way, but when the track is once blazed it is easy enough to follow and find the path.

As I do not wish to fill the pages of this journal with personal explanations, my contributions in NATURE to this subject must cease with this note. It is not my purpose at present to refute the imputations cast upon Agassiz by the editors of Forbes's Life and Letters; he can well afford to pass them over as he has done thus far, in silent contempt, the more so since, fortunately for Agassiz, the editors have given us from Forbes's own letters all that was necessary to show a course of duplicity, on Forbes's part, towards the man with whom "he served his apprenticeship in glacier observation," which is happily rare among scientific men. ALEXANDER AGASSIZ

Probosces capable of sucking the Nectar of *Anagræcum sesquipedale*

MR. W. A. FORBES, in the number for June 12 started the question, whether moths are known to inhabit Madagascar with probosces capable of such an expansion, as to obtain the last drops of the nectar secreted in the lower part of the whip-like nectaries of *Anagræcum sesquipedale*.

As long as a direct answer to this question has not been given, it may be of some interest to state in general the existence of moths provided with probosces sufficiently long for the honey-spurs in question.

Some days ago I received a letter from my brother, Fritz Müller (Itajahy, Prov. St. Catharina, Brazil), in which he says: "I recently caught a Sphinx (not determinable by Burmeister's 'Brazilian Sphingidae'), the proboscis of which has a length of about 0.25 metres—a length not approached by any honey-tube of this country known to me. I enclose the proboscis." Being unable to get the name of this species of Sphinx, I append the illustration of its proboscis, magnified in the proportion 7 : 1.

This proboscis, in its contorted condition forming a roll of 10-11 millimetres in diameter, and showing at least 20 elegant windings, in its expanded condition attains a length of between 10 and 11 inches, and would consequently be adapted to the nectaries of *Anagræcum sesquipedale*, which have been found by Darwin 11½ inches long, with only the lower inch and a half filled with nectar. Darwin indeed says, with regard to the fertilisation of *Anagræcum sesquipedale* (p. 198 of his work on Orchids): "there must be moths with probosces capable of extension to a length of between 10 and 11 inches."

Lippstadt, July 1

HERMANN MÜLLER

An Order of Merit

YOUR leading article in the last number of NATURE on the subject of a proposed "Order of Merit for Scientific Men," recalls the views (in exact correspondence with your own) entertained by my brother-in-law, the late J. Beete Jukes. These were expressed by him in no uncertain terms on the occasion of

publishing an address on the Geological Survey, delivered in Dublin in 1863.

I take the liberty of sending you a print for your perusal, and to refer to note B, at p. 21. I was glad to see the subject so well dealt with in your article.

ALF. H. BROWNE

5, West Hill, Highbury

"Men of science have of late years pandered too much to the utilitarian quackery of the age, and it is time that some one should stand up to protest against it. Government and the House of Commons should be told that Science must be supported and encouraged for her own purely abstract purposes, independently of all utilitarian applications. The necessary preliminary, indeed, to these utilitarian applications is the discovery and establishment of abstract scientific truth by men who look to that alone, and whose whole faculties and lives are devoted to it. The men who afterwards make the practical applications of it often attain, indeed, far wider reputations than the real men of science, and become to the popular gaze the representatives of Science itself. The higher class are rarely much known to the public during their lives, and are not usually men who would experience any satisfaction if they were nick-named Knights or labelled with C.B., or would feel inclined to accept any other crumbs that might fall from the table of the politically great and powerful. Nor would they commonly care much for pecuniary rewards, unless as a means to enable them to do their work without drudging for the support of themselves or their families. They are the men, however, who in the end rule the world, and doubtless they are often sustained in their labours by a consciousness of this fact.

"It would manifestly conduce to the public good and the national honour if such men, when they do arise amongst us, should be sought out, recognised as public benefactors, and allowed means to do that work which their faculties, and theirs only, enable them to perform." ("Her Majesty's Geological Survey of the United Kingdom," &c., by J. Beete Jukes, F.R.S. 1867.)

