which is, neglecting the constant coefficient of amplitude, geometrically represented by another harmonic curve of identical form, but shifted on so that it begins at a point *a*, or a quarter of the length of the curve O O from the origin. In this second curve the heights of the ordinates represent the varying velocities of the diaphragm, the velocity being nothing at *a* when the displacement at A is a maximum, and being at a maximum at *b* when the diaphragm in flying back passes through its point of rest or has no displacement. Now of these two curves the former corresponds in phase to the movement of the diaphragm of the transmitting telephone, while the second curve corresponds to the variations of velocity, and therefore of the current transmitted, and consequently also corresponds to the motions of the diaphragm of the receiving telephone. Hence it is easy to understand that there exists a difference of phase of one-quarter of an undulation between the movements of the diaphragms of the transmitting and receiving telephones, which will be either a retardation or an apparent acceleration of phase according to the sense in which the transmitted currents traverse the coil of the receiving telephone. These considerations apply only to the telephone of Bell or its modification by Gower, in which the vibrations of the transmitting diaphragm generate the current. They do not apply to the transmitters of Edison and Hughes, which merely regulate the current. In these instruments the strength of the current is proportional to the *displacement*, not to the *velocity;* hence there is no retardation of phase.

The memoir of Helmholtz, which, by introducing certain considerations respecting the mutual inductive actions exercised upon one another by the individual turns of wire in the coil of the telephone, arrived at a somewhat different conclusion, and was principally devoted to the question of the timbre of the transmitted sounds. The previous researches in physiological acoustics of this distinguished physicist had shown that differences of phase affecting individual tones of a compound "clang" do not produce any effect which the ear can detect. This important law the present writer has, however, shown elsewhere to be true only when one ear receives the sound, and to hold no longer in the case of binaural hearing. The equations of Helmholtz indicated the unexpected result that the difference of phase between the vibrations of transmitter and receiver was a quantity so small that practically it might be altogether disregarded, and he arrived at the conclusion that all sounds were transmitted by the telephone with an equal proportionate degree of intensity independent of their pitch, and therefore with unaltered timbre. Here again, however, the writer of this article has shown that the relation between the thickness and diameter of the vibrating diaphragm affects the distribution of the magnetism induced in it by the magnet, as to whether it is lamellar or radial in character, and that this distribution has influence on the timbre of the sound emitted by the receiving telephone, the notes of higher pitch being better given by the disk in whose magnetisation the lamellar distribution preponderates, while the lower ones are better given with a preponderating radial magnetisation. The whole question of timbre of the emitted sounds requires further careful study.

The experiments which M. König has executed entirely confirm the *à priori* reasoning of du Bois-Reymond as to the existence of a difference of phase. Instead of using two vibrating diaphragms, Dr. König takes two tuning-forks accurately tuned to unison, each of them being placed in front of the magnet of a telephone whose disk has been removed, and which are united in the usual manner by wires. The first of the forks being set into vibration with a violin-bow, the second immediately begins to vibrate. The phase of each of the forks is next observed. This has been done in several ways: firstly, by direct comparison of each fork in turn with the vibration-microscope; secondly, by applying the well-known optical method of Lissajous, compounding together the two vibrations rectangularly by throwing a ray of light on to small mirrors attached to the two forks, and reflected from one to the other and then on to a screen. The figure thus produced exhibited unmistakably a difference of phase of an exact quarter of an undulation. A further experiment on compound tones was made with the same general arrangements; two forks, differing by three octaves, being made to take up, one as transmitter the other as receiver, sounds whose higher vibrations were eight times as rapid as the fundamental tone. Here again the difference of phase experimentally found for the higher tone was one quarter of a vibration.

