LETTERS TO THE EDITOR

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Prof. Tyndall on Germs

I AM very glad I wrote to you putting my questions to Prof. Tyndall. It has drawn from him a letter, full of all sorts of hints and prophecies and information and pleasant observations on details with which I had not thought of troubling him; and there is even a delicate bit of flattery for poor me, of whom the Professor knows nothing. It is really quite a gem of a letter, a beautiful example of that "tour piquant" referred to by M. Pasteur, which the Professor gives to everything he touches, and which we at home know how to value as well as any Frenchman. There is only one fault in it, and that is that the Professor, in the exuberance of his kindness, has unfortunately forgotten to answer my modest questions. But why does he liken himself to Horatius, and talk of enemies yet to be dealt with? Horatius did not sing his pæan before going into battle. And how can Prof. Tyndall have any enemies? I thought that scientific investigators were all brothers. I regarded Prof. Tyndall as a brother keeping a bright look out due north, and Dr. Bastian as a brother with his eye firmly set towards the south, while Dr. Sanderson seemed to me to be a remarkably silent brother gazing somewhere about sou'-sou'-west-and-by-south-a-quarter-south.

But to be serious. Briefly put, the situation seems to be this.

Prof. Tyndall has propounded a theory—no mere speculation raising a trifling controversy to be settled privately with Dr. Sanderson or Dr. anybody else, but a momentous theory on which, as he says, "the lives of men depend," and the truth of which it concerns all men to sift. It is not addressed to any scientific coterie, but widely published for the benefit of the outside world, the like of me among the rest.

The theory as propounded stands or falls with the assertion that when, with due precautions, an organic fluid is boiled for a few minutes in a flask, which is then hermetically sealed, it is impossible to obtain bacterial putrefaction.

What Prof. Tyndall declares to be impossible, that Dr. Sanderson declares that he has done.

If Dr. Sanderson is right there is an end of the theory, and the lives of men must rest on some other basis.

If Prof. Tyndall is right, Dr. Sanderson (not to put too fine a point upon it) has blundered in his very careful experiments.

Anxious to know where I should look for the truth, I ventured to ask Prof. Tyndall which alternative he adopted. Instead of helping me out of my difficulty he has responded with a flourish of rhetoric about not crossing swords with Dr. Sanderson. It is plainly from no want of courtesy that Prof. Tyndall has declined to satisfy my curiosity. He can't help being courteous; and to the class to which I belong—simple students who hang upon the lips of Professors for their scientific sustenance—he invariably overflows with courtesy.

I am sure he would have answered me if he could.

Even now I should be grateful (and so I believe would many more of us outsiders) if on second thoughts he should resolve to put his rhetorical sword into the unadorned scabbard of common sense, and kindly try to answer three plain questions:—

1. Does he accept Dr. Sanderson's experiments, and give up his theory?

2. Does he reject Dr. Sanderson's experiments as untrustworthy, and why?

3. Can he suggest any third view which will reconcile his theory with established facts?

Unless Prof. Tyndall feels constrained by his regard for human life to give me a reply, I will not press him to do so, if it would be in the least embarrassing. Only, if there is to be an answer, I hope this time it will be direct to the point. Perhaps, after all, it is not absolutely necessary. Silence is sometimes more eloquent than speech.

Feb. 19

INQUIRER

[This letter was unavoidably delayed last week.—ED.]

The Mechanical Action of Light

IN his recent lecture at the Royal Institution upon the Mechanical Action of Light, Mr. Crookes stated that his investi-

gations into this subject had enabled him to measure the repulsive force of light, and he calculated that the sun's light exercised a repulsive force upon the surface of the earth of 3,000 millions of tons, a force sufficient, he said, to drive the earth into space, were it not for the attraction of gravitation.

Let us look for a moment at this conclusion of Mr. Crookes. Granting that gravitation and a (hypothetical) tangential force cause the planets to revolve round the sun, the continuous action of a repulsive force emanating from the sun and impinging upon the surfaces of the planets, would cause them to spin round upon their axes just as a ball spins round when it is propelled along a resisting surface. This rotation would be in the same direction—right to left—as the revolution of the planetary bodies in their orbits.

But such an explanation of the rotation of the planets upon their axes will not, unfortunately, hold good, as upon this hypothesis their axes ought to be perpendicular to their orbits, whereas, with the exception of Jupiter, the equators of the planets are largely inclined to their orbits. If, then, the rotatory movement of the earth is not caused by the friction of a repulsive force emanating from the sun, it is clear that the effect of the 3,000 millions of tons which Mr. Crookes says continuously press against that half of the earth's surface which is exposed to the sun's rays, would be to retard not only the earth's diurnal rotation, but also its annual movement round the sun. Now there is no evidence whatever of retardation from any such cause, either as regards the earth or the planets with whose movements we are most familiar.

