obvious explanation of them, and has found, not only no sign of any deliberate mutilation, but overwhelming evidence of the reality of the ancient Egyptian's profound reverence for the bodies of his dead relations, and of his horror of mutilating them, even for the purpose of making the incisions necessary for embalming.

Since this correspondence began, a full discussion of all the data relating to the subject under consideration has been published in the report for 1907-8 on the Archaeological Survey of Nubia. There the facts will be found recorded for the use of anyone interested in the matter.

G. ELLIOT SMITH.

Effect of Heat on Soils.

MR. A. D. HALL in his opening address to the Agricultural Sub-section of the British Association at Sheffield mentioned "a process of heating the soil before sowing" and "a process of firing the soil preparatory to sowing the crop," both of which seem very similar to "burning bush" as practised in the West Indies. On reading Russel and Hutchinson's paper on "Partial Sterilisation of Soil" in *Journ. Agric. Sci.* for October, 1909, it struck me that their work afforded a probable explanation of "burning bush," and I now make the suggestion in the hopes of obtaining some evidence. The process seems to be similar to that known as *chena* in Ceylon, *ladang* in Malaya, and *jhuming* in India (see J. C. Willis, "Agriculture in the Tropics," pp. 1 and 2), and perhaps someone with experience of the East could throw further light on the question.

It is a common practice in Trinidad for a small cultivator to rent a piece of abandoned land, which is cleared by burning the "bush" in the dry season. In the wet season the clearing is generally planted with maize, which is usually followed by cassava, yams, taniais, &c. In a few years the cultivation is no longer remunerative, and the land, once abandoned, quickly goes back to "bush." One explanation is that the ashes of the burnt "bush" supply a certain amount of mineral plant food in a readily available form (the nitrogen in the plants burnt would, of course, be lost). This plant food would presumably produce an increased yield, but the effect would not last many years. Another—and I think a more probable—explanation is that the burning results in a partial sterilisation of the soil, with a greater production of ammonia. The fact that maize is the first crop grown after the "bush" has been burnt is significant, since it is a crop which is benefited by a nitrogenous manure.

C. HAROLD WRIGHT.

Government Laboratory, Trinidad, B.W.I.,
October 8.

The Colours and Spectrum of Water.

A PERUSAL of the articles which have appeared in your journal on this subject (*NATURE*, vol. lxxxiii., pp. 48, 68, 188, 487, and vol. lxxxiv., p. 87) leads me to ask if the spectrum of water has ever been thoroughly investigated. It is difficult to believe that this has not been done, and yet there is but little allusion to the important bearing the character of the spectrum must have upon the colour. I should be glad to learn what is the origin of a feature I have sometimes observed in the spectrum, as seen with a "miniature spectroscope" by Browning. This is a dark band or line at about wave-length 6000, which I noticed in June, 1887, in Sark, in clear water, both in the sea and in fresh water. The latter was bluish-green, and the sea was green. So far as this line would have any influence on the colour of the water, it would tend to make it blue; but the colour would be much more influenced by the very strong general absorption in the whole of the red and orange beyond the dark line. In the sea water this general absorption extended more feebly to the D line of the solar spectrum, and even to the dry-air band δ .

In the deep green water of Lago Maggiore the spectrum was almost identical with that of the sea at Sark; I made the wave-length of the dark line about 6050.

The Lake of Geneva is noted for the deep blue of its

clear water. In it I failed to see the dark line, but the general absorption of the red end of the spectrum was very striking.

The bluest water I ever saw—bluer than the Mediterranean, Red Sea, or Indian Ocean (I have not been to Capri)—was, however, the Blaue See, near Kandersteg, and the Lago di Garda. In the former, pieces of pot at the bottom appeared pure Prussian-blue. During a stay of a few days by the Lago di Garda I could not make out the law of its changes in apparent colour, but at times it was far bluer than the bluest sky I ever saw. It usually had a slightly greenish tinge. Both these lakes are extremely clear. I regret not having observed their spectrum.

T. W. BACKHOUSE.

West Hendon House, Sunderland, October 19.

Luminous Paint.

MR. C. A. EMERY, of Marlborough College, has directed my attention to a passage in Livy (xxxix., 13) where the following occurs:—

"Matronas Baccharum cum ardentibus facibus decurrere ad Tiberim, demissasque in aquam faces, quia vivum sulphur cum calce insit, integra flamma efferre."