Incidentally two very important facts have been observed by Dr. König. In experimenting he found that a tuning-fork, vibrating in front of the magnet of a telephone *whose circuit is closed*, comes to rest in a much shorter time than the same fork vibrating freely away from the telephone; also that this weakening of the sound is greater in proportion as the distance of the fork from the pole of the magnet is smaller, and also is greater for small amplitudes of vibration than for large ones. These results are not without interest in their bearing upon Mr. Edison's recent attempt to construct a dynamo-electric machine, in which the moving parts should be attached to a large vibrating tuning-fork instead of to a rotating axis. Doubtless the inventor's idea was to get rid of the friction accompanying rotation; for, as the vibrations of the tuning-fork are very nearly simple harmonic motions, and as the simple harmonic motion is the only type which can be propagated without loss by friction through a body, the motions of whose parts are coincident in phase, it might be anticipated that there would be less waste of energy in a "harmonic" engine than in a rotatory one. The important fact however remained behind that by far the greatest part of the work of driving a dynamo-electric machine was not spent in overcoming friction, but in doing the work of moving closed conductors across a magnetic field, a work which, to produce an equal amount of current, requires equal power, whether the motion be one of rotation or of "harmonic" vibration. Many years ago Foucault demonstrated the reality of this resistance to motion by spinning his gyroscope between the poles of an electromagnet; and with a Gramme machine, and also with a Holtz machine, the increased effort necessary to sustain rotation when work is being done is a familiar fact. Dr. König has now demonstrated the existence of a similar phenomenon in the case of the vibrations of the tuning-fork, which comes much sooner to rest when it is doing electrical work than when it is doing no work.

SILVANUS P. THOMPSON

ON THE EOCENE FLORA OF BOURNEMOUTH

On several previous occasions these columns have called attention to the eocene plant remains obtained at Bournemouth. The Palæontographical Society has undertaken their publication, but as this must be spread over many years, it may not be undesirable to note from time to time the principal additions to the flora as they come to light.

The specimens which I have collected this year may reach about a thousand. Among the more important are two from the marine beds east of Boscombe. One is a portion of the stem of a cactus measuring two feet three inches by three inches, showing eighty bosses of spines cleared from the matrix. A section which I have made of this presents a flattened ellipse in which the pulp is replaced by sand and the woody stem has sunk down to the lower side, though still preserving the characteristic radiating structure. The cuticle is now thin and glossy black, and bears the spines, varying from two to a dozen on each boss, arranged in the usual spiral order. Heer described similar spines from Bovey as those of a palm, notwithstanding that the regularly spiral arrangement of the clusters is perfectly shown in Mr. Fitch's drawings.

The second of the specimens is the largest of several branches with leaves, of a Sequoia-like conifer, which abounds in the higher beds east of Bournemouth Pier, yet has not been found in those west of it. The foliage and branching might be almost equally taken for *Sequoia gigantea, Araucaria Cunninghami, Creptomeria japonica,* or *Arthrotaxis selaginoides.* The stem is slightly curved and does not branch for ten inches, but then forks into six slightly diverging branchlets, each some six inches long. Two of these terminate in swollen buds which would perhaps have borne cones, and another ends in a compact cluster of budding needles without any swelling, and might have produced the male flower. This branchlet, and the great number of others that have been formed with it, were evidently shed from the trees exactly as they are seen to fall from the similar conifers at Kew. Nothing beyond branches clothed with leaves have been found, and we have only the peculiar Araucaria-like swelling of some of the terminal buds to guide us. On the other hand, branches very strongly resembling these have been found by Baron Ettingshausen at Häring with Sequoia cones attached. I think however that this resemblance to Sequoia should not at present have too much value attached to it, because both genera appear to have lived contemporaneously, perhaps from Oolitic times, until the present day.

Ettingshausen has detected what he considers the flower and a scale of Sequoia among the specimens just obtained from the Lower Bournemouth beds, so that the view I put forward that some of the coniferous twigs associated with Bovey ferns were identical with *Sequoia Couttsiæ* of Bovey is somewhat confirmed. It is again most fortunate that I was able last year to obtain a twig of one of the commonest Alum Bay conifers, formerly referred to Taxites, Cupressites, &c., with the peculiar fruit of Podocarpus, recognised by Dr. Carruthers, attached to it, and it now seems probable that there are several distinct podocarps in our eocenes.