Geological Subsidence and Upheaval

SIR J. HIRSCHER thought that the earth's crust floats upon an ocean of molten matter, and that the washing of detritus from the land into the sea, by altering the relative weight of different portions of the shell, occasions a subsidence of the ocean's bed and an upheaval of the land, which may be either gradual and insensible, like the process of denudation, or spasmodic and by fits and starts producing earthquakes and sometimes volcanic eruptions.

This theory was at one time adopted, at least partially, by Sir C. Lyell, but is not mentioned in the latest edition of his "Principles," and is generally rejected by geologists as at variance with the opinion held by Sir W. Thomson and others in regard to the internal solidity of the earth. But this objection may be avoided by modifying Sir J. Herschel's theory. We may repudiate his hypothesis that a great fiery ocean exists below the outer crust. We may arrive at many of the important conclusions which he drew from this hypothesis, and which he described as all that a geologist could require, by admitting either that *solid rocks are plastic*, or that *some of the lower and warmer strata of the earth are more pliable than the upper*.

As to the plasticity of solid bodies, it may be sufficient to refer to the experiments of M. Tresca (Comp. Rend. de l'Acad., 1864-65, and Annales du Conservatoire, No. 21). Dr. Tyndall (Glaciers of the Alps, p. 9) suggests the possibility that the contortions of the strata in the valley of Lauterbrunnen may have been produced by pressure acting throughout long ages on the rocks in their present hard and solid condition.

Again, the lower strata of our globe may be rendered more pliable than the superincumbent rocks by the great internal heat, although it may be insufficient to fuse them or even to maintain them in a viscous condition. Many of the geological effects of a molten ocean may thus be produced.

The theory that volcanic eruptions are caused by water percolating through superficial cracks may, perhaps, give a clue to the reason why volcanoes often occur in a great circle round the globe and in diametrically antipodal positions. When other causes concur to modify the form of the earth, the tidal strain occasioned by the sun and moon may often be required to overcome the *vis inertiae*; this strain being greatest in the great circles of the globe perpendicular to the direction in which the sun and moon happen to be, cracks would probably occur most readily in these circles.

It seems at least a curious coincidence that some areas of recent

subsidence, e.g. coral reefs and islands, are parts of the earth's surface which have lately increased rapidly in weight; and it may be worthy of consideration whether coral and volcanic islands have contributed to deepen the bed of the ocean.

J. F. ANDERSON

Cauterets, Hautes Pyrenees, July 12

Curious Rainbow

AN unusual atmospheric effect was witnessed here to-day, which I had a good opportunity of observing. The sun was about 8° from the horizon, shining brightly upon a heavy shower which had a background of dark clouds. The result was, of course, a double rainbow of remarkable brilliancy. In addition, however, to the ordinary circular and concentric bows, there was a third of an elliptical form, the two ends of which respectively sprang from the two ends of the inner arc, while the elliptical curve cut the outer arc at each extremity of a chord, which was parallel to, and which intersected the normal radius at a point about two-thirds of its length above, the diameter that formed the common base. The top of the elliptical bow was thus the outermost of the three, but the space between its inner margin and the outer margin of the second bow, although quite distinct, was not large.

The appearance of the third bow was due to light reflected from the sea. The sun being low, the resulting line of reflection was long, and it was the linear character of the source of light which gave the elliptical form to the bow it occasioned.

Dunskaith, Ross-shire, July 10

GERGE J. ROMANES

CHLOROPHYLL COLOURING-MATTERS *

II.

