some respects, by its more ovate shape and its peculiarly serrated margin.

Ulmus Tasmanicus, n.s. Fig. 7.

Leaf ovate-lanceolate; base somewhat imperfect in the only specimen obtained, but evidently rounded; mid-rib strong towards base, and rapidly diminishing towards acuminate apex, where it is extremely delicate though sharply raised; secondary nerves simple or furcate (about 12 pairs) emerging from mid-rib at an angle of about 60 degrees, and proceeding upwards and outwards more or less irregularly straight or curved, terminating in a marginal tooth or serrature; margin somewhat coarsely and irregularly serrate, one or two serratures between extremities of secondary nerves. Viewed from the exposed lower surface of leaf, the left side has the fourth and sixth, and the right side the fifth, seventh, and eighth secondary nerves, furcate: Tertiary nerves at right angles to the secondary rare and very delicate, losing themselves in an extremely fine reticulation.

Length, about 5in.; greatest breadth, about $1\frac{3}{4}$in.; distance between each of the lowest four secondary nerves at base about 12 milimetres, the spaces gradually lessening upwards. Tertiary Sandstone, Mount Bischoff.

Although it is somewhat hazardous to determine the true position of a plant in the absence of fruit, I have referred the above leaf to the genus *Ulmus*, on account of the very characteristic form and neuration. I have frequently observed winged seed-impressions very like that of the Elm at One Tree Point.

Figs. 2, 3, 9, 10, 11 represent leaf impressions found associated with those described from the Tertiary Sandstones, Mount Bischoff, but whose position is as yet undetermined.

THE TASMANIAN EARTH TREMORS, 1883-4-5.

By A. B. Biggs.

(*Read 9th June*, 1885.)

The minute earth-shocks which have, during the past two years, been adding a new chapter to the history of our island, have been to me from the first a special subject of observation and study. The principal points which I set myself, if possible, to ascertain, were—First.—To arrive at an estimate of the actual magnitude of the surface-motion of the earth; and second.—To gain some idea of the position of the source, or focus, of the disturbance. I soon found, however, that the subject was much more difficult than I anticipated, from (a) the extreme minuteness of the actual earth movement, and (b) the large discrepancies in the reports from different localities, both as to the *time* and the *direction* of the shocks.

I have referred to the *minuteness* of these shocks. As the result of my experiments in the way of measurement I am satisfied that a greatly exaggerated notion of their magnitude generally prevails. To correct this notion, I offer a brief description of some of the apparatus which I have employed in the detection and measurement of the tremors. I at first tried some of the most approved forms of seismometer, amongst others the crossed U tubes of mercury—also a plate of glass, smoked on its upper surface, resting on three marbles, free to roll on a level glass slab. A pin point rested lightly on the smoked surface. This has recorded only once. I also tried a saucer of mercury, covered with a layer of treacle. Of the above, and other forms of apparatus unsuccessfully tried, a fuller description is given in my original MS. in the Society's library. The *negative* evidence furnished by these failures has, however, an important bearing upon the question. The fact that a small wooden style about 3in. in length, stood on one end, reduced to a base of $\frac{1}{10}$in. in diameter, has been overturned by only a few of the strongest shocks, speaks volumes.

It was evident that, for positive results, I must contrive something far more sensitive than the foregoing.

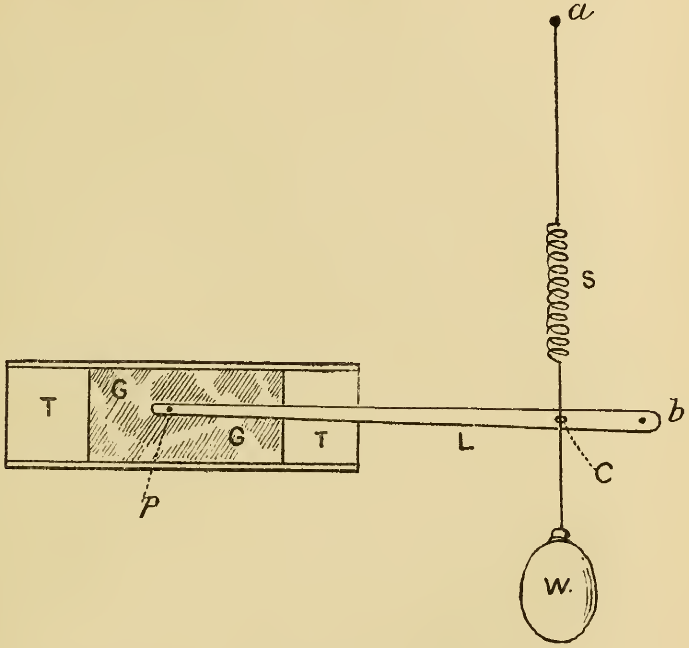
The following apparatus, which I now employ, I have found fairly successful:—

For Time Signal.—A free pendulum is suspended by copper wire, the end projecting below the bob and passing through a loop of platinum wire, this loop being only just large enough to allow the pendulum wire to pass through freely, without touching. The loop and pendulum wire each form part of an electric circuit. The least oscillation of the pendulum brings it into contact with the loop, thereby closing the circuit. This rings an electric bell, and at the same instant breaks the current which drives an electric clock, thereby showing the time of shock.

