

struts, even if arranged as he first conceived them to be, could not have failed by flexure during the wildest hurricane.

4. Sir George finds fault with the connection of the brackets, and "can hardly imagine that trains could be run through at speed." I should have been pleased to have explained the connection to Sir George, but he has not sought to know anything about the details of the bridge, and, I am sure, would be much puzzled to give your readers even the vaguest possible description of the connection, which he nevertheless stigmatises as "not very perfect."

In conclusion, as Sir George has not done so himself, I would warn any young student who may have read the investigation contained in the appendix to the first letter, that the methods therein proposed would lead to an over-estimate of the strength of struts of ordinary proportions by from 200 to 300 per cent. This warning is the more necessary, as the general tenour of Sir George Airy's letter might make a student imagine that he erred, if anything, in the direction of excess of caution, whereas the application of the principles laid down by him would, in the case of the Forth bridge, result in the compression members being made only one-third of the strength considered expedient by Mr. Fowler and myself.

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P.S.—It may interest some of your readers to know that the maximum force recorded during recent storms by our wind pressure plates at the Forth has been 20 lbs. per square foot, upon the small and light plate having an area of 2 square feet, and 12½ lbs. upon the large and heavy one, with an area of 300 square feet. The same ratio holds good down to pressures of 2 lbs. per square foot, and it appears pretty certain that the higher blasts are of such momentary duration and of such unequal distribution, that even a small sized railway bridge could never experience ordinary anemometer pressures. Other reasons for a reduced pressure on a large surface have been advanced by Dr. Siemens in a recent number of the *Comptes Rendus*. Nevertheless, in this instance of the Forth bridge we have assumed that a 56 lbs. hurricane will act simultaneously over the whole width of the Forth, with a resultant lateral pressure of no less than 8000 tons upon the main spans. We have further assumed that the said hurricane might blow down one side of the Forth, whilst a dead calm prevailed on the other side, and have even provided for the twisting action upon the piers and superstructure due to a 56 lbs. hurricane blowing up the Forth on one side, and down it on the other. To ascertain what lateral pressure a 56 lbs. hurricane would cause, we tested, both in air and in water currents, a large model of the bridge, with cross-bracing complete, and ascertained its equivalent in square feet of flat surface. Under any of the conditions of wind pressure enumerated above, combined with any distribution of the rolling load, the resultant stresses upon superstructure, holding down bolts and piers will be far within the safe working limits as determined by our experiments upon the respective materials.—B. B.

Altitude and Weather

IN NATURE, vol. xxvii. p. 176, you notice the remarkable warm and dry weather September 21 last on Ben Nevis, during an anticyclone, and, as at the foot the air was relatively cold and humid, you see in the heat and dryness on the mountain an effect of descending air currents. In this you are quite right, but I do not think you are right in estimating that this air was saturated at a certain height above Ben Nevis. The fact is this: the increase of temperature from a certain height above sea-level to the latter being *de facto* much less than the dynamical increase of a stratum of air, due to compression sinking down, a downward current of air will be generally warm, and relatively dry. It does not matter if it sinks along the slopes of mountains (as the foehn), or vertically, as modern meteorology considers it to be the case in anticyclones. There is only one great difference: the air currents down a slope may be, and often are, very violent, and only when they are so, their relative heat and dryness are felt, while the downward currents in an anticyclone are so gentle that they are seldom felt or directly registered, and that mostly the thermometer and hygrometer are our only means of detecting them. On account of their slow motions, the effect of these downward currents during anticyclones is little felt in valleys and plains, as (1) they are even more retarded near great land surfaces; (2) in the colder time of the year, especially when the ground is covered with snow, the radiation from the soil lowers the temperature of the lower strata. Thus during anticyclones in winter a very low temperature is generally experienced

in plains and valleys, due to radiation, and a very high temperature and low humidity on isolated mountains, due to descending currents of air.

These conditions are best realised during protracted and considerable anticyclones, and it was Prof. Hann's merit to have explained this fact.¹ The exceedingly protracted anticyclone of December, 1879, in Central Europe, was especially favourable to the proof of the existence of descending currents, as the cold was great in the valleys, even in the high ones, like the Engadine and the Davos, but the air was warm and very dry on isolated mountains. An example from the best mountain observatory of that time, the Puy de Dôme, and the foot of it, will suffice, nine days, December 20-28, 1879, at 6 a.m.

	Feet.	Temp. F.	Relative humidity	Amount of Cloud
Puy de Dôme,	4813	38·8	38	1·3
Clermont (base)	1273	8·2	91	0·7

There is all reason to think that in these days there was no saturated stratum of air even considerably above, say the Puy de Dôme.

I must remark that Prof. Hann, in his last work, "Der Föhn in Bludenz," does not sustain his former opinion that great precipitations on the windward side of mountains is necessary to the appearance of a foehn on the leeward side. His opinion now is, that a considerable barometric gradient and the drawing in of air from considerable heights are alone necessary, for even if the air on the mountains is not abnormally warm, it will come down warm and relatively dry.

A. WOJIKOFF

Ofizerskaja, St. Petersburg, December 15-27, 1882

The Fertilisation of the Speedwell

I FEAR that Dr. H. Müller's passage in Schenk's "Hand-buch" would occupy too much space to be given here in full; but I can condense what he says into a few lines. Dr. Müller takes the *Veronica chamædrys* as representing a type of flowers in which the anthers have to be brought into a position to strike the body of the insect by the action of the insect itself. He finds the same arrangement in the *V. urticæfolia*. These flowers are visited by insects of various kinds, but their structure is, he thinks, explained only by what takes place when they are visited by *Syrphidæ*. When one of these insects visits such a flower, it hovers for some seconds before it, then settles upon the lower lobe of the corolla, without noticing the style which is coloured like the corolla, and which is now under the insect's body. It then crawls higher to reach the nectary, and in doing so bends down the stamens—which are also coloured like the corolla—until the anthers strike against the under part of the insect's body. The pollen thus obtained is carried to another flower, and brought into contact with the stigma when the insect first alights; and fresh pollen is again obtained by the attempts to reach the nectary. Dr. Müller either knows from observation or assumes that in the *V. chamædrys* anthers and stigma are mature at the same time. He attaches importance to the fact that both stamens and style are coloured like the corolla, and therefore appear to escape the observation of the insect; and the thinness of the base of the stamen is also noticed by him as one feature in the adaptation of the flower to the visits of *Syrphidæ*. He does not refer to the looseness of the corolla. Mr. Stapley's suggestion that this may play some part in the work of cross-fertilisation is an ingenious one, and calls for further research.

As to the *V. hederæfolia*, Dr. Müller mentions it as one of the plants that have a tendency to keep their flowers half-shut in cold and rainy weather, and thus to become cleistogamous.

I am sorry that I misunderstood Mr. Stapley's first letter upon any point; but he has misunderstood mine also, if he thinks I was not aware he wished to call attention "to the adaptation of the flower for cross-fertilisation." I wrote as briefly as I could, and naturally assumed that he would understand I was not thinking merely of the fact that Diptera drew down the stamens.

ARTHUR RANSOM

Bedford, December 23, 1882

THE SACRED TREE OF KUM-BUM

THE dissipation of illusions is always a little painful, even after repeated experience of the process. I must confess, then, to some feeling of injury at learning from Mr. Keane's interesting review in NATURE, vol.

¹ *Zeitschr. für Meteorologie*, p. 129, 1876.