I do not in the least question that under certain circumstances light may repel solid, liquid, or gaseous bodies, and, indeed, if Mr. Crookes' general conclusions be confirmed, it may be found that the rapid extension of the tails of comets as they approach the sun may be due to the repulsive action of the sun's rays. As this force would be inversely as the square of the distance, the effect of the sun's light, acting in a straight line upon the highly attenuated matter of which a comet's tail is composed, would repel it with enormous velocity in a direction opposite to the sun as the comet approached its perihelion.

Manchester, February

GEORGE HICKS

Metachromism and Allied Changes

THE laws of metachromism, enunciated by Mr. W. Ackroyd (in his recent paper read before the Chemical Society, NATURE, vol. xiii. p. 298), have an apparent parallel in the order of colours shown by various series of combinations; there being but few exceptions to the following rule, in its application to binary compounds. *Increase of the electro-negative element produces a colour change towards the red end of the spectrum, and vice versa.* Thus the sub-oxides are generally blue, and the per-oxides yellow; the sub-sulphides white or yellow; and the per-sulphides red.

The examples which lead to this generalization are as follows:—

K₂O blue grey, K₂O white, K₂O₄ chrome yellow.
K₂S reddish yellow, K₂S₂ orange, K₂S₃ liver brown.
K₂Cl blue, KCl white, Na and Rb chlorides the same.
Na₂O blue, Na₂O₂ yellowish white, Na₂O₄ orange.
Cs₂O blue, Cs₂O white.
(H₄N)₂S₂ yellow, (H₄N)₂S₃ orange yellow.
CeO white, Ce₂O₃ fawn red.
U₃O₄ green, U₂O₃ brick red.
FeCl₂ white, FeCl₃ brown.
Cr₂O₃ green, CrO₃ yellow green, CrO₅ red.
MnO olive, Mn₂O₄ red brown, Mn₂O₃ brown black.
MnS dark green, MnS₂ brown red.
SnO olive brown, SnO₂ yellow.
SnS blue grey, SnS₂ yellow.
MoO purple brown, MoO₂ dark brown.
MoS₂ lead grey, MoS₃ dark brown.
MoCl₂ deep blue, MoCl₄ dark red.
W₂O₃ blue, WO₃ yellow.
Sb₂O₃ grey white, Sb₂O₅ pale yellow.
Sb₂S₃ blue black, Sb₂S₅ orange yellow.
Bi₂O₃ yellow, Bi₂O₅ brown.
Cu₂Cl₂ white, CuCl₂ liver colour.
PbO yellow, Pb₂O₄ red, PbO₂ brown.
PbS lead grey, PbS₂ red.
Ti₂O yellow, Ti₂O₃ brown.
HgI green, HgI₂ yellow or red.
Au₂O dark green, Au₂O₃ brown.
PtCl₂ olive, PtCl₄ orange.
OsCl₂ green, OsCl₄ red.

With fuller information on the rare and unstable compounds, than is to be found in ordinary text books (such as "Miller's Elements"), no doubt numerous other instances might be noticed; these, however, will suffice to exemplify the general law.

With regard to black compounds, I have not included them in the above list, as they only complicate it unnecessarily; the ordinary term *black* being used to describe a very minute amount of any possible colour. It is only when the predominating colour is observed (as blue-olive-red-black, &c.), that the description is of any value. "Brown" is likewise ill-defined in its spectral position, as various tints called brown generally include a small quantity of any colour except blue. Black and brown compounds are therefore inconclusive without a spectral examination of the colour in each case.

The real anomalies to the above law are the following compounds. CrCl_2 white, Cr_2Cl_6 violet; MnCl_2 pale pink, MnCl_3 green brown; As_2S_3 red, As_2S_5 yellow; HgCl yellow white, HgCl_2 white; AuI yellow, AuI_3 green. In these five pairs the law is apparently reversed, but they cannot be said to nullify an induction from thirty-five pairs, as enumerated above. A conformity of six cases in seven to a rule is sufficient to establish it as a law, from which the modifying causes have not yet been eliminated.

One or two other cases might at first seem to be also exceptions; but as they are really *salts*, in which the electro-positive of the base is the same element as that of the acid, they are not necessarily to be compared with binary compounds.

In the order of the colours, it will be observed that, in numerous cases, white occurs between blue and yellow compounds; and there is only one instance of a violet compound with more of the electro-negative element than its white connection. As white light comes (in the natural arrangement) between blue and yellow, this order is more in accordance with the spectrum than is the order of metachromism announced by Mr. Ackroyd, in which white occurs before violet.

It may be worth notice that the electro-negative elements (whose increase reddens the compounds) are, on the whole, rather more red and yellow than the electro-positive, many of which are bluish, and even dark blue, as Na.

This order of colours in successive compounds of a series (which might be called *taxichromism*) was observed by the writer some few years ago, in connection with the spectral order in metachromism; the latter, however, was only traced through a dozen or so, of the oxides, without pursuing the subject farther.