So sensitive is this apparatus, that, although fixed against a solid brick wall, it will respond to a moderate push against the wall with the hand. Of course this is affected only by horizontal movements. The pendulum is about 10in. in length. I at first tried a much longer one, about 7ft. or 8ft., but this seldom acted, in fact only indicated the very strongest tremors.

My signal apparatus for *vertical* earth movement consists of a delicate coiled spring of great elasticity, suspending a small weight through which the spring wire passes, the end

VERTICAL SEISMOMETER



S........ Coiled spring. suspended at a.

L.......... Lever. pivoted at b. and connected with spring wire at C.

P.............. Pin. through end of lever. the point resting against glass... ....G.

T............. Tin slide: turned over at edges for glass (G) to slide in.

of the wire being almost in contact with the surface of mercury contained in a small metallic cup. The least vertical jar brings the lower point of the spring into contact with the mercury, thus completing an electric current and recording the signal. Although I adjust this so that I can scarcely see space between the end of the wire and its reflected image in the mercury, many sensible tremors occur which it fails to register. I find by experiment that the floor of the room near the wall responds to a foot-fall through a space six times that for which the instrument is set as above, the floor being a good solid one; the actual measures of adjustment being ·005in. for tremors, and ·03in. for floor.

I have now to describe what may be more properly designated as my *seismometers*. For *horizontal* earth-movement I have a free pendulum, with a bob of about 4lb. weight. Projecting perpendicularly under the bob is a fine tube, in which slides a pin, point downwards. The point of the pin rests, by its own weight, upon the smoked surface of a piece of glass, which is elevated to the pin by three screws underneath. Any oscillation of the pendulum, or rather of the glass itself (as it is really the tablet, and not the style, that partakes of the earth motion) will of course record itself on the glass. The marks are afterwards measured by micrometer and the direction noted. By far the largest reading of this instrument was recorded on 13th July, last year; namely, 0·28in. (whole oscillation), the next largest being 0·09in. on 7th December, 1883.

My *vertical* seismometer is on the same principle as the electric vertical signal (described before) namely, a weight suspended by a coiled spring. Between the spring and the weight is a horizontal lever, attached at one-fifth of its length from the pivotted end; the free end carrying a pin point which traverses the surface of a piece of smoked glass. The motion is thus multiplied 5 times. The highest reading of this instrument occurred on the same date as that of the other, being 0·15in., on 13th July, 1884. (This to be divided by 5 for actual motion).

The transit instrument should be a most delicate indicator of any permament displacement. I have, from time to time, had to make minute readjustments, necessitated probably by the operation of the tremors. The cause is, however, uncertain, and the indications extremely minute.

The Rev. Canon Brownrigg, by a fortunate chance, obtained an *optical* observation of one of these tremors in the telescope. He was observing a star, when he noticed that the image began to dance in the field of view. He was puzzled as to what had shaken the telescope, until, on going into the house, he learned that a shock had occurred at about the time he was observing. By a necessarily rough judgment, the angular motion as compared with the angular value of the field-bar, would be about 1 minute of arc. This is as if a flag-pole 20ft. in height vibrated through a space of $\frac{1}{14}$in. at the top.

Now, in apparent opposition to the conclusion to be drawn from the foregoing as to the minuteness of the shocks, we have certain well avouched facts. I first take the somewhat startling circumstance reported in the Launceston papers by Mr. Geo. T. Hudson, as observed by him during the strong tremor of 13th December, 1883. He says:—"Our attention was directed particularly to the mud-flat" (in the river) . . . "which was a state of rapid agitation, moving apparently up and down in a most remarkable and alarming manner, the motion apparently coming from beneath, and from N.W. to. S.E. The rapid motion continued for fully one minute, and then gradually lessened, until it finally ceased. The wave motion was exceedingly severe, the tide being very low, etc." It is not surprising that such a sight as this should impress the beholder with the idea that it must be caused by some very violent agitation. I think, however, that it may be explained without any such inference. The tide was low, the mud-flat was still wet and glistening, and of course it reflected vividly the rays of sunlight. Under these circumstances the smallest motion of the surface would be immensely magnified, the magnification increasing with the distance from which it was viewed, in addition to the fact that the deviation of the reflected ray would be doubled by reflection. On this very occasion I was with a party taking tea, in a summer-house in a garden almost adjoining the spot from which Mr. Hudson viewed the phenomenon, and yet the tremor passed totally unperceived by any of us.