I THINK there can be no doubt that the spectra of the various yellow substances given in Pl. II., Figs. 3, 4, and 6 of Dr. Kraus's work, are due to a variable mixture of xanthophyll, yellow xanthophyll, and lichnoxanthine. These can be separated, and do occur in different kinds of plants, either alone or mixed in such variable proportions that the spectra of the solutions show the absorption-bands, not only in variable positions, but also much less distinctly in some cases than in others. This difference is ascribed by the author, not to a variation in the relative proportion of two or more substances, each having definite and unvarying characters, but to the modification of one single substance, due to some unknown cause, assigning as a reason for this supposition that the chemical reactions are the same, and that the positions of the absorption-bands vary so gradually from one extreme to the other that no distinct demarcation can be detected. Now this is so very fundamental a question in such studies, and, according as it is decided, would modify the conclusions so much, that it is requisite to discuss it somewhat fully. No doubt the position of the absorption-bands seen in the spectra of solutions in different liquids does differ very considerably, but I feel persuaded that the spectrum of the same chemical compound, dissolved in the same liquid, is the same in all cases; and that, if there is any difference between the spectra of two similar solutions, it is due to a difference in the substances themselves. I would restrict the term *modification* to those changes sometimes produced by the action of weak alkalis or acids, or by deoxidizing reagents, which are only of a temporary nature, so that when the solution is restored to its original state, the spectrum is seen to be just as at first. We really do require such a term, and I have myself constantly used it in this sense. There is, however, no such relation between the different colouring-matters belonging to what I have called the xanthophyll group; and, though the presence or absence of oily substances may, and sometimes does, materially influence the position of the absorption-bands seen in the spectra of plants themselves, yet, when dissolved in a relatively large quantity of a solvent, this effect is altogether overcome. As I have shown in my late paper the position of the

* Continued from p. 204.

absorption-bands in the different members of the xanthophyll group is very different, and yet it would be easy so to mix them as to have a perfect series of connecting links, and in my opinion the variations from what appear to be independent compounds may be explained in an extremely simple and satisfactory manner, without supposing that the optical characters are subject to any such variations as are ascribed to them by the author. Whenever I have met with these variations I have looked upon them as presumptive evidence of there being a mixture, and have always been able to prove the truth of this principle by subsequent conclusive experiments. The following example will serve very well to explain my views. Many yellow flowers are coloured by a variable mixture of what I have called xanthophyll, yellow xanthophyll, and lichnoxanthine. The former occurs separately in the Alga, *Porphyra vulgaris*, the second in such pale yellow flowers as the yellow *Chrysanthemum*, and the last in the yellow fungus, *Clavaria fusiformis*. The absorption-bands of these two kinds of xanthophyll are in a very different position, and the lichnoxanthine gives no bands, only an uniform absorption, extending over about one half of the spectrum from the blue end. The chemical reactions are also equally distinct. On dissolving each in absolute alcohol, and adding a little hydrochloric acid, the first fades slowly, without being first changed into another yellow substance, and without turning blue or green; the second is first altered into another yellow substance, giving a spectrum with two absorption-bands in a different position, and then turns to a deep blue, whilst the last remains unchanged for a much longer time, and fades very slowly. Now, of course, if all these were mixed together in variable quantities, we should get results varying according to the relative amount of each. The absorption-bands due to the two kinds of xanthophyll would lie in an intermediate position, according to the relative amount of each constituent, and would be more or less indistinct, according as there was more or less of the lichnoxanthine; and on adding a little hydrochloric acid to the solution in alcohol the colour would turn to a more or less blue green, and subsequently fade to a pale or deeper yellow, according to the relative quantity of each constituent.

In order to make my meaning more clear, let us suppose that we were to take a mixture of equal quantities of xanthophyll and yellow xanthophyll. Using the notation I have so often explained in former papers, the centres of the absorption-bands of the spectra of a solution in bisulphide of carbon would then be—

Xanthophyll	6½	8
The above mixture	6½	8½
Yellow xanthophyll	7	8½

Now on exposing solutions of xanthophyll or yellow xanthophyll to the sun both fade, and if examined when very little colour was left undecomposed, the bands would be seen to be in the same position as at first, the solution being in fact just as if a large part of the colour had been removed, or as if it had been much diluted. In the case of the mixture this would not be the case. Xanthophyll is more rapidly decomposed than yellow xanthophyll, so that when very little colour was left the bands would be no longer in the original position, but in the same place as those of yellow xanthophyll, showing that a small quantity of this is left, when all the other has been destroyed. If some lichnoxanthine had been mixed with the solution, after longer exposure to the sun no absorption-bands would be seen, only the general absorption due to that substance. Moreover if we took equally deep coloured solutions in absolute alcohol of the same three different specimens, and added a little hydrochloric acid to each, the xanthophyll would fade till it was colourless, the yellow xanthophyll would turn to a fine blue,