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On a Quartz Thread Gravity Balance

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VII. *On a Quartz Thread Gravity Balance.*

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[PLATES 13, 14.]

THE problem of replacing dynamical by statical methods of studying the variations of the earth's gravitational force has long occupied the attention of astronomers and physicists, and a good many attempts have been made to construct an instrument which should enable relative measurements of gravitational force to be carried out with a smaller expenditure of time and trouble than is incidental to the observation of pendulums.

The only kind of force which is practicable as a means of opposing gravitation in the construction of such an instrument as is here contemplated, is that derived from the elastic properties of matter.

Accordingly the problem is reduced to that of constructing either a spring balance of sufficient accuracy, or of making use of the elastic properties of a gas.

In both cases, when we approach the limits of accuracy obtainable by pendulum observations, we have to face very great difficulties arising from the necessity of ascertaining the temperature of the apparatus within very narrow limits, and this obviously implies the even greater difficulty of insuring that the apparatus shall have the same temperature at every point.

If we add to this the consideration that the apparatus must be reasonably portable, and of such construction that it is not possible to disturb its mechanism by the shaking inseparable from transport, it is evident that we have to face a mechanical and physical problem of considerable difficulty.

Until Mr. BOYS discovered the unique properties of fused quartz, in 1887,[*] no material having elastic properties of the requisite simplicity and constancy was available, and from this cause, if from no other, all attempts at constructing a statical instrument of reasonable accuracy must necessarily have failed—as they all did. But even setting this aside, we are satisfied that all the designs—some of them of great

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[*] 'Phil. Mag.,' June, 1887, p. 489.

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ingenuity—which have hitherto been submitted, must necessarily have failed from the difficulty of securing in them the requisite uniformity of temperature.

With regard to forms of apparatus in which advantage is taken of the elastic properties of gases, it is proper to observe that, though it is possible to arrange a compensation effective to the first order of the variations due to change of temperature, yet the apparatus must almost necessarily be too large to satisfy the condition as to the temperature being sufficiently uniform throughout.

Very closely associated with the problem above discussed is that of making an instrument by which small secular variations in the gravitational force at any one place may be ascertained, though in this case the problem is very considerably simplified, because the question of portability does not arise, and arrangements may be made for minimising fluctuations of temperature. In this case, however, the observations should be of a higher order of accuracy than is necessary during a gravity survey.

As soon as we became familiar with the properties of fused quartz, it became obvious to us, as well as to others, that by taking advantage of these qualities it might be possible to construct a balance having sufficiently permanent elastic properties to give a practical solution of the problem of constructing a gravity meter.

A committee of the British Association, which in 1886 had invited designs for a gravity meter, reported in 1889 that work had been suspended pending a trial of fused quartz. Our own attempts to construct a gravity balance began in September, 1889, and have continued uninterruptedly ever since.

When we began to work at the matter we formed an impression that the problem of observing small variations in the intensity of gravity at any one place would prove simpler than that of constructing a portable instrument, and consequently we first turned our attention in this direction. We worked at instruments of what may be called the non-portable balance class for two years, at the end of which time we had satisfied ourselves that we were not likely to attain to sufficient sensitiveness, and accordingly we turned our attention to the construction of a portable instrument. We experimented in this direction for some time, with the result that in October, 1892, we began to construct what we hoped would prove to be a final form of instrument, but it was not until September, 1893, that we had got it forward enough to commence systematic observing.

With regard to portability we may say that we have travelled with the present balance over 6,000 miles. Of seven quartz threads which we have had in actual use since the balance was completed in September, 1893, only one has broken. During the first journey in February, 1894, and through the negligence of a person who had undertaken to look after it, the balance was knocked off its stand and practically destroyed; the only part which was not broken was the thread. The present thread was mounted on the 10th of September, 1896. It has travelled over 4,600 miles by cart, railway, and steamer. Observations have been taken with this thread at four stations in New South Wales, at Melbourne in Victoria, and at Hobart and Launceston in Tasmania. On November 23rd, when hurrying to catch the train at Springwood, N.S.W., one of the handles of the box containing the balance broke, and one end of the box fell through a distance of about two feet on to the asphalt platform. The reading was found to be altered when we got to Sydney, but the thread was undamaged. As the final result of the present investigation we have determined the value of "gravity" at Hornsby, a station 21 miles from Sydney and 472 feet above the laboratory, relatively to that at Sydney, in three journeys, with a maximum difference less than one part in 500,000. For the purpose of a survey the evidence shows that a single observation with the balance will enable g to be determined relatively to a standard value to within one part in 100,000. There is a great probability, however, that the error would be less than one part in 200,000.

Before proceeding to the discussion of the subject of this paper we desire to perform the pleasant duty of thanking those who have assisted us in one way or another, and first amongst these is Mr. JAMES COOK, F.R.A.S., our mechanical assistant. Mr. COOK made the whole of the instrument, except the thermometric appliances, including his own working drawings from our sketch designs. It is not too much to say that had we not been so fortunate as to have commanded his great mechanical skill and accuracy we should in all probability have failed in our undertaking.

To Mr. J. J. E. DURACK, Deas Thomson Scholar of the University of Sydney, and to Miss FLORENCE MARTIN, we owe our thanks for much observational assistance. Through the enlightened liberality of the Commissioners of the Railways of New South Wales we were provided with free passes over the government railways. To them, and to the Secretary for Railways, Mr. HUGH MACLACHLAN, we desire to tender our thanks, not only for our free passes, but for the unfailing kindness and courtesy which they showered upon us.

Amongst those who have assisted us with advice upon special points we desire especially to thank our colleague, Professor GURNEY, Mr. G. H. KNIBBS, Mr. E. F. J. LOVE, Mr. G. F. FLEURI, M. GUILLAUME, Mr. GRIFFITHS, and Mr. CHARD, L.S.

During our journeys we were provided with facilities for making our observations by Professor LYLE, of the University of Melbourne; Professor MCAULAY, of the University of Tasmania; Mr. ALEXANDER MORTON, Curator of the Tasmanian Museum at Hobart; Mr. W. J. BAIN, of Launceston; Sir PHILIP FYSH, (late) Premier of Tasmania, and Dr. WIGAN, Acting Mayor of Armidale in 1897.

The Surveyor-General of New South Wales also deserves our thanks for having placed a disused theodolite at our disposal, the circle of which was employed for a long time as part of our balance.

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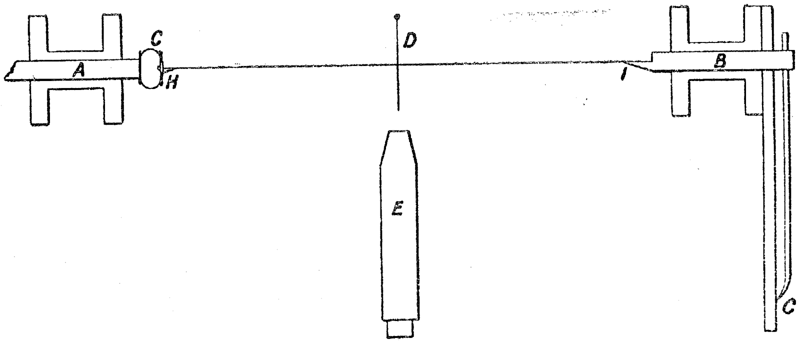
GENERAL DESCRIPTION OF THE INSTRUMENT AND PRELIMINARY REMARKS.

A and B (fig. 1) are two metallic rods capable of adjustment by sliding along their common axis, but having no freedom to move transversely; in our earlier experiments we made use of the head stock and back centre of a watchmaker's lathe.

Fig. 1



C is a coach spring attached to A, and carrying a point H, to which the end of the quartz thread is fastened.

B is a brass axle capable of turning about the axial line HI, which also represents the quartz thread; it is attached rigidly to an arm carrying a vernier G. The vernier moves over a divided circular arc—a sextant being in fact employed.

The quartz thread has as nearly as possible a diameter of ·0015 inch = ·0038 centim., and is very uniform; it is soldered up to the points prepared for it at H and I.

The distance of A from B is regulated so that the thread is stretched tight by the spring at C.

The length HI is 30·5 centims.

D is a piece of gilded brass wire 5·3 centims. long, and its mass is 0·018 gram; it is attached to the thread by soldering, the thread lying in a little kink in the wire.

It is adjusted so that its centre of gravity lies a little to one side of the thread.

E is a microscope attached to the frame of the machine so as to occupy an invariable position with respect to the supports A and B.

The microscope is provided with a riding level showing 2″ of arc per division of 2·5 millims. at ordinary temperatures.

The whole apparatus is provided with levelling screws, so that the axis of collimation of the microscope can be brought into an invariable position by means of the riding level, which invariable position is very nearly or quite horizontal. Also the thread itself can be adjusted to lie in a horizontal plane by means of a subsidiary level, which is permanently attached to the frame work, and is at right angles to the riding level.

The apparatus is thermally insulated so far as possible, and a platinum wire thermometer wound upon very thin glass lies alongside the thread.

There is also an arrangement for arresting the end of the lever for security during transport.

The gravitational moment of the lever about the thread requires to be adjusted by the addition of small drops of fusible metal solder. The moment is so adjusted that about three whole turns of each end of the thread are required to keep the lever horizontal. This adjustment is made once for all; during the process of observing the thread is twisted from one end only, viz., the end attached to the sextant arm. It will be shown directly (in the section dealing with the theory of the instrument) that the equilibrium of the lever becomes unstable when its centre of gravity rises a few degrees above the horizontal plane passing through the thread.

The microscope is so arranged with respect to the lever that, when the riding level is horizontal, the image of the end of the lever lies on the cross wire; and the lever itself is almost, but not quite, in the position of instability.

By turning the vernier arm it is possible to increase or diminish the twist of the thread from I up to the lever. When the thread is increasingly twisted, the side of the lever on which is the centre of gravity rises until the position of instability is reached and the lever upsets; it is caught, however, by the arrester, and is not allowed to fall right over. When the intensity of gravitational force increases, the centre of gravity of the lever falls, and the thread has to be twisted by the vernier arm in order to bring the lever back to its former position.

The immediate subject of observation is the amount of twisting or untwisting necessary to bring the lever to its sighted position.

In order to calibrate the instrument, it is necessary to know the total twist of the thread, that the thread is uniform, or at all events of a definite shape, the position of the centre of gravity of the lever, and the exact position occupied by the lever with respect to the ends of the thread. As these quantities are not exactly ascertainable, we prefer to calibrate the instrument by observing the change of reading which occurs when it is taken from Sydney to Melbourne—the gravitational data of both of these places being sufficiently well established.

It appears from the theory below that this information, together with the vernier reading at a third station and the corresponding temperature readings at each station, give us all the data necessary for calculating the value of gravity at the third station.

The necessity of observing the temperature arises from the fact that the rigidity of fused quartz threads increases with increase of temperature. The effect is, of course, complicated by the changes which take place in the dimensions of the threads. The nett result is that as the temperature rises the "stiffness" of the threads increases.*

The dimensions of the lever also change, and in such a manner as to partially compensate the changes of "stiffness" taking place in the thread. In the actual instrument the resilience as well as the figure of the coach spring vary with temperature, the line of collimation of the microscope may also vary relatively to the level, and the metal framework may also twist and vary in shape in any manner. All these effects are found to be capable of collection into a single temperature coefficient—which, so far, we have found to be sufficiently well expressed by a single term—the temperature scale adopted being the platinum scale of CALLENDER.

Many attempts were made to annul the effect of variation of temperature by constructing levers composed of two bars of metals of different expansibilities, an account of which will be found in the appendix, but all such attempts came to nothing.

The sensitiveness of the instrument is at present such that to compensate for a change of one part in 100,000 in the value of g it would be necessary to move the vernier arm through 2·12 sextant (doubled) minutes = 1·060 minutes of arc. Also a change of temperature of one-tenth of a degree alters the circle reading by 3·15 sextant minutes, gravity being invariable.

Although quartz is immeasurably superior to any other known material as regards the constancy of its elastic properties, it must not be supposed that it is entirely free from elastic defects. As a matter of fact, it is only just good enough for the present purpose. One of the great difficulties we have had to overcome has arisen from the fact that it is only by the most judicious choice of dimensions that it is possible to reduce the viscous yielding of the thread to within practicable limits, and even as it is, although the thread of the present instrument has been twisted for more than two years, and indeed purposely overtwisted for part of that time, we are still obliged to apply an important though practically constant correction on this account. In other words, the reading of the sextant arc is still slowly decreasing, and though the rate of decrease is now constant for all practical purposes, it has to be taken strictly into account in interpreting the indications of the instrument, even when consecutive observations are separated by an interval of time as short as a single day.

It will, no doubt, occur to the reader that we ought to use a finer thread, in order to get rid of this source of inconvenience. This, however, experience has shown us, we are not at liberty to do, for a finer thread means either a finer lever or one soldered to the thread at a point nearer to its own centre of gravity. The former

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* THRELFALL, 'Phil. Mag.,' July, 1890.

solution is inconsistent with the necessity of having the lever strong enough to bear arresting without change of form, and the latter has the drawback that it tends to magnify the effect of such variations as do actually occur in the shape of the lever. In short, the design of a gravity balance becomes a matter of compromise just like any other engineering undertaking. In calculating the moment of the gravitational forces brought to bear upon the thread by the weight of the lever, it must not be forgotten that the effective weight is reduced in consequence of the flotation of the lever by the air surrounding it. At an accuracy of one part in a hundred thousand in the estimation of g the effect of variations of barometric pressure or humidity would become sensible, and it is therefore necessary to protect the lever against variations of air density. This is done by enclosing the whole apparatus in an air-tight casing within which the air is kept at a constant density. In practice the density selected corresponds to a pressure slightly below the minimum external pressure of the air. The apparatus must also clearly be filled with dry air to avoid the deposit of dew. As a consequence, the twisting of the thread necessary to afford a reading of the instrument, has to be carried out by means of a shaft working through a stuffing box; and the requisite accuracy in the estimation of the twist given to the thread makes it necessary that the rod shall work almost without friction in the stuffing box.

We have devised a sort of mercury and tallow joint which has got over the difficulty fairly well; and which is also applied to the stuffing box through which the rod of the arrester works.

An aneroid in connection with the internal space enables any leakage to be detected.

The constructional difficulties above indicated were further increased by our determination to have no iron or steel work about the machine: this was due to fear of magnetic interference; with which, however, we have never been troubled.

The degree of portability attained will be understood from the following statement of the sizes and weights of the various appliances requisite for an observation of gravity, as, for instance, during a survey when everything must be provided.