The shake of 13th July, 1884, was unquestionably by far the greatest shock of the whole series, and the only one that I know of that furnished any permanent and unquestionable exhibition of its violence. The principal tokens it left behind, in Launceston, were the throwing down a finial from the top of one of St. Andrew's Church spires, and the displacement of the masonry of another by about an inch, the throwing down of the newly-erected chimney of a bakery, also the top brick or tile of the chimney of Mr. Lay's house. All these effects however, may, I think, be accounted for without supposing anything more than a very minute shaking. (See M.S.)

On the day following, I visited some of the china and glass warehouses, where I saw goods so piled that I should have thought that merely walking heavily across the shop would cause a smash; yet I was told that nothing had been disturbed. In fact, I was informed at Mr. Hubbard's establishment, that the rumbling of the carts along the street caused more commotion amongst the crockery than the generality of the tremors.

I have dwelt at considerable length upon this branch of the subject, namely, the extreme actual minuteness of the earth-movement, because I have to combat an almost universal impression that is directly opposed to my deductions. The question will naturally arise—How are we to account for this impression? I am inclined to think these tremors are *vibrational*—our nerve-system is peculiarly sensitive to vibrations. The vibration of a great organ pipe may be felt as well as heard. A tap with a walking stick upon the end of a 20ft. log will be distinctly felt by a person sitting on the other end. I have, while reclining on a spring sofa, felt the vibration from a passing cart during its passage along the whole length of the block.

Mr. Jackson, machinist, of Patterson-street, Launceston, informs me that he spent some years in Japan, having left only recently, where he was employed mechanically in the construction of Mr. Milne's successful seismometers. He assures me that our tremors are very insignificant in comparison with those prevalent in Japan, our strongest shocks only being comparable to their weak ones.

With regard to the *position of the focus*, I assumed that there were two modes of arriving at a solution of the question, the one being a careful analysis of *the lines of direction*, and the other a *comparison of the times* at which the shock was felt in different localities. The first method, however, was found to fail in practice, probably owing chiefly to the fact that the molecular motion of a body in vibration is distinct and different from the actual wave progression. The second mode was rendered difficult and uncertain by the fact that the time quotations were evidently in many cases inaccurate and inconsistent.

Undoubtedly the tremor of 13th July, last year was, as I have said, by far the greatest of the series, and, from the wide range over which it was felt, was the first to furnish sufficient data from which to form anything like an approximate estimate of the position of its focus. I made a very careful analysis of the time records of this wave, which was published in the *Launceston Examiner* of 22nd July. The principle which I applied to this investigation I more particularly specified in a subsequent communication to the same paper, dated September 2, 1884, as follows:—I assumed that the earth-wave," should progress in a more or less irregular expanding circle, having the focus of disturbance in the centre; consequently there should be points situated all round the circle where the tremor should be felt at the same instant, just as a ripple from a stone thrown into a pond progresses all round from the centre outwards." Of course, difference of conductivity in different strata would tend to distort, more or less, the supposed circle; still, over long distances, I think we may fairly assume a pretty nearly equal average in different directions.

In applying this principle, it was necessary to first "smooth" the time curves, by averaging the different time reports from contiguous localities (I had 54 to work upon). I now quote from the article referred to (22 July):—

"It would appear that the wave first struck the coast line of N.S. Wales, from Sydney to Eden, nearly simultaneously, at 2 p.m. (1·44 p.m. our time). It next reaches Ringarooma at 1·55. A nearly synchronous line next passes through Wilson's Promontory, George Town, Launceston, Midland, Hobart, and the Huon, at 1·57. Lastly, it reaches Bischoff at 2·1. Now, the nearest focus I can find for these synchronous curves is about the East Coast of New Zealand. Setting one leg of a pair of compasses upon Mount Egmont, the other leg would strike these curves with considerable accuracy. These curves are separated by a distance of about 260 miles (roughly) from inner to outer. An earthwave has been estimated to travel at from 1200 to 1300 miles per hour. The shock reached Sydney 17 minutes before it reached Bischoff. This, at 20 miles per minute, would give 340 miles. But I think we have good reason for estimating the progress of the wave at a much slower rate, inasmuch as nearly all reports agree in describing the sound as *preceding* the shock, showing that the earth wave was slower than the sound wave. Taking this circumstance into account, I think the time taken in passing from curve to curve is not inconsistent with the foregoing hypothesis."