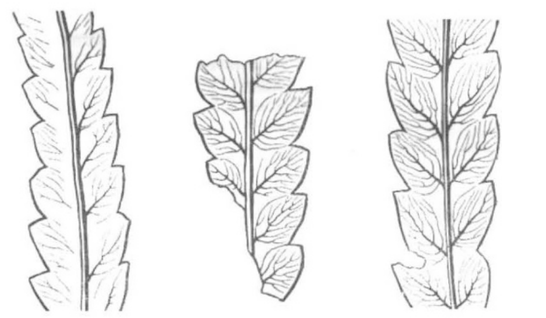
The remains of palm obtained this year are few but instructive. I was fortunate in obtaining from a small isolated patch of clay imbedded in sand, the spathe of a palm; a slab ten inches square covered with over twenty fruit stalks; and about eighteen inches of the upper part of the broad pinna of a feather palm. There is hardly room to doubt that these all belong to the same species, and its accurate determination in that case is a matter of almost certainty.

One exceptionally large fossil dicotyledon was obtained. This is a peltate, bluntly lobed leaf fifteen inches long from the foot of the leaf stalk to the tip, and ten inches across, and is considered by Ettingshausen to be near Cecropia.

Another striking specimen is not only a perfectly new, but one of the finest ferns yet discovered. My attention was called to it by a lady, who was watching my work and whose quick eye caught sight of the unusual venation even before I did, and we gradually brought to light an almost perfect palmate pinna, large enough to occupy a plate in the monograph now being published by the Palæontographical Society. The position of the sori bodering each lobe is distinctly traceable, and this character with its membranous texture and very slender rachis place it almost unmistakably in Adiantum,[1] while the anastomosing veins further define it as belonging to the sub-genus Hewardia, now confined to tropical America. I am the more pleased with this discovery since small mutilated fragments had already attracted my attention and been figured, without our possessing any satisfactory clue to their identity. I have named it *Hewardia regia.*

[1] Or possibly Lindsæa, sub-genus Schizoloma.

While on the subject of ferns, I am pained to have to refer again to a statement I made in this paper with respect to the well-known eocene representative of *Osmunda javanica.* The Rev. Prof. Heer cannot take the expression of an opinion different to his own, in the spirit in which it is meant, however courteously it may be expressed, and I regret that I have hitherto had the misfortune to feel compelled to differ from his conclusions upon almost every subject. In a footnote to a small pamphlet entitled "Die Aufgaben der Phyto-Palæontologie," which was only accidentally brought under my notice, he replies in a manner which renders further discussion impossible. He affects to suppose that I, a much younger man, would venture to differ from him without having reasons founded on new and positive data to justify my doing so. I select one of the instances in which he thinks proper to tell me I do not speak the truth, not because this one is more easy of proof, but because it immediately concerns my present work for the Palæontographical Society. I have accurately traced the figure of what he calls *Pecopteris lignitum,* the figure of his *Dryandra rigida,* and a piece of a fossil Osmunda from Bournemouth. They are so like each other and unlike anything else that nothing need be added. Heer's voluminous work



Dryandra rigida, Heer (Skopau). *Aspidium lignitum,* Heer (Skopau). *Osmunda lignitum* (Bournemouth).

