

The Newtonian Constant of Gravitation.

I SHOULD be obliged if you would allow me to make a correction in my lecture at the Royal Institution, published in NATURE, Aug. 2, 9, and 23, on page 331. I have stated that in pieces of apparatus geometrically similar but of different dimensions, the disturbances due to uncertain convection currents are likely to be in the proportion of the seventh power of the linear dimensions. Having discussed this at some length lately with Prof. Poynting, I find that I was in error, and that in reality the disturbances would be proportional to the fifth power of the linear dimensions if the circulation of the air were so extremely slow as to be steady. If, however, its velocity were sufficient to give rise to unsteadiness, the rate at which momentum would be given to the suspended portion of the apparatus would depend on the square of the velocity, at least in part, and as the part depending on the square increased in importance the disturbance would gradually rise to the eighth power. So long, therefore, as the apparatus is small enough to prevent terms involving the square of the velocity from being appreciable, the ratio of the disturbance to the couple to be measured or the stability is the same whatever the size; but as soon as the apparatus exceeds this, then the disadvantage of size very rapidly becomes evident.

Of course the objection due to the great increase of time which must elapse between the handling of apparatus and its being fit for observations to be made, which accompanies increase of size, remains.

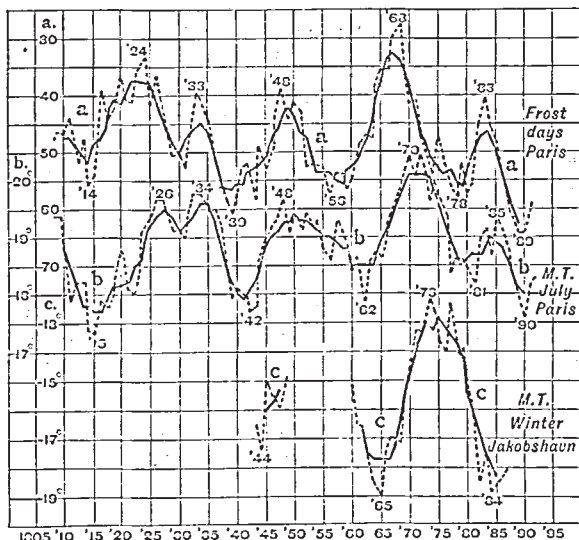
As the consideration of the relation between disturbance and couple to be measured, and its variation with linear dimensions, is a matter of great importance in the design of most instruments in which the movements of a suspended system supply the means of measurement, there is an additional reason for correcting in these columns the error that I made.

C. V. BOYS.

On Some Temperature-Variations in France and Greenland.

THE relations indicated in the diagram sent herewith are, I think, instructive; and they might perhaps be found to contain some useful clues to coming weather.

This diagram has two kinds of curves, dotted line, and continuous. Both are smoothed curves. In the former, the actual



values have been smoothed with averages of five; and in the latter, those averages have, in their turn, been treated in the same way. High points in all the curves denote heat; low points cold.

The first pair of curves (a) show, by averages, the variation in the number of frost days in Paris in October to April of each cold season since 1806. (I designate each cold season by the

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year in which it ends; 1806 meaning 1805 6, &c.). These are inverted curves, the numbers increasing downwards. They present a succession of (say) five obvious waves, which, with regard to the crest intervals, are neither wholly regular nor wholly irregular, the intervals of the smoother curve being, in series, 12, 15, 18, and 17 years.

The second pair of curves (b) show the variation in mean temperature of July at Paris during the same period; and we may perceive in these a general correspondence to the first curves, with, however, a distinct tendency to lag somewhat.

There is a good deal of general similarity, of course, between the weather of Paris and our own, and between the longer waves of variation of July and those of the whole summer. Hence we find, e.g., that a once-smoothed curve of mean temperature of summer at Greenwich presents obvious minima in the years 1814, 1839, 1862, and 1881. Compare this with the Paris July curve.

It is known that in our climate a severe winter tends to be followed by a cool summer; but the facts here presented are, it will be perceived, of a somewhat different order, and wider scope.

The third pair of curves (c) relate to Jakobshavn, on the west coast of Greenland, and show the smoothed variations in mean temperature of winter (December-February) for a series of years. These curves are short compared with the others, and are interrupted at one part; but so far as they go, they seem to present a similar variation, with further lag; so that, as compared with the Paris frost curve, we find the phases have come to be nearly opposite. Our European winters, indeed, seem to be generally opposite to those of Greenland. This is pointed out, as regards Vienna, by Dr. Hann in the paper from which those Jakobshavn figures are obtained. (Met. Zeits. 1890, p. 113.)

By way of comparing these curves, it may be useful to note the lowest points of the three once-smoothed curves; and the intervals between those of the same curve and of different curves. (The intervals, in years, are given in brackets.)

Paris, frost,	1814 (25),	1839 (17),	1856 (22),	1878 (11),	1889
	(1)	(3)	(6)	(3)	
Paris, July,	1815 (27),	1842 (20),	1862 (19)	1881 (9),	1890
	(2)	(3)	(3)	(3)	
Jakobshavn, winter,	1844 (21),	1865 (19),	1884.		

The fact of this lagging correspondence would appear to suggest that the general variations of our winter seasons are, in some measure, a key to those of approaching summers, and also, if the Jakobshavn correspondence were confirmed by a longer series of data, to those of approaching winters in Greenland.

An explanation of these curious facts may perhaps be supplied by those who have a comprehensive knowledge of polar meteorology and its relations.

A. B. M.

New Element in the Sulphur Group.

DR. B. BRAUNER, of Prag, in 1888 made a careful investigation of the atomic weight of tellurium, an account of which will be found in C. S. J. 320, p. 382. In accordance with Messrs. Newland's and Mendeléeff's Periodic Law, tellurium should have, if pure, an atomic weight of 125, or even lower.

Prof. D. Mendeléeff takes Sb = 122, Te = 125, I = 127. Taking the latest numbers for antimony and iodine (Sb = 120, I = 126.8), Dr. Brauner proceeds to investigate the atomic weight of Te, for which he finds by a great number of experiments the number 127.65. As is only to be expected from such a staunch advocate of the Periodic Law, he at once came to the conclusion that neither himself nor former experimenters (Berzelius, in 1812, 1818 and 1832; von Hauer, in 1857, &c.) had been dealing with the pure element. As he puts it, tellurium is not an element.

Tellurium prepared from the dibromide gave the high atomic weight of 130. Prepared from the tetrabromide, which latter was distilled in vacuo, the resulting element being distilled in a current of hydrogen, Te = 127.65. Under these circumstances, he says, no doubt one constituent of "tellurium" partly escapes, thus reducing the atomic weight. He terms tellurium the gadolinium of the sulphur group.

In the following year, 1889, Prof. Mendeléeff predicted (Faraday Lecture, C. S. J. 323, p. 649) an element with atomic weight 212, which he calls Dvi (= Bi-) tellurium, for which he suggests the symbol Dt, and predicts the following