This he renders:—

"The Bacchantes would run down to the Tiber with burning torches, and plunging them into the water would take them out with the flame unextinguished, because they were covered with a mixture of living sulphur and lime."

The expression "vivum sulphur" is thus explained by Pliny:—

"In Italia quoque invenitur sulphur. Genera quatuor: vivum, quod Graeci apyron vocant, nascitur solidum, hoc est, gleba: quo solum ex omnibus generibus medicum utuntur. Solum (cetera enim liquore constant, et conficiuntur oleo incocata) vivum effoditur, translucetque, et viret."—Plinii Hist. Nat. Lib., xxxv., 50.

It would seem, therefore, that calcium sulphide was discovered, and its luminosity observed, about nineteen hundred years before Marggraf, who in 1750 A.D. prepared the substance by "calcining gypsum with combustible matter." Eighteen years later Canton obtained the same effect by "igniting calcined oyster-shells with sulphur." (Inverted commas from Roscoe and Schorlemmer's "Treatise on Chemistry," under "Calcium and Sulphur.")

As neither of these manufacturing methods is difficult, it seems reasonable to suppose that one or other might have been known in Livy's day, and from the Latin text a modification of Canton's method seems probable.

Except for the above, I can find no direct reference to the discovery of calcium sulphide.

I noticed when reading Charles Reade's novel "The Cloister and the Hearth" that one of the more striking incidents depended on the use of a phosphorescent paint. As this powerful story is of the fifteenth century, I considered the phosphorescent paint an anachronism, for Brandt is said to have discovered phosphorus in 1669, and the "Bologna stone" (barium sulphide) was discovered only a few years earlier. But Reade may have been aware of the passage in Livy or may have known about Pliny's "Pholas"—the rock-boring mollusc (Piddock)—the phosphorescence of which is said to remain long after death.

Possibly some readers of *NATURE* may be able to throw more light on the origin of "luminous paint."

R. G. DURRANT.

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Velocity of Negative Ions in Hydrogen at Atmospheric Pressure.

SOME time ago Prof. Chattock and I attempted to measure the velocity of the ions from a point discharging in pure and impure hydrogen at atmospheric pressure by the wind-pressure method, in the hope of finding that the negative ion was influenced by the presence of small traces of oxygen. The results were given in *Phil. Mag.*, April, in which it was shown that the change which the com-

plete elimination of oxygen caused was surprisingly great. Thus the apparent specific velocity of the negative ion in pure hydrogen was >230 cm. sec.⁻¹, volt cm.⁻¹, but it rapidly dropped to 7.6 on the addition of oxygen up to 1 per cent. The velocity of the positive ion was unaffected by traces of oxygen impurity, and was 5.8 throughout.

The change in the case of negative discharge might have been caused in two ways:—(a) by a great diminution with purity in the size of the negative ion in hydrogen; (b) by back discharge from the plate. If the latter were true the wind-pressure method breaks down, and the above values of velocity are not real. As it was certain that some back discharge was present, it seemed reasonable at the time to attribute the whole effect to this cause, but some recent work of Franck shows that it was probably not correct to do so.

Franck has shown (*Verh. d. D. Phys. Ges.*, xii., 291 and 613, 1910) that in gases such as argon and nitrogen the specific velocities of the negative ions obtained by α rays rapidly increased as the last traces of oxygen were removed. Thus in pure argon and nitrogen he obtained values as high as 206.4 and 144.6 respectively. As in the above, however, the positive ions were unaffected.

In the light of these results it is probable that the negative ions in point discharge in hydrogen, like those in other oxygen-free gases, are either corpuscles or are very small, although in our work the unknown amount of back discharge present prevented the determination of their true specific velocity.

These results throw considerable light on various phenomena occurring in point discharge, and I hope to publish later a more complete discussion.

A. M. TYNDALL.

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An Irish Pteridosperm.

READERS of NATURE familiar with the many valuable additions to knowledge made by British palaeobotanists within the last twenty years will be interested to know that in the course of rearrangement of the fossil plants in my charge in the botanical division of the National Museum in Dublin I have found, while comparing the specimens of *Sphenopteris* in this collection with those in the collection of the Geological Survey of Ireland (of which my colleague, Prof. Grenville Cole, is director), that in the latter collection there is a specimen of *Sphenopteris Hoeninghausi* from the Coal-measures of Glengoose, co. Tipperary, which shows all the characteristic features of *Lyginodendron Oldhamium* (including its spines, sclerotic network, venation, and conchoidal pinnule segments). Moreover, the specimen shows, in direct continuity with the vegetative part, the *Calymmatotheca Stangeri* condition regarded first by Scott, and now by many others, as probably the seed-producing part of *Lyginodendron*, from which the *Lagenostoma Lomaxi* seed has already, as Oliver and Scott have satisfactorily shown, in all probability fallen out. The specimen in question, if my interpretation is right, proves the correctness of the conjecture that *C. Stangeri* is part of the true pteridosperm *Lyginodendron*. I hope to publish shortly an illustrated account of the find.