has certainly not tended to simplify the determination of this particular fossil. He had described it as *Aspidium lignitum,*[1] *Dryandra rigida,*[2] and *Pecopteris lignitum,*[3] supposing it to be a Hemitelia, and not until two years after Stur[4] had proved it to be an Osmunda, does it appear in one of his works, without further explanation, as *Osmunda lignitum.*[5] Yet the fossil agrees with the well-known *O. javanica,* which ranges from Kamschatka to Java, so exactly, and in such minute particulars (as detailed in the second part of our monograph upon ferns, in course of publication) that it seems impossible to excuse such a series of mistakes. With unexampled carelessness he has permitted the lithographer, in every one of the works quoted, to distort and make the leaf an impossible one by colouring the lower pair of veins as if they were the margins of the leaf. Having decided, in his own mind, in describing the flora of Bovey Tracey, that this Osmunda was a tree fern, he connected with it, stems, young shoots, and what he calls rhizomes, which never belonged to it, the latter resembling the stem of the Australian grass tree. Two very characteristic statements are founded on this erroneous belief, one that "in the shade of the forest throve numerous ferns, one species of which (*Pecopteris lignitum*) seems to have formed trees of imposing grandeur," the other, that *its* stems with those of Sequoia "certainly contribute the greatest amount of lignite." The real facts are that this was not at all an arborescent fern, and that no vestiges even of the trunks of

[1] "Beitrag zu näher. Kenntn. d. Sächs-thüring. Braunkfl." (Pl. ix. Fig. 2)
[2] *Idem.* (Pl. x. Fig. 15.)
[3] *Phil. Trans.* vol. clii. p. 1047, 1861.
[4] O. Grutschreiberi, *Stur. Jahrbuch k.k. geol.-Reichsanstalt,* vol. xx. p. 9.
[5] *Jahrbuch der k. ungar.-geol. Anstalt,* vol. ii. 1872.

tree-ferns have ever been found in English eocenes. In the same way on the evidence of three seeds, which he supposes to be grape stones, and some cactus spines, we read that "the trees of the ancient forest were evidently festooned with vines, beside which the prickly Rotang-palm twined its snake-like form." Indeed, in addition to the error he committed in calling them miocene, all Heer's determinations of the Bovey Tracey plants require revising.

The Alum Bay leaf bed, familiar to geologists for twenty or thirty years, appears at last to have almost given out, for the leaf bearing pipe-clay is washed away to such an extent that a fortnight's stay scarce yielded a dozen of the commoner leaves. The unusual rainfall has also nearly obliterated the Hempstead section, and the face of the hill resembles a glacier of mud, which has carried trees and bushes, in place of rocks, into the sea. A lady, my brother, and myself had the misfortune to select that route home, returning from Gurnet Bay when darkness was coming on. The only passage over the deep and perfectly soft mud streams lay through the dead brushwood which fringed them. The tide was high on one side, and up the escarpment on the other lay mud and brushwood of the most impenetrable character, while with a tide still rising and darkness increasing, it appeared as hopeless to attempt to retrace our steps as to press on.

The following, from my note-book, has even less connection with fossil leaves, but the experience may be of use to geologists visiting the district.

At Alum Bay a large area of weathered chalk, usually supposed inaccessible, can be explored without much danger, for it is almost everywhere possible to descend to the sea-level between the Needles and the beacon on Freshwater Down. The face of the cliffs is traversed by numerous faintly marked tracks, which it is difficult to suppose could ever have been of service except to smugglers, for the shore line is rocky and not used by fishermen. Those who appreciate the bolder coast scenery of our white chalk will be repaid by climbs even of 500 or 600 feet, to the perfect solitude of the water's edge. If accompanied by ladies, a rope will be found a proper precaution and useful in lessening the exertion to them. One of the easiest ways is directly under the beacon, and there is a path down into Scratchells Bay, just inside the railing of the fort, whence at low tide the second of the Needles can be reached.

