to be in millimetres, for the longer (which I will call A), 228·4; for the shorter (which I will call G), 227·3; the length of 9 English inches in millimetres being 228·6. The "handle" penetrates into the sheath about 17·7 mm. These handles are tapered to the end penetrating the sheath. The two "sheaths" are not alike in interior form: the sheath A is hollowed out conically to a depth of 46·5 mm., the remainder of its length having a hole drilled through it of about 2·6 mm. diameter; the sheath G is 70 mm. long, is hollowed out to a depth of 50·5 mm., and has no hole through the bottom or end part.

I was led to conjecture that these tubes or "sheaths" might be musical pitch-pipes; and on blowing across their orifices, the shorter produced the sound of high G, the longer or perforated one that of high A. By stopping with the finger the hole which passes through the bottom of the "sheath" A, the sound of high G sharp was produced. These notes I tested with a pitch-pipe.

This led me to further conjecture that they should present an interior diameter in accordance with the condition laid down therefor by Lyng-Lun; that is, such as to give an interior circumference equal to 9 grains of *Sorghum rubrum* laid lengthways. Having carefully calibrated the interior diameters, I obtained a series of values, giving for the interior circumference of the G sheath or tube a mean value of 28·32 mm., and for that of the A tube 28·44. Through the kindness of Messrs. J. Carter and Sons, of High Holborn, London, I obtained a sample of *Sorghum rubrum*, and operating on this, as also on a sample obtained in Dublin, I got for the length of nine millet seeds placed end to end the following values in millimetres: 26, 26·50, 27·10, 27·38, and 28. I took as mean the value 27 mm. (the exact mean being 26·995 mm.), so that the difference from that of the interior circumferences found is only 1·44 mm. in the mean. I may add that from a series of ten measurements kindly made micrometrically by Dr. McNab, it appears that the lengths of the grains measured by him vary, and would give for the lengths of nine placed end to end, the limits 28·804 mm. and 24·689 mm.

I consider therefore that, so far, the interior circumferences determined point to the "sheaths" being pitch-pipes having the standard interior dimension laid down by Lyng-Lun.

I thought it worth while furthermore to verify the cubical contents of the tubes in millet seeds. The pipe G gave a contents of 421 red millet seeds, and the pipe A of 375; or a mean capacity in seeds of 398. The white seeds gave me for G 400.

What, however, is interesting, is that the end of the "handle" which penetrates the G sheath is hollowed out cylindrically, and this space holds 39 red seeds, and would seem to represent a standard of one-tenth volume. The corresponding end of the A sheath gives for two measurements 37 seeds, or as near as possible one-tenth the capacity of that sheath measured in seed contained.

With the measures of lengths of millet seed determined, we may attempt to fix the probable or approximate length of the fundamental pitch-pipe; that is, the length of 81 millet seeds placed end to end. Taking 27 mm. as the approximate length of nine grains, this length, or rather depth, would be $9 \times 27 = 243$ mm. Now there is found for the combined lengths of the "sheaths" and "handles" when placed end to end, the values:

$$\begin{array}{rcl} \text{mm.} & \text{mm.} & \text{mm.} \\ \text{For the G tube, } 227\cdot3 + 17\cdot65 & = & 244\cdot95, \\ \text{,, A tube, } 228\cdot4 + 17\cdot7 & = & 246\cdot10; \end{array}$$

both differing little from the approximate value found above.

Taking, on the other hand, the mean interior circumferences of the two tubes as probably representing the lengths of nine millet seeds, we have from the measurement

$$\begin{array}{rcl} \text{mm.} & \text{mm.} & \\ \text{Of the G pipe, } 28\cdot32 \times 9 & = & 254\cdot88, \\ \text{,, A-pipe, } 28\cdot44 \times 9 & = & 255\cdot96. \end{array}$$

It is worthy of remark that by multiplying the mean, end or bottom, diameters of the two sheaths, $\frac{12\cdot96 + 12\cdot47}{2} = 12\cdot715$,

by 20, the product comes out 254·3 mm.

Such coincidences can hardly be fortuitous, and to some extent justify the presumption that the two jade instruments which I originally took to be "brushes," are either original

standard measures of very great antiquity, or copies more or less exact therefrom. A further determination of the lengths of the Chinese red millet seed is evidently desirable as a matter of metrical and historical research.

J. P. O'REILLY

The Cambridge Cholera Fungus

IN reply to Dr. Klein's letter, I wish to state that although the specimens figured by Prof. Roy in the Royal Society's Proceedings appear to be branched, the one shown to me did not.

