greater, in about the ratio of ·275 to ·158, than it was close to the limb; these figures representing approximately the amount of polarisation respectively at ten minutes from the limb, and close to it, the total light being unity.

We may therefore conclude that

1. The corona is radially polarised.
2. This polarisation increases as we recede from the limb.

The bright lines seen in the spectrum of the corona inform us that *part* of the light we receive from it is intrinsic, that is, that the gases composing it are incandescent, and from their proximity to the sun we should scarcely expect anything else; yet this fact in no way renders it impossible that much of the light we receive from the corona should be reflected or scattered by minute particles of, perhaps, denser matter, probably incipient cloud, suspended within it, as such particles are supposed to exist in the earth's atmosphere, in order to account for the polarisation and blue colour of the sky. This supposition, when we remember that the temperature, and hence the amount of intrinsic light, must decrease as we recede from the sun, would amply account for the increase of polarisation with distance from the limb. There are, however, two other causes which may be named as adequate to produce this effect. Probably in reality the increase is due to the three causes combined.

When the plates were inclined so as to neutralise the corona polarisation, I saw faint dark centred bands on the portion of the moon's disc in the field. I did not observe any when the plates were at right angles to the axis of the telescope, but I think I should have noticed them had they existed; so that although there was a sensible amount of light on the moon's disc sufficient to show bands when polarised by the glass plates, I do not think it was perceptibly polarised itself. This would tend to show that the light was reflected from the moon itself, and not from the intervening atmosphere.

G. K. WINTER

Erratum of the Errata, or, "A Few Millions"

I AM indebted to Mr. A. Cowper Ranyard, of London, for calling public attention to errors existing in the illustrative appendix to a research entitled Acoustical Experiments, &c., which article of mine the Editor of NATURE honoured with a republication in his journal on May 9, 1872.

The existence of these errors has been known to me since a few weeks after the original publication of my paper; but as they did not affect in the least the subject proper of the research, and would be apparent to any one who might take the trouble to review the calculations, I allowed them to pass unnoticed, and even now would not pursue the subject further had Mr. Ranyard really corrected my errors; but he has *himself* committed the error of "A Few Millions" (the title of his communication*) which he would attribute to *me* when, in these words, he undertakes the correction of my figures. "Taking the velocity of light as 185,300 miles per second, and the wave-length of D_1, as given by Angström, at 0·00058950 millimetres, gives 5,058,700,000,000,000 vibrations per second, or a little more than *five thousand millions of millions*, instead of a little less than *six hundred millions of millions* vibrations per second, as given by Dr. Mayer." The following is the correct calculation:—

$$185{,}300 \text{ miles} = \frac{298{,}212{,}000{,}000 \text{ mm.}}{\cdot 0005895 \text{ mm.}} = 505{,}870{,}000{,}000{,}000$$

and 5,058,700,000,000,000 (Mr. A. C. Ranyard's result) minus 505,870,000,000,000 (Mr. Mayer's result) gives Mr. Ranyard 4,552,830,000,000,000 tremors.

Thus it appears that both Mr. Ranyard and myself commit errors in simple arithmetic, but I am sure that our mutual friends will not attribute them to want of sufficient mathematical culture to accomplish "a simple rule-of-three sum." (A. C. R.) He that is without sin let him first cast a stone. I, however, do not wish Mr Ranyard's errors in any way to extenuate my own greater negligence which has disfigured the appendix of my paper, containing, as it does, "some strange numerical errors, which perhaps it will be well to point out, lest some of your readers should make use of the numbers given at the end of the paper without previously testing them." (A. C. R.) I will therefore ask my readers to substitute for the last paragraph under the heading of "Quantitative Relations in the Experiments and Analogical Facts in the Phenomena of Light," the following:—

"We will now examine the analogical phenomena in the case of light. Let fork No. 1, giving 256 vibrations a second stand for 508,730,000,000,000 vibrations a second, which will be the number of vibrations made by the ray D_1 of the spectrum, if we adopt 300,000 kilometres per second as the velocity of light. Then fork No. 3 will represent 504,750,000,000,000 vibrations per second, which latter give a wave-length ·0000048 millimetre longer than that of D_1, and belongs to a ray removed from D_1, towards the red end of the spectrum, by eight times the distance which separates D_1 from D_2. We saw that fork No. 3, giving 254 vibrations a second, had to move towards the ear with a velocity of 8·734 feet to give the note produced by 256 vibrations per second, emanating from a fixed fork; so, if a star, which only sends forth those rays which vibrate 504,750,000,000,000 times a second, should move towards the eye with a velocity of 2,442 kilometres, or 1,517 miles, its colour would change to that given when D_1 emanates from a stationary soda-flame."

ALFRED M. MAYER

* See NATURE, June 20.

Rev. John Ward on Atmospheric Germs

THIS worthy was Vicar of Stratford-upon-Avon, from 1662 till his death in 1681. He was a man of general knowledge, and was specially skilled in the diseases of women and children. It is not known that he obtained the archiepiscopal licence to practise physic, but he certainly practised the healing art, and he records his intention "to inquire whether a man may get of the archbishop a licence to practise *per totam Angliam!*" His diary, 1648-1679, is sensible and entertaining. It is chiefly known as containing a notice of Shakespeare, with the only extant account of the cause of his death, viz., "a feavour" caused by a carouse with Drayton and Ben Jonson. The Diary is in the Library of the Medical Society of London. It was edited by Dr. Charles Severn, and published by Colburn, in 1839.

The following extract is remarkable:—

"Venenum pestilens est congeries minimarum animalcularum per aerem volitantium, quæ corpora humana per respirationem aut poros subeuntes, eorum partes corrodunt et corrumpunt, ex iisque ad alia corpora volitantes, seu ad alia quocunque modo delatæ, et quasi contagio propagatæ, etram illa inficiunt, corrodunt, corrumpunt, sicut priora, e quibus evenerunt. . . . Supra fœnum cubare noxium multis fecit, non solum in peste, sed etiam in aliis morbis."

Of course *in peste* means "in the case of the plague."

C. M. INGLEBY

Coefficients of the Linear Expansion of Solids

AT the British Association which met last year in Edinburgh I suggested a *thermometer of translation* which should record the amount of the successive rises of temperature during the year. For this purpose a body possessing great expansibility with a fine needle point at its upper end, was proposed to be placed on a sloping frame, and made of a material possessing small expansibility, and protected from the changes of temperature, and having its upper surface finely serrated. When the body expanded, its upper end bearing the needle point would extend higher up on the frame, and when contraction commenced the projecting needle point would continue its hold of the teeth on the frame, preventing shortening at its upper end, so that the centre of gravity of the mass would be raised. In this way the successive increments of heat would be registered by successive *creeps* of the body upwards on the frame.

It has occurred to me that the same principle might be advantageously adopted for measuring the linear expansion of different solids.

In order to double the readings for expansion, clamping screws attached to upright rods fixed at the ends of the body would be better than the needle point for detention during contraction, which was proposed for the thermometer of translation. The bar to be experimented on would be placed on rollers in a vessel containing water or steam of different temperatures. The screws would be tightened at the lower end of the bar, and slackened at the upper before expansion, and tightened at the upper and slackened at the lower before contraction. After the contraction had fully taken place, the bar would be again heated and again cooled, and this process would be repeated until the total amount of translation became easily measurable. Although the amount of translation produced in any case by a single experiment might be scarcely appreciable, yet we can by cumulative repetition increase the amount of translation to any extent without increasing the errors of observation, for a single final reading is sufficient for the whole series of expansions however numerous