At Bournemouth we had a rather narrow escape. I foresaw that during this year's digging unusual caution would be necessary, owing to the heavy and saturated state of the cliff. I was obliged, however, to go through some fifteen feet of sand to reach a lower bed from which I expected to get pinnæ of *Goniopteris Bunburyi*. I had dug out a piece of this bed from end to end; a distance of about twenty feet by three or four feet wide; and the cliff above this narrow excavation consisted of some fifteen feet of vertical coarse sand, capped by indurated ironstone, and a thick black clay bed, above which the cliff sloped away at an angle. To expose a little more of the leaf bed we ventured at one point to slightly undermine the verticality of the cliff, before replacing the sand and clay we had dug out. During a pause for lunch sand fell twice upon the leaf bed cleared for work and was shovelled off. On a sudden loose pieces seemed to be falling all along the face of our pit, and with no more warning than an impulse to throw ourselves out of danger, huge boulders of clay and ironstone tore by—which from their weight were afterwards immovable to us—our excavation was completely filled in, and our tools still lie buried under the *débris*. I was helplessly buried for a few minutes up to my shoulders in sand, anticipating another slip, which fell soon after I was extricated.

J. STARKIE GARDNER

RECENT EXPERIMENTS ON RADIATION

EXPERIMENTS on radiation have a twofold interest. Accurate measurements of the increase of radiation due to an increase of temperature have of course a great theoretical value, but in addition to this, there is the practical question of a possible measure of temperature by means of the radiation of a body. It is this practical question with special reference to the temperature of the sun which seems chiefly to induce experimenters to study the subject with improved methods. It has led at any rate Mr. Rossetti to furnish a most valuable contribution to the study of radiation.[1]

Newton was the first to give a formula connecting the quantity of heat radiated by a body with the temperature of the body; but his formula was not sufficiently accurate, and has been replaced by another first given by Dulong and Petit. But Dulong and Petit's formula also breaks down when the difference of temperature between the radiating body and the inclosure is large.

Mr. Rossetti, trying to improve on Dulong and Petit's formula, deduces from his experiments the following for the radiation of lampblack:—

$$y = a\,T^2\,(T - \theta) - b\,(T - \theta),$$

where y is proportional to the thermal effect of the radiation, a and b are constants, and T and θ are the temperatures of the body and the inclosure, as measured on the absolute scale. This formula seems certainly to be as far superior to Dulong and Petit's as this latter was to Newton's. The last term generally is but small compared to the first, and Mr. Rossetti believes it to be due to the effect of the surrounding air, although we do not quite see how this can be. The following experiments prove how accurately the formula may be made to represent the facts. The constants a and b were obtained by measuring the radiation of a Leslie's cube filled with water or mercury, and gradually heated up to 300°. A piece of copper foil covered with lampblack was then heated in a flame of alcohol. The temperature of the flame lies between 390° and 400°; and two numbers obtained by means of the above formula were found to lie between these limits. The radiation of a red hot copper sphere was then determined, and its temperature independently measured by means of a calorimeter. The temperatures obtained by the two methods were 762°·1 and 763°·6 respectively.

In order to find the temperature of the copper sphere account was of course taken of the emissive power of copper as compared with lampblack. For this purpose, Mr. Rossetti has invented an ingenious method to determine this emissive power of various metals at the temperature of the Bunsen flame. That a formula obtained by means of experiments made between 0° and 300° C. should give such accurate results for a temperature of 760° is already a good proof for the usefulness of the formula, but Mr. Rossetti has pushed his verification even further. A cylinder of oxychloride of magnesium was heated in a flame of coal-gas and oxygen. The temperature was found to be about 960°, and in a flame it was found to be 2,167° and 2,397° in two experiments. Platinum melted easily in the flame, and hence the temperature could not have been far wrong.

Before Mr. Rossetti can apply his formula to determine the sun's temperature, he has to determine the absorptive effect of our atmosphere; but we shall not enter here into this part of the question. The sun's *effective temperature* is the temperature he would have, if he had the emissive power of lampblack. Mr. Rossetti finds this effective temperature by his formula to be a little below 10,000° C. Taking account of the fact that the sun himself is surrounded by an absorbing atmosphere, and accepting some data given by Secchi for the amount of this absorption, the temperature of the photosphere is found to be above 20,000° C.

[1] Reale Acc. dei Lincei (3) II. 6 Jan. 1878.