In this analysis I am, of course, not responsible for the correctness of the data, only for the conclusions deduced therefrom.

Next in importance, both as to strength of shock, and also with regard to the area over which it is traceable with reasonable precision, is that of 19th September last. Applying the same principles of investigation as before, it would appear that this wave first reached the N. East Coast at about 8·30 p.m.; then Launceston at 8·37½; Longford, Corners, Campbell Town, and Ross, at 8·38; and Prahran and Cape Schanck, at 8·39 (our time). A synchronous curve of 8·39 would pass through Prahran, Cape Schanck, and George Town,—a little west of Launceston, by Campbell Town, and somewhat east of Hobart. The focus of such a curve would be about 500 miles east of Cape Howe. (See my notes, *Launceston Examiner*, 2nd October).

So far then as the time record of these two great shocks may be relied on, they agree in indicating a focus far away to the eastward, and that the supposition that *these* emanated from anywhere near the Straits Islands is quite untenable.

Although, as I think, the conclusion at which I have arrived with reference to these particular shocks is inevitable from their time record, I by no means contend for a general application of that conclusion to the whole series. I have long felt a strong leaning to the probability that the home of a great proportion of these shocks is somewhere in the vicinity of the N.E. of Tasmania. But I am inclined to think it not only possible, but even probable, *that they do not necessarily spring from any one point in particular.* It is noteworthy that indications of direction have been extremely varied and perplexing. For some time after the commencement of these tremors, the prevailing direction appeared to be from somewhere near North-West, suggesting Sunda as their source. The time record, especially from August to the beginning of December, 1883, showed a prevailing progression towards the East and South, the shocks most frequently reaching Launceston before the East Coast and Hobart. Then there appeared a general change of direction, and from about April or May, 1884, as a general rule, they struck the N.E. Coast first. In my published meteorological remarks for May, 1884, I wrote as follows :—" I have observed during the past two or three months a marked change in the character of these tremors, inasmuch as they now appear to proceed from no direction in particular, and frequently give indications of a *twisting* motion." This would appear to indicate the transition stage.

Mr. Arthur Green, from whom I have received valuable assistance, has an ingenious and effective little pendulum seismometer, whose indications were frequently confirmatory of the above-mentioned peculiarity of motion. Its tracings about the time mentioned above often exhibited a more or less circular figure, as if actuated by simultaneous transverse forces. The shock of 21st March last exhibited a somewhat similar characteristic. My seismometer (horizontal) record was a circular mark of about $\frac{1}{20}$ inch diameter, which a magnifier showed to be crossed and re-crossed in all directions.

On the question as to the origin of these tremors, it appears to me that three hypotheses present themselves for consideration :—

1. The operation of *local volcanic* forces—that is, that the source is under our feet.

2. The *vibrations from some distant centre*, or centres, of volcanic activity.

3. Minor forces, comparatively local, excited into *sympathetic activity*, by some distant or general disturbance.

The first of these, I think, we may dismiss. As I remarked in discussing the 13th July tremor, "immediately over the focus of the disturbance the motion would be *vertical*, the earth-wave becoming more and more inclined as it recedes from that point.

Now, my seismometer readings indicate *great inclination*, or very minute vertical motion in comparison with the horizontal. This has been the general indication of my seismometers.

The *second* hypothesis is rather favoured by the records of the great shocks 13th July, and 19th September; but, against that, we have no knowledge of any sudden or violent convulsion in the direction indicated. Still, that there are forces at work in that direction is evidenced by the boiling springs, as well as at least one active volcano in New Zealand. It is, I think, not improbable that deep subterranean action of no great violence might propagate its vibrations to a long distance.

The *third* hypothesis fits in with the fact, which should not be overlooked, that during the whole period of these tremors, the entire globe has been in a state of abnormal disquiet. Within the period, there have been terrible earthquakes at Ischia and Sunda, besides lesser in Persia, Asia Minor, Spain, England and other places; also numerous volcanoes abnormally active, and new ones breaking out in all directions from Iceland and Alaska northwards, to New Zealand at least southwards. (Those at the extreme south we do not know about.) If the origin of our tremors were some fixed centre, I think we should not have had so much trouble in determining its position. After making all allowance for errors in time, I think the indications go to show that the minor tremors at least proceed from various directions. This again favours the third hypothesis.