Dr. Klein is of course perfectly right as to his statements concerning branching Bacteria; and his remarks, if he rigorously distinguishes between real and false branching, are true also of all the Schizomycetes. At the same time, the existence of such a form as *Cladothrix dichotoma* is not without interest, more especially since Cienkowski has described for it an involution form.

WALTER GARDINER

Clare College, Cambridge, January 31

As bearing on the subject of the "Cambridge cholera fungus," it may interest some readers to learn that methylene-blue has long been known as a good stain for fungi. My friend Mr. T. Hick, Botanical Lecturer at Owens College, showed me, some years ago, beautiful preparations of moulds stained with this substance, and I have frequently used it for the same purpose, as also chinoline-blue, known as "blue No. 13" of the aniline dyes. It is impossible to keep an aqueous solution of the latter for any length of time free from fungoid growth, the hyphae of which, at a certain stage of development, exactly resemble the forms described by Messrs. Roy, Brown, and Sherrington, and my specimens, when grown on a slice of potato, developed into *Aspergillus glaucus*. Remembering the very varied appearances assumed by the barren hyphae of fungi, depending on nature of substratum, relative amount of moisture, &c., I believe that morphological agreement of vegetative parts by no means proves specific identity, even when both can be examined in a fresh state—a great advantage, as the chemical and physical properties of the hyphae can be compared; but an expression of opinion as to relationship based on the comparison of barren hyphae with drawings is simply valueless, and only proves a very slender acquaintance with the characters of admitted specific value in the determination of fungi. Members of the Chytridiaceae are common only in books; during years of practical mycological work I have only once met with a species belonging to this group, and this one I could not succeed in staining with either methylene- or chinoline-blue, but Bismarck-brown gave good results. I was inclined to attach a certain amount of value to this selective power exercised by fungi in connection with dyes, until I discovered that the hyphae producing the zygospores of *Syzygites megalocarpa* could not be stained with blue, but readily with methyl-green, while the hyphae of the conidial stage (grown by us from the zygospore) readily absorbed methylene-blue, but had no affinity for green. Interstitial swellings and knob-like outgrowths are not uncommon on mycelium belonging to widely separated groups, especially when the spores are caused to germinate under abnormal conditions, as described in the *Journal of Botany* for October 1882. The protoplasm frequently becomes concentrated in these portions, which are then cut off from the thread by a septum, and serve as centres for a fresh growth, when placed under favourable conditions. The absence, presence, or relative number of septa vary much in the same plant at various ages and under different conditions of growth.

Kew

G. MASSEE

Earthquakes

IN NATURE of October 14 (p. 570) you published a letter from Prof. O'Reilly regarding the great earthquake of Carolina, and drawing attention to the tendency of earthquake-lines to assume the direction of great circles. So far his observations were identical with a theory I had myself elaborated, and which I embodied in a paper written at the beginning of the year 1884, now in the hands of the Committee of the Geological Society of London, but never presented to the Society. So long ago as

that period I had drawn attention to what I pointed out as the two principal earthquake great circles—one, the Japan and Rocky Mountain system, with one of its poles in 170° W. long., 25° S. lat.; the other, the Himalayic, with its north pole approximately in 45° N. lat., 160° W. long. The former has been frequently described, and Scrope ("Volcanoes," p. 303) suggested a theory to explain its occurrence. The latter is little less remarkable, and is at the moment even more interesting, as, with the exception of the Carolina earthquake, all the great earthquakes and volcanic eruptions of the last five years may be referred to it. I may instance the cases of Krakatã, Kashmir, the Caucasus, Spain, Cotopaxi, New Zealand, and the recent Mediterranean disturbance, all of which occurred within a few degrees of the line or actually on it. Now it is remarkable that this line is marked through a considerable portion of its course by the presence of disturbed Miocene rocks, so much so that I have felt justified in calling it the Miocene line.

The paper referred to contained a theory too long to be worked out in the compass of a letter, but founded on the changes in form which must occur when a plastic body falls by the action of gravity towards a primary. A little consideration will show that, as the action of gravity is inversely proportionate to the square of the distance, the forward portion of such a body will be continually pulled away from the posterior, and an original sphere will in its descent become deformed into a prolate spheroid. Now the result is, I believe, calculable for a body like the earth, even under the present conditions of its annual approach to the sun, in other words its *fall* from aphelion to perihelion. During periods of extreme eccentricity of the orbit the fall and consequent deformation were much greater. The main factors in the calculation are of course: (1) the distance from the primary of the commencement of the fall; (2) the diameter of the falling body; (3) the distance fallen; and (4) the comparative masses of the primary and the attracted body. Beyond this, consideration has to be given to what we may term the specific resistance to deformation of the particular body. The latter, indeed, seems to be the principal factor in determining the amplitude and periodicity of earthquakes.

