

as to have any practical bearing on the behaviour of a secondary battery.

University College, Liverpool.

OLIVER J. LODGE

### On the Conservation of Solar Radiation

IT appears to me a difficulty arises with regard to Dr. Siemens' theory when we consider the original condition of the earth and of the other planets. What, in fact, has become of the great amount of energy which was present in the form of heat in those bodies?

Just as in the case of the sun, the rotation of the earth would produce a continuous cycle current, the decrease of rotatory energy being perhaps counterbalanced by shrinkage, the radiant heat would become transformed into the potential energy of dissociation, and this energy again would be given back to the earth in the form of heat in another part of the circuit where the elements recombine. Now it is quite impossible that the whole of the heat radiated should be used in this way, for after a lapse of years we should find a considerable diminution of potential, or (perhaps) rotatory energy, and we therefore should be forced to the conclusion that the earth became continually hotter. Hence some of the radiant heat escaped must have escaped into space, never to return.

Is it then a feasible solution that more heat is radiating from the sun than is necessary for the dissociation of the elements? If so, then at least we should have a satisfactory explanation of its slowly-diminishing activity.

G. B. S.

THE writer of this letter is right in concluding that in accordance with my hypothesis the earth also must throw out a stream of matter equatorially into space; and if your correspondent will refer to my article in the *Nineteenth Century* of April last, he will find that at p. 522 I speak of such a terrestrial outflow, with which I connect the phenomena of Aurora Borealis. If at any period of the world's history the rotatory velocity of the earth has been much greater than it is now, and its surface-temperature sufficiently high to cause ignition of combustible gases, it may be reasonably supposed that it had the power of recuperating its heat of radiation. The amount of heat so recuperated would, under all circumstances, be less than that received back by combustion, and the result of gradual diminution of temperature would be that on a certain day the temperature must have fallen below the point of ignition, from which day forward no further recuperation of heat could be expected. The process of cooling would then proceed at a very rapid ratio, until the surface-temperature had reached another point of comparative constancy, at which the radiation into space was balanced by the heat received by solar radiation, and which is our present condition.

C. W. SIEMENS

12, Queen Anne's Gate, S.W., October 16

### The Great Comet and Schmidt's Comet

THERE can be no doubt of the elongation of the nucleus of the Great Comet in the direction of the axis of the tail, in which direction it is three times as long as in a direction at right angles thereto.

The place of the comet this morning, at 6h. om. G.M.T., was

R.A. = 10h. 18m. 53 $\pm$  5 secs.

P.D. = 103° 31' 35"  $\pm$  10".

A neighbouring object was carefully observed, through haze, as a star of reference; its place was

R.A. = 10h. 18m. 53s.

P.D. = 102° 30' 0"

On consulting the Catalogue, it appears there is no star in this place. The object observed was probably Schmidt's Comet, discovered on the 8th of this month, but not since heard of here.

Unfortunately the above are absolute, not differential measures, but they have been corrected by measures of  $\lambda$  Draconis, also observed as a star of reference; its place is

R.A. = 10h. 4m. 46s.

P.D. = 101° 46' 27".

WENTWORTH ERCK

Sherrington House, Bray, October 16

[The nearest bright star to Mr. Erck's place is L. 20158, 6 $\cdot$ 7 mag. in Gould; R.A. for 1882, 10h. 17m. 32s., N.P.D. 102° 47'.  $\lambda$  Draconis is evidently a slip of the pen for  $\lambda$  Hydræ.—ED.]

### The B.A. Unit

I WISH to call the attention of readers of NATURE who are interested in the experiments which have recently been made for the determination of the B.A. unit of resistance, to a paper by F. Kohlrausch, read before the Academy of Sciences at Göttingen, September 6, 1882, "On the Measurement by Electrical means of the mean Area of the windings of a Coil." Prof. Kohlrausch has applied his method to redetermine the mean area of the coils of the earth inductor used by him in his experiments on the value of the B.A. unit in 1874. He finds the area of this coil to be 387,200 sq. cm.; the value used in 1874, calculated from the geometrical measurements of Weber in 1853, was 392,800 sq. cm. In consequence the value of the B.A. unit as determined from his experiments requires alteration, and, making the necessary corrections, Prof. Kohlrausch obtains

1 B.A. unit =  $\cdot 990 \times 10^9 \frac{\text{cm.}}{\text{sec.}}$ , agreeing much more nearly

with the values found by Rowland, Rayleigh and Schuster, and myself.