	Length.	Breadth.	Depth.	Weight.
	ft. ins.	ins.	ins.	lbs.
Box containing balance	2 9½	19	15¼	106
Box of resistance coils	2 4½	10	10	23
Box of accessories	2 1½	17½	11½	82
Legs of tripod stand	...	...	...	15
		Total weight		226

The above weights and dimensions refer to a balance of gun-metal and copper, and accessories taken just as they came to hand from the laboratory. Both weights and dimensions might be greatly reduced if so desired. The weights certainly might be easily halved.

It is necessary to observe either upon a stone, cement, earthen or asphalte floor. Boards, however strong, do not form a sufficiently inelastic support.

INSTRUMENTAL DETAILS.

The drawings on Plate 13 are to scale, and ought to be self explanatory. Notes, however, have been added on the page facing the drawings in order to make it easier to follow the latter. It will be seen that the general principle of construction is as follows:—All the essential parts are rigidly held together by a system of bars forming a complete mechanism. This is then thrust into a tube of copper, which it fits precisely, and the tube of copper is further surrounded by a packing of paper to insure some degree of thermal insulation. The paper in its turn is surrounded by an outer tube of brass, and this is held down to the base by brass clips. The copper tube is closed at one end by the brass cover carrying the vernier arm and mercury stuffing box, which constitute the circle end of the machine. At the other end there is a smaller brass cover, through which projects the end of the arrester shaft and the end of the thermometer; or, rather, the thick terminals and the end of the ebonite shank on which the glass tube is mounted. The aneroid tube also passes to the interior of the apparatus through this end.

The copper tube has two openings at opposite ends of a horizontal diameter, and into these openings are fastened, at one side a window, at the other the microscope focussed upon the end of the lever.

The under frame is provided with levelling screws so spaced as to fit the grooves of the top of a Kew magnetometer tripod. The instrument having been reconstructed several times does not now occupy the best position on the under frame, and this has necessitated the addition of a heavy lead counterpoise. Without this counterpoise the weight carried by the back levelling screw is too great to permit of the screw being turned with sufficient ease for exact levelling. The counterpoise is not shown in the drawing, but appears in the photograph (see Plate 14).

Attachment of the inner mechanism to the copper tube.—The bars connecting the bearing (which supports the "spring" end of the thread) to the bearing which carries the vernier arm are of gun-metal, and the bearing which carries the spring fits with considerable accuracy into the copper tube, carefully bored for the purpose. During our earlier observations the spring bearing was quite free to slide up or down the copper tube according as the temperature rose or fell. After one of our journeys, however, we thought that there was some evidence of the spring end of the thread having moved, and it appeared possible to connect this motion with the freedom of the spring bearing. Accordingly at present the bearing is wedged tight to the copper tube.

Stuffing Boxes.

The idea was to render the brass work incapable of being amalgamated by plating it with nickel, which for ordinary purposes may be regarded as incapable of amalgamation. The fit of the shafts both of the vernier arm and of the arrester is made as good as possible without being at all stiff. The joints between the shafts and the cylindrical holes in which they work are further filled in with tallow. Outside a cup-shaped depression is arranged, and this is filled with mercury. As there is a partial vacuum inside the apparatus, the tendency is for the mercury to drive the tallow through the joint and then to follow it. The joint is very fine, however, and the surface tension of the mercury tends to oppose this motion. The practical result is that we obtain a smoothly working motion and a sufficient air tightness so far as inwardly directed air pressures are concerned.

Microscope.

The microscope is supported on a flange soldered to the thick copper tube. There is also a flange on the tube within which the microscope is fixed, and the adjustments of the microscope are partly made by sliding one flange over the other, the clamping screws which press the opposing surfaces together being passed purposely through holes much larger than the screw diameters. The flanges are ground to fit, and are put together with a little tallow for the sake of securing air tightness.

The microscope itself is an ordinary Zeiss microscope tube furnished with the "A" objective, giving a magnification of about 100 diameters. The adjustments are made as follows. It is required to adjust the microscope so that the image of the end of the lever will be sharply in focus, and bisect the cross wire when it is a small definite amount below its position of instability. For the purpose of arriving at this adjustment the object-glass is first soldered by its screw mounting to a length of brass tube which fits inside the stronger tube very well. The outer tube is attached by soldering and screwing to the flange, and carries the eye-piece itself mounted in a short length of tube in which the cross wires are fixed. The eye-piece is focussed on the cross wires as usual, the object-glass is placed in position, and the outer tube is levelled by means of the riding level. The focussing is then accomplished by sliding the object-glass tube in or out, and when the desired adjustment has been obtained, the flange is unclamped and the object-glass tube sweated in position with tinman's solder, by which means it becomes rigidly attached to the outer tube. The outer lenses of the object glass are also fixed in their mountings by means of wax, but the workmanship of the cell is so good that the wax acts merely as a means of preventing the leakage of air. No doubt some change in the position of the axis of collimation occurs as the temperature rises. This will do no harm if the position of the axis of collimation can be regarded as a function of the temperature, but the chance of irregularities must be put up with so long as brass mounts are used: with platinum a greater degree of certainty is to be expected. It is fair to say, however, that we have no reason to suspect the axis of collimation of movement rather than any other part of the apparatus. When the preliminary focussing has been accomplished, and the soldering satisfactorily finished, the microscope is again mounted and levelled, and the final adjustment made so that the lever upsets when its pointed end rises above the cross wire by one diameter of the point. It generally happens that the focus is not quite correct, but this can now be set right by means of the eye-piece. Finally the eye piece (which is waxed and otherwise fastened to a tube several inches long sliding very tightly inside the outer tube) is finally waxed air tight and firm. All the parts of the microscope are now firmly fixed to the outer tube, and the whole is correctly focussed upon the end of the lever when the latter is in position. It remains to explain how the outer tube of the microscope is prepared. It must be remembered that the outer tube carries the riding level, and that the whole theory rests upon the assumption that the axis of collimation can be brought into an invariable position with respect to the horizontal. In order to avoid spending too much time over levelling the instrument when it is in actual process of observation, it is of some assistance to have the tube of the microscope and the V-grooves of the riding level so perfect that, in ordinary phraseology, the level will reverse. The microscope tube was ground and lapped by us on one of BROWN and SHARPE's cutter grinders, which are not recommended by the makers for producing a cylindrical surface. However, by applying care and attention and rotating the tube between centres we have succeeded in making a tube so cylindrical that we are unable by any of our appliances to detect any defect of form. We have to thank the mechanical assistant of the laboratory, Mr. JAMES COOK, for making the V-grooves so perfect that the sensitive level we employ does in fact reverse when mounted on the microscope tube.

The riding level is a very substantial affair, the frame is of brass cut away as much as possible for the sake of lightness. A cross level is attached to the frame by adjusting screws. The main level is mounted in a copper tube (see p. 232).

Quartz Thread and Lever and Attachments.

This being the essential part of the instrument, requires considerable care both in its design and its manufacture. The general methods have been described by BOYS,* also THRELFALL ;† but it will suffice for the present purpose to refer the reader to a book on 'Laboratory Arts,'‡ Sections 80 to 91.

We have already referred to the fact that it is necessary to attend to the dimensions of the thread employed, and will not repeat ourselves except to point out that to arrive at the best relative dimensions as the result of a compromise necessarily implies an immense amount of experimenting. At one time the quartz

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* 'Phil. Mag.,' June, 1887, p. 489. 'Journal of the Society of Arts,' 1889. 'Journal of the Physical Society,' 1894. 'Phil. Mag.,' 1894, vol. 37, p. 463.

† 'Phil. Mag.,' July, 1890.

‡ THRELFALL, Macmillan, 1898.

behaved so badly that we actually took the trouble to assure ourselves by trial of its superiority over other materials. We mounted a very fine steel wire, kindly sent to us by Mr. ELLERY of the Melbourne Observatory, on another balance, and observed that its rate of viscous subsidence was about a hundred times greater than we were accustomed to in the case of quartz. We repeated the experiment with a very fine platinum wire with a similar result.

It must not be supposed that all fused quartz, as derived from clear rock crystal, has the same properties. Almost every crystal examined by us contains both sodium and lithium—the latter in large spectroscopic quantity—indeed, we first noticed it from the colour it gave to the blowpipe flame. The observation of the almost universal presence of lithium in quartz was first made by TEGETMEIER,* a fact of which we were ignorant when we made the observation. There also appears to us to be a distinct though small difference in the viscosity of various samples of fused quartz, and this independently of the sodium or lithium they may contain. It has been our practice to select the most infusible quartz independently of the amount of lithium it contains, for lithium seems to be burned out by continued heating in the oxy-gas flame: whether this is really the case or whether the lithium forms a compound which does not give the flame re-action, we have not attempted to inquire. We have aimed at securing the greatest possible uniformity in the thread and a mean diameter of about ·0038 centim. We do not pass a thread unless the diameter is uniform from end to end within the limits of observation (exceeding those of measurement), looking at samples taken from each end of the thread through a microscope magnifying 100 diameters. We have also spent a great deal of time in trying to make certain that there were no drawn-out air bubbles in the thread, and though we have succeeded in arriving at the satisfaction of both these conditions simultaneously, still our present practice is to ignore very small bubbles, and to direct our attention principally to obtaining uniformity. Since, however, we now adjust the torsion of one end of the thread only, we are inclined to think that for the future we would put up with a slight taper in the thread, provided of course that the twisting for adjustment was done from the thinner end.

The bow-and-arrow method of Mr. BOYS gives better threads for our purpose than the catapult method ('Laboratory Arts'), which does not lend itself to the production of threads of great uniformity. There may also be some difference of tempering, owing to the different rates at which the threads are pulled out and cooled in the two methods considered. A great deal of time may be spent in preparing the thread, for at present we have no method of predetermining its diameter. All that we can do is to be guided by experience, and shoot threads till one is got satisfying the conditions. The process is so uncertain that we have on occasion got a thread within a few days, and on others we have spent a fortnight over it. The thread which forms part of the instrument to which the observations refer took a fortnight's continuous

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* 'Wied. Ann.,' 41, p. 19, 1890.

shooting. The best way of carrying out the examination is to draw the thread, comparing it all along its length with a bit broken from one end laid beside it under the cover slip of a microscope slide, the space between the slide and cover slip being filled with stained glycerine.

When a sufficiently perfect thread is obtained, it is silvered all over, as described in 'Laboratory Arts,' p. 222, and the ends are then coppered and soldered up to the supports. The centre of the thread is then coppered for about a centimetre of its length. The lever is adjusted in position, being held tight by a temporary clamp mounted from the back girder. It is then soldered up to the thread by tinman's solder. The excess of silver is removed with nitric acid, and the thread is well washed. It is surprising how difficult it is to do this thoroughly; we use a brush made of glass, but we are not sure that it is the best way.

Lever.

We have made levers of many metals, but at present use one of gilded brass wire of the smallest diameter we could get with our draw-plates, say ·005 inch. The conditions to be satisfied are (1) magnetic indifference, (2) undeformability by the arrester, (3) lightness. At first we used to make levers of aluminium foil, shaped like a cross; we then tried soft annealed copper, in the hope of doing away with secular changes of shape, but it was not stiff enough to resist the arresting.

Adjustments of the Lever.—Having decided to use three turns in each half of the thread, it is necessary to adjust the lever so that the line joining the centre of gravity of the lever to the thread is nearly horizontal when the thread is twisted with this amount of twist.

It is shown in the part of this paper dealing with the theory of the instrument that with this twist in the thread the lever becomes unstable when the line joining its centre of gravity to the thread rises about three degrees above the horizontal. We have adjusted the lever in the present case so that it upsets when the thread is twisted by about three whole turns. The adjustment is made by weighting the lever with a small drop of fusible metal, rather larger than a pin's head.

In making a new machine we think it would be worth while to make the lever out of a thickish needle of fused quartz. We have occasionally thought that the thread was slipping in its silvering, but though there is a distinct danger of this happening, we have not had a really clear case. Some attempts to guard against the possibility of this, by grinding the thread flat on one side, failed on account of the way in which the thread was weakened.

The Spring and its Adjustments.

It is almost if not quite essential that one end of the quartz fibre shall be carried by a spring, otherwise the difficulty of manipulation becomes intolerable. It is also clear that the spring must satisfy some peculiar conditions. The thread must be stretched by a constant, or nearly constant force, and the spring, while freely allowing this to occur, must be incapable of moving so that the point of attachment of the thread shall become displaced transversely. The spring must also be subject to so much damping that it will not vibrate during transport, otherwise the rate of subsidence of the thread may be affected and errors introduced. We have attempted to meet these conditions in the following manner. The spring, as shown in the drawings (Plate 13), consists of a pair of elliptical springs crossing each other at right angles, the point of support for the thread being at the centre of the crossing. The point to which the thread is attached is very light and might be lighter with advantage, so as to increase the effect of the damping.

There are four bars of spring steel (watch main-spring, in fact), which are attached to the plate carrying the coach springs so as to stand up perpendicular to the plate. The free ends of these springs are attached by wire links to the thread support. The object of this disposition is to damp down the free transverse vibrations of the support. For a long time this formed the complete apparatus, which will be referred to hereafter as the "rosette" spring.

In October, 1898, however, we had reason to believe that some small residual irregularities were traceable to a transverse movement of the thread attachment. We therefore put on three stays, made of very fine glass hairs, so as to hold the support to the three girders of the main framework. The stays were cemented in position by paraffin, and the wire linkages were also cemented at their points of contact by means of paraffin. This has been completely successful, as the observations will show. We are, however, of opinion that we could improve this part of the apparatus still further, for paraffin is not an ideal cement. In fastening the thread up to the support, and afterwards in stretching the thread, it is very convenient to have a slow adjustment. This is supplied by fastening the spring system on the end of a bar; the bar is passed through the end bearing and is adjusted by means of a nut working on a thread cut on the bar. The proper tension of the thread having been arrived at, the nut and bar are soldered up to the end bearing. In view of the great difficulty of preparing a good thread, it is well worth while to provide every possible convenience for adjustment, and for minimising the risk of breakage.

Thermometry.