It is difficult for the geologist at this remote spot in the Far East to keep in touch with the daily progress of geology at home, but there is one probably counterbalancing advantage—in the enlarged view he has to take of the mid-Tertiary epoch as a factor in geologic change.

THOMAS W. KINGSMILL

Shanghai, November 30, 1886

THE CALENDAR AND GENERAL DIRECTORY OF THE SCIENCE AND ART DEPARTMENT

THERE is a general impression on the Continent, and even in England, that English teachers of science carry on their work with little direct relation to one another. Twenty-five years ago this impression was not incorrect, but any one who will take the trouble to read the "Calendar and General Directory of the Science and Art Department for the year 1887," lately published, will see that it is no longer true, and that very important steps have been taken towards the establishment of an organised and efficient system of scientific instruction. At South Kensington we have now a School of Science, which maintains the most intimate connection with a vast number of science schools and classes in all parts of the United Kingdom. Here we have at least the germs of a proper system, and it depends upon the country itself whether we are to remain content with what has been achieved, or are to continue the work we have begun until it can be pronounced completely adequate to the needs of modern times.

The movement which has led to these results may be said to have begun in 1853, when the Department of Practical Art was expanded into the Department of Science and Art. The immediate object of this change was to secure that the advancement of practical science should be directly encouraged, and it was decided that the end could be most surely attained by "the creation in the metropolis of a school of the highest class, capable of affording the best instruction and the most perfect train-

ing," and by help rendered to local institutions for scientific education. For some time it seemed not improbable that the scheme would be, at least in part, a failure. No general system of making grants applicable to the whole country was devised until 1859. Experimental schools were established by special Minutes, the arrangement usually being that the teachers were to receive payments from the Department in the nature of certificate allowances, and that their incomes, from fees, subscriptions, and other sources, were to be guaranteed by the Department for a certain number of years at amounts varying in different places. In this way science schools were opened at Aberdeen, Birmingham, Bristol, Barking, Leeds, Newcastle-on-Tyne, Poplar, Stoke-on-Trent, St. Thomas's Charterhouse, Truro, Wigan, and Wandsworth. It was found, however, that there were but few places where a man could earn his living by teaching science alone; and in 1859 the only science classes in operation under the Department, irrespective of the Navigation Schools, were those at Aberdeen, Birmingham, Bristol, and Wigan, the number of persons in attendance being 395. Then a new plan was tried. In 1859, when the late Lord Salisbury was Lord President, the first General Science Minute was passed, enabling any place to establish science classes, and to obtain State aid according to certain fixed rules. The effect of this measure surpassed the hopes of those by whom it had been suggested. A number of new schools and classes were rapidly formed, so that, in May 1861, at the first general and simultaneous examination of classes, there were 38 classes with 1330 pupils, not including some 800 pupils in classes not under certificated teachers. Since that time there has been constant progress, as the following table will show:—

1862 ...	70 schools with	2,543 pupils in	140 classes
1872 ...	948	36,783	2803
1882 ...	1403	68,581	4881
1885 ...	1542	78,810	5649

In some schools there are classes both for science and for art, and in such cases it is interesting to note the relative proportion of the number of pupils in the two departments. At the School of Science and Art in Reading, for instance, there are 200 art students, and only 90 students of science. At the Central Board School, Rochdale, on the other hand, while there are only 50 students of art, there are 190 science students. In some instances the numbers are evenly, or almost evenly, balanced. At a school in Deptford there are in each department 160 students, and at another, in Bristol, art has 360 students, science 350.

The number of teachers varies, of course, very considerably. The following tables, compiled from the details presented in the "Calendar and General Directory," show the number of schools which have each three or more teachers:—

I.

Schools in which both Science and Art Classes are held

Schools	110	83	40	32	24	15	14	9	4	1	2	3	2	1
Teachers	3	4	5	6	7	8	9	10	11	12	13	14	15	18

II.

Schools in which Science Classes only are held

Schools	105	36	24	11	1	2	4	2	1	1	1
Teachers	3	4	5	6	7	8	9	10	11	13	14

The progress of the central institution—the "school of the highest class, capable of affording the best instruction"—has been in its own way not less remarkable. The removal of some of the courses of the School of Mines to South Kensington greatly increased the