Bromley, Kent

W. M. FLINDERS PETRIE

Seasonal Order of Colour in Flowers

I THINK it may be useful to mention, in reference to several letters on this subject in NATURE, that light appears to have no direct influence on the tints of flowers. I quote the following from Sachs's Textbook, Engl. transl., p. 675:—

"As long as sufficient quantities of assimilated material have been previously accumulated, or are produced by green leaves exposed to the light, flowers are developed even in continuous deep darkness which are of normal size, form, and colour, with perfect pollen and fertile ovules, ripening their fruits and producing seeds capable of germination."

W. T. THISELTON DYER

Rainbow Projected on Blue Sky

AN instance of this phenomenon, which is referred to as rare in NATURE (vol. vii. p. 68), occurred to-day, Feb. 22, 4.30 P.M. The sky was almost quite clear and a light shower of rain falling, caused one to look upwards for the clouds whence it proceeded, but the air was uniformly clear near the zenith, though bordering the horizon all round there were some detached cumuli, and a few thin filmy modifications higher in N.E. There were no visible signs of the origin of the falling drops. On turning round to east, a solar bow was seen, for the most part on a background of azure. It was a *complete* bow and moderately intense. Near the vertex it rested on thin clouds, as did the extremities on the horizon, but they had no sensible effect on the phenomenon, for it was observed that as they receded from the upper portion, the bow remained intact and equally bright in all its parts. The arms spanned areas of blue sky. It was very transient, like the shower, and fading rapidly, was gone within 10 sec. after it was first observed. At 4.45 P.M., the left side of an incomplete bow was seen in N.E. on clear sky, except at the lower extremity:

no rain accompanied it. It was as evanescent as the first. The weather was very unsettled and showery with low barometer. Thermometer 50°, with brisk wind from W.S.W.— $3\frac{1}{2}$ inches of rain had fallen during the previous 9 days.

WILLIAM F. DENNING

Ashleydown, Bristol, Feb. 22

OUR ASTRONOMICAL COLUMN

OLBERS' COMET OF 1815.—The comet discovered by Olbers, at Bremen, on March 6, 1815, and with which his name has been usually associated, belongs to the group, the members of which revolve in periods a few years less than the period of the planet Uranus. The deviation from parabolic motion was remarked independently a few months after the discovery by Bessel, Gauss, Olbers, Nicolai, and Triesnecker. Bessel calculated elliptic elements so early as the middle of May, and was followed by Gauss in June, and the former subsequently investigated, as completely as was practicable at the time, the elements resulting from the whole course of observation and the effect of perturbation in the actual revolution.

When first detected, the comet is said to have been barely visible in a good achromatic, and according to the *Zeitschrift für Astronomie*, was not discernible without telescopic aid at any time; it is, however, upon record that in Russia it was seen with the naked eye. The last observation was made by Gauss at Göttingen, on August 25. At discovery on March 6, at 10 P.M., it was in R.A. $49^{\circ}3$ and N.P.D. $61^{\circ}3$, distant from the sun 1.47 and from the earth 1.30 , and at Gauss's final observation it was in R.A. $217^{\circ}1$, N.P.D. $84^{\circ}5$, distant from the sun 2.10 , and from the earth 2.36 . Representing the intensity of light at discovery (calculated according to the usual expression

$\frac{1}{r^2 \Delta^2}$) by unity, a maximum of 1.4 was attained on May 3, and it had diminished on August 25 to 0.16 , or one-sixth of that at discovery.

Bessel's determination of the elements of this comet and of the perturbations to the next return will be found in *Abhandl. der Berl. Acad. Mathem. Cl.* 1812-15. His definitive figures are as follow:—

Perihelion Passage, 1815, April 25.99867 M.T. at Paris.	
Longitude of the perihelion	$149^{\circ} 1' 56''$
" " ascending node	$83^{\circ} 28' 34''$
Inclination to ecliptic	$44^{\circ} 29' 55''$
Excentricity	0.9312197
Semi-axis major	17.63383

These elements apply to the date of perihelion passage; the longitudes from mean equinox of 1815.

To the above value of the semi-axis corresponds a period of revolution of 74.049 years. The distance in perihelion is 1.213 (the earth's mean distance from the sun being taken as unity), and the aphelion distance 34.055 ; the minor semi-axis is found to be 6.427 . The ascending node is situate near the orbit of the planet Mars, and if the actual form of orbit is due to planetary attraction, it is probably to be ascribed to a near approach of the comet to Mars at some distant period. At the other node the radius-vector is 3.81 in the region of the minor planets. The comet, it will be seen, recedes beyond the orbit of Neptune, but near the aphelion it has a depression of nearly 40° below the plane of the ecliptic.

According to Bessel, the mean motion at the instant of perihelion passage in 1815 corresponded to a period of revolution of 27046.9 days, and he found that this would be diminished 824.5 days by the united action of the planets Jupiter, Saturn, and Uranus, thus obtaining 26222.4 days for the actual revolution, and fixing the next arrival at perihelion to 1887, February 9.4. Beyond doubt, however, this date may now admit of a closer determination, and very probably we may soon hear of a further investigation being undertaken. The original observations made at the Observatory of Paris, and those at one or two other observatories in greater or less detail, are