We believe that we wasted some time in endeavouring to obtain consistent results with mercury-in-glass thermometers. We were very unwilling to adopt platinum thermometry on account of the loss of portability which is its inevitable concomitant. Through the kindness of Mr. Griffiths we obtained three of Tonnelot's thermometers, which were studied very carefully by M. Guillaume before they were sent to Mr. Griffiths. They were again studied by Mr. Griffiths before they were sent out to us, with the result that he wrote to say he believed they were the three finest thermometers ever made. We mention this to show that we really went to some trouble in the matter, and did not take to platinum thermometry until it was actually forced upon us. We desire to thank M. GUILLAUME and Mr. GRIFFITHS for the trouble they took in the matter. The reason why mercury-in-glass thermometers fail for our purpose is that they are too slow when of the necessary sensitiveness, that they do not give the mean temperature of the thread, and that the zero corrections present difficulties in field work. We are glad to acknowledge that it was M. GUILLAUME'S opinion from the first that platinum thermometry would be the most suitable for us. The instrument we at present employ is made simply according to the instructions of Mr. CALLENDER,* but we have made one or two small modifications in the resistance box which was constructed to work it. The platinum wire, of the diameter recommended by Mr. CALLENDER, is wound in a double spiral on a very thin glass tube, 1 centim. in diameter. The tube is mounted on an ebonite plug through which the leads pass, and which serves to screw it in to the inner tube of the balance. The tube is unsupported, except by the plug, and is arranged to lie parallel to the thread, and at a distance of 2 centims. more or less below it, *i.e.*, 2 centims. from thread to centre of tube.

When we first adopted the platinum thermometer we were of the opinion that the temperature of the thermometer would lag behind that of the thread, but experience has shown that the opposite is the case. When the temperature is rising to a maximum the maximum is always reached and passed some few minutes before the lever reaches its highest point.

The apparent slowness of the thread in taking up the temperature of the surrounding space may be due to the following circumstance. Though we are not in possession of the complete mathematical theory of a stretched, weighted, and twisted thread, we have found by experience that an increase of tension of the thread, as by tightening the rosette spring, acts in the same way as an increase in the twist of the thread. Consequently, if the spring takes some time to reach the surrounding temperature, its effect will be the same as if the temperature of the thread itself were lagging, at all events so far as the observer at the microscope is concerned. This appears as follows. The action of a rise of temperature on the tension of the spring is complex. The expansion of the bars increases the tension on the thread; the decrease in the elastic forces of the spring produces an opposite effect. On the whole the former is probably the larger effect and therefore predominates. Consequently, the rise of temperature will stretch the thread; this will act like an increase of twist, hence less twist will be required to keep the lever in its sighted position, but this is exactly the effect produced by the temperature increase in the rigidity of the thread.

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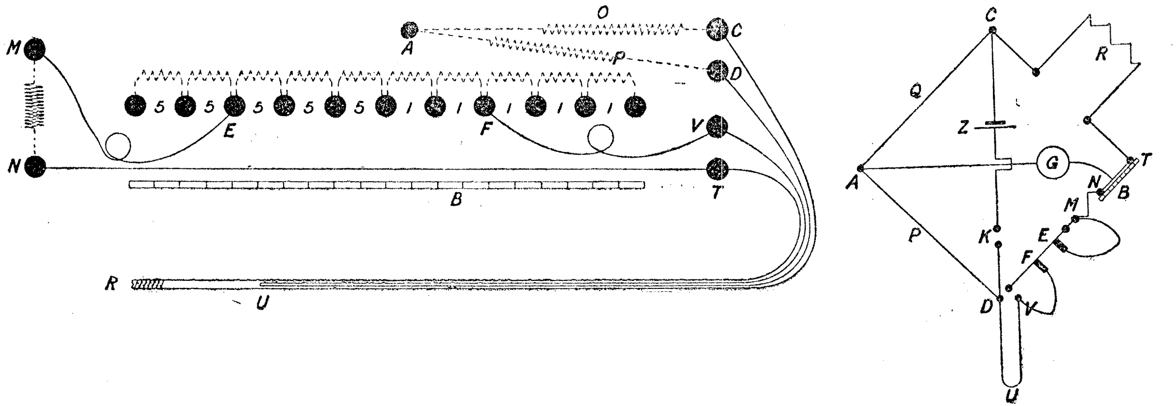
* 'Phil. Mag.,' July, 1891.

Resistance Box.

The resistance standards are made of manganin wire according to St. Lindeck's indications.* After the box had been in use for a year it was recalibrated and found to have changed slightly, sufficiently to affect estimates of temperature supposed accurate to ·002 centim. degrees. The arrangement will be seen from the diagram (fig. 2). We have five coils each of one "box unit," in our case 0·1166725 "legal ohm." We have also five coils five times as great, *i.e.*, equal to five box units of resistance each. The box unit is a resistance equal to the one-hundredth of the change of resistance of the platinum thermometer, when its temperature is changed from 0° C. to 100° C. The manganin bridge wire lies over a glass scale of equal parts which is graduated over a length corresponding to a resistance of one box unit. This

Fig. 2.



Connections of Resistance Box.

A. The point of junction of the two equal arms P and Q.
B. The contact of the galvanometer connection to the bridge wire.
C. The junction of the resistance, Q, to the flexible lead of the thermometer.
D. The junction of the resistance, P, to the flexible "dummy" lead for compensating the resistance of the thermometer leads.
E. The connection of the short flexible lead to one end of the variable resistance.
F. The connection of the other short flexible lead to the other end of the variable resistance.
G. The galvanometer.
K. The battery key.
M. One end of the coil balancing the thermometer resistance at 5° C.
N. The other end of the coil.
P. One of the equal arms.
Q. The other equal arm.
R. The platinum wire resistance, constituting the thermometer.
T. The junction of one of the flexible leads of the thermometer and the divided bridge wire.
U. The end of the loop of the "dummy" lead.
V. Junction of the "dummy" lead and the short flexible lead.
Z. The battery.

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* 'B. A. Reports,' 1892, p. 139.

length is divided into two hundred parts, a movement of the slide over one part thus corresponding to a change of temperature of one-hundredth of a degree. Each division is 1·73 millim. long. In order to avoid the difficulty which arises from the alteration of resistance of plug contacts, we have made a slight innovation. We have placed all the coils in series, and between each of them we have brought a stout clamping terminal to the top of the box. Connection is made to the coils which it is desired to use by means of stout terminals attached to flexible leads. The diagram shows how the resistance of these leads is compensated. The "dummy" leads used to compensate the thermometer leads are made too short by the length of the flexible leads, which are, of course, made of the same wire as the dummy leads. Assuming that the temperatures of the dummy leads and of the flexible leads are the same, this gives a perfect compensation.

The advantage of choosing the particular value of the box unit above mentioned lies in the resulting fact that, setting aside corrections, the box is direct reading; the disadvantage is that with any other thermometers the box would lead to inconvenient arithmetic. The apparatus is only intended to measure temperatures between 5° C. and 35° C., consequently the large coil of the box has the same resistance as the thermometer at 5° C. The contact on the bridge wire is also of manganin, to reduce thermoelectric action, and there is no difficulty in subdividing the bridge divisions to tenths by estimation, *i.e.*, in reading to one-thousandth of a degree. We use one of AYRTON and MATHER'S D'Arsonval galvanometers and find it very sensitive and portable.

The following constants will end our discussion of this matter :—

	Legal ohms.
Resistance of platinum thermometer at 100 C.	45·99173
,, ,, ,, 0° C.	34·32448
Difference	11·66725
Resistance of equal arms	10·027

Testing Resistances.

(1.) The resistance of each of the box units was ascertained in terms of the resistance of one division of the bridge wire, the wire having been previously tested for uniformity in the usual way.

(2.) The same process was gone through with the "fives."

(3.) The five box units in series were tested against each of the "fives" in succession.

(4.) The value of the large coil was ascertained in terms of the platinum thermometer at 0° C. through the intermediary of another box.

We do not desire to dwell on this part of our work, as it is foreign to the main object of this paper, but we must state that the comparisons were made with care and repeated six times. From the comparisons a table was prepared showing the corrections to be applied at each point of the scale. The arrangement of coils adopted allows us to measure each temperature in two ways, and at every five degrees in three ways; this provides a valuable check against large errors or mistakes.

It may possibly be considered that as we have to refer to a table of corrections in any case, we wasted time in adjusting the coils with the nicety we employed. This is not the case, however, for in the process of observing it is necessary to make many approximate estimates of temperature as well as the final maximum, and the uncorrected direct readings are very useful for this purpose. We must acknowledge the assistance we received from a paper by Mr. GRIFFITHS in 'Nature,'* in which the construction of a box of coils and a standard thermometer is described.

We usually observe by closing the galvanometer circuit before we close the battery circuit. A special experiment showed that this was legitimate with our inductionless coils and thermometer, and thus we are not troubled by residual thermoelectric effects; rendered small in any case by the design of the contact maker.

The Arrester.

The arrangement made for arresting the lever is in reality more simple than the description would seem to indicate. We have a framework adjustable to the bars of the main supports by means of clamp screws. This framework carries two jaws worked up and down by means of a pinion with teeth cut by an involute cutter. The rack teeth are therefore triangular, the tops being cut off; it is necessary to have a good smooth motion. The lower jaw of the arrester is convex so that the lever is held against a convex surface. The upper jaw is wedge-shaped, and presses the lever against the convex surface of the lower jaw. This arrangement was adopted with a view to preventing the lever getting bent by the arrester. The active surfaces of the jaws are connected to the racks through the intermediary of spiral springs, and the upper jaw has a stronger spring than the lower jaw. By a simple mechanical arrangement the lever is always held by the same pressural force, and this depends on the elasticity of the springs, and not on the exact angle through which the arrester shaft may have been turned. The arrester frame is adjusted until the lever is practically arrested in its observing position and is not displaced out of the field of view of the microscope. What happens is this. As the jaws close the upper jaw reaches a fixed position. On further turning the arrester handle the spring of the jaw gets compressed but the jaw remains fixed. Then the lower jaw closes on the upper one, and on further turning its spring gets compressed. An essential part of this construction is that the spring of the lower jaw must be weaker than that of the upper one. The arrester shaft of course works through a Hook's joint. Outside the thermometer end of the machine a milled head is placed, and this works a key which

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* November 14, 1895.

passes through a stuffing box and fits on the end of the arrester shaft. The milled head may be clamped by a clamp screw on the end of the balance case, and thus we can make sure that the arrester does not work loose during transport. The thread being quartz in a perfectly dry atmosphere will of course insulate perfectly. We have a check on the possible electrification of the lever because we can set it swinging so as to touch the open jaws, and we can then observe whether any change in the reading occurs. We have often suspected that we were troubled by electrostatic effects but we have no conclusive evidence that this was really the case.

The whole of the inside of the balance is blacked so as to increase the surface conductivity, and so enable the temperature to equalise itself with the greatest possible facility.

In fitting the instrument together the surfaces are coated with soft wax before being screwed up. This makes all the connections air-tight. The last operation after putting the machine together is to exhaust it down to about 24 inches of mercury pressure, and then admit air dried by sulphuric acid and phosphorus pentoxide. The air is also passed over potash and through a filter of cotton wool, the object being to have dry dust-free air in the balance case.

The process of exhaustion and readmission of air is repeated many times; in making a new instrument we would have two openings into the balance and draw dry air through it instead of exhausting and readmitting air. Finally, the balance is left with air at a pressure of about 25 inches of mercury, but, of course, the actual pressure selected depends on the temperature of the balance at the time; a temperature of about 25° C. would be suitable for the pressure named. It is, of course, our object to have such a degree of exhaustion that there will always be an inward pressure wherever the balance is taken, and yet not to have a greater pressure difference than is necessary. A survey in mountainous regions would naturally require a higher degree of exhaustion.

Mounting the Riding Level.

The mounting of a sensitive bubble tube, such as is employed in our riding level, is a matter calling for attention. We have availed ourselves of the experience of our friend, Mr. G. Knibbs, and, acting on his advice, have mounted the glass tube in a stout tube of copper, and packed it in position by means of glass wool. The packing is tight enough to put any displacement of the bubble tube out of the question, and yet the tubes are free to follow their own tendencies in the matter of expansion or contraction. We have proved by experience that a level mounted on the tube of the balance case is not sufficiently to be relied upon, and this at least suggests that some of the irregularities we have observed may be due to warping of the metal work under the influence of changes of temperature. In the event of such warping exceeding the elastic limits of the material, we should have permanent sets which would be most noticeable after extreme temperature changes. When we come to discuss the observations we shall see that there appears to have been some effect of the kind.

Thermal Insulation.

It has already been mentioned that the copper tube, which forms the inner case of the balance, is surrounded by a packing of paper, about half-an-inch thick, over which the outer brass tube fits. It is clear that heat is able to enter or leave through the metallic ends, the sextant part being especially difficult to insulate. We, therefore, put the whole affair into a box of thin copper sheet and packed the interspace with cotton wool. It is, however, obvious that the sextant arm and the microscope must project, and, consequently, it is quite a question whether it is really worth while to insulate the other part so carefully as we have tried to do.

Packing and Transport.

The balance in its copper box is lifted by its movable handles on to a tray provided with mild steel handles, extending upwards above the top of the copper box. The tray carrying the balance is then lifted into a pine box which it just fits. Rigid connection between the machine and the box is secured by means of two hard wood strips which slip and dovetail into notches at the top of the box. When the copper box is in position the wooden strips lie exactly above it, and there are brass screws which pass from the strips into the frame of the balance inside the thin copper box, which is not strong enough in itself to form a proper connection. In this way all relative motion of the instrument and the box is avoided. The riding level is taken off the balance and screwed to the box by special screws. The pine box containing the balance is supported on a set of sofa springs, which are attached to a false bottom, and some side support is given to the box by certain iron rods which are attached to the false bottom and extend upwards, so surrounding the box by a kind of iron framework. Connection between the box and the framework is secured by means of rubber buffers, and in this way the box is prevented from swinging about on its spring bottom.

The balance is handled by means of two handles screwing into the framework through two holes in the copper lid of the outer box.

THEORY OF THE BALANCE.

The tension of the spring which stretches the thread is so large in comparison with the weight of the lever that the thread is very nearly straight; we will suppose that it is exactly so.

Let HI (fig. 1, p. 218) represent the plan of a thread lying in a horizontal plane with a lever having mass, but of linear dimensions, fixed at the middle point of the thread. Let the thread be considered uniform in diameter, and homogeneous. Let the centre of gravity of the lever be at D, and at first consider the lever as hanging vertically, so that D is vertically below the thread. Under these circumstances, let there be no twist in either half of the thread.

Let θ be the angle through which the circle end of the thread, I, is rotated from its initial position, in other words, let θ be the twist in the circle end of the thread when the lever is kept vertical in its initial position.

Let ϕ be the corresponding quantity referring to the spring end of the thread.

Let ψ be the angle which the lever makes at any moment with the vertical plane drawn downwards through the thread.

Let l be the distance of the centre of gravity of the lever from the thread.