R. T. GLAZEBROOK

Trinity College, Cambridge, October 13

### The African Rivers and Meteorology

THINKING that the following extract from a letter written from the Niger Delta may be of interest to your readers, I beg leave to offer it for insertion.

"As yet there has been little water in the Niger, the rise up to the present (August 29) has not been over 3 feet in the lower river, and they say no rise has taken place in the upper river as yet. The upper river commences at Locayo, or where the Benue or Chadda joins the Niger, and continues thence on to Timbuctoo. So far as I can foresee, there will be a famine in the Niger Valley this year, as there has been a complete failure of the first crop from drought, and there has been no chance of putting in the second crop for the same reason."

The regimen of the waters of such great rivers as the Nile, the Niger, and the Congo, both as to quantity and periods of rise and fall, must be closely related to the meteorological conditions of the highlands of Africa, so little known to us, so extensive, and as yet so inaccessible for observation. May it not, therefore, be assumed that the comparative and continuous study and observation of those rivers as regards their volumes and periods of rise and fall, would be likely to furnish most valuable data for the prediction or forecast of weather in Europe. Thinking so, I have suggested to my correspondent the advisability of keeping a systematic record of the rise of the river Niger, and, if possible, of the temperature and other conditions of the water, with a view to their utilisation for meteorological purposes, and from this point of view I have thought that the above communication may present some interest.

J. P. O'REILLY

Royal College of Science for Ireland, Dublin, October 13

### A "Natural" Experiment in Complementary Colours

YESTERDAY evening I was reading Goethe's account of his visit to the falls of Schaffhausen ("Journey to Switzerland in 1797"). After mentioning that the morning was a misty one, and describing the general effect of the cataract, he adds: "Wenn die strömenden Stellen grün aussehen, so erscheint der nächste Gischt leine purpur gefärbt." I had certainly never heard of this phenomenon before, but it naturally occurred to me that it was probably an effect of complementary colours. Less than two hours afterwards I opened NATURE for the week, and found precisely the same phenomenon, with the same explanation as given by Mr. C. T. Whitmell. The point is interesting, as giving testimony to Goethe's close and accurate observation of colour phenomena; while the coincidence involved seems also to be worth recording.

WALTER R. BROWNE

October 13

### Ventilation of Small Houses

I HAVE been much interested in the reports of the Sanitary Institute. May I call attention to the fact that the majority of the smaller houses in our large towns have no means of ventilation except through the rooms. There are no ventilators or staircase windows, and the back house door opens into the kitchen. In a three-storied house the staircase is lit by the fanlight over the front door and a skylight in the roof, neither of

which opens; this arrangement gives little enough light and no air. Can it be healthy? Ought it to be? It is at least most disagreeable.

A. H.

October 15

### ON THE PROPOSED FORTH BRIDGE

AN interesting account of the plan of the railway bridge for crossing the Forth at Queensferry, as designed by our distinguished engineer, Mr. Fowler, with the association of Mr. Baker, was given by Mr. Baker to the British Association at their late meeting at Southampton. Supported as it was, to the advantage of those present, by the exhibition of the model of the proposed bridge, it must have given extensive information on the character of the structure. Yet it seems to me that, amidst many valuable particulars, on the strength of materials, their mode of application in this instance, and similar important subjects—it would hardly impress sufficiently, upon the minds of hearers or readers, the vastness of the scheme, the novelty of its arrangements, and the dangers (yet untried) to which, conjecturally, it may be subject. I have thought therefore that I might, without impropriety, offer to the editor of *NATURE* some remarks on points which after careful consideration have suggested themselves to me. For some particulars I am indebted to the courtesy of Mr. Fowler himself, and I greatly value this kindness.

It is known that at Queensferry the separation of the river-banks, or rather that of the piers next to the banks, at the elevation required for the railway, approaches to a mile. This space is divided by three piers (for which there are excellent foundations on rock and hard clay) into four parts, but only the two middle parts concern us now. They are exactly similar, and are treated in exactly the same way; and subsequent allusions, referring ostensibly to one, are to be considered as applicable to both. Each of the three piers is an iron frame, 350 feet high, the central pier 270 feet wide (in the direction of length of the bridge), and each of the others 150 feet. These lofty frames are braced, each upper angle on one side to lower angle on the other side, with no other diagonal bracing, but with a simple tie at mid-height. The lengths of the diagonal bracing are respectively about 430 and 360 feet. The water-spaces between two piers are each about 1700 feet; and the engineering question now is, how this space of 1700 feet (roughly one-third of a mile) is to be bridged for the passage of a railway.