T. JOHNSON.

Royal College of Science, Dublin, October 24.

Fermat's Theorem.

THE following proof of this theorem may be of some interest. Take the scale of notation the radix of which is x , and write down all the numbers of p digits, any or all of which may be zero. The number of these numbers is x^p . From one number we can, in general, derive $p-1$ others by cyclical permutation, the exceptions being those numbers that are periodic with a period that is a submultiple of p . Suppose p to be a prime, so that its only submultiple is unity. Then all the numbers except the x numbers that have their digits the same can be arranged in sets of p each (which are easily seen to be mutually exclusive). Hence the number of these numbers, which is $x^p - x$, is divisible by p , and if x is prime to p we see immediately that $x^{p-1} - 1$ is divisible by p , which is

Fermat's theorem. It is clear that this proof depends on permutations and combinations, not really on scales of notation, which, indeed, we have only used because of the clearness that they lend to its statement.

H. C. POCKLINGTON.

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The Uganda-Congo Boundary.

WITH reference to the note on this subject in NATURE of September 1, has not the writer fallen into a slight error in quoting (p. 268) the definition of limits in the "Berlin Act" as applicable to the Congo State? Reference to the text of the Act will show that the passage quoted relates, not to the State, but to the Free Trade area in the Congo basin and neighbouring territories constituted at that time, with limits by no means coincident with those of the State. In fact, the "Berlin Act" had nothing to do with the State (as such), which was constituted, not by the conference, but by agreements with individual Powers negotiated about the same time.

The frontier originally claimed by the State in this region, and definitely accepted by several of the Powers, was the thirtieth meridian, for however unsatisfactory this might be, there could hardly, in 1885, have been a question of the adoption of the water-parting, which would at that date have involved far more uncertainty than the meridian. It was even doubtful to which of the two basins Lake Edward belonged. The mistake in 1894 seems to have consisted, not in the gratuitous introduction of the thirtieth meridian, but in its partial retention (viz. in the Ruwenzori district), to the detriment of Uganda, while replaced farther north by the water-parting, greatly to the advantage of the Congo State.

EDWARD HEAWOOD.

1 Savile Row, September 16.

IT is quite true that the definition quoted in the note was that of the Free Trade area, but as the recognition of the Congo State, on our part, contained no definition of frontiers, we were entitled in 1894 to maintain that, in default of any specific deviation agreed upon mutually, the two frontier lines were identical. So far as I am aware, up to the date of the treaty of 1894 we had not admitted, nor, indeed, seriously considered, any claim on behalf of the Congo State to territories outside the Congo basin.

The history of the whole series of transactions is somewhat complicated and difficult, but it seems that, whatever were the intentions of the signatories to the Berlin Act and of the framers of the original agreements with the Congo State, any distinction between the State and the Free Trade area disappeared at an early period of their history, and had ceased to exist by 1894.

THE WRITER OF THE ARTICLE.

An Agaric with Sterile Gills.

THE occurrence of agarics with sterile gills is well known in certain species; it has been noticed most in those with purple spores. A few days ago I met with two specimens of *Panaeolus campanulatus* in Sutton Park, near Birmingham, in which the gills were of a pinkish-grey colour, somewhat closely resembling the tint of the dry pileus. There was a total absence of the usual variegated, grey and black, appearance. The pileus was large and well developed, measuring $1\frac{1}{2}$ inches high and broad, stipe quite 4 inches long, and presenting all the characters of that of *P. campanulatus*. On examining sections of the gills it was seen that numerous basidia were present, projecting beyond the paraphyses; very many of them had the four sterigmata of full size, but not one over the whole of the gills of both specimens had produced a spore, nor were any produced afterwards, so long as the fungi were preserved. Unfortunately, they had been gathered before I saw them, so that it was impossible to ascertain if there was any visible cause for the sterility.

W. B. GROVE.

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