Let m be the mass of the lever.

Let g be the value of the earth's acceleration at the point considered.

Let τ be the moment of the forces exerted on the lever by either half of the thread when twisted through unit angle.

The equation of equilibrium, neglecting for the present the effect of variations of temperature, is

$$mgl \sin \psi = \tau (\theta - \psi) + \tau (\phi - \psi) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

If the system is in equilibrium and we increase θ, the lever will take up a new equilibrium position, provided that $\frac{d\psi}{d\theta}$ is finite. If $\frac{d\psi}{d\theta}$ is infinite any increase of θ will make the lever upset. Now, from Equation (1) above

$$\frac{d\psi}{d\theta} = \frac{\tau}{mgl \cos \psi + 2\tau},$$

so $\frac{d\psi}{d\theta}$ will be infinite when

$$mgl \cos \psi + 2\tau = 0,$$

i.e., when

$$\cos \psi = -\frac{2\tau}{mgl}.$$

Also from the original equation

$$mgl = \frac{\tau (\theta - \psi) + \tau (\phi - \psi)}{\sin \psi},$$

so $\frac{d\psi}{d\theta}$ will be infinite when

$$\cot \psi = -\frac{2}{(\theta - \psi) + (\phi - \psi)}.$$

In the case of the instrument as constructed, we know that $(\theta - \psi)$ and $(\phi - \psi)$ are both approximately equal to 6π, so that the upsetting position of the lever is approximately given by $\cot \psi = -\frac{1}{6\pi}$, or the lever is above the horizontal plane through the thread by about three degrees when upsetting takes place. This is in exact accordance with our observations in so far as they are able to test it, and indicates that the simplified theory is fairly applicable to the actual instrument.

The accuracy with which a setting of the microscope may be made upon the mark at the end of the lever depends upon the value of $d\psi/d\theta$, and is, therefore, greater the nearer the position of the lever at the moment of observation is to the upsetting position. When the observing position of the microscope has been chosen, ϕ and ψ become constant. In the present instrument it is found possible to observe so close to the upsetting position of the lever that the microscope can be set much closer than the circle can be read. It is because of this fact that we have adopted the plan of increasing the effective circle sensitiveness by giving the thread three whole turns. The circle has a radius of 7 inches, and with our present experience, we are inclined to think that it would be better for the future to use a larger circle and reduce the twist of the thread.

The sensitiveness of the instrument to gravitational changes is given by the value of $d\theta/dg$, and from (1) this is

$$\frac{d\theta}{dg} = \frac{ml \sin \psi}{\tau}.$$

This is greater the nearer the position of the lever chosen for observation is to the horizontal plane, in so far in accordance with the last result.

Effect of Variation of Temperature.

We find that the relation between θ and the temperature as given by our platinum thermometer scale, for the small changes in the latter factor which we encounter in practice, is a linear one, within the limits of accuracy of our observations. We may therefore include all the effects of a change of temperature in a single coefficient. Equation (1) may be written

$$\theta = \frac{mgl \sin \psi}{\tau} - (\phi - 2\psi).$$

As ϕ and ψ are independent of the temperature, and as θ decreases as the temperature rises, to make this a working formula we must re-write it thus

$$\theta = \frac{mgl \sin \psi}{\tau}(1 - \alpha t) - (\phi - 2\psi) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where α is the temperature coefficient and t is the platinum temperature. In consequence of the fact that the constants in this equation can only be approximately determined, we are limited to the consideration of relative values of g, so we may write the equation

$$\theta = \mathrm{K}g(1 + \alpha t) - \mathrm{C},$$

where K and C are constants.

Let θ_s be the reading corresponding to g_s the value of g at Sydney.
,, θ_m ,, ,, g_m ,, g ,, Melbourne.
,, θ_p ,, ,, g_p ,, g ,, any other place.

We have then

$$\theta_s = \mathrm{K}g_s(1 - \alpha t) - \mathrm{C}$$

with corresponding values for θ_m and θ_p.

For any one value of t we have

$$\frac{\theta_s - \theta_p}{\theta_s - \theta_m} = \frac{g_s - g_p}{g_s - g_m},$$

or

$$g_s - g_p = (g_s - g_m)\frac{\theta_s - \theta_p}{\theta_s - \theta_m}.$$

From the reading at any station at any temperature we may therefore determine the difference of g between that station and a standard station, say Sydney; provided we know the reading at Sydney at that temperature, the difference of reading between Sydney and Melbourne at that temperature and the difference in the values of g at Sydney and Melbourne.

To get an idea of the numbers involved in the use of our present instrument, we may take the Sydney reading at 21° on October 6, 1898, as 83°. We have then

Reading at Sydney 83°, corresponding to $g = 979{\cdot}639$, as given by Mr. Love.
,, Melbourne 82°, ,, $g = 979{\cdot}916$, ,, ,,
,, the Pole 70°·5, ,, $g = 983{\cdot}11$, Everett's 'Units and Physical Constants.'
,, the Equator 88°·6, corresponding to $g = 978{\cdot}1$, Everett's 'Units and Physical Constants.'

The readings are those which would be given by our sextant arc, the temperature being 21° C.

From Equation (2) we get

$$\frac{\delta\theta}{\delta t} = -\frac{ml\alpha\sin\psi}{\tau}g.$$

If δt is taken equal to 1° C. on our thermometric scale, we have by observation

$$\frac{\delta\theta}{\delta t} \text{ at Sydney} = -31{\cdot}50 \text{ sextant minutes.}$$

We can at once deduce that

$$\frac{\delta\theta}{\delta t} \text{ at Melbourne would be } -31{\cdot}51.$$

and

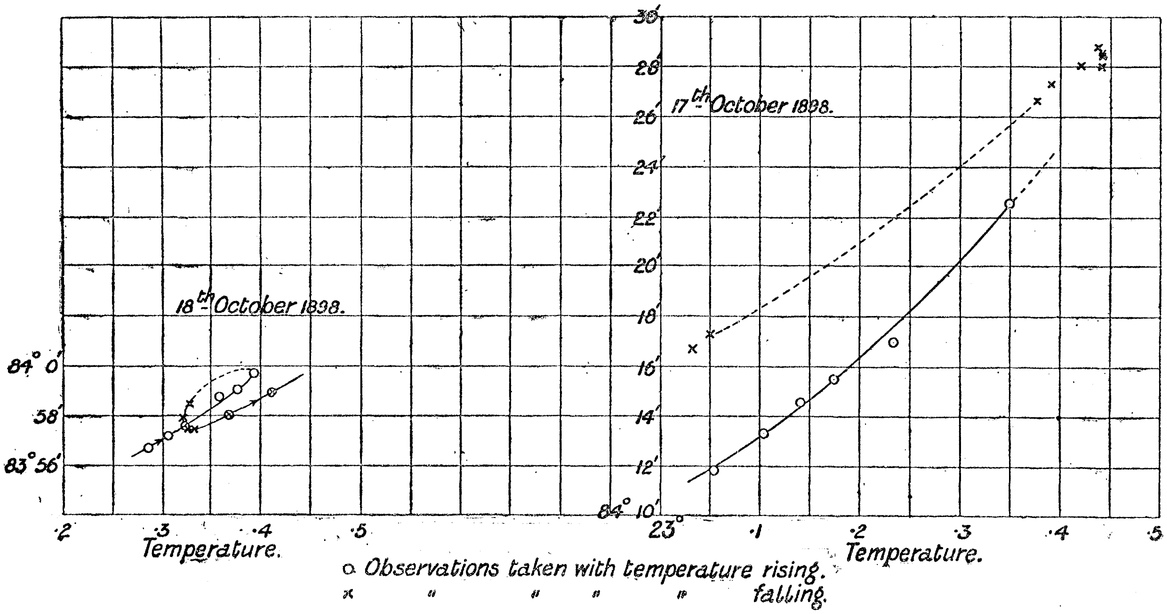
,, ,, the Pole ,, $-31{\cdot}61$.
,, ,, ,, Equator ,, $-31{\cdot}41$.

These values being so nearly the same, it is obvious that an approximate value of g at any station is all that is necessary to enable all observations to be reduced to a common temperature, which, if desired, may then be used to obtain a second approximation; this, however, is not necessary with our present accuracy of observation.

Observations.

Supposing that the line of collimation of the microscope occupies a fixed position, the variables which have to be observed at any one place are the temperature of the interior of the instrument as given by the platinum thermometer, and the amount of twist in the thread necessary at any moment to bring the image of the end of the lever coincident with a cross wire in the eye-piece of the microscope. This latter factor is given by the reading of the position of the vernier arm on the sextant arc. We have found that the relation between these variables depends very greatly not only on whether the temperature is rising or falling, and on the rate of the change, but also, theoretically at all events, on all the previous variations of temperature since the last steady state. In our instrument, the platinum wire, after a change of temperature, assumes its new resistance appropriate to the final temperature more quickly than the lever takes up its new position.

Plot 1.



In Plot 1 are given observations showing the relation between the temperature and the position of the vernier arm when the temperature is first rising and afterwards falling. The change of temperature on October 17th was due to a "southerly burster" coming up after a hot westerly wind. On the 18th the change is the natural change which takes place every evening, the second rise of temperature being due to the room being heated by the lights used for observation. These observations gave us what we call a natural minimum. These examples are sufficient to explain the reason of our ordinary procedure which is described in the next paragraph.

Method of Observing.

In the afternoon, when the temperature of the room just commences to fall, a watch is kept by one observer on the temperature of the box. When the rise of temperature of the box becomes very slow, the other observer prepares to take observations of the lever end. The preparation consists in carefully levelling the instrument along a line parallel to the thread, placing the long striding level on the microscope, and levelling cross ways until the sum of the readings of the two ends of the bubble of this level is within a few tenths of a division of the sum found when the level was last reversed. When the observer watching the temperature considers that the temperature of the box will reach its maximum value within the next few minutes, the lever is unarrested, a note being generally made of the time at which this is done.

A careful watch being kept on the levels, the image of the end of the lever is taken slightly above the cross wire by screwing the tangent screw of the vernier arm, the motion of the screw is then reversed, and the edge of the image brought down to coincidence with the cross wire. (There is a difference in the reading of a coincidence, depending on whether the image of the lever end is brought *up* to the wire or *down* to it, of from 30″ to 40″ (sextant), so that settings are habitually made downwards.) An observation of temperature is taken when the coincidence is exact. Immediately a setting is made, the positions of the ends of the bubble of the microscope level are noted, and the time, temperature and level readings entered. A reading of the position of the vernier arm is then made at leisure. A complete observation takes from three to four minutes. On levelling again, if necessary, it is seen that the image of the end of the lever has moved upwards, so the former procedure is repeated, until the end of the lever remains steadily in coincidence with the cross wire. An independent entry is made in the note-book of the time at which the temperature commences to fall. The image of the lever end is watched, levelling at intervals, until it begins to come down, to ensure that the maximum reading has been obtained. The lever is now arrested, and the vernier put at the constant reading of 85° (as it happens), so that the amount of twist in the thread may be kept constant with the exception of the time, never longer than an hour, occupied in an observation. The aneroid and air thermometer readings are entered, the level reversed and then removed from the microscope, and the observation is complete.

Specimen Observation.

NATURAL Maximum. December 20th, 1898.

Time. P.M.	Temperature of instrument.	Readings of end of level.	Circle reading.	Remarks.
h. m. s.	°C.		° ′ ″	
1 1 0	21·509	...	...	Air temperature 22°·1
2 12 0	21·779	...	...	,, ,, 22°·4
2 42 0	21·890	...	...	,, ,, 22°·5
3 53 20	22·033	6·1 41·1	81 26 50	
3 57 30	22·033	6·2 41·1	81 26 50	
4 2 30	22·033	6·2 41·1	81 27 0	Lever continues to rise, but temperature is steady
4 21 0	22·033	6·2 41·1	81 27 20	
4 24 0	Begins to fall			
4 26 0	22·31	6·2 41·1	...	Lever turned, air temperature 21°·6

Aneroid 28·2. Lever clamped at 85°
Level reversed. B end to eye-piece. Readings of bubble 6·2 41·1
,, ,, A ,, ,, ,, ,, 6·2 41·1
,, ,, B ,, ,, ,, ,, 6·1 41·1

The time between the first indication of a fall of temperature and a fall of the image of the lever end may be anything between 0 and 12 minutes, depending on the rate at which the temperature is changing. The temperature when the image of the lever end first gave signs of coming down, has been 0·02° below its maximum value, though it is generally much less. We assume that the maximum value of the temperature corresponds to the maximum reading of the vernier arm—an assumption which can only be rigidly justified by the accordance of the results which the procedure gives. To distinguish the observation of a maximum temperature and a maximum reading when the temperature alters naturally, from readings taken in other ways, we call the former a natural maximum observation. As the hour at which the temperature rate reverses varies greatly from day to day, the preliminary watching of the temperature is rather tedious.

A minimum reading may be obtained in a similar way by waiting until both the temperature of the air and of the box are falling, the temperature of the air being lower than that of the box. This occurs, of course, every evening after sunset. It is easy now to increase the temperature of the air of the room, by using lamps or small stoves; the lights necessary for observation are generally sufficient. The temperature of the box, and the position of the vernier arm, then go through minimum values which are observed as before. A reading taken under these circumstances we call a natural minimum. The actual observing for a natural minimum does not take half-an-hour.

NATURAL Minimum. Sydney, December 18th, 1898.

Time. P.M.	Temperature of instrument.	Readings of end of level.	Circle reading.	Remarks.
h. m. s.	°C.		° ′ ″	
6 38 0	23·110			
7 56 0	22·950	...	...	Air temperature 22°·3, temperature steady
8 9 0	22·950	...	...	Temperature beginning to turn
8 12 0	22·954	6·5 41·0	81 54 30	
8 16 0	22·960	6·4 41·0	81 54 40	Independent settings (2)
8 21 0	22·969	6·3 40·9	81 54 40	" "
8 24 30	22·975	6·4 41·0	...	Lever has turned

Aneroid 28·00. Air temperature 23°·7. Lever clamped at 85°
Level reversed. B end to eye-piece. Readings of bubble 6·7 41·1
" " A " " " " 6·6 41·1
" " B " " " " 6·3 40·9

By far the greater number of our observations have been made by artificially heating the lower part of the case by placing a batswing gas burner, or a lamp, on the floor directly under the centre of the Kew magnetometer tripod, with a view of getting the temperature of the box between 2° and 3° above that of the air of the room. The source of heat is taken away after about three-quarters of an hour, and a maximum reading is obtained within the next hour. It is surprising that such barbarous treatment as heating the instrument from *below only*, with a naked flame, should have given results worth recording, but from the plots of the observations taken in this way it will be seen that they are most accordant. They will be referred to as artificial maxima. The success of this method of observing depends entirely on the perfect freedom of expansion of every part of the instrument. For reasons, which we give afterwards, we now only observe either natural maxima or minima.