If we might admit the probability of our being affected by shocks from the antipodes, it would almost seem as if our tremors had some indirect connection with earthquakes that have occurred the other side of the world. For comparison, I give below the dates of earthquakes in other places, with the nearest period of special activity in Tasmania:—

On 28th July, 1883, occurred the earthquake of *Naples*, and on 30th we had several tremors during the day, some of them felt generally throughout the island.

On the 26th August was the outbreak at *Sunda*.

We had an increase on 28th, culminating on 30th.

October 19th.—*Asia Minor.* Maximum in Tasmania on 26th, 27th, 28th. (Nothing very special.)

December 22.—Severe shock at *Lisbon.* 24th.—Launceston (several tremors), and New Zealand.

February 10, 1884.—*Asiatic Turkey* (severe). *Tasmania,* for several consecutive days, both before and after, culminating in a violent one on the 14th, with several minor shocks same day.

April 22.— *England.* Tasmania, all through the month; strong ones on 12th, 14th, 24th, 25th, and 26th; and on 27th my electric signal was going repeatedly, almost continuously.

May 19.—*Persia.*—Severe. Tasmania.—Repeated and almost continuous signals on 21st, 22nd, and 23rd.

January 26, 1885.—*Alhama* (Spain).—"Severe." "Tremendous report. One person killed."

27, 28.—*Southern Syria.*

27.—*Valparaiso* (morning).—Severe and long.

31.—*Algiers.*—Eight houses destroyed.

30-31.—*Tasmania* (midnight).—Strong, after a month or more of comparative quietness.

It will be seen by the foregoing list that a considerable interval elapses between the earthquakes elsewhere and their apparent effect upon us, far too long to admit of the supposition that it is the *direct* wave that reaches us. But may we not reasonably conceive of intermediate sources of energy (whatever may be their nature, for I am not going to commit myself to any volcanic theory in particular) which require only the slightest jar to disturb into operation, such, for instance, as fissures ready to collapse, or internal reservoirs of water, or gases, just ready to burst their bounds? I simply throw out these suggestions for what they are worth; I will not presume to dogmatise.

From the circumstance that very few of these shakings are felt down in the mines, the opinion has been pretty widely entertained, that they are merely superficial. I cannot accept this inference. In the mines the men are engaged about their work on solid ground with nothing but solid rock or earth around them—nothing to clatter or shake, or to give any indication of slight movement. On the contrary, above ground we are probably comfortably reclining, in the quiet of evening (when a great proportion of these tremors are noticed) perhaps on a spring sofa, in a more or less shaky tenement, and surrounded with glassware and nick-nacks that vibrate and respond to the slightest motion. No wonder, therefore, that minute shakings, totally unperceived by workers underground, are obstrusively apparent to us.

It has been observed that earth-shocks have frequently been associated with sudden and sometimes violent barometrical disturbance, from which I have been led to examine our own barometrical readings in connection with our periods of special telluric disturbance. I have failed however to discover any relationship between them. These periods appear to be associated indifferently with high or low, rising or falling barometer.

I have on my notes several other points which I believe would have been of interest, but I must forego them, as I fear this paper will be deemed too long already.

I would take this opportunity of thanking my numerous correspondents, who have from time to time assisted me by their communications.

NOTES ON JEAN JULIEN HOUTEN DE LaBILLARDIERE.

By Baron F. Von Mueller, K.C.M.G.

(*Read 14th July*, 1885.)

Jean Julien Houton de LaBillardiere, born in Alençon (Orne), 28th October, 1755; died in Paris, 8th January, 1834. He graduated in medicine in the University of Montpellier, but subsequently devoted his studies almost exclusively to botany. For this purpose he traversed first the European Alps, and travelled, then, through some portion of Britain. In 1786 and 1788 he was sent by Louis XVI. on a botanic exploration of Syria, which brought him also to the Lebanon. The literary result of this journey was his work, "Icones plantarum Syriæ rariores," the first part of which appeared in 1791. When in 1792 the first search expedition was sent out under Admiral d'Entrecasteaux to ascertain the fate of Count La Pérouse and his crew, M. de LaBillardiere became botanist of the expedition, and had thus the splendid opportunity of rendering known much of the vast vegetation of South-west Australia (King George's Sound having only in the year before been discovered by Captain Vancouver), and also of the southern part of Tasmania, he being the first to explore phytologially the region where now the town of Hobart stands, although Bruni Island was visited during Cook's second and third expeditions in 1773 and 1777 already. At the war time LaBillardiere's collections were confiscated in Java; but on his return to France were restored to him through the influence of Sir Joseph