The plan proposed is, to attach to each side of each frame (that is, to each side which will face a traveller entering upon the bridge) a framed cantilever or bracket about 675 feet long (that is, exceeding in length an English furlong by 15 feet), attached at top and bottom to the iron frame above mentioned, but having no other support in its entire length of 675 feet. To give the reader a practical idea of the length of this bracket, I remark that the length of St. Paul's Cathedral, outside to outside, is exactly 500 feet; and thus this bracket, which is to project over the water without any support whatever, is longer than the Cathedral by 175 feet. This in itself is enough to excite some fear, supposing the bracket to support merely its own weight. But further, the bracket bears also the very considerable weight of the roadway and rails. It is also heavily loaded on its point. The two opposing brackets from the two iron frames cover 1350 feet, but the whole space to be covered is 1700 feet, leaving 350 feet yet to be supplied for the support of the railway. To furnish this, a lattice-girder carrying a railway is provided, rather more than 350 feet long, whose extremities rest upon the tips of the two brackets.

This statement is enough, I think, to justify great alarm. No specimen, I believe, exists of any cantilever protruding to a length comparable, even in a low degree,

to the enormous brackets proposed here. The only structures of this class, in ordinary mechanics, known to me, are the swing-bridges for crossing dock-entrances, and the like, and these are absolutely petty in the present comparison.

I now advert to the weights of the principal portions of the bridge, and the strains which they will create. I understand that the weight of the two parallel braced sides of one bracket is about 3360 tons, to which is to be added the weight of roadway and rails for 675 feet, on which I have no information. I proceed to inquire what strains, in the nature of horizontal pull at the top of the pier and horizontal push at the bottom of the pier, will be caused by this weight. If the weight were evenly dispersed over the triangular bracket, its centre of gravity would be distant from the pier by one-third of the distance of the point from the pier. But as no vertical bar near the pier is included in the weights above, I must take a larger factor, say  $\frac{2}{3}$ . The vertical weight being 3360 tons, acting at a distance from the pier of  $\frac{2}{3} \times 675$  feet, and the separation of the points of connection with the pier being 350 feet, it is easily seen that the horizontal pull at the top and push at the bottom are each about 2600 tons. The inclined tension along the great upper bar of the cantilever and the inclined thrust along the great lower bar of the cantilever are therefore each about 2670 tons. The extremities of the great upper bar and the great lower bar being connected at the point of the bracket, and (for a moment) no other weight being supposed to act, there is no tension or thrust at that point, and therefore the tension and the thrust increase gradually, according to the attachment of their loads, from nothing at the point of the bracket to 2670 tons at connection with the pier.

But the point of the bracket is permanently loaded with half the weight of the intermediate 350-foot railway, or 363 tons, and occasionally loaded with the whole weight of a railway train, say for a passenger train 150 tons (a mineral train would be heavier). The vertical weight of 513 tons thus introduced would be met by a tension of 1004 tons through the whole length of the great upper bar, and a thrust of 1004 tons through the whole length of the great lower bar. Thus we have—

For the great upper bar, a tension increasing from 1004 tons near its point, to 3674 tons near the pier.

For the great lower bar, a thrust increasing from 1004 tons near its point, to 3674 tons near the pier.

The second of these statements particularly requires attention.

Mechanical students and professional engineers are accustomed to estimate by numerical measure the magnitude of a horizontal or nearly horizontal thrust, but persons in ordinary life scarcely attach a clear meaning to such a phrase. I am therefore compelled to make a somewhat violent explanatory supposition, with the hope that it may convey a practical impression as to the meaning of the statements just given.

The great lower bar is in fact a nearly flat frame, braced from side to side, about 120 feet wide at the bottom, and about 40 feet wide at the top, and 690 feet long. Suppose this structure to be planted vertically, say in St. Paul's Churchyard, without any bars, chains, or any thing else, below its vertex, to prevent motion edgewise, but with bracing (which, under ordinary circumstances, would suffice, but which will be the subject of further remark) to prevent its moving flatwise. Its top would be 310 feet higher than the top of the cross of St. Paul's Cathedral. Suppose a weight of 1000 tons to be placed on its very top, and additional weights (if necessary) to be placed at its sides, till the whole weight pressing the ground is 3600 tons. In this state its condition is exactly that of the great lower bar, as regards the crushing and distorting tendency of the weights (although the upper weight itself ought to be considered as partially protected from lateral movement by the great