ARTIFICIAL Maximum. Hornsby, December 15th, 1898.

Time. P.M.	Temperature of instrument.	Readings of end of level.	Circle reading.	Remarks.
h. m. s.	°C.		° ′ ″	
9 23 30	26·958			
9 30 0	27·030			
9 32 0	27·030	...	...	Temperature commencing to fall
9 34 0	27·040	6·5 40·9	84 14 0	
9 39 10	27·059	6·6 40·9	84 14 40	
9 46 30	...	...	...	Lever well down

Aneroid 28·20. Air temperature 24°·5. Clamped at 80°
Level reversed. B end to eye-piece. Readings of bubble 6·9 41·0
" " A " " " " 6·6 40·8
" " B " " " " 6·9 41·0

The adequacy of the temperature observation, and of the instrumental adjustments, will now be discussed.

Temperature Corrections.—Any given temperature measurement is clearly affected with the sums of the errors of the coils employed. The accuracy with which the coils are compared with each other and the bridge wire is chiefly a matter of galvanometry. We made our comparisons with the galvanometer used in the temperature observations, and, consequently, cannot hope to attain the highest possible accuracy of which the method is capable. A long series of comparisons shows that the working accuracy, or rather consistency, attained in the temperature measurements is within 0°·01. If we suppose that any observation is in error by this amount, the resulting uncertainty may be stated as 0·3 minute (sextant). This would lead to an error in the estimation of the value of g of one part in 700,000 very nearly.

Accuracy of Setting on the Lever and Reading the Circle.—The position of the vernier arm can be read to 10 seconds of the graduation of the arc, or to 5 seconds of arc. The magnification of the microscope is such that if we move the vernier arm through 10 seconds (sextant), the end of the lever is displaced on the cross wires by a comparatively large amount—an amount two or three times larger than the least we could see. We may say then, that the accuracy of setting is such that we are not affected with any errors on this score comparable with those which occur in reading the vernier. With regard to the latter we will suppose that, taking the sensitiveness as before, an error of 10 seconds in reading the vernier arm would lead to an error in the estimation of g of one part in 1,300,000. This is about half the temperature error.

Errors of Levelling.—The indications of the instrument depend on the assumption that the line of collimation of the microscope can be brought to the same position with respect to the horizontal at each observation. We have already discussed the precautions taken to insure this being the case; but, as there pointed out, there is a certain outstanding theoretical uncertainty on this score, and, moreover, there is no way of applying a check. Setting this aside, we consider that the axis of the microscope tube occupies the same relative position with respect to the horizontal plane whenever the level reverses. The readings of the position of the ends of the bubble can be got with certainty to within 0·2 of a division. It is not always possible to adjust the level during an observation so that the bubble occupies its reversing position. In practice our observations have been made with the maximum error of 0·6 in the position of the bubble. By trial, we have found that if we displace the bubble by 9·0 divisions we alter the circle reading by 150 sextant minutes. It follows that an error of levelling of one division of the level scale would introduce an error of 0·3 sextant minute, or one part in 700,000 in the value of g.

If all these three maximum errors conspire, we shall obtain a value of g in error by one part in about 300,000. It must not be forgotten, however, that with the exception of the temperature error, we have taken as possible errors quantities which could only be realised by bad observing and by omitting to apply a level correction, which, hitherto, we have not found requisite.

Reduction of Observations.

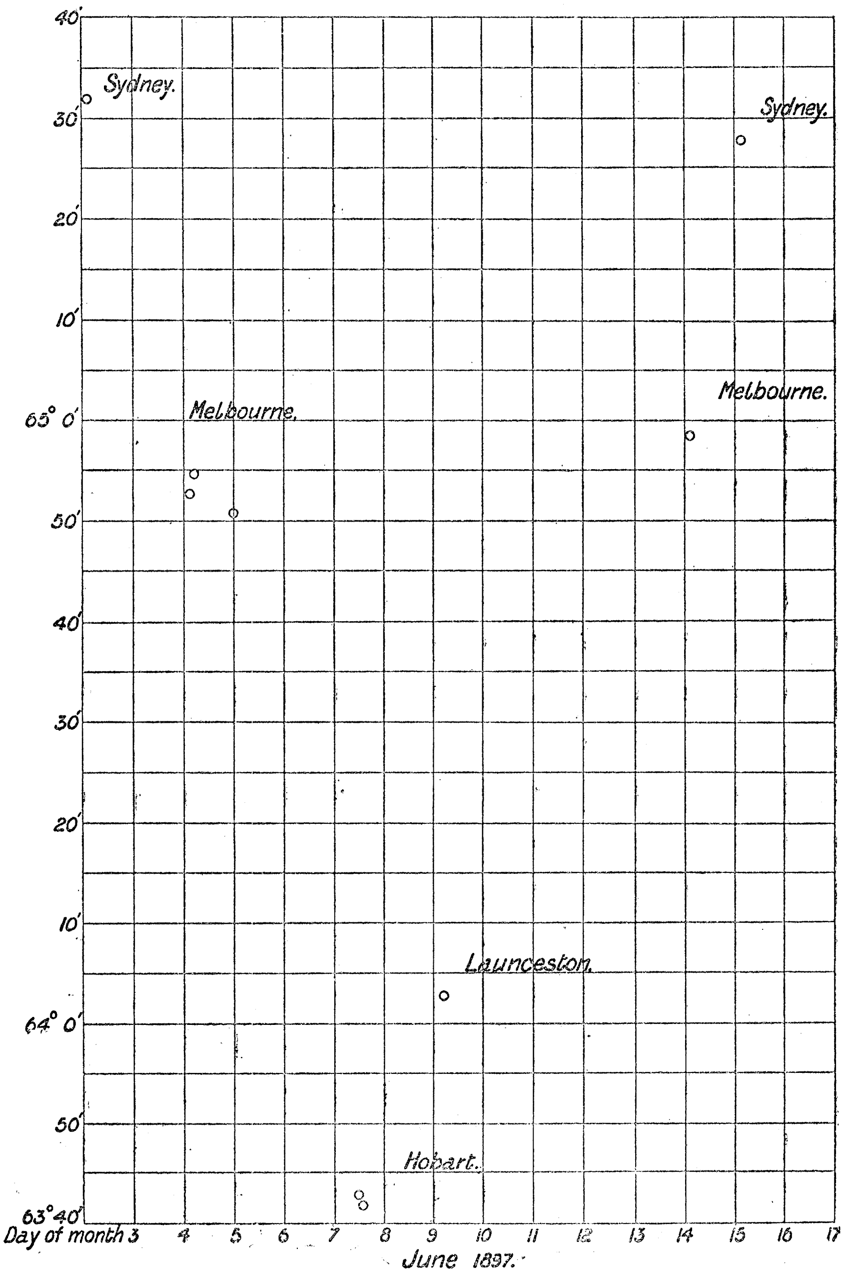
The readings of the instrument at any one place differ from day to day from two causes—from change of temperature and from the slow elastic after-working of the thread and its supports. When a new thread is set up, or any alteration made in the lever or supports, the first thing to be determined is the effect of temperature on the readings. When a considerable number of daily observations have been collected, in the first place, the maximum temperatures of each day's observation, as entered, are corrected according to a scheme drawn up from the comparison of the coils of the resistance box. The maximum readings of the position of the vernier arm are then all reduced to one temperature by an assumed temperature coefficient, on the assumption that the relation between the temperature and readings is a linear one. A plot is then made with time as abscissa and readings as ordinates. If there appear to be systematic errors connected with the temperature, a new coefficient is taken, and the observations again reduced. This procedure is repeated until the systematic errors cease to be apparent—sometimes a lengthy process. If there was no elastic after-working, the plot of the observations from day to day at any one place should lie on a line parallel to the axis of time. If elastic after-working exists the line joining the plots of the observations will be more or less sloped to the same axis.

DISCUSSION OF RESULTS.

There are two conditions which a balance of this kind must fulfil for it to be a working instrument—firstly, it must give accordant readings at any one place from day to day; and secondly, the readings must not be affected by the vibration inseparable from transport. We shall adopt the historical method of treatment, discussing the deviations from the rigorous fulfilment of the two conditions in the order in which they were observed.

The first journey made with the instrument (other than preliminary ones) was commenced in June, 1897. The instrument was taken from Sydney to Melbourne by train, and set up in a cellar of the Physical Laboratory of the University. It was then taken to Hobart by steamer, observed in a cellar of the Museum and in the University Physical Laboratory, then to Launceston by train and observed in the strong room of the Custom House, then to Melbourne by steamer, and to Sydney again by train. On this trip simultaneous observations of temperature and twist were taken over long periods with the temperature rising or falling more or less rapidly, as we had not at this time discovered that it was essential only to observe maximum or minimum readings. From the numerous observations taken we have selected those obtained under conditions most nearly approaching those which we now know to be required. They are given in Plot 2. In the long series of observations taken discontinuities appeared, amounting in one instance to 7 sextant minutes. At this time the two axles to which the thread was attached were connected by a rectangular frame, intended to make both ends of the thread twist by

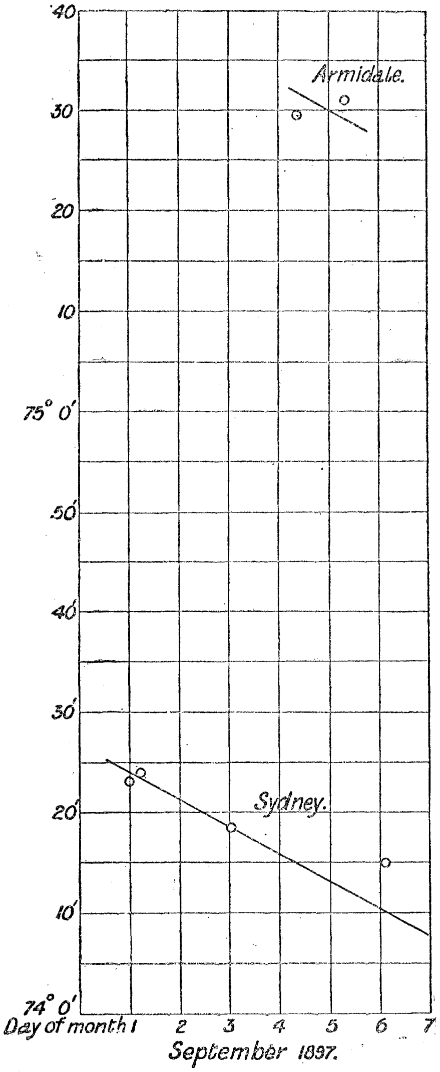
Plot 2.



Readings reduced to 16° C.

the same amount. This frame was put in as the result of an after-thought, after the instrument had been completed, and there was not then room for an adequately designed girder. We attributed the discontinuities observed to the bending of this frame, and it was taken out immediately on our return to Sydney, with the result that there has not been any appreciable discontinuity in a series of readings since that date. The difference between the reading at Sydney on June 2nd, and that at Melbourne on the 4th and 5th, may be taken at 39 (sextant minutes), and that between the observation at Melbourne on June 14th, and that at Sydney on the following day, as 29 (sextant minutes). The mean of these, 34 (sextant minutes), confirms, so far as it can be said to confirm anything, a result which we obtained in the following year, as will be seen later. We had quite concluded before we returned

Plot 3.

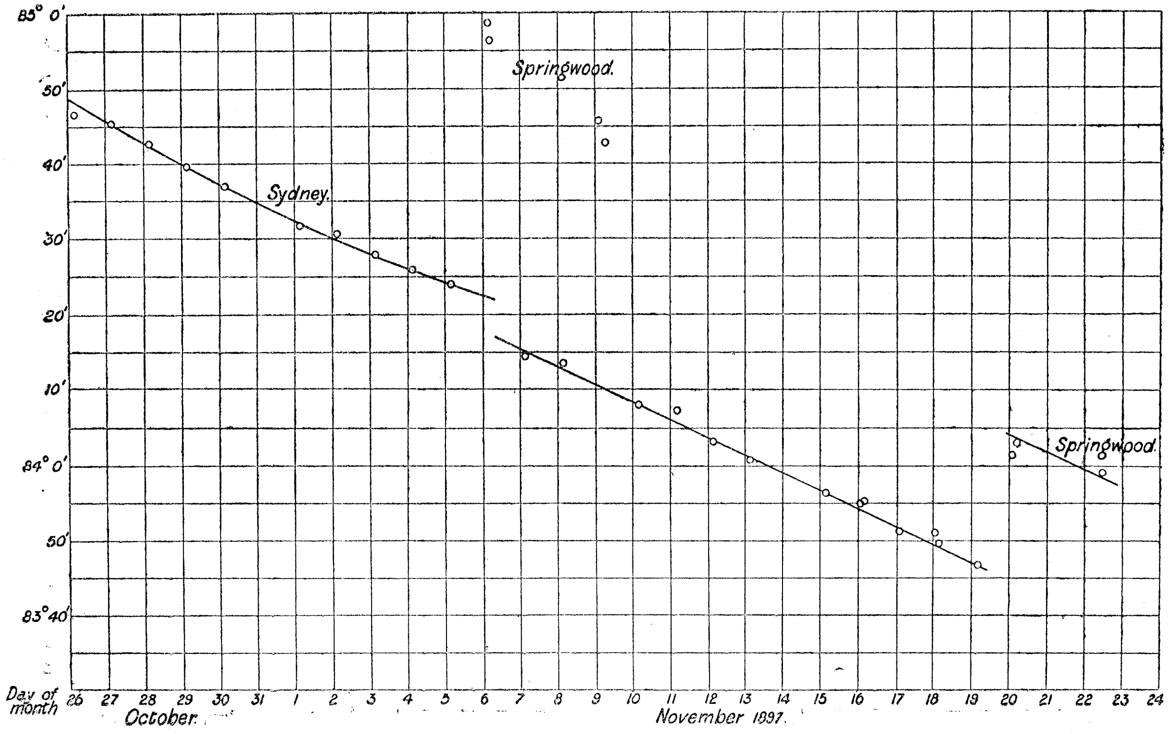


Readings reduced to 14° C.

to Sydney that the observations would not be of high value, but they afforded us some evidence that the instrument could be transported under actual conditions without serious derangement.

