Report of the Committee, consisting of Mr. G. H. DARWIN, Professor Sir WILLIAM THOMSON, Professor TAIT, Professor GRANT, Dr. SIEMENS, Professor PURSER, Professor G. FORBES, *and* Mr. HORACE DARWIN, *appointed for the Measurement of the Lunar Disturbance of Gravity.*

On an instrument for detecting and measuring small changes in the direction of the force of gravity, by GEORGE H. DARWIN, *M.A., F.R.S., formerly Fellow of Trinity College, Cambridge, and* HORACE DARWIN, *M.A., Assoc. M. Inst. C.E.*

[This report is written in the name of G. H. DARWIN merely for the sake of verbal convenience.]

I. *Account of the experiments.*

WE feel some difficulty as to the form which this report should take, because we are still carrying on our experiments, and have, as yet, arrived at no final results. As, however, we have done a good deal of work, and have come to conclusions of some interest, we think it better to give at once an account of our operations up to the present time, rather than to defer it to the future.

In November, 1878, Sir William Thomson suggested to me that I should endeavour to investigate experimentally the lunar disturbance of gravity, and the question of the tidal yielding of the solid earth. In May, 1879, we both visited him at Glasgow, and there saw an instrument, which, although roughly put together, he believed to contain the principle by which success might perhaps be attained. The instrument was erected in the Physical Laboratory of the University of Glasgow. We are not in a position to give an accurate description of it, but the following rough details are quite sufficient.

A solid lead cylinder, weighing perhaps a pound or two, was suspended by a fine brass wire, about 5 feet in length, from the centre of the lintel or cross-beam of the solid stone gallows, which is erected there for the purpose of pendulum experiments. A spike projected a little way out of the bottom of the cylindrical weight; a single silk fibre, several inches in length, was cemented to this spike, and the other end of the fibre was cemented to the edge of an ordinary galvanometer-mirror. A second silk fibre, of equal length, was cemented to the edge of the mirror at a point near to the attachment of the former fibre. The other end of this second fibre was then attached to a support, which was connected with the base of the stone gallows. The support was so placed that it stood very near to the spike at the bottom of the pendulum, and the mirror thus hung by the bifilar suspension of two silks, which stood exceedingly near to one another in their upper parts. The instrument was screened from draughts by paper pasted across between the two pillars of the gallows; but at the bottom, on one side, a pane of glass was inserted, through which one could see the pendulum-bob and galvanometer-mirror.

It is obvious that a small displacement of the pendulum, in a direction perpendicular to the two silks, will cause the mirror to turn about a vertical axis.

A lamp and slit were arranged, as in a galvanometer, for exhibiting the movement of the pendulum, by means of the beam of light reflected from the mirror.

No systematic observations were made, but we looked at the instrument at various hours of the day and night, and on Sunday also, when the street and railway traffic is very small.

The reflected beam of light was found to be in incessant movement, of so irregular a character that it was hardly possible to localise the mean position of the spot of light on the screen, within 5 or 6 inches. On returning to the instrument after several hours, we frequently found that the light had wandered to quite a different part of the room, and we had sometimes to search through nearly a semicircle before finding it again.

Sir William Thomson showed us that, by standing some 10 feet away from the piers, and swaying from one foot to the other, in time with the free oscillations of the pendulum, quite a large oscillation of the spot of light could be produced. Subsequent experience has taught us that considerable precautions are necessary to avoid effects of this kind, and the stone piers at Glasgow did not seem to be well isolated from the floor, and the top of the gallows was used as a junction for a number of electric connections.

The cause of the extreme irregularity of the movements of the pendulum was obscure; and as Sir William Thomson was of opinion that the instrument was well worthy of careful study, we determined to undertake a series of experiments at the Cavendish Laboratory at Cambridge. We take this opportunity of recording our thanks to Lord Rayleigh[1] for his kindness in placing rooms at our disposal, and for his constant readiness to help us.

The pressure of other employments on both of us prevented our beginning operations immediately, and the length of time which we have now spent over these experiments is partly referable to this cause, although it is principally due to the number of difficulties to be overcome, and to the quantity of apparatus which has had to be manufactured.

In order to avoid the possibility of disturbance from terrestrial magnetism, we determined that our pendulum should be made of pure copper.[2] Mr. Hussey Vivian kindly gave me an introduction to Messrs. Elkington, of Birmingham; and, although it was quite out of their ordinary line of business, they consented to make what we required. Accordingly, they made a pair of electrolytically-deposited solid copper cylinders, $5\frac{1}{2}$ inches long, and $2\frac{3}{4}$ inches in diameter. From their appearance, we presume that the deposition was made on to the inside of copper tubes, and we understand that it occupied six weeks to take place. In November, 1879, they sent us these two heavy masses of copper, and, declining any payment, courteously begged our acceptance of them. Of these two cylinders we have, as yet, only used one; but should our present endeavours lead to results of interest, we shall ultimately require both of them.

Two months before the receipt of our weights, the British Association had reappointed the Committee for the Lunar Disturbance of Gravity, and had added our names thereto. Since that time, with the exception

[1] Professor Maxwell had given us permission to use the 'pendulum room,' but we had not yet begun our operations at the time of his death.

[2] We now think that this was probably a superfluity of precaution.

of compulsory intermissions, we have continued to work at this subject. My brother Horace and I have always discussed together the plan on which to proceed; but up to the present time much the larger part of the work has consisted in devising mechanical expedients for overcoming difficulties. In this work he has borne by very far the larger share; and the apparatus has been throughout constructed from his designs, and under his superintendence, by the Cambridge Scientific Instrument Company.

Near the corner of a stone-paved ground-floor room in the Cavendish Laboratory there stands a very solid stone gallows, similar to, but rather more massive than, the one at Glasgow. As it did not appear thoroughly free from rigid connection with the floor, we had the pavement raised all round the piers, and the earth was excavated from round the brick basement to the depth of about 2 feet 6 inches, until we were assured that there was no connection with the floor or walls of the room, excepting through the earth. The ditch, which was left round the piers, was found very useful for enabling us to carry out the somewhat delicate manipulations involved in hanging the mirror by its two silk fibres.

Into the middle of the flat ends of one of our copper weights (which weighed 4,797 grammes, with spec. gr. 8·91) were screwed a pair of copper plugs; one plug was square-headed and the other pointed. Into the centre of the square plug was soldered a thin copper wire, just capable of sustaining the weight, and intended to hang the pendulum.

A stout cast-iron tripod was made for the support of the pendulum. Through a hole in the centre of it there ran rather loosely a stout iron rod with a screw cut on it. A nut ran on the screw and prevented the rod from slipping through the hole. The other end of the copper wire was fixed into the end of the rod.

The tripod was placed with its three legs resting near the margin of the circular hole in the centre of the lintel of the gallows. The iron rod was in the centre of the hole, and its lower end appeared about six inches below the lower face of the lintel. The pendulum hung from the rod by a wire of such length as to bring the spiked plug within a few inches of the base of the gallows. This would of course be a very bad way of hanging a pendulum which is intended to swing, but in our case the displacements of the end of the pendulum were only likely to be of a magnitude to be estimated in thousandths or even millionths of an inch, and it is certain that for such small displacements the nut from which the pendulum hung could not possibly rock on its bearings. However, in subsequent experiments we improved the arrangement by giving the nut a flange, from which there projected three small equidistant knobs, on which the nut rested.

The length of the pendulum from the upper juncture with the iron rod down to the tip of the spike in the bob was 148·2 c.m.

An iron box was cast with three short legs, two in front and one behind; its interior dimensions were $15 \times 15 \times 17\frac{1}{2}$ c.m.; it had a tap at the back; the front face ($15 \times 17\frac{1}{2}$) was left open, with arrangements for fixing a plate-glass face thereon. The top face ($15 \times 17\frac{1}{2}$) was pierced by a large round hole. On to this hole was cemented an ordinary earthenware 4-inch drain pipe, and on to the top of this first pipe there was cemented a second. The box was thus provided with a chimney 144 c.m. high. The cubic contents of the box and chimney were about $3\frac{1}{2}$ gallons.

The box was placed standing on the base of the gallows, with the chimney vertically underneath the round hole in the lintel. The top of the chimney nearly reached the lower face of the lintel, and the iron rod of the pendulum extended a few inches down into the chimney. The pendulum wire ran down the middle of the chimney, and the lower half of the pendulum bob was visible through the open face of the iron box. The stone gallows faces towards the S.E., but we placed the box askew on the base, so that its open face was directed towards the S.

The three legs of the box rested on little metal discs, each with a conical hole in it, and these discs rested on three others of a somewhat larger size. When the box was set approximately in position, we could by an arrangement of screws cause the smaller discs to slide a fraction of an inch on the larger ones, and thus exactly adjust the position of the box and chimney.

A small stand, something like a retort stand, about 4 inches high, stood on a leaden base, with a short horizontal arm clamped by a screw on to the thin vertical rod. This was the 'fixed' support for the bifilar suspension of the mirror. The stand was placed to the E. of the pendulum bob, and the horizontal arm reached out until it came very close to the spike of the pendulum.

The suspension and protection from tarnishing of our mirror gave us much trouble, but it is useless to explain the various earlier methods employed, because we have now overcome these difficulties in a manner to be described later. The two cocoon fibres were fixed at a considerable distance apart on the edge of the mirror, and as they were very short they splayed out at nearly a right angle to one another. By means of this arrangement the free period of oscillation of the mirror was made very short, and we were easily able to separate the long free swing of the pendulum from the short oscillations of the mirror.

The mirror was hung so that the upper ends of the silks stood within an eighth of an inch of one another, but the tip of the spike stood $\frac{1}{8}$ or $\frac{1}{16}$ of an inch higher than the fixed support. The plate-glass front of the box was then fixed on with indiarubber packing.

It is obvious that a movement of the box parallel to the front from E. to W. would bring the two fibres nearer together; this operation we shall describe as sensitising the instrument. A movement of the box perpendicular to the front would cause the mirror to show its face parallel to the front of the box; this operation we shall describe as centralising. As sensitising will generally decentralise, both sets of screws had to be worked alternately.

The adjusting screws for moving the box did not work very well; nevertheless, by a little trouble we managed to bring the two silks of the bifilar suspension very close to one another.

After the instrument had been hung as above described, we tried a preliminary sensitisation, and found the pendulum to respond to a slight touch on either pier. The spot of light reflected from the mirror was very unsteady, but not nearly so much so as in the Glasgow experiment; and we were quite unable to produce any perceptible increase of agitation by stamping or swaying to and fro on the stone floor. This showed that the isolation of the pier was far more satisfactory than at Glasgow.

We then filled the box and pipes with water. We had much trouble with slow leakage of the vessel, but the most serious difficulty arose from the air-bubbles which adhered to the pendulum. By using boiled water we obviated this fairly well, but we concluded that it was a great mistake to have a flat bottom to the pendulum. This mistake we have remedied in the final experiment described in the present paper.

The damping effect of the water on the oscillations of the pendulum and of the mirror was very great, and although the incessant dance of the light continued, it was of much smaller amplitude, and comparatively large oscillations of the pendulum, caused by giving the piers a push, died out after two or three swings. A very slight push on the stone piers displaced the mean position of the light, but jumping and stamping on the pavement of the room produced no perceptible effect. If, however, one of us stood on the bare earth in the ditch behind, or before the massive stone pier, a very sensible deflection of the light was caused; this we now know was caused by an elastic depression of the earth, which tilted the whole structure in one or the other direction. A pull of a few ounces, delivered horizontally on the centre of the lintel, produced a clear deflection, and when the pull was 8lbs., the deflection of the spot of light amounted to 45 c.m. We then determined to make some rough systematic experiments.

The room was darkened by shutters over all the windows, and the doors were kept closed. The paraffin lamp stood at three or four feet to the S.E. of the easterly stone pier, but the light was screened from the pier.

We began our readings at 12 noon (March 15, 1880), and took eight between that time and 10.30 P.M. From 12 noon until 4 P.M. the lamp was left burning, but afterwards it was only lighted for about a minute to take each reading. At 12 the reading was 595 m.m., and at 4 P.M. it was 936 m.m.[1]; these readings, together with the intermediate ones, showed that the pendulum had been moving northwards with a nearly uniform velocity. After the lamp was put out, the pendulum moved southward, and by 10.30 P.M. was nearly in the same position as at noon.

During the whole of the two following days and a part of the next we took a number of readings from 9 A.M. until 11 P.M. The observations when graphically exhibited showed a fairly regular wave, the pendulum being at the maximum of its northern excursion between 5 and 7 P.M., and probably furthest south between the same hours in the morning. But besides this wave motion, the mean position for the day travelled a good deal northward. We think that a part of this diurnal oscillation was due to the warping of the stone columns from changes of temperature. An increase of temperature on the south-east faces of the piers carried the lintel towards the north-west, and of this displacement we observed only the northerly component. The lamp produced a very rapid effect, and the diurnal change lagged some two hours behind the change in the external air. The *difference* between the temperatures of the S.E. and N.W. faces of the pier must have been very slight indeed. At that time, and indeed until quite recently, we attributed the whole of this diurnal oscillation to the warping of the piers, but we now feel nearly certain that it was due in great measure to a real change in the horizon.

We found that warming one of the legs of the iron tripod, even by contact with the finger, produced a marked effect, and we concluded that the mode of suspension was unsatisfactory.

[1] I give the numbers as recorded in the note-book, but the readings would sometimes differ by 2 or 3 m.m. within half-a-minute. The light always waves to and fro in an uncertain sort of way, so that it is impossible to assign a mean position with any certainty.

Although we had thus learnt that changes of temperature formed the great obstacle in the way of success, there were a good many things to be learnt from the instrument as it existed at that time.

After the box and pipes had been filled for some days the plate-glass front cracked quite across, and a slow leakage began to take place; we were thus compelled to dismount the whole apparatus and to make a fresh start.

It is obvious that to detect and measure displacements of the pendulum in the N. and S. direction, the azimuth of the silks by which the mirror is suspended must be E. and W., and that although any E. and W. displacement of the pendulum will be invisible, still such displacement will alter the sensitiveness of the instrument for the N. and S. displacements. In order to obviate this we determined to constrain the pendulum to move only in the N. and S. azimuth.

Accordingly we had a T-piece about 4 inches long fixed to the end of the iron rod from which the pendulum hung. The two ends of a fine copper wire were soldered into the ends of the T-piece; a long loop of wire was thus formed. The square-headed plug at the top of the pendulum-bob was replaced by another containing a small copper wheel, which could revolve about a horizontal axis. The bearings of the wheel were open on one side.

When the wheel was placed to ride on the bottom of the wire loop, and the pendulum-bob hooked on to the axle of the wheel by the open bearings, we had our pendulum hanging by a bifilar suspension. The motion of the pendulum was thus constrained to take place only perpendicular to the plane of the wire loop.

The iron tripod was replaced by a slate slab large enough to entirely cover the hole in the lintel of the gallows. Through the centre of the slab was a round hole, of about one inch in diameter, through which passed the iron rod with the T-piece at the lower end. The iron rod was supported on the slate by means of the flanged nut above referred to. There was also a straight slot, cut quite through the slab, running from the central hole to the margin. The purpose of this slot will be explained presently.

In the preceding experiment we had no means of determining the absolute amount of displacement of the pendulum, although, of course, we knew that it must be very small. There are two methods by which the absolute displacements are determinable; one is to cause known small displacements to the pendulum and to watch the effect on the mirror; and the second is to cause known small horizontal forces to act on the pendulum. We have hitherto only employed the latter method, but we are rather inclined to think that the former may give better results.

The following plan for producing small known horizontal forces was suggested by my brother.

Suppose there be a very large and a very small pendulum hanging by wires of equal length from neighbouring points in the same horizon; and suppose the large and the small pendulum to be joined by a fibre which is a very little shorter than the distance between the points of suspension. Then each pendulum is obviously deflected a little from the vertical, but the deflection of the small pendulum varies as the mass of the larger, and that of the larger as the mass of the smaller. If m be the mass of the small pendulum, and M of the large one, and if a be the distance between the points of suspension, then it may be easily shown that if a be increased by a small length δa, the increase of the linear deflection of the large pendulum is $m\delta a/(m+M)$. If l be the length of either pendulum, the angular deflection of the larger one is $m\delta a/l(m+M)$, and this is the deflection which would be produced by a horizontal force equal to $m\delta a/l(m+M)$ of gravity. It is clear, then, that by making the inequality between the two weights m and M very great, and the displacement of the point of suspension very small, we may deflect the large pendulum by as small a quantity as we like. The theory is almost the same if the two pendulums are not of exactly the same length, or if the length of one of them be varied.

Now in our application of this principle we did not actually attach the two pendulums together, but we made the little pendulum lean up against the large one; the theory is obviously just the same.

We call the small pendulum 'the disturber,' because its use is to disturb the large pendulum by known forces. A small copper weight for the disturber weighed ·732 grammes, and the large pendulum-bob, with its pulley, weighed 4831·5. Therefore the one was 6600 times as massive as the other. The disturber was hung by a platinum wire about $\frac{1}{1000}$th of an inch in diameter, which is a good deal thinner than a fine human hair.

We must now explain how the disturber was suspended, and the method of moving its point of suspension.

Parallel to the sides of the slot in the slate slab there was riveted a pair of brass rails, one being V-shaped and the other flat; on these rails there slid a little carriage with three legs, one of which slid on one rail, and the other two on the other. A brass rod with an eyelet-hole at the end was fixed to the centre of the carriage, and was directed downwards so that it passed through the centre of the slot. The slot was directed so that it was perpendicular to the T-piece from which the pendulum hung, and the brass rod of the little carriage was bent and of such length, that when the carriage was pushed on its rails until it was as near the centre of the slab as it would go, the eyelet-hole stood just below the T-piece, and half-way between the two wires. A micrometer screw was clamped to the slab and was arranged for making the carriage traverse known lengths on its rails, and as the wires of the pendulum were in the E. and W. plane, the carriage was caused to travel N. and S. by its micrometer screw.

One end of the fine platinum wire was fastened to the eyelet, and the other (as above stated) to the small disturbing weight. The platinum wire was of such length that the disturber just reached the pulley by which the big pendulum hung. We found that by pushing the carriage up to the centre, and very slightly tilting it off one rail, we could cause the disturber-weight to rest on either side of the pulley at will. If it was left on the side of the pulley remote from the disturber-carriage, it was in gear, and the traversing of the carriage on its rails would produce a small pressure of the disturber on to the side of the pulley. If it was left on the same side of the pulley as the disturber-carriage, the two pendulums were quite independent and the disturber was out of gear.

On making allowance for the difference in length between the pendulum and the disturber, and for the manner in which the thrust was delivered at the top of the pendulum, but omitting the corrections for the weights of the suspending wires and for the elasticity of the copper wire, we found that one turn of the micrometer screw should displace the spike at the bottom of the pendulum through 0·0001 mm. or $\frac{1}{245000}$th of an inch. The same displacement would be produced by an alteration in the direction of gravity with reference to the earth's surface by $\frac{1}{70}$th of a second of arc.

A rough computation showed that the to and fro motion of the pendulum in the N.S. azimuth, due to lunar attraction, should, if the earth be rigid, be the same as that produced by $2\frac{2}{3}$ turns of the micrometer screw.

We now return to the other arrangements made in re-erecting the instrument.

A new mirror, silvered on the face, was used, and was hung in a slightly different manner.

The fluid in which the pendulum was hung was spirits and water. The physical properties of such a mixture will be referred to later. In order to avoid air-bubbles we boiled $3\frac{1}{2}$ gallons of spirits and water for three hours *in vacuo*, and the result appeared satisfactory in that respect.

After the mirror was hung, the plate-glass front to the box was fixed and the vessel was filled by the tap in the back of the box. The disturber was not introduced until afterwards, and we then found that the pendulum responded properly to the disturbance.

As the heat of a lamp in the neighbourhood of the piers exercised a large disturbance, we changed the method of observing, and read the reflection of a scale with a telescope. The scale was a levelling staff divided into feet, and tenths and hundredths of a foot, laid horizontally at 15 feet from the piers, with the telescope immediately over it.

Since the amount of fluid through which the light had to pass was considerable, we were forced to place a gas-flame immediately in front of the scale; but the gas was only kept alight long enough to take a reading.

After sensitising the instrument we found that the incessant dance of the image of the scale was markedly less than when the pendulum was hung in water. A touch with a finger on either pier produced deflection by bending the piers, and the instrument responded to the disturber.

The vessel had been filled with fluid for some days, and we had just begun a series of readings, when the plate-glass front again cracked quite across without any previous warning. Thus ended our second attempt.

In the third experiment (July and August, 1880) the arrangements were so nearly the same as those just described that we need not refer to them. The packing for the plate-glass front was formed of red lead, and this proved perfectly successful, whereas the indiarubber packing had twice failed. As we were troubled by invisible leakage and by the evaporation of the fluid, we arranged an inverted bottle, so as always to keep the chimney full. We thought that when the T-piece at the end of the shaft became exposed to the air, the pendulum became much more unsteady, but we now think it at least possible that there was merely a period of real terrestrial disturbance.

From August 10 to 14 we took a series of observations from early morning until late at night. We noted the same sort of diurnal oscillatory motion as before, but the outline of the curve was far less regular. This, we think, may perhaps be explained by the necessity we were under of leaving the doors open a good deal, in order to permit the cord to pass by which Lord Rayleigh was spinning the British Association coil.

Notwithstanding that the weather was sultry the warping of the stone columns must have been very slight, for a thermometer hung close to the pier scarcely showed a degree of change between the day and night, and the *difference* of temperature of the N. and S. faces must have been a very small fraction of a degree. At that time, however, we still thought that the whole of the diurnal oscillation was due to the warping of the columns.

We next tried a series of experiments to test the sensitiveness of the instrument.

As above remarked the image of the scale was continually in motion, and moreover the mean reading was always shifting in either one direction or the other. At any one time it was possible to take a reading to within $\frac{1}{10}$th of a foot with certainty, and to make an estimate of the $\frac{1}{100}$th of a foot, but the numbers given below are necessarily to be regarded as very rough approximations.

As above stated, the gallows faced about to the S.E., and we may describe the two square piers as the E. and W. piers, and the edges of each pier by the points of the compass towards which they are directed.

On August 14, 1880, my brother stood on a plank supported by the pavement of the room close to the S.W. edge of the W. pier, and, lighting a spirit lamp, held the flame for ten seconds within an inch or two of this edge of the pier. The effect was certainly produced of making the pendulum-bob move northwards, but as such an effect is fused in the diurnal change then going on, the amount of effect was uncertain. He then stood similarly near the N.E. edge of the E. pier, and held the spirit flame actually licking the edge of the stone during one minute. The effect should now be opposed to the diurnal change, and it was so. Before the exposure to heat was over the reading had decreased ·15 feet, and after the heat was withdrawn the recovery began to take place almost immediately. We concluded afterwards that the effect was equivalent to a change of horizon of about $0''\cdot15$.

When the flame was held near but not touching the lintel for thirty seconds, the effect was obvious but scarcely measurable, even in round numbers, on account of the unsteadiness of the image.

When a heated lump of brass was pushed under the iron box no effect whatever was perceived, and even when a spirit flame was held so as to lick one side of the iron box during thirty seconds, we could not be sure that there was any effect. We had expected a violent disturbance, but these experiments seemed to show that convection currents in the fluid produce remarkably little effect.

When a pull of 300 grammes was delivered on to the centre of the lintel in a southward direction, we determined by several trials that the displacement of the reading was about ·30 feet, which may be equal to about $0''\cdot3$ change of horizon.

Two-thirds of a watering-can of water was poured into the ditch at the back of the pier. In this experiment the swelling of the ground should have an effect antagonistic to that produced by the cooling of the back face of the pier, and also to the diurnal changes then going on. The swelling of the ground certainly tilted the pier over, so that the reading was altered by ·10 foot. A further dose of water seemed to have the same effect, and it took more than an hour for the piers to regain their former position. As the normal diurnal change was going on simultaneously, we do not know the length of time during which the water continued to produce an effect.

On August 15 we tried a series of experiments with the disturber. When the disturber was displaced on its rails, the pendulum took a very perceptible time to take up its new position, on account of the viscosity of the fluid in which it was immersed.

The diurnal changes which were going on prevented the readings from being very accordant amongst themselves, but we concluded that twenty-five turns of the screw gave between ·4 foot and ·3 foot alteration in the reading on the scale. From the masses and dimensions of the pendulum and disturber, we concluded that 1 foot of our scale corresponded with about 1″ change in horizon. Taking into account the length of the pendulum, it appeared that 1 foot of our scale corresponded with $\frac{1}{1400}$th of a mm. displacement of the spike at the bottom of the pendulum. Now as a tenth of a foot of alteration of reading could be perceived with certainty, it followed that when the pendulum point moved through $\frac{1}{14000}$th of a mm. we could certainly perceive it.

During the first ten days the mean of the diurnal readings gradually increased, showing that the pendulum was moving northwards, until the reading had actually shifted 8 feet on the scale. It then became necessary to shift the scale. Between August 23 and 25 the reading had changed another foot. We then left Cambridge. On returning in October we found that this change had continued. The mirror had, however, become tarnished, and it was no longer possible to take a reading, although one could just see a gas flame by reflection from the mirror.

Whilst erecting the pendulum we had to stand on, and in front of, the piers, and to put them under various kinds of stress, and we always found that after such stress some sort of apparently abnormal changes in the piers continued for three or four hours afterwards.

We were at that time at a loss to understand the reason of this long-continued change in the mean position of the pendulum, and were reluctant to believe that it indicated any real change of horizon of the whole soil; but after having read the papers of MM. d'Abbadie and Plantamour, we now believe that such a real change was taking place.

By this course of experiments it appeared that an instrument of the kind described may be brought to almost any degree of sensitiveness. We had seen, however, that a stone support is unfavourable, because the bad conductivity of stone prevents a rapid equalisation of temperature between different parts, and even small inequalities of temperature produce considerable warping of the stone piers. But it now seems probable that we exaggerated the amount of disturbance which may arise from this cause.

A cellar would undoubtedly be the best site for such an experiment, but unfortunately there is no such place available in the Cavendish Laboratory. Lord Rayleigh, however, placed the 'balance room' at our disposal, and this room has a northerly aspect. There are two windows in it, high up on the north wall, and these we keep boarded up.

The arrangements which we now intended to make were that the pendulum and mirror should be hung in a very confined space, and should be immersed in fluid of considerable viscosity. The boundary of that space should be made of a heat-conducting material, which should itself form the support for the pendulum. The whole instrument, including the basement, was to be immersed in water, and the basement itself was to be carefully detached from contact with the building in which it stands. By these means we hoped to damp out the short oscillations due to local tremors, but to allow the longer oscillations free to take place; but above all we desired that changes of temperature in the instrument should take place with great slowness, and should be, as far as possible, equal all round.

We removed the pavement from the centre of the room, and had a circular hole, about 3 feet 6 inches in diameter, excavated in the 'made earth,' until we got down to the undisturbed gravel, at a depth of about 2 feet 6 inches.

We obtained a large cylindrical stone 2 feet 4 inches in diameter and 2 feet 6 inches in height, weighing about three-quarters of a ton. This we had intended to place on the earth in the hole, so that its upper surface should stand flush with the pavement of the room. But the excavation had been carried down a little too deep, and therefore an ordinary flat paving stone was placed on the earth, with a thin bedding of cement underneath it. The cylindrical block was placed to stand upon the paving stone, with a very thin bedding of lime and water between the two stones. The surface of the stone was then flush with the floor. We do not think that any sacrifice of stability has been made by this course.

An annular trench or ditch a little less than a foot across is left round the stone. We have lately had the bottom of the ditch cemented, and the vertical sides lined with brickwork, which is kept clear of any contact with the pavement of the room. On the S. side the ditch is a little wider, and this permits us to stand in it conveniently. The bricked ditch is watertight, and has a small overflow pipe into the drains. The water in the ditch stands slightly higher than the flat top of the cylindrical stone, and thus the whole basement may be kept immersed in water, and it is, presumably, at a very uniform temperature all round.

Before describing the instrument itself we will explain the remaining precautions for equalisation of temperature.

On the flat top of the stone stands a large barrel or tub, 5 feet 6 inches high and 1 foot 10 inches in diameter, open at both ends. The diameter of the stone is about 2 inches greater than the outside measure of the diameter of the tub, and the tub thus nearly covers the whole of the stone. The tub is well payed with pitch inside, and stands on two felt rings soaked in tar. Five large iron weights, weighing altogether nearly three-quarters of a ton, are hooked on to the upper edge of the tub, in order to make the joint between the tub and the stone watertight. Near the bottom is a plate-glass window; when it is in position, the window faces to the S. This tub is filled with water and the instrument stands immersed therein.

We had at first much trouble from the leakage of the tub, and we have to thank Mr. Gordon, the assistant at the Laboratory, for his ready help in overcoming this difficulty, as well as others which were perpetually recurring. The mounting of the tub was one of the last things done before the instrument was ready for observation, and we must now return to the description of the instrument itself.

We used the same pendulum-bob as before, but we had its shape altered so that the ends both above and below were conical surfaces, whilst the central part was left cylindrical. The upper plug with its pulley is replaced by another plug bearing a short round horizontal rod, with a rounded groove cut in it. The groove stands vertically over the centre of the weight, and is designed for taking the wire of the bifilar suspension of the pendulum; when riding on the wire the pendulum-bob hangs vertically.

Part of this upper plug consists of a short thin horizontal arm about an inch long. This arm is perpendicular to the plane of the groove, and when the pendulum is in position, projects northwards. Through the end of the arm is bored a fine vertical hole. This part of the apparatus is for the modified form of disturber, which we are now using.

The support for the pendulum consists of a stout copper tube $2\frac{7}{8}$ inches in diameter inside measure, and it just admits the pendulum-bob with $\frac{1}{8}$th inch play all round. The tube is 3 feet 6 inches in height, and is closed at the lower end by a diaphragm, pierced in the centre by a round hole, about $\frac{1}{4}$ inch in diameter. The upper end has a ring of brass soldered on to it, and this ring has a flange to it. The upper part of the brass ring forms a short continuation $\frac{3}{4}$ of an inch in length of the copper tube. The ring is only introduced as a means of fastening the flange to the copper tube.

The upper edge of the brass continuation has three V notches in it at 120° apart on the circumference of the ring. A brass cap like the lid of a pill-box has an inside measure $\frac{1}{4}$ inch greater than the outside measure of the brass ring. The brass cap has three rods which project inwards from its circumference, and which are placed at 120° apart thereon. When the cap is placed on the brass continuation of the upper tube, the three rods rest in the three V notches, and the cap is geometrically fixed with respect to the tube. A fine screw works through the centre of the cap, and actuates an apparatus, not easy to explain without drawings, by which the cap can be slightly tilted in one azimuth. The object of tilting the cap is to enable us to sensitise the instrument by bringing the silk fibres attached to the mirror into close proximity.

Into the cap are soldered the two ends of a fine brass wire; the junctures are equidistant from the centre of the cap and on opposite sides of it; they lie on that diameter of the cap which is perpendicular to the axis about which the tilting can be produced.

When the pendulum is hung on the brass wire loop by the groove in the upper plug, the wires just clear the sides of the copper tube.

It is clear that the tilting of the cap is mechanically equivalent to a shortening of one side of the wire loop and the lengthening of the other. Hence the pendulum is susceptible of a small lateral adjustment by means of the screw in the cap.

To the bottom of the tube is soldered a second stout brass ring; this ring bears on it three stout brass legs inclined at 120° to one another, all lying in a plane perpendicular to the copper tube. From the extremity of each leg to the centre of the tube is $8\frac{1}{2}$ inches. The last inch of each leg is hollowed out on its under surface into the form of a radial V groove.

There are three detached short pieces of brass tube, each ending below in a flange with three knobs on it, and at the upper end in a screw with a rounded head. These three serve as feet for the instrument. These three feet are placed on the upper surface of our basement stone at 120° apart, estimated from the centre of the stone. The copper tube with its legs attached is set down so that the inverted V grooves in the legs rest on the rounded screw-head at the tops of the three feet, and each of the feet rests on its three knobs on the stone. The bottom of the copper tube is thus raised $5\frac{1}{2}$ inches above the stone. By this arrangement the copper tube is retained in position with reference to the stone, and it will be observed that no part of the apparatus is under any constraint except such as is just necessary to geometrically determine its position.

The screws with rounded heads which form the three feet are susceptible of small adjustments in height, and one of the three heads is capable of more delicate adjustment, for it is actuated by a fine screw, which is driven by a toothed wheel and pinion. The pinion is turned by a wooden rod, made flexible by the insertion of a Hook's joint, and the wooden rod reaches to the top of the tub, when it is mounted surrounding the instrument.

The adjustable leg is to the N. of the instrument, and as the mirror faces S. we call it the 'back-leg.' When the copper support is mounted on its three legs, a rough adjustment for the verticality of the tube is made with two of the legs, and final adjustment is made by the back-leg.

It is obvious that if the back-leg be raised or depressed the point of the pendulum is carried southwards or northwards, and the mirror turns accordingly. Thus the back-leg with its screw and rod affords the means of centralising the mirror. The arrangements for suspending the mirror must now be described.

The lower plug in the pendulum-bob is rounded and has a small horizontal hole through it. When the pendulum is hung this rounded plug just appears through the hole in the diaphragm at the bottom of the copper tube.

A small brass box, shaped like a disk, can be screwed on to the bottom of the copper tube, in such a way that a diameter of the box forms a straight line with the axis of the copper tube. One side of the box is of plate glass, and when it is fastened in position the plate glass faces to the S. This is the mirror-box; it is of such a size as to permit the mirror to swing about 15° in either direction from parallelism with the plate-glass front.

The fixed support for the second fibre for the bifilar suspension of the mirror may be described as a very small inverted retort-stand. The vertical rod projects downwards from the underside of the diaphragm, a little to the E. of the hole in the diaphragm; and a small horizontal arm projects from this rod, and is of such a length that its extremity reaches to near the centre of the hole. This arm has a small eyelet-hole pierced through a projection at its extremity.

The mirror itself is a little larger than a shilling and is of thin plate glass; it has two holes drilled through the edge at about 60° from one another. The mirror was silvered on both sides, and then dipped into melted paraffin; the paraffin and silver were then cleaned off one side. The paraffin protects the silver from tarnishing, and the silver film seen through the glass has been found to remain perfectly bright for months, after having been immersed in fluid during that time. A piece of platinum wire about $\frac{1}{1000}$th of an inch in diameter is threaded twice through each hole in opposite directions, in such a manner that with a continuous piece of wire (formed by tying the two ends together) a pair of short loops are formed at the edge of the mirror, over each of the two holes. When the mirror is hung from a silk fibre passing through both loops, the weight of the mirror is sufficient to pull each loop taut.

A single silk fibre was threaded through the eyelet-hole at the end of the blunt point of the pendulum-bob, and tied in such a way that there was no loose end projecting so as to foul the other side of the bifilar suspension. The other end of the silk fibre was knotted to a piece of sewing silk on which a needle was threaded.

The pendulum was then hung from the cap by its wire loop, outside the copper tube, and the silk fibre with the sewing silk and needle attached dangled down at the bottom. The cap, with the pendulum attached thereto, was then hauled up and carefully let down into the copper tube. The sewing silk, fibre, and blunt end came out through the hole in the diaphragm.

We then sewed with the needle through the two loops on the margin of the mirror, and then through the eyelet-hole in the little horizontal arm. The silk was pulled taut, and the end fastened off on to the little vertical rod, from which the horizontal arm projects.

The mirror then hangs with one part of the silk attached to the pendulum-bob and the other to the horizontal arm.

The two parts of the silk are inclined to one another at a considerable angle, so that the free period of the mirror is short, but the upper parts of the silk stand very close to one another. The mirror-box encloses the mirror and makes the copper tube watertight.

There is another part of the apparatus which has not yet been explained, namely, the disturber. This part of the instrument was in reality arranged before the mirror was hung.

We shall not give a full account of the disturber, because it does not seem to work very satisfactorily.

In the form of disturber which we now use the variation of horizontal thrust is produced by variation in the length of the disturbing pendulum, instead of by variation of the point of support as in the previous experiment. It was not easy to vary the point of support when the pendulum is hung in a tube which nearly fits it.

The disturber-weight is a small lump of copper, and it hangs by fine sewing silk. The silk is threaded through the eyelet in the horizontal arm which forms part of the upper plug of the pendulum; thus the disturber-weight is to the N. of the pendulum. The silk after passing between the wires supporting the pendulum has its other end attached to the cap at the top at a point to the S. of the centre of the cap. Thus the silk is slightly inclined to the plane through the wires. The arrangement for varying the length of the disturbing pendulum will not be explained in detail, but it may suffice to say that it is produced by a third weight, which we call the 'guide weight,' which may be hauled up or let down in an approximately vertical line. This guide weight determines by its position how much of the upper part of the silk of the disturber shall be cut off, so as not to form a part of the free cord by which the disturbing weight hangs.

The guide weight may be raised or lowered by cords which pass through the cap. If the apparatus were to work properly a given amount of displacement of the guide weight should produce a calculable horizontal thrust on the pendulum. The whole of the arrangements for the disturber could be made outside the copper tube, so that the pendulum was lowered into the tube with the disturber attached thereto.

After the mirror was hung and the mirror-box screwed on, a brass cap was fixed by screws on to the flange at the top of the copper tube. This cap has a tube or chimney attached to it, the top of which rises five inches above the top of the cap or lid from which the pendulum hangs. From this chimney emerges a rod attached to the screw by which the sensitising apparatus is actuated, and also the silk by which the guide weight is raised or depressed.

The copper tube, with its appendages, was then filled with a boiled mixture of filtered water and spirits of wine by means of a small tap in the back of the mirror-box. The mixture was made by taking equal volumes of the two fluids; the boiling to which it was subjected will of course have somewhat disturbed the proportions. Poiseuille has shown [1] that a mixture of spirits and water has much greater viscosity than either pure spirits or pure water. When the mixture is by weight in the proportion of about seven of water to nine of spirits, the viscosity is nearly three times as great as that of pure spirits or of pure water. As the specific gravity of spirits is about ·8, it follows that the mixture is to be made by taking equal volumes of the two fluids. It is on account of this remarkable fact that we chose this mixture in which to suspend the pendulum, and we observed that the unsteadiness of the mirror was markedly less than when the fluid used was simply water.

The level of the fluid stood in our tubular support quite up to the top of the chimney, and thus the highest point of the pendulum itself was 5 inches below the surface.

The tub was then let down over the instrument, and the weights hooked on to its edge. The plate-glass window in the tub stood on the S. opposite to the mirror-box. The tub was filled with water up to nearly the top of the chimney, and the ditch round the stone basement was also ultimately filled with water. The whole instrument thus stood immersed from top to bottom in water.

Even before the tub was filled we thought that we noticed a diminution of unsteadiness in the image of a slit reflected from the mirror. The filling of the tub exercised quite a striking effect in the increase of steadiness, and the water in the ditch again operated favourably.

We met with much difficulty at first in preventing serious leakage of the tub, and as it is still not absolutely watertight, we have arranged a water-pipe to drip about once a minute into the tub. A small overflow pipe from the tub to the ditch allows a very slow dripping to go into the ditch, and thus both vessels are kept full to a constant level. We had to take this course because we found that a rise of the water in the ditch through half an inch produced a deflection of the pendulum. The ditch, it must be remembered, was a little broader on the S. side than elsewhere.

In May, 1881, we took a series of observations with the light, slit and scale. The scale was about 7 feet from the tub, and in order to read it we found it convenient to kneel behind the scale on the ground. I was one day watching the light for nearly ten minutes, and being tired with kneeling on the pavement I supported part of my weight on my hands a few inches in front of the scale. The place where my hands came was on the bare earth from which one of the paving stones had been removed. I was surprised to find quite a large change in the reading. After

[1] *Poggendorf's Annalen*, 1843, vol. 58, p. 437.

several trials I found that the pressure of a few pounds with one hand only was quite sufficient to produce an effect.

It must be remembered that this is not a case of a small pressure delivered on the bare earth at say 7 feet distance, but it is the difference of effect produced by this pressure at 7 feet and 8 feet; for of course the change only consisted in the change of distribution in the weight of a small portion of my body.

We have, however, since shown that even this degree of sensitiveness may be exceeded.

We had thought all along that it would ultimately be necessary to take our observations from outside the room, but this observation impressed it on us more than ever; for it would be impossible for an observer always to stand in exactly the same position for taking readings, and my brother and I could not take a set of readings together on account of the difference between our weights.

In making preliminary arrangements for reading from outside the room we found the most convenient way of bringing the reflected image into the field of view of the telescope was by shifting a weight about the room. My brother stood in the room and changed his position until the image was in the field of view, and afterwards placed a heavy weight where he had been standing; after he had left the room the image was in the field of view.

On the S.W. wall of the room there is a trap-door or window which opens into another room, and we determined to read from this.

In order to read with a telescope the light has to undergo two reflections and twelve refractions, besides those in the telescope; it has also to pass twice through layers of water and of the fluid mixture. In consequence of the loss of light we found it impossible to read the image of an illuminated scale, and we had to make the scale self-luminous.

On the pavement to the S. of the instrument is placed a flat board on to which are fixed a pair of rails; a carriage with three legs slides on these rails, and can be driven to and fro by a screw of ten threads to the inch. Backlash in the nut which drives the carriage is avoided by means of a spiral spring. A small gas-flame is attached to the carriage; in front of it is a piece of red glass, the vertical edge of which is very distinctly visible in the telescope after reflection from the mirror. The red glass was introduced to avoid prismatic effects, which had been troublesome before. The edge of the glass was found to be a more convenient object than a line which had been engraved on the glass as a fiducial mark.

The gas-flame is caused to traverse by pulleys driven by cords. The cords come to the observing window, and can be worked from there. A second telescope is erected at the window, for reading certain scales attached to the traversing gear of the carriage, and we find that we can read the position of the gas-flame to within a tenth of an inch, or even less, with certainty.

From the gas the ray of light enters the tub and mirror-box, is reflected by the mirror, and emerges by the same route; it then meets a looking-glass which reflects it nearly at right angles and a little upwards, and finally enters the object-glass of the reading telescope, fixed to the sill of the observing window.

When the carriage is at the right part of the scale the edge of the red glass coincides with the cross wire of the reading telescope, and the reading is taken by means of the scale telescope.

Arrangements had also to be made for working the sensitiser, centraliser, and disturber from outside the room.

A scaffolding was erected over the tub, but free of contact therewith, and this supported a system of worm-wheels, tangent screws, and pulleys by which the three requisite movements could be given. The junctures with the sensitising and centralising rods were purposely made loose, because it was found at first that a slight shake to the scaffolding disturbed the pendulum.

The pulleys on the scaffolding are driven by cords which pass to the observing window.

On the window-sill we now have two telescopes, four pulleys, an arrangement, with a scale attached, for raising and depressing the guide weight, and a gas tap for governing the flame in the room.

After the arrangements which have been described were completed we sensitised the instrument from outside the room. The arrangements worked so admirably that we could produce a quite extraordinary degree of sensitiveness by the alternate working of the sensitising and centralising wheels, without ever causing the image of the lamp to disappear from the field of view. This is a great improvement on the old arrangement with the stone gallows.

We now found that if one of us was in the room and stood at about 16 feet to the S. of the instrument with his feet about a foot apart, and slowly shifted his weight from one foot to the other, then a distinct change was produced in the position of the mirror. This is the most remarkable proof of sensitiveness which we have yet seen, for the instrument can detect the difference between the distortion of the soil caused by a weight of 140 lbs. placed at 16 feet and at 17 feet. We have not as yet taken any great pains to make the instrument as sensitive as possible, and we have little doubt but that we might exceed the present degree of delicacy, if it were desirable to do so.

The sensitiveness now attained is, we think, only apparently greater than it was with the stone gallows, and depends on the improved optical arrangements, and the increase of steadiness due to the elimination of changes of temperature in the support.

From July 21 to July 25 we took a series of readings. There was evidence of a distinct diurnal period with a maximum about noon, when the pendulum stood furthest northwards; in the experiment with the stone gallows in 1880 the maximum northern excursion took place between 5 and 7 P.M.

The path of the pendulum was interrupted by many minor zigzags, and it would sometimes reverse its motion for nearly an hour together. During the first four days the mean position of the pendulum travelled southward, and the image went off the scale three times, so that we had to recentralise it. In the night between the 24th and 25th it took an abrupt turn northward, and the reading was found in the morning of the 25th at nearly the opposite end of the scale.

On the 25th the dance of the image was greater than we had seen it at any time with the new instrument, so that we went into the room to see whether the water had fallen in the tub and had left the top of the copper tube exposed; for on a previous occasion this had appeared to produce much unsteadiness. There was, however, no change in the state of affairs. A few days later the image was quite remarkable for its steadiness.

On July 25, and again on the 27th, we tried a series of observations with the disturber, in order to determine the absolute value of the scale.

The guide weight being at a known altitude in the copper tube we took a series of six readings at intervals of a minute, and then shifting the guide weight to another known altitude, took six more in a similar manner; and so on backwards and forwards for an hour.

The first movement of the guide weight produced a considerable disturbance of an irregular character, and the first set of readings were rejected. Afterwards there was more or less concordance between the results, but it was to be noticed there was a systematic difference between the change from 'up' to 'down' and 'down' to 'up.' This may perhaps be attributed to friction between certain parts of the apparatus. We believe that on another occasion we might erect the disturber under much more favourable conditions, but we do not feel sure that it could ever be made to operate very satisfactorily.

The series of readings before and after the change of the guide weight were taken in order to determine the path of the pendulum at the critical moment; but the behaviour of the pendulum is often so irregular, even within a few minutes, that the discrepancy between the several results and the apparent systematic error may be largely due to unknown changes, which took place during the minute which necessarily elapsed between the last of one set of readings and the first of the next. The image took up its new position deliberately, and it was necessary to wait until it had come to its normal position.

Between the first and second set of observations with the disturber, it had been necessary to enter the room and to recentralise the image. We do not know whether something may not have disturbed the degree of sensitiveness, but at any rate the results of the two sets of observations are very discordant.[1]

The first set showed that one inch of movement of the gas-flame, which formed the scale, corresponds with $\frac{1}{15}$th of a second of arc of change of horizon; the second gave $\frac{1}{8}$th of a second to the inch.

As we can see a twentieth of an inch in the scale, it follows that a change of horizon of about $0''\cdot005$ should be distinctly visible. In this case the point of the pendulum moves through $\frac{1}{40000}$th of a millimeter. At present we do not think that the disturber gives more than the order of the changes of horizon which we note, but our estimate receives a general confirmation from another circumstance.

From the delicacy of the gearing connected with the back-leg, we estimate that it is by no means difficult to raise the back-leg by a millionth of an inch. The looseness in the gearing was purposely kept so great that it requires a turn or two of the external pulley on the window-sill before the backlash is absorbed, but after this a very small fraction of a turn is sufficient to move the image in the field.

We are now inclined to look to this process with the back-leg to enable us to determine the actual value of our scale, but this will require a certain amount of new apparatus, which we have not yet had time to arrange. In erecting the instrument we omitted to take certain measurements which it now appears will be necessary for the use of the back-leg as a means of determining the absolute value of our scale, but we know these measurements approximately from the working drawings of the

[1] See, however, the postscript at the end of this part.

instrument. Now it appears that one complete revolution of a certain tangent-screw by which the back-leg is raised should tilt the pendulum-stand through almost exactly half a second of arc, and therefore this should produce a relative displacement of the pendulum of the same amount. We have no doubt but that a tenth of the turn of the tangent-screw produces quite a large deflection of the image, and probably a hundredth of a turn would produce a sensible deflection. Therefore, from mere consideration of the effect of the back-leg we do not doubt but that a deflection of the pendulum through a $\frac{1}{200}$th of a second of arc is distinctly visible. This affords a kind of confirmation of the somewhat unsatisfactory deductions which we draw from the operation of the disturber.

Postscript.—The account of our more recent experiments was written during an absence from Cambridge from July 29 to August 9. In this period the gradual southerly progression of the pendulum-bob, which was observed up to July 28, seems to have continued; for on August 9 the pendulum was much too far S. to permit the image of the gas-flame to come into the field of view of the telescope. On August 9 the image was recentralised, and on the 9th and 10th the southerly change continued; on the 11th, however, a reversal northwards again occurred. During these days the unsteadiness of the image was much greater than we have seen it at any time with the new instrument. There was some heavy rain and a good deal of wind at that time. We intend to arrange a scale for giving a numerical value to the degree of unsteadiness, but at present it is merely a matter of judgment.

It seems possible that earthquakes were the cause of unsteadiness on August 9, 10, and 11, and we shall no doubt hear whether any earthquakes have taken place on those days.

After August 11 we were both again absent from Cambridge. On August 16 my brother returned, and found that the southerly progression of the pendulum-bob had reasserted itself, so that the image was again far out of the field of view. After recentralising he found the image to be unusually steady.

This appeared a good opportunity of trying the effect of purely local tremors.

One observer therefore went into the room and, standing near the instrument, delivered some smart blows on the brickwork coping round the ditch, the stone pavement, the tub, and the large stone basement underneath the water. Little or no effect was produced by this. Very small movements of the body, such as leaning forward while sitting in a chair, or a shift of part of the weight from heels to toes, produced a sensible deflection, and it was not very easy for the experimenter to avoid this kind of change whilst delivering the blows. To show the sensitiveness of the instrument to steady pressure we may mention that a pressure of three fingers on the brick coping of the ditch produces a marked deflection.

On August 17 I returned to Cambridge, and noted, with my brother, that the image had never been nearly so steady before. The abnormal steadiness continued on the 18th. There was much rain during those days.

On the afternoon of the 19th there was a high wind, and although the abnormal steadiness had ceased, still the agitation of the image was rather less than we usually observe it.

The image being so steady on the 17th, we thought that a good opportunity was afforded for testing the disturber. At 6.15 P.M. of that day we began the readings. The changes from 'up' to 'down' were made as quickly as we could, and in a quarter of an hour we secured five readings when the guide weight was 'up,' and four when it was 'down.'

When a curve was drawn, with the time as abscissa, and the readings as ordinates, through the 'up's,' and similarly through the 'down's,' the curves presented similar features. This seems to show that movement of the disturber does not cause irregularities or changes, except such as it is designed to produce.

The displacement of the guide weight was through 5 c.m. on each occasion.

The four changes from 'up' to 'down' showed that an inch of scale corresponded with $0''{\cdot}0897$, with a mean error of $0''{\cdot}0021$; the four from 'down' to 'up' gave $0''{\cdot}0909$ to the inch, with a mean error of $0''{\cdot}0042$. Thus the systematic error on the previous occasions was probably only apparent.

Including all the eight changes together, we find that the value of an inch is $0''{\cdot}0903$ with a mean error of $0''{\cdot}0030$.

A change in the scale reading amounting to a tenth of an inch is visible without any doubt, and even less is probably visible. Now it will give an idea of the delicacy of the instrument when we say that a tenth of an inch of our scale corresponds to a change of horizon[1] through an angle equal to that subtended by an inch at 384 miles.

II. *On the work of previous observers.*

In the following section we propose to give an account of the various experiments which have been made in order to detect small variations of horizon, as far as they are known to us; but it is probable that other papers of a similar kind may have escaped our notice.

In a report of this kind it is useful to have references collected together, and therefore, besides giving an account of the papers which we have consulted, we shall requote the references contained in these papers.

In Poggendorf's 'Annalen' for 1873 there are papers by Professor F. Zöllner, which had been previously read before the Royal Saxon Society, and which are entitled 'Ueber eine neue Methode zur Messung anziehender und abstossender Kräfte,' vol. 150, p. 131, 'Beschreibung und Anwendung des Horizontalpendels,' vol. 150, p. 134. A part of the second of these papers is translated, and the figure is reproduced in the supplementary number of the 'Philosophical Magazine' for 1872, p. 491, in a paper 'On the Origin of the Earth's Magnetism.'

The horizontal pendulum was independently invented by Professor Zöllner, and, notwithstanding assertions to the contrary, was probably for the first time actually realised by him; it appears, however, that it had been twice invented before. The history of the instrument contains a curious piece of scientific fraud, of which we shall give an account below.

The instrument underwent some modifications under the hands of Professor Zöllner, and the two forms are described in the above papers.

[1] We use the expression 'change of horizon' to denote relative movement of the earth, at the place of observation, and the plumb-line. Such changes may arise either from alteration in the shape of the earth, or from displacement of the plumb-line; our experiments do not determine which of these two really takes place.

The principle employed is as follows:—There is a very stout vertical stand, supported on three legs. At the top and bottom of the vertical shaft are fixed two projections. Attached to each projection is a fine straight steel clock spring; the springs are parallel to the vertical shaft of the stand, the one attached to the lower projection running upwards, and that attached to the upper one running downwards. The springs are of equal length, each being equal to half the distance between their points of attachment on the projections.

The springs terminate in a pair of rings, which stand exactly opposite to one another, so that a rod may be thrust through both.

A glass rod has a heavy weight attached to one end of it, and the other end is thrust through the two rings. The rings are a little separated from one another, and the glass rod stands out horizontally, with its weight at the end, and is supported by the tension of the two springs. It is obvious that if the point of attachment of the upper spring were vertically over that of the lower spring, and if the springs had no torsional elasticity, then the glass rod would be in neutral equilibrium, and would stand equally well in any azimuth.

The springs being thin have but little torsional elasticity, and Professor Zöllner arranges the instrument so that the one support is very nearly over the other. In consequence of this the rod and weight have but a small predilection for one azimuth more than another. The free oscillations of the horizontal pendulum could thus be made extraordinarily slow; and even a complete period of one minute could be easily attained.

A very small horizontal force of course produces a large deflection of the pendulum, and a small deflection of the force of gravitation with reference to the instrument must produce a like result. He considers that by this instrument he could, in the first form of the instrument, detect a displacement of the horizon through $0''{\cdot}00035$; in the second his estimate is $0''{\cdot}001$.

The observation was made by means of a mirror attached to the weight, and scale and telescope.

The maximum change of level due to the moon's attraction is at St. Petersburg $0''{\cdot}0174$, and from the sun $0''{\cdot}0080$ [C. A. F. Peters, 'Bull. Acad. Imp. St. Pétersbourg,' 1844, vol. 3. No. 14]; and thus the instrument was amply sensitive enough to detect the lunar and solar disturbances of gravity.[1]

Professor Zöllner found, as we have done, that the readings were never the same for two successive instants. The passing of trains on the railway at a mile distant produced oscillations of the equilibrium position.

[1] We are of opinion that M. Zöllner has made a mistake in using at Leipsig Peters' results for St. Petersburg. Besides this he considers the changes of the vertical to be $0''{\cdot}0174$ on *each* side of a mean position, and thus says the change is $0''{\cdot}0348$ altogether. Now a rough computation which I have made for Cambridge shows that the maximum meridional horizontal component of gravitation, as due to lunar attraction, is $4{\cdot}12 \times 10^{-8}$ of pure gravity. This force will produce a deflection of the plumb-line of $0''{\cdot}0085$, and the total amplitude of meridional oscillation will be $0''{\cdot}0170$. The maximum deflection of the plumb line occurs when the moon's hour-angle is $\pm$ 45° and $\pm$ 135° at the place of observation. The change at Cambridge when the moon is S.E. and N.W. is $0''{\cdot}0216$. The deflection of the plumb line varies as the cosine of the latitude, and is therefore greater at Cambridge than at St. Petersburg. Multiplying ·0216 by sec 51° 43′ cos 60° we get ·0174, and thus my calculation agrees with Peters'.

He seems to have failed to detect the laws governing the longer and wider oscillations performed. Notwithstanding that he took a number of precautions against the effects of changes of temperature, he remarks that 'the external circumstances under which the above experiments were carried out must be characterised as extremely unfavourable for this object (measuring the lunar attraction), so that the sensitiveness might be much increased in pits in the ground, provided the reaction of the glowing molten interior against the solid crust do not generate inequalities of the same order.'

Further on he says that if the displacements of the pendulum should be found not to agree in phase with the theoretical phase as given by the sun's position, then it might be concluded that gravitation must take a finite time to come from the sun.

It appears to me that such a result would afford strong grounds for presuming the existence of frictional tides in the solid earth, and that Professor Zöllner's conclusion would be quite unjustifiable.

Earlier in the paper he states that he preferred to construct his instrument on a large scale, in order to avoid the disturbing effects of convection currents. We cannot but think, from our own experience, that by this course Professor Zöllner lost more than he gained, for the larger the instrument the more it would necessarily be exposed in its various parts to regions of different temperature, and we have found that the warping of supports by inequalities of temperature is a most serious cause of disturbance.

The instrument of which we have given a short account appears to us very interesting from its ingenuity, and the account of the attempts to use it are well worthy of attention, but we cannot think that it can ever be made to give such good results as those which may perhaps be attained by our plan or by others. The variation in the torsional elasticity of the suspending springs, due to changes of temperature, would seem likely to produce serious variations in the value of the displacements of the pendulum, and it does not seem easy to suspend such an instrument in fluid in such a manner as to kill out the effects of purely local tremors.

Moreover, the whole instrument is kept permanently in a condition of great stress, and one would be inclined to suppose that the vertical stand would be slightly warped by the variation of direction in which the tensions of the springs are applied, when the pendulum bob varies its position.

In a further paper in the same volume, p. 140, 'Zur Geschichte des Horizontalpendels,' Zöllner gives the priority of invention to M. Perrot, who had described a similar instrument on March 31, 1862, ('Comptes Rendus,' vol. 54, p. 728), but as far as he knows M. Perrot did not actually construct it.

He also quotes an account of an 'Astronomische Pendelwage,' by Lorenz Hengler, published in 1832, in vol. 43 of 'Dingler's Polytechn. Journ.,' pp. 81–92. In this paper it appears that Hengler gives the most astonishing and vague accounts of the manner in which he detected the lunar attraction with a horizontal pendulum, the points of support being the ceiling and floor of a room 16 feet high. The terrestrial rotation was also detected with a still more marvellous instrument.

Zöllner obviously discredits these experiments, but hesitates to characterise them, as they deserve, as mere fraud and invention.

The university authorities at Munich state that in the years 1830–1 there was a candidate in philosophy and theology named Lorenz Hengler, of Reichenhofen, 'der weder früher noch später zu finden ist.'

At p. 150 of the same volume Professor Šafařik contributes a 'Beitrag zur Geschichte des Horizontalpendels.' He says that the instrument takes its origin from Professor Gruithuisen, of Munich, whose name has 'keinen guten Klang' in the exact sciences.

This strange person, amongst other eccentricities, proposed to dig a hole quite through the earth, and proposes a catachthonic observatory. Gruithuisen says, in his 'Neuen Analekten für Erd- und Himmelskunde' (Munich, 1832), vol. 1, part i.: 'I believe that the oscillating-balance (Schwung-wage) of a pupil of mine (named Hengeller), when constructed on a large scale, will do the best service.'

Some of the most interesting observations which have been made are those of M. d'Abbadie. He gave an account of his experiments in a paper, entitled 'Études sur la verticale,' 'Association Française pour l'avancement des Sciences, Congrès de Bordeaux, 1872,' p. 159. As this work is not very easily accessible to English readers, and as the paper itself has much interest, we give a somewhat full abstract of it. He has also published two short notes with reference to M. Plantamour's observations (noticed below), in vol. 86, p. 1528 (1878), and vol. 89, p. 1016 (1879), of the 'Comptes Rendus.' We shall incorporate the substance of his remarks in these notes in our account of the original paper.

When at Olinda, in Brazil, in 1837, M. d'Abbadie noticed the variations of a delicate level which took place from day to day. At the end of the two months of his stay there the changes in the E. and W. azimuth had compensated themselves, and the level was in the same condition as at first; but the change in the meridian was still progressing when he had to leave.

In 1842, at Gondar, in Ethiopia, and at Saqa, he noticed a similar thing. In 1852 he gave an account to the French Academy ('Comptes Rendus,' May, p. 712) of these observations, as well as of others, by means of levels, which were carried out in a cellar in the old castle of Audaux, Basses Pyrénées.

Leverrier, he says, speaks of sudden changes taking place in the level of astronomical instruments, apparently without cause. Airy has proved that the azimuth of an instrument may change, and Hough notes, in America, capricious changes of the Nadir.

Henry has collected a series of levellings and azimuths observed at Greenwich during ten years, and during eight of the same years at Cambridge ('Monthly Notices, R.A.S.,' vol. 8, p. 134). The results with respect to these two places present a general agreement, and show that from March to September the western Y of the transit instrument falls through $2''\cdot 5$, whilst it deviates at the same time $2''$ towards the north. Ellis has made a comparison of curves applying to Greenwich, during eight years, for level and azimuth. He shows that there is a general correspondence with the curves of the external temperature ('Memoirs of the R. Ast. Soc.,' vol. 29, pp. 45–57).

In the later papers M. d'Abbadie says that M. Bouquet de la Grye has observed similar disturbances of the vertical at Campbell Island, lat. 52° 34′ S.. M. Bouquet used a heavy pendulum governing a vertical lever, by which the angle was multiplied.[1] He found that the

[1] I do not find a reference to M. Bouquet in the R.S. Catalogue of scientific papers. It appears from what M. d'Abbadie says that certain observations have

great breakers on the shore at a distance of two miles caused a deviation of the vertical of $1''\cdot1$. On one occasion the vertical seems to have varied through $3''\cdot2$ in $3\frac{1}{4}$ hours.

M. d'Abbadie also quotes Elkin, Yvon Villarceau, and Airy as having found, from astronomical observations, notable variations in latitude, amounting to from $7''$ to $8''$.

As M. d'Abbadie did not consider levels to afford a satisfactory method of observation of the presumed changes of horizon, he determined to proceed in a different manner.

The site of his experiments was Abbadia, in Subernoa, near Hendaye. The Atlantic was 400 meters distant, and the sea level 62 meters below the place of observation. The subsoil was loamy rock (*roche marneuse*), belonging to cretaceous deposits of the South of France. Notwithstanding the steep slope of the soil, water was found at about 5 meters below the surface.

In this situation he had built, in 1863, a concrete cone, of which the external slope was one in ten (une inclinaison d'une dixième). The concrete cone is truncated, and the flat surface at the top is 2 meters in diameter. It is pierced down the centre by a vertical hole or well 1 meter in diameter. This well extends to within half a meter of the top, at which point the concrete closes in, leaving only a hole of 12 centimeters up to the flat upper surface.

From the top of the concrete down to the rock is 8 meters, and the well is continued into the rock to a further depth of 2 meters: thus from top to bottom is 10 meters.

A tunnel is made to the bottom of the well in order to drain away the water, and access of the observer to the bottom is permitted by means of an underground staircase. Access can also be obtained to a point halfway between the top and bottom by means of a hole through the concrete. At this point there is a diaphragm across the well, pierced by a hole 21 centimeters in diameter. The diaphragm seems to have been originally made in order to support a lens, but the mode of observation was afterwards changed. The diaphragm is still useful, however, for allowing the observer to stand there and sweep away cobwebs.

The cone is enclosed in an external building, from the roof of which, as I understand, there hangs a platform on which the observer may stand without touching the cone; and the two staircases leading up to the top are also isolated.[1]

On the hole through the top of the cone is riveted a disk of brass pierced through its centre by a circular hole 21 mm. in diameter. The hole in the disk is traversed across two perpendicular diameters by fine platinum wires; at first there were only two wires, but afterwards there were four, which were arranged so as to present the outline of a right-angled cross. The parallel wires were very close together, so that the four wires enclosed in the centre a very small square space.

At the bottom of the well is put a pool of mercury. The mercury was at first in an iron basin, but the agitation of the mercury was found sometimes to be so great that no reflection was visible for an hour to-

been made with pendulums in Italy, but that it does not distinctly appear that the variations of level are simultaneous over wide areas. No reference is given as to the observers.

[1] This passage appears to me a little obscure, and I cannot quite understand the arrangement.

gether. At the suggestion of Leverrier the iron basin was replaced by a shallow wooden tray with a corrugated bottom, and a good reflection was then generally obtainable. Immediately over the mercury pool there stood a lens of 10 c.m. diameter and 10 meters focal length, and over the brass disk there stood a microscope with moveable micrometer wires in the eyepiece, and a position circle. The platinum wires were illuminated, and on looking through the microscope the observer saw the wires both directly and by reflection. The observations were taken by measuring the azimuth and displacement of the image of the central square relatively to the real square enclosed by the wires.

One division of the micrometer screw indicated a displacement of vertical of $0''\cdot03$, so that the observations were susceptible of considerable refinement.

The whole of the masonry was finished in 1863, and M. d'Abbadie then allowed the structure five years to settle before he began taking observations. The arrangements for observing above described were made in 1868 and 1869.

In the course of a year he secured 2,000 observations, and the results appear to be very strange and capricious.

Throughout March, 1869, the perturbations of the mercury were so incessant, that observations (taken at that time with the iron basin) were nearly impossible; on the 29th he waited nearly an hour in vain in trying to catch the image of the wires. Two days later the mercury was perfectly tranquil. On April 6 it was much agitated, although the air and sea were calm. A tranquil surface was a rare exception.

In 1870 the corrugated trough was substituted for the iron basin; and M. d'Abbadie says :—

'Cependant, ni le fond inégal du bain rainé ni sa forme ne m'ont empêché d'observer, ce que j'appelle des *ombres fuyantes.* Ce sont des bandes sombres et parallèles qui traversent le champ du microscope avec plus ou moins de vitesse, et qu'on explique en attribuant au mercure des ondes très ténues, causées par une oscillation du sol dans un seul sens. Le plus souvent ces *ombres* semblent courir du S.E. au N.O., approximativement selon l'axe de la chaîne des Pyrénées; mais je les ai observées, le 15 Mars 1872, allant vers le S.O. À cette époque le mercure était, depuis le 29 février, dans une agitation continuelle, comme mon aide l'avait constaté en 1869, aussi dans le mois de Mars.'[1]

He observed also, from time to time, certain oscillations of the mercury too rapid to be counted, which he calls 'tremoussements.' There were also sudden jumpings of the image from one point to another, or 'frétillements,' indicating a sudden change of vertical through $0''\cdot49$ to $0''\cdot65$.

He observed many microscopic earthquakes, and in some cases the image was carried quite out of the field of view.

He also detected the difference of vertical according to the state of the tide in the neighbouring sea; but the change of level due to this cause was often masked by others occurring contemporaneously.

From observations during the years 1867 to 1872 (with the exception of 1870) he finds that in every year but one the plumb-line deviated northwards during the latter months of the year, but in 1872 it deviated to the south.

[1] M. d'Abbadie writes to me that this phenomenon was ultimately found to result from air-currents (Nov. 5, 1881).

He does not give any theoretical views as to the causes of these phenomena, but remarks that his observations tend to prove that the causes of change are sometimes neither astronomical nor thermometrical.

The most sudden change which he noted was on October 27, 1872, when the vertical changed by $2''\cdot4$ in six hours and a quarter. Between January 30 and March 26 of the same year the plumb-line deviated $4''\cdot5$ towards the south.

We now come to the valuable observations of M. Plantamour, which we believe are still being prosecuted by him. His papers are 'Sur le déplacement de la bulle des niveaux à bulle d'air,' 'Comptes Rendus,' June 24, 1878, vol. 86, p. 1522, and 'Des mouvements périodiques du sol accusés par des niveaux à bulle d'air,' 'Comptes Rendus,' December 1, 1879, vol. 89, p. 937.

The observations were made at Sécheron, near Geneva, at first at the Observatory, and afterwards at M. Plantamour's house. After some preliminary observations he obtained a very sensitive level and laid it on the concrete floor of a room in which the variations of temperature were very small. The azimuth of the level was E. and W., and the observations were made every hour from 9 A.M. until midnight. Figures are given of the displacement of the bubble during April 24, 25, and 26, 1878. The results indicate a diurnal oscillation of level, the E. end of the level being highest towards 5.30 P.M.; the amplitudes of the oscillations were $8''\cdot4$, $11''\cdot2$, $15''\cdot75$ during these three days. It also appeared that there was a gradual rising of the mean diurnal position of the E. end during the same time.

The level was then transported to a cellar in M. Plantamour's house, when the temperature only varied by half a degree centigrade. The bubble of the level often ran quite up to one end. A new and larger level was obtained, together with the great 'chevalet de fer,' which is used by the manufacturers in testing levels. Both levels were placed E. and W., at about two meters apart. During May 3 and 4, 1878, the bubble travelled eastward without much return, and it is interesting to learn that simultaneous observations by M. Turretini, at the Level Factory, three kilometers distant, at Plainpalais, showed a similar change.

Between May 3 and 6 the level actually changed through $17''$. Up to the 19th the level still showed the eastward change.

M. Plantamour remarks that the eastern pier of a transit instrument is known to rise during a part of the year, but not by an amount comparable with that observed by him, and that the diurnal variations are unknown.

After further observations of a similar kind one of the levels was arranged in the N. and S. azimuth.

The same sort of diurnal oscillations, although more irregular, were observed, but the hours of maximum were not the same in the two levels. During the four days May 24 to 28 the maximum rising of the north generally took place about noon. This is exactly the converse of what we have recently observed.

In the second paper he remarks:

'Dans le sens du méridien, les mouvements diurnes sont très rares irréguliers et toujours très faibles, le niveau en accuse parfois, quand il n'y en a point de l'est à l'ouest, et inversement, quand ces derniers sont très prononcés, on n'en aperçoit que très rarement du sud au nord.'

In our experiment of March 15 to 18, 1880, we found that the pendulum stood furthest north about 6 P.M., so that at that time the S. was most elevated; and in the short series of observations during the present summer the maximum elevation of the S. took place about noon.

On October 1, 1878, M. Plantamour began a new series of observations, which lasted until September 30, 1879. The levels were arranged in the two azimuths as before, and the observations were taken five times a day, namely, at 9 A.M., noon, 3, 6, and 9 P.M. The mean of these five readings he takes as the diurnal value.

During October and November the eastern end of the level fell, which is exactly the converse of what happened during the spring of the same year; he concludes that the eastern end falls when the external temperature falls.

When a curve of the external temperature was placed parallel with that for the level, it appeared that there was a parallelism between the two, but the curve for the level lagged behind that for temperature by a period of from one to four days.

This parallelism was maintained until the end of June, 1879, when it became disturbed. From then until the beginning of September the E. rose, but in a much greater proportion than the rise of mean temperature. It must be noted that July was a cold and wet month.

Although the external temperature began to fall on August 5, the E. end continued to rise until September 8. This he attributes to an accumulation of heat in the soil. The total amplitude of the annual oscillation from E. to W. amounted to $28''\cdot08$.

There was also a diurnal oscillation in this azimuth which amounted to $3''\cdot2$ on September 5. The east end appeared to be highest between 6 and 7.45 P.M., and lowest at the similar hour in the morning.[1]

The meridional oscillations were much smaller, the total annual amplitude being only $4''\cdot89$. From December 23, 1878, until the end of April, 1879, there was a correspondence between the external temperature curve and that for N. and S. level. We have already quoted the remark on the diurnal meridional oscillations.

M. Plantamour tells us that in 1856 Admiral Mouchez detected no movement of the soil by means of the levels attached to astronomical instruments. On the other hand, M. Hirsch established, by several years of observation at Neuchâtel, that there was an annual oscillation of a transit instrument from E. to W., with an amplitude of $23''$, and an azimuthal oscillation of $75''$. Similar observations with the transit instrument were made at the observatory at Berne in the summer of 1879.

It is to be regretted that M. Plantamour does not give us more information concerning the manner in which the iron support for the levels was protected from small changes of temperature, nor with regard to the effect of the observer's weight on the floor of the room. We have concluded that both these sources of disturbance should be carefully eliminated.

[1] It seems that M. Plantamour sent a figure to the French Academy with the paper, but no figure is given. This figure would doubtless have explained the meaning of some passages which are somewhat obscure. Thus he speaks of the *minimum* occurring between 6 and 7.45, but it is not clear whether minimum means E. highest or E. lowest. I interpret the passage as above, because this was the state of things in the observations recorded in the first of the two papers. There is a similar difficulty about the meridional oscillations.

Some interesting observations were made at Pulkova on a subject cognate to that on which we are writing. M. Magnus Nyrén contributed, on February 28, 1878, an interesting note to the Imperial Academy of St. Petersburg, entitled 'Erderschütterung beobachtet an einem feinem Niveau 1877 Mai 10.'[1] On May 10 (April 28), 1877, at 4.16 A.M., a striking disturbance of the level on the axis of the transit was observed by M. Nyrén in the observatory at Pulkova. The oscillations were watched by him for three minutes; their complete period was about 20 seconds, and their amplitude between $1''\cdot5$ and $2''$. At 4.35 A.M. there was no longer any disturbance. He draws attention to the fact that it afterwards appeared that one hour and fourteen minutes earlier there had been a great earthquake at Iquique. The distance from Iquique to Pulkova is 10,600 kilometers in a straight line, and 12,540 kilometers along the arc of a great circle. He does not positively connect the two phenomena together; but he observes that if the wave came through the earth from Iquique to Pulkova it must have travelled at the rate of about 2·4 kilometers per second. This is the speed of transmission through platinum or silver.

M. Nyrén thinks the wave-motion could not have been so regular as it was, if the transmission had been through the solid, and suggests that the transmission was through the fluid interior of the earth.

It appears to us that this argument is hardly sound, and that it would be more just to conclude that the interior of the earth was a sensibly perfectly elastic solid; because oscillations in molten rock would surely be more quickly killed out by internal friction than those in a solid. However, M. Nyrén does not lay much stress on this argument. He also draws attention to the fact that on September 20 (8), 1867, M. Wagner observed at Pulkova an oscillation of the level, with an amplitude of $3''$, and that seven minutes before the disturbance there had been an earthquake at Malta. On April 4 (March 23), 1868, M. Gromadzki observed an agitation of the level, and it was afterwards found that there had been an earthquake in Turkestan five minutes before.

Similar observations of disturbances had been made twice before, once by M. Wagner and once by M. Romberg; but they had not been connected with any earthquakes—at least with certainty.

Dr. C. W. Siemens has invented an instrument of extraordinary delicacy, which he calls an 'Attraction-meter.' An account of the instrument is given in an addendum to his paper 'On determining the depth of the sea without the use of the sounding-line' ('Phil. Trans.,' 1876, p. 659). We shall not give any account of this instrument, because Dr. Siemens is a member of our committee, and will doubtless bring any observations he may make with it before the British Association at some future time.

III. *Remarks on the present state of the subject.*

Although our experiments are not yet concluded, it may be well to make a few remarks on the present aspects of the question, and to state shortly our intentions as to future operations.

Our experiments, as far as they go, confirm the results of MM. d'Abbadie and Plantamour, and we think that there can remain little

[1] *Bull. Acad. St. Pét.*, vol. 24, p. 567.

doubt that the surface of the earth is in incessant movement, with oscillations of periods extending from a fraction of a second to a year.

Whether it be a purely superficial phenomenon or not, this consideration should be of importance to astronomical observers, for their instruments are necessarily placed at the surface of the earth. M. Plantamour and others have shown that there is an intimate connection between the changes of level and those of the temperature of the air; whence it follows that the principal part of the changes must be superficial. On the other hand, M. d'Abbadie has shown that it is impossible to explain all the changes by means of changes of temperature. It would be interesting to determine whether changes of a similar kind penetrate to the bottom of mines, and Gruithuisen's suggestion of a catachthonic observatory seems worthy of attention, although he perhaps went rather far in the proposition that the observatory should be ten or fifteen miles below the earth's surface.

It may appear not improbable that the surface of the soil becomes wrinkled all over, when it is swollen by increase of temperature and by rainfall. If this, however, were the case, then we should expect that instruments erected at a short distance apart would show discordant results. M. Plantamour, however, found that, at least during three days, there was a nearly perfect accordance between the behaviour of two sets of levels at three kilometers apart; and during eight years there appeared to be general agreement between the changes of level of the astronomical instruments at Greenwich and Cambridge. It would be a matter of much interest to determine how far this concordance would be maintained if the instrument of observation had been as delicate as that used by M. d'Abbadie or as our pendulum.

M. Plantