) would give an adequate heat factor for coniferous forests.　　VILHJALMUR STEFANSSON.

New Court, Middle Temple, E.C.4,
　　　September 5.

Can the Geostrophic Term account for the Angular Momentum of a Cyclone?

IN meteorological discussion it is sometimes implied that the rotative velocity of the air comprising a cyclone is primarily accounted for by the geostrophic term in the equation of motion.

If considerations of a second order of magnitude be ignored this hypothesis is capable of simple treatment in its main features and is worth examination. Imagine an initial circulation round an axis, of any magnitude whatever, and consider an elemental mass δm at distance r from the axis.

The radial velocity of this element is then denoted by dr/dt taken positive outwards.

The increase in the angular momentum of δm about the axis in time δt due to the geostrophic term is

$$2\omega \sin \phi \cdot r \cdot \delta m \cdot \delta t \frac{dr}{dt},$$

taken positive clockwise in the Northern Hemisphere.

In the limit this becomes $\omega \sin \phi \cdot \delta m(2r \cdot dr)$, or $\omega \sin \phi \cdot d(r^2 \cdot \delta m)$.

If ϕ be taken as constant, and we sum up for the whole mass of the cyclone, we see that the increase in the total angular momentum in a given time is equal to the product of $\omega \sin \phi$ into the corresponding increment in the moment of inertia about the axis.

The extent to which the moment of inertia can vary is represented by the deepening or filling up of the cyclone, and a rough calculation shows that the possible angular momentum so accounted for is very small, and is, moreover, of the opposite sign to that required by the hypothesis under consideration.

In the above analysis two things have been ignored: (1) The question of the variability of ϕ; (2) the resistance of the earth's surface. If it be supposed that there is a systematic difference between the latitude of the surface inflow and that of the outflow above, there is the possibility of the existence of a term of appreciable magnitude in the case of a large cyclone. On this point observational evidence is weak, and all that can be said is that for a small system the effect must be small, and for large ones we have no reason to suppose it to be large.

The resistance of the earth's surface continually tends to reduce the rotational velocity, and the magnitude of the term concerned is, moreover, large compared with (1), which is of the nature of a differential effect.

On the whole, it seems clear that the 'angular momentum of a rotating system cannot be accounted for by the geostrophic term, and that its origin must be sought in the initial relative velocities of masses of air subsequently included in the circulation.
　　　　　　　　　　L. H. G. DINES.

Benson, Wallingford, August 31.

Zoological Nomenclature: Spirifer and Syringothyris.

IN accordance with prescribed routine, the Secretary of the International Commission of Zoological Nomenclature has the honour herewith to notify the members of the zoological profession that Miss Helen M. Muir Wood, of the British Museum of Natural History, has submitted the generic names Spirifer, Sow, 1816, and Syringothyris, Winchell, 1863, to the International Commission, for suspension of rules, with the view of retaining *Anomia striata* Martin as genotype of Spirifer, and *Syringothyris typa* (s. *Spirifer carteri* Hall) as genotype of Syringothyris.

The argument is presented: (1) that under the rules *Anomia cuspidata* Martin is type of Spirifer, and Syringothyris is synonym of Spirifer: (2) but for seventy years, practically all authors have, in conscious opposition to the rules, taken *A. striata* as type of Spirifer, and *Spirifer carteri* s. *Sy. typa* as type of Syringothyris: (3) so many species are involved in this instance that the application of the rules would present greater confusion than uniformity.

The secretary will postpone vote on this case for one year, and invites expression of opinion for or against suspension in the premises.
　　　　　　　　　　C. W. STILES,
　　　　　　　　　　　　Secretary.

Hygienic Laboratory, Washington, D.C.

Colour Vision and Colour Vision Theories.

PROF. PEDDIE states in his letter in NATURE of September 8, p. 362, that the facts that I have given as totally opposed to the trichromatic theory can be explained by it. If he will show how this can be done I can then deal with his explanations. Directly the trichromatic theory is put in a definite form its failure becomes evident. For example, it has been stated frequently by others that the construction of the trichromatic theory given to explain simultaneous and successive contrast will not explain colour blindness, and *vice versa*. There is no fact that directly supports the trichromatic theory. In numerous cases papers written to support the trichromatic theory are found on examination to give facts strongly adverse to it. Prof. Frank Allen has written a number of papers supporting the trichromatic theory. In a paper on the Primary Colour Sensations (*Philosophical Magazine*, vol. xxxviii., July 1919, p. 81) Prof. Allen writes: "But it is difficult to understand why the exceedingly complex region between $\lambda \cdot 470 \mu$ and $\lambda \cdot 570 \mu$ should exhibit, as it does, persistency curves with only one elevation in the green." The reader should note that it is only on the trichromatic theory that this region is complex. On my theory it is quite simple and the results should be as stated.
　　　　　　　　　　F. W. EDRIDGE-GREEN.

London, September 10.