The removal of the frame connecting the two axles leaves one-half of the thread with a constant twist, while the other half has during each observation a slightly variable amount of twist depending on the temperature and the intensity of the gravitational force. Any change of gravitational force has now to be compensated by a change in the twist of one-half of the thread, consequently the vernier arm has now to be moved through twice the angle necessary with the former arrangement; the reading-sensitiveness of the instrument was, in fact, doubled by this change.

Plot 4.



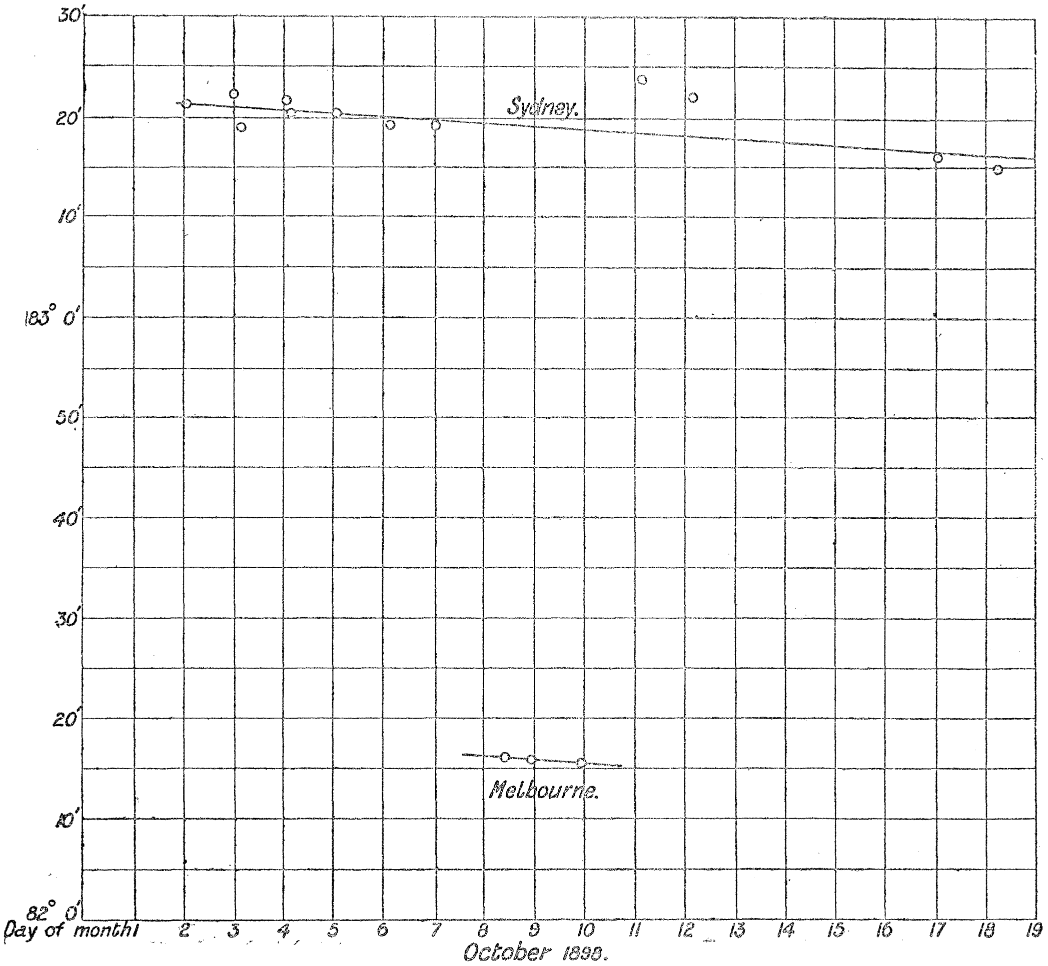
Readings reduced to 25° C.

A gravitational survey, connecting the towns of the eastern Queensland seaboard with Sydney, was now projected, the first connecting station chosen being Armidale, a town on the table-land in the northern part of New South Wales. The observations given in Plot 3 show an immense improvement in the behaviour of the balance, though they give evidence of a break of 5 sextant minutes in the reading on returning to Sydney—due to the shaking which the instrument had experienced in transport. We had, by this time, found out the necessity of observing only when the temperature was at a maximum or a minimum.

On the return from Armidale it was found that some air had leaked into the instrument. In trying to discover the leak, through carelessness, the delicate soldering of the lever to the thread was severely strained. The accident had the effect of permanently increasing the reading by 10°.

On November 6th the instrument was taken by train to Springwood, a station 48 miles west of Sydney, 1216 feet above sea level. It was here observed in the cellar of the Oriental Hotel, and was brought back to Sydney the same night. Plot 4 shows that a break of 5·0 sextant minutes had occurred in the Sydney reading, and that the daily rate due to elastic after-working (about 2·5 sextant minutes per day) had slightly increased. The balance was again taken to Springwood. On its return the next day there was no definite evidence of a further break. But a third journey on November 20th, returning to Sydney on the 23rd, strengthened the evidence as to a permanent change taking place due to the travelling. When putting the instrument into the train at Springwood on the 23rd, one of the handles of the box broke, and one end of the box fell about two feet on the asphalte. The thread was not broken, but the observation of the 23rd at Sydney differed by 1° (sextant) from

Plot 5.



Readings reduced to 21° C.

its reading on the 19th. It was considered that the breaks in the readings when the instrument was "travelled" were due to a looseness in the joint fixing the lever to the thread, which might have been caused by the accident after coming from Armidale, and observations were discontinued until the joint could be re-made.

In February, 1898, the bars carrying the thread were taken out of the outer case and the lever was re-soldered to the thread. This necessitated the little lump of fusible metal at the end of the lever being altered in order to keep the twist in the thread the same as before. In putting the instrument together again the further end of the internal framework was wedged to the copper tube. Previously permanency of relative position between the microscope and the thread depended on the attachment of the inner framework to the copper tube by the screws at the circle end only.

Other work did not permit observations to be commenced until the end of September. The temperature coefficient was now 26·7 sextant minutes per degree centigrade. Plot 5 shows that the daily rate due to elastic after-working had decreased from 2·5 minutes in September, 1897, to 0·3 minute in September, 1898. (The present thread was mounted in September, 1896.) Satisfactory observations were made in Melbourne on October 8th, 9th, and 10th. The observations on our return to Sydney show a difference of 5·5 minutes compared to the ones taken before going to Melbourne, but the change, instead of being permanent as it was in 1897, completely came out by the 17th.

The journey to Melbourne was undertaken with a view to finding the sensitiveness of the instrument. The rate, as given by the observations at Melbourne, which were completely satisfactory, is the same as that at Sydney. We may suppose it likely then that the change in the Sydney reading is due to something which occurred on the return journey. The difference between the readings at Sydney and at Melbourne, taken from the plot, is 63 sextant minutes.

If three whole turns in the thread, or 2160 (sextant degrees), are required to keep the lever horizontal against a g of 980, assuming that the relation between the necessary twist and g is a linear one, in spite of the accumulated after-working, 72 sextant minutes would be required to compensate for a change of g of 0·277, which is the difference between g at Melbourne and Sydney, as given by Mr. LOVE.*

We have then for the difference of reading between Sydney and Melbourne for the instrument, as at present constructed, calculated from the number of twists in the thread, known only approximately, 72 sextant minutes.

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* Mr. E. F. J. LOVE, who has given attention to the determination of gravity at Sydney and Melbourne, both experimentally and by discussing the results by other observers, was good enough to give us the following values of g as the most probable :—

Values of gravity at Melbourne Observatory	979·916
Values of gravity at Sydney Observatory	979·639
Difference	0·277

From the unsatisfactory comparison in June, 1897, remembering that the sensitiveness is doubled since that date, 68 sextant minutes.

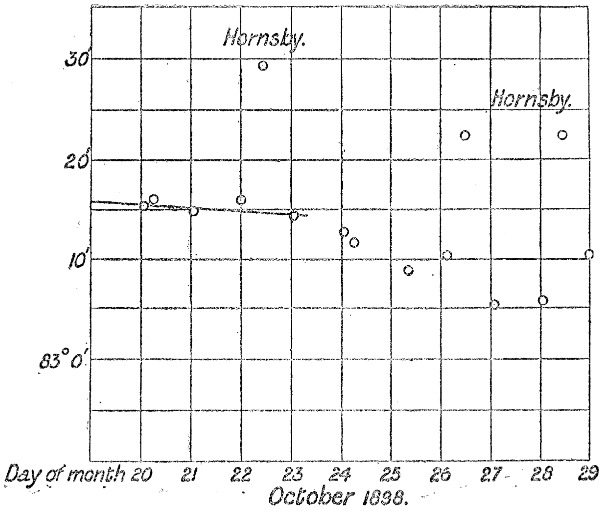
From the comparison in October, 1898, 63 sextant minutes.

Until the difference has been more accurately determined, we may take say 60 sextant minutes as the difference of readings at Sydney and at Melbourne.

Mr. Love considers that the value of g at Melbourne is 979·916, and that the Sydney-Melbourne difference is 0·277. If the change in the reading of our instrument between Sydney and Melbourne is 60 sextant minutes, then a change in the value of g of 1 part in 100,000 would be represented by a change of reading of 2·12 sextant minutes.

There is now ample evidence before the reader in the plots of the observations taken in November, 1897, and from the 2nd to the 18th October, 1898, to show that, apart from discontinuities due to travelling, it is extremely unlikely that any single observation would differ from the mean reading at any one station by 1 sextant minute.

Plot 6.



Readings reduced to 21° C.

There is only one observation, that on the afternoon of October 3rd, 1898, which differs from the mean Sydney reading by 2 sextant minutes. If, therefore, the instrument can be carried from place to place without altering its behaviour, we may say that the value of g at any station may be determined relatively to that at some standard station by a single observation, with extreme probability to 1 part in 200,000, and with certainty to 1 part in 100,000.

We had, therefore, at this time to determine the cause of the effect produced by travelling the instrument. We determined to take the balance to Hornsby Junction, a station 21 miles north of Sydney, 592 feet above sea level.

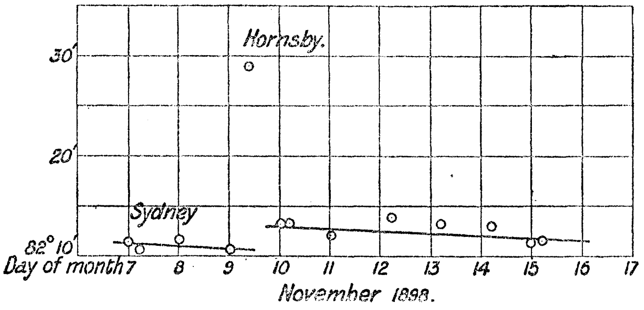
The observations at Hornsby were made at night in the lamp room of the station, and the balance was brought back to the laboratory in the morning. The result of the observations is shown in Plot 6. Without correcting for daily rate, the Sydney-Hornsby differences of reading, as determined from these observations, are 13·5, 15·0, 12·0, 16·9, 16·4, 12·0 sextant minutes, and confirm the existence of a serious change produced by travelling. We now know that the difference is 18·2, so that from one of these observations the difference might be in error by 6·2 sextant minutes.

The only points of the instrument which we could think of as being affected by the travelling, were: the end of the rosette spring to which one end of the thread is attached, and the lever itself, the after-end only of which is clamped, leaving the forward-end not incapable of vibration. Although the end of the rosette spring was stayed by wires to four upright pieces of watch spring, it was still capable of some vibration in a direction at right angles to the length of the thread.

On October 29th, 1898, the inner framework was taken out. The arrester springs were strengthened, so that the lever was held more firmly than before. The junctions of the little wire stays of the rosette spring were paraffined at both ends to prevent the slightest play in the links, and three fine glass hairs were attached with paraffin, one end in the centre of the rosette spring and the other on one of the bars of the framework, the three glass hairs lying in a plane at right angles to the line of the thread. The point of the rosette spring may now be considered to have no freedom of movement in a direction at right angles to the length of the thread.

The instrument was re-mounted, and observations commenced on November 7th. It was found that the temperature coefficient had increased, an increase being what one would expect if the resilience of the spring system had increased, as it must have done owing to the additional constraint imposed by the three glass hairs.

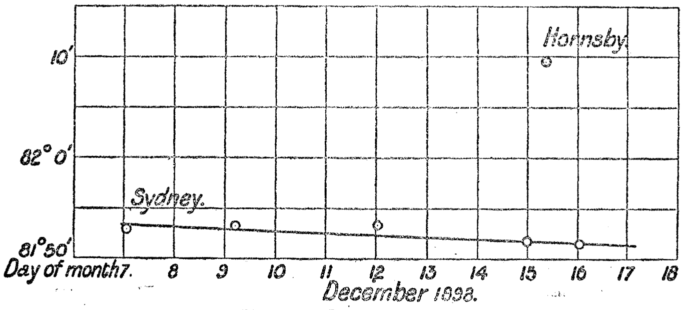
Plot 7.



Readings reduced to 23° C.

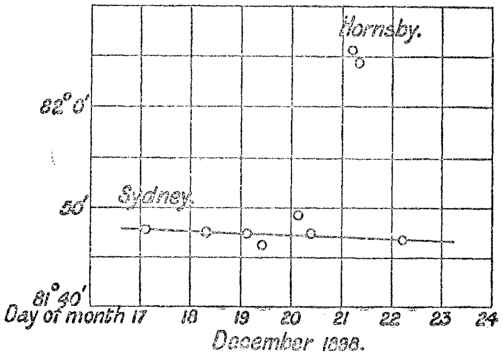
It was very soon seen that the changes due to travelling had been greatly diminished, the change in the Sydney reading, after an observation at Hornsby on November 9th, being only 2·6 sextant minutes. It was found, however, that by daily observation at Sydney, from November 10th to December 7th (those up to November 15th being shown in Plot 7), that discrepancies amounting to 4 sextant minutes on each side of the mean line now appeared in a most erratic way. This we traced to the method of observing by heating the instrument from below with a naked flame, as before described. When we observed only natural maxima and minima, the readings at Sydney regained their old regularity, with the exception of one observation on the afternoon of December 20th, which is 2 sextant minutes from the mean line. In our present instrument there is a fault in design, inasmuch as the microscope is fixed to a copper tube, while the thread is carried by a gun-metal framework which we now try to fix firmly at both ends to the copper tube. It is only to be expected that, owing to the different expansibilities of the two metals, the instrument may be twisted in a most erratic way during changes of temperature, and this effect will be exaggerated if the changes of temperature are rapid. Moreover, by the addition of glass tie bars the character of the spring system has been entirely changed.

Plot 8.



Readings reduced to 23° C.

Plot 9.



Readings reduced to 23° C.

Two more journeys were made to Hornsby, and the observations are shown in Plots 8 and 9. It is seen from these plots that the readings are not now affected by travelling, for, without correcting for daily rate, we have the following differences between an observation at Hornsby and the observation before and after at Sydney :—

November 9th	18·3 sextant minutes.	
„ 10th	15·9 „ „	
December 15th	17·9 „ „	
„ 16th	18·3 „ „	
„ 20th	17·7 „ „	Taking the mean Hornsby reading.
„ 22nd	18·3 „ „	

We may reject the observation of November 10th. Discrepancies twice as great as this from the mean were discovered afterwards to be due to observing immediately after too rapid temperature changes.

When we correct for daily rate and take the mean reading at Sydney, the Sydney-Hornsby difference comes out—

November 9th	18·5 sextant minutes.
December 15th	18·1 „ „
„ 21st	18·1 „ „

We have, therefore, determined the value of g at Hornsby relatively to that at Sydney in three journeys, with a maximum difference of 0·4 sextant minute, or to less than 1 part in 500,000 in the value of g.

This sensitiveness refers, of course, only to the Sydney-Hornsby difference. For the purposes of a survey the mean reading at the standard station may be determined with extreme accuracy—with an error at any rate negligible with respect to the probable error of a single observation at any one station. Travelling does not now affect the instrument, so that the accuracy of a determination of g, from a single observation at any station, depends on the possible deviation of a single observation from the mean.

APPENDIX B.

NOTES ON EXPERIMENTS MADE WITH VARIOUS FORMS OF GRAVITY BALANCES.

In September, 1888, we made calculations as to the sensitiveness of a balance with a horizontal thread twisted several times. We then constructed several trial instruments, mounting our threads and appliances on an old watchmaker's lathe-bed. One end of the thread was fastened to a rod working in a barrel against a spiral spring, and the other end was attached to the centre of a bar provided with a large roughly-divided circle.

A large number of trials were made as to the right thickness of the thread, &c., and much trouble was experienced in preventing the thread from breaking after having been exposed to twist and tension during several days. The thread and levers were massive compared with those we now employ. One of the earliest methods of cementing employed by us was by means of clean fusible metal. Attempts were also made to grind the thread slightly flat so as to give a sort of key to the cement.

We then began to attempt to compensate the natural increase in stiffness which occurs as the temperature of a quartz thread rises. It was clear that compensation to the requisite extent could only be attained by the use of some form of compound lever. Professor GURNEY suggested to us to try a lever consisting of three bars. Two of these were to be horizontal and situated at a short distance one above the other in the same vertical plane, they were to be of metals of as dissimilar expansibilities as possible, the less expansible one being uppermost. One end of each lever was to be attached rigidly to a cross piece to which the thread could be fastened. At the free ends of the levers the following disposition was to be made: a bar was to be pivoted from the end of the lever of the less expansible metal, and was to bear against the wedge-shaped end of the lever made from the more expansible metal. When the system of two bars was horizontal, the pivoted bar was vertical. As the temperature rose the more expansible bar would deflect the pivoted bar, and so increase the moment round the thread. Many levers were made on this principle, for which purpose we instructed ourselves in the art of the watchmaker, and finally managed to make very small levers, using aluminium and platinum-iridium as the two dissimilar metals. We finally made a compound lever weighing less than a decigram, and having all its dimensions correct to produce the compensation required. After some experimenting with this lever, it was abandoned on account of the inevitable want of permanency of form to which its hinged joint gave rise.

During the experiments with compound levers an iron box was made which would contain the whole appliance, and which was air-tight and could be exhausted. Most of our earlier trials were made in this box; it lent itself especially to the investigation of the flotation of the levers by air of different densities. We had hoped to be able to deduce the sensitiveness from the flotation effects, but the cement of uncertain density occupied so large a relative volume in comparison with the lever that these attempts failed.

By May, 1891, we had sufficient experience to hope to detect the lunar disturbance of gravity. At this time we used a lever of aluminium shaped like a cross, the thread being cemented across the shorter arm by shellac. A mirror was mounted on the lever so as to be vertical when the lever was horizontal; the mirror was close to the thread, being carried by the head of the cross. The balance, in the air-tight box, was placed in a cellar and supported on a very heavy stand, made by filling a large iron cylinder (a mine case for 500 lbs. of guncotton) with sand and stones. The temperature was taken by means of a very fine mercury in glass thermometer, the bulb of which was placed inside the iron box. The temperature of the cellar was very uniform, but was disturbed considerably by the presence of the observer. The sensitiveness of the instrument was ascertained by weighting the lever with shreds of fine wire; the calculation involved a knowledge of the distance of the shred of wire from the thread, a quantity which could not be got with any great accuracy.

The highest sensitiveness attained was that we got an observable deflection corresponding to an increase of gravity by 7×10^{-6} of itself. This involved reading to one-tenth of a millimetre on a scale distant 3 metres from the mirror $\equiv$ 3 seconds of arc. The observations, of course, included a study of the daily rate of the system, as well as of its temperature coefficient. We varied the form of the lever, the dimensions and twist of the thread, &c. Exceedingly good mirrors were made and mounted during this work, and we found that the best cementing material for thin mirrors is a mixture of equal parts of white and red lead made into a paste with a little boiled linseed oil. The mirrors were fastened on with a trace of this cement; they were left for about a day, and then stoved at 100° C. till hard. Every kind of cement deforms thin mirrors, but this paint deforms them less than anything else we tried. A variation was attempted on the following lines. Two threads were mounted, one many times the diameter of the other, the thicker one supports the working lever, the other acting as a torsionless axle. From the end of the working lever, distant, perhaps, 2 centims. from the thread, the finest possible thread also of quartz was stretched to join the second lever mounted on the thin thread, and was cemented to it quite close up to the thread. The second lever carried the mirror, and any motion of the first lever was magnified in the ratio of the distances of attachment of the connecting thread from the two main threads. We only got one-tenth the sensitiveness of our best single system by this arrangement, which was besides very cumbersome and difficult to set up. The finest quartz thread is too stiff to act properly as a flexible connection and acts more like a rigid bar.

During the latter part of 1891 we made an adjustable system of MICHELSON'S arrangement of interference mirrors, and tried to increase the sensitiveness of our angular measurements by observing the motion of the interference fringes. The general result arrived at after much patient work was that the method presented no advantages in practice; and this even when we replaced our single mirror by two small mirrors separated as far as the mechanical conditions permitted.

By March, 1892, we became convinced that it was hopeless to attempt to disentangle the lunar effect from the instrumental irregularities, even if we could bring the sensitiveness up to the necessary point; of which there seemed to be no hope. The research was therefore abandoned.

Investigations with the view of constructing our present form of portable instrument were begun in May, 1892. For this purpose we mounted a thread on the spiral head of our milling machine, and supported the fixed end of the thread on the back centre. The twisting of the thread was observed by a mirror and scale, the mirror being connected to the rod carrying the end of the thread to be twisted; and a lever and mirror were mounted at the centre of the thread as before. The machine was provided with a special slow motion of rotation for the spiral head. The position of the lever was observed by a telescope, using the method of reflected images. These trials led to the present form of balance, the construction of which was commenced in August, 1892. The instrument was not ready till July, 1893, and the first thread was not mounted till September, 1893. This was a catapult thread, and was twisted with $2\frac{3}{4}$ turns. The aluminium lever carried a mirror, and this was observed by reflexion of cross wires as before. At first the thread was intended to be twisted from one end only to compensate changes of gravity—exactly as in our present form of instrument—but as irregularities occurred we were led to put in an extra set of bars so as to twist both ends of the thread at once. We had also an idea that the theory would be sufficiently simplified to enable us to avoid the necessity for any calibration, the total twist being capable of exact estimation in consequence of the presence of a theodolite circle, which at that time occupied the place since taken by the sextant arc. We also hoped that an exactly symmetrical distribution of twist in the thread would reduce the viscous yielding, and so more than compensate for the loss of sensitiveness due to twisting two ends of the thread instead of one.

The lever was adjusted by drops of paraffin, which are easier to regulate than drops of solder. The whole structure of the instrument was mounted on a turntable so as to allow us to take observations in any azimuth, for the purpose of eliminating magnetic effects if such should appear. In October, 1893, this machine was taken to Armidale, with the result mentioned at the commencement of the paper. The temperature was taken at this time by a mercury-in-glass thermometer.

During the time that the instrument was undergoing repair and alteration, in accordance with the experience obtained, we constructed a fresh instrument in which the whole of the working parts were immersed in mineral sperm oil. The machine was only intended as a trial instrument, and was put together out of brass tube, &c., but it was well made. We observed it in the cellar with the appliances formerly used in the attempt to discover the lunar disturbance of gravity. It was found that the readings went through a regular daily cycle which was ultimately traced down to the action of minute convection currents. The regularity of these minute currents was one of the most surprising things we have ever met with. In order to get an idea as to whether it would be possible to observe at sea, we mounted the machine on a swing, and satisfied ourselves that no amount of damping would enable accurate observations to be made under such circumstances. A subsidiary set of experiments were made in connection with this matter in order to find the resisting properties of different cements when immersed in oils, *i.e.*, to find out whether they were gradually softened by the oils. These experiments lasted for two years, and showed us that the resistance of shellac is surprisingly great. Mineral oils did not appear to have any influence at all, and turpenes only a very slight effect, the other cements tried were not nearly so resisting.

Experiments of one sort or another with the oil balance were continued till January, 1894.

Meanwhile the portable instrument had been restored, and in November, 1893, the catapult thread was replaced by a shot thread, this was under observation during the beginning of 1894. We now had a balance much worse than the one that had been broken: the thread appearing to show a viscous yielding in the wrong direction. We attributed this to the shellac, so in January, 1894, an experimental thread was mounted on yet another balance. This thread was drawn down so as to be thick in the middle and at both ends, with the view of reducing the stress intensities in the shellac. The results of this experiment were such as to lead us to think the abnormal behaviour of the portable balance was to be traced to the thread or lever, and not to the shellac. However, by February the balance seemed to have settled down, and it was again taken to Armidale. On this occasion, however, the results were quite disappointing, and the cross wire images were so difficult to observe that it was clear that a new mirror or method of observing was necessary. The viscous subsidence from whatever cause arising was also unsatisfactory, so that we decided to mount a fresh thread, choosing now a much finer one. For two months of incessant work we struggled with fine threads, finally mounting what we thought a very good one in April, and this was then observed till July, when we have the following note: "This thread, the very finest we have had with a very small lever, has been most unreliable. It seems from its behaviour (especially when the lever becomes unstable) that the centre of gravity of the lever has been moving relatively to the thread, perhaps the fine thread has been moving through the shellac."* A defect in the arrester rendered this possible.

In consequence of this observation a separate experiment on another balance was made from May till September, using a soldered thread. We were led by this experiment to solder up a fine thread to the portable balance and to mount on it a soft copper lever. On August 21st, after continued observation, we were forced to the following conclusion:—"From the general appearance of the observations it is clear that the readings are becoming less with lapse of time—or the thread requires more twist to bring the lever to its sighted position—the effect to be anticipated . . . The readings are, however, irregular . . . The thread is not to be compared with others we have had whose variations were within a degree, whereas this thread has altered its reading by ten degrees. The thread is so fine that the centre of gravity of the lever must be extremely close to it, so that the very smallest change in the lever or attached mirror will greatly affect the reading. The very fine threads have not been a success." As a result of this experience we abandoned the lever and mirror in favour of a microscope, and also brought the arrester to its present form. During September and October, 1894, several threads were mounted, and these all broke before we could get a series of observations. These persistent failures led us to examine the threads more closely than we had done hitherto, both as to uniformity and freedom from air bubbles. Threads of the following diameters all broke: ·00126, ·0015, and

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* We are now inclined to think that a good deal of the difficulty which we attributed to the imperfect elasticity of the threads was in reality due to bad means of attachment, imperfect spring, and bad temperature estimations.

·00154 inch. Also the twist was far less than our previous experiments had indicated as allowable. Thus, we thought that we ought to have been allowed to put in a twist of one turn per centimetre, and we only put in a twist of ·204 turn per centimetre. This showed that either we had got hold of a bad sample of quartz, in spite of all efforts to the contrary, or that the conditions of continuous stress obtaining in a balance affect the elastic properties in some hitherto unrecognised way.

Now the thin thread which did so badly from June till August, 1894, had a diameter of ·0004 inch or ·00102 centim. The length of all the threads was 12 inches. We concluded that the diameter of the thread should lie between ·0035 and ·001 centim., using levers of the mass hitherto employed, so as to avoid breakage on the one hand or irregularity on the other. The cause of breakage was also seen to be related to the sag produced by the use of too heavy a lever. It was at this time that we made the comparative examination of different samples of quartz referred to in the text. We did not terminate these investigations till the end of 1894, and it was not till March, 1895, that we succeeded in obtaining a thread to satisfy us. The diameter of this thread lay between ·0014 and ·0015 inch, and was made of our most infusible quartz and mounted with a very light straight wire lever. We observed this thread till September, 1895, finding, amongst other things, that the flotation correction was very small, as had been anticipated. The viscous yielding was large, and we suspected that the silvering had not been removed sufficiently close to the coppering (from fear of the nitric acid getting in and gradually loosening the thread); this was tested by a reapplication of nitric acid, and the reading promptly changed by 40° of twist.

Finding the great weight of the instrument a drawback, we re-made the base and replaced the theodolite circle at the twisting end by a sextant arc. By November we had traced such irregularity as still persisted, to the sticking of a barrel spring, which caused the tension of the thread to vary irregularly whenever the temperature changed. We therefore designed and made the "rosette" spring referred to in the text, but were so unfortunate as to break the thread in dismounting it; we spent no less than two months in getting a fresh thread to satisfy the conditions. On January 22nd, 1896, this thread was mounted, with about three whole turns at each end. It was observed continually until July 9th, when it was pleased to break. The daily rate of subsidence of this thread was 6 sextant minutes in March, and fell to 2·5 in the latter part of June. There appeared to be some effect depending on the time which elapsed between the releasing of the lever and the time at which the reading was taken. We did not find the reason of this at the time, but we now know that it was due to the heating up of the balance by the observer. However, we wasted some time over it.

A new thread was got in September, 1896, after some weeks' shooting. This thread is still in use and is the best we ever got. We had a great deal of trouble in stopping leaks in the apparatus, which was beginning to show signs of wear in the bearings. In April, 1897, we detected a mechanical irregularity due to this wear, the axle of the sextant arm was working with too much friction ; this was remedied. During May the instrument was considered to be fit to travel, and we took it to Bowenfels, a place in the Blue Mountains, some 2000 feet above Sydney, and here we made some promising observations. We have to thank our friend, Mr. FLINT, for allowing us to use his house as an observing station, and for helping us in every way. When we came to discuss these observations, we found that it was now necessary to improve the thermometry, and we accordingly made and tested the platinum thermometer. By dint of some very continuous work we were able to get away to Melbourne in June. An account of our work from this date is contained in the paper.

DESCRIPTION OF PLATES.

PLATE 1.

N.B.—The drawing does not show the long inner tube carrying the eye-piece inside the outer microscope tube, otherwise it is a working drawing.

INDEX TO LETTERING ON THE DRAWING. HORIZONTAL SECTION.

A. Axle carrying rosette spring. B. Axle carrying vernier arm. C. Rosette spring. D. Wire lever.

EE. Glass tube carrying platinum wire thermometer wound in a double spiral.

FF. Main bearings. GGG. One of the three bars holding the bearings together.

H. Pinion of arrester working into two racks carrying the arrester jaws. I. Jaws of the arrester.

JJJJ. Copper tube bored to fit the main bearings and rigid with respect to the inner mechanism.

KKKK. Paper insulation round the copper tube. LLLL. Outer tube of polished brass.

MMM. Levelling screws forming the supports of the balance on the tripod stand.

NNNN. Ground and lapped tube carrying the microscope.

OOO. Points at which connection is made to the thread by soldering.

PPPP. Mercury stuffing boxes on the vernier and arrester shafts.

QQQQ. (Also in the plan of the graduated arc.) Screws attaching the inner mechanism to the copper tube.

RR. (Also in the plan of the graduated arc.) Part of the framework of the webbing of the sextant arc.

SS. Cap of mercury stuffing box on vernier axle.

T. (Also in plan.) One of the screws fastening the vernier arm to a strengthening piece screwing to the vernier axle.

U. (Also on the plan of the graduated arc.) Clamping washer and lock nut with left-handed thread to insure rigid connection between the vernier arm and the axle.

V. Clamp screw of arrester shaft.

W. Slow-motion screw used in the preliminary adjustment of the tension of the thread, afterwards clamped by the clamp screw and soldered up to an invariable position with respect to the frame.

Y. Box key on the arrester shaft. This key can be drawn out by unscrewing the stuffing box from the end of the balance case. The drawing shows the box key too large to pass through the hole, this is wrong.

ZZZZ. Screw clamps or clips holding the balance proper to the under frame.

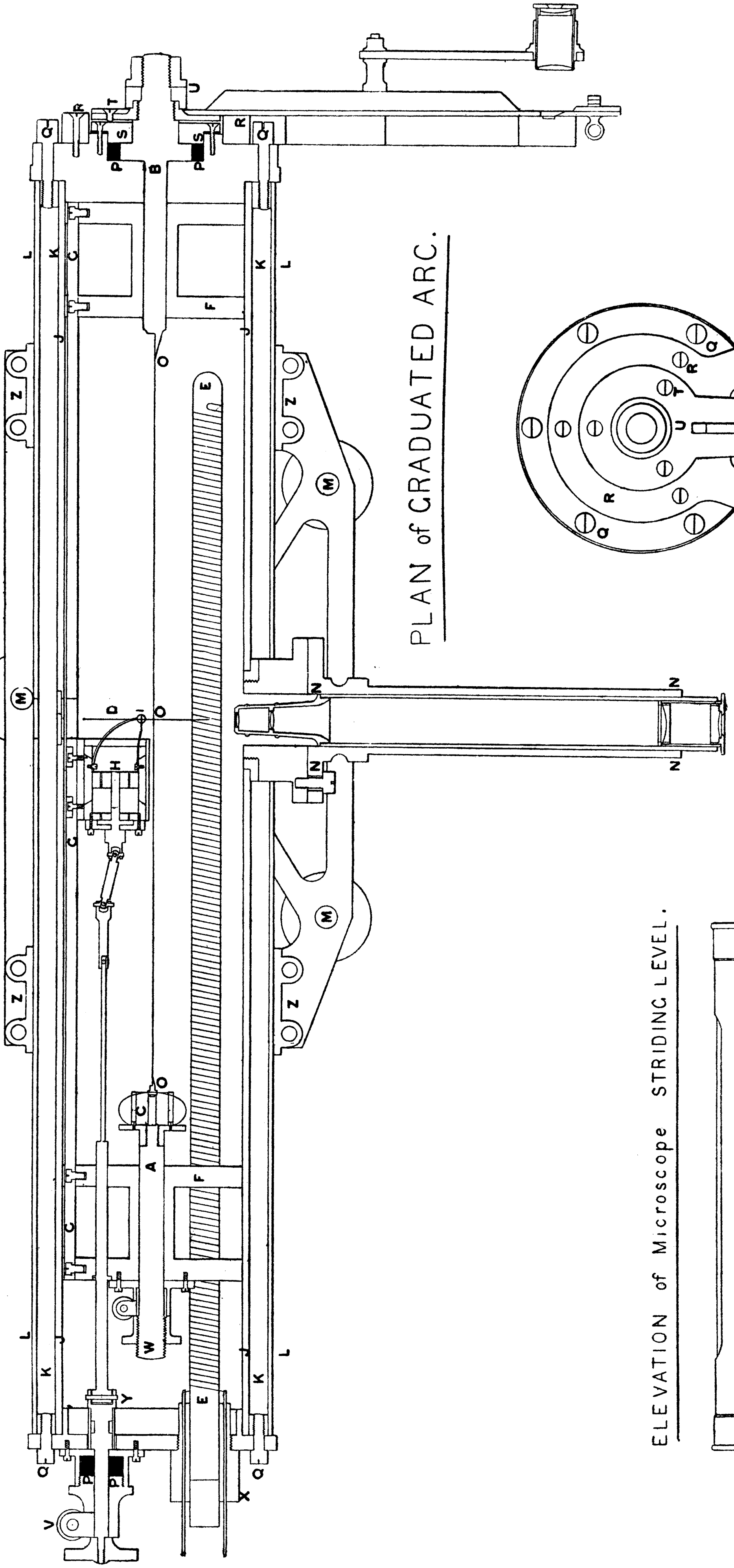
INDEX TO LETTERING OF THE ELEVATION OF THE MICROSCOPE AND STRIDING LEVEL.

L. Level. KK. Adjustments for the level tube. C. Cross level.

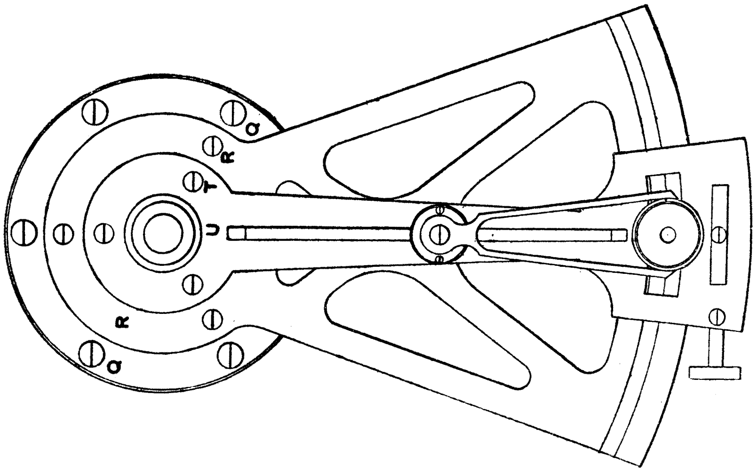
VV. The sides of the V grooves of the level carriage. MM. Worked tube carrying he microscope.

PLATE 2.

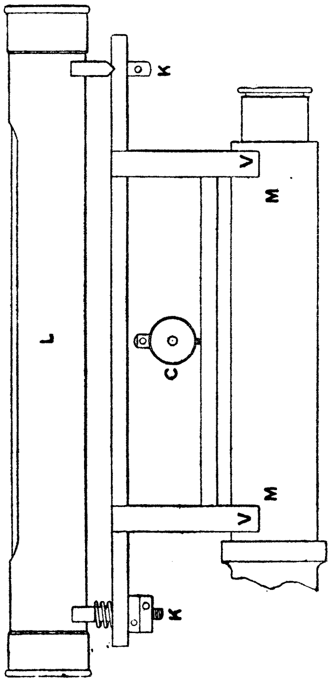
Reproduction of photographs of the apparatus.



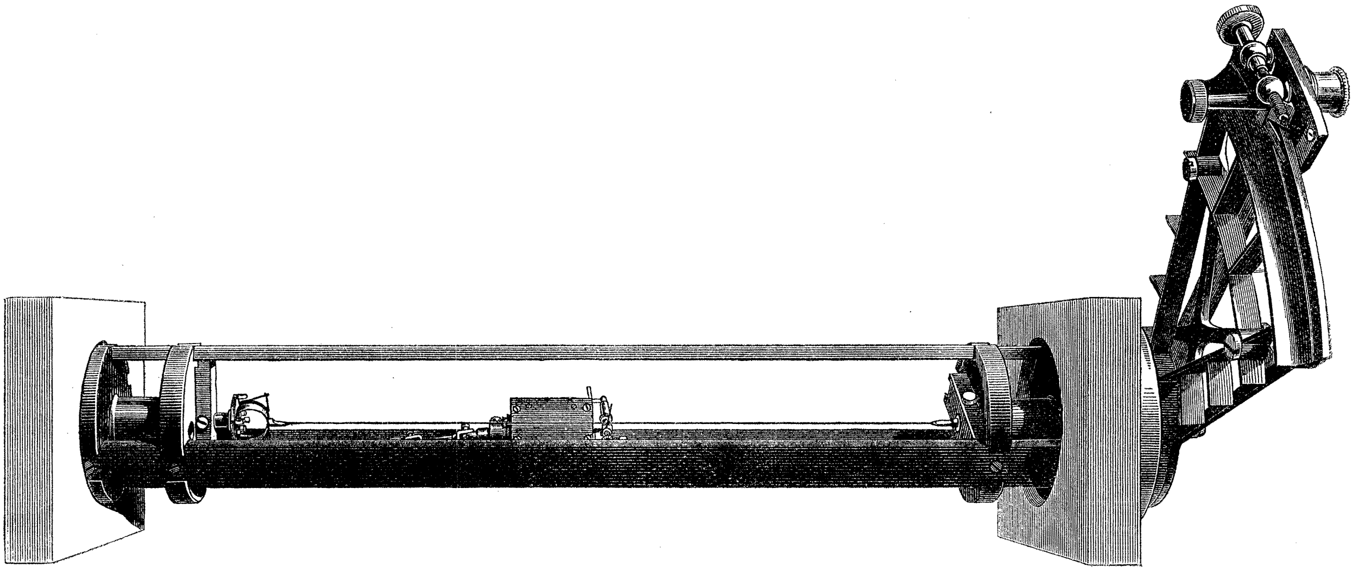
HORIZONTAL SECTION of GRAVITY BALANCE. ⅕ - SIZE

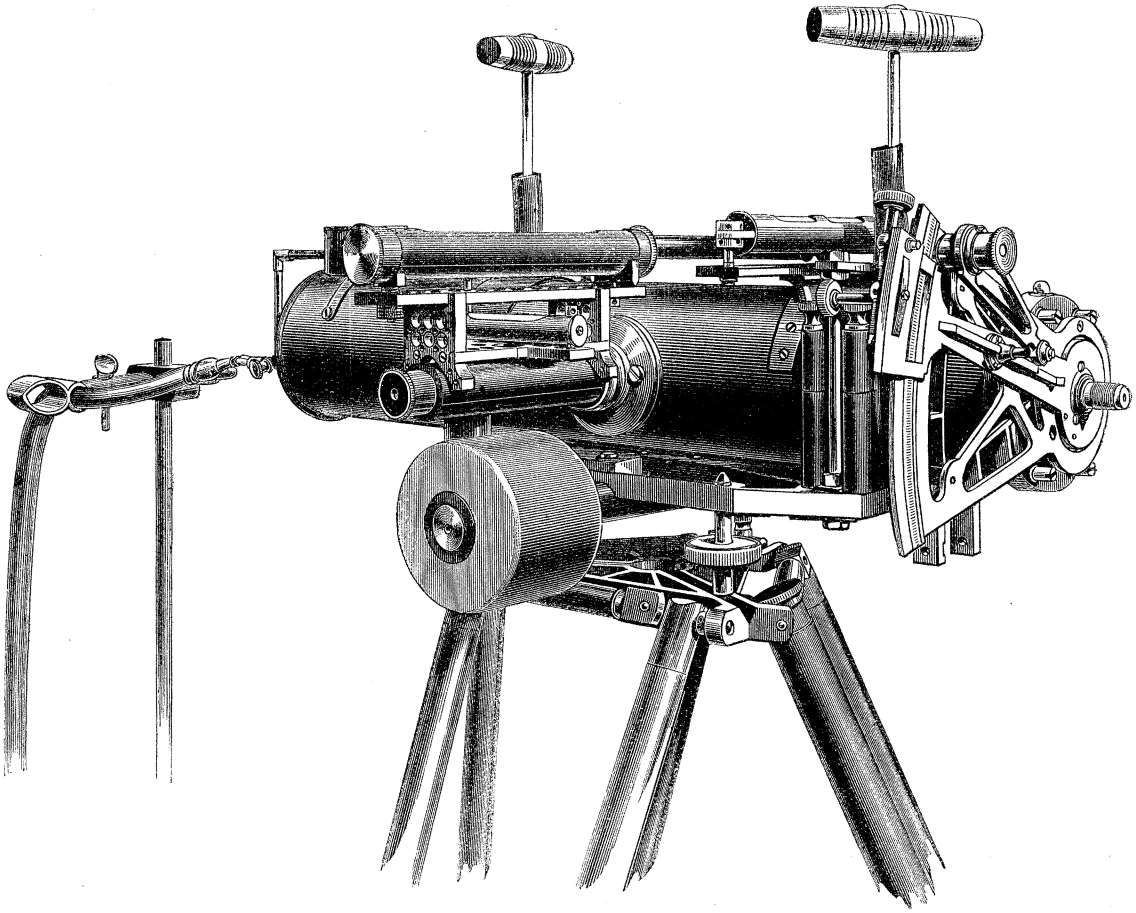


PLAN of GRADUATED ARC.



ELEVATION of Microscope STRIDING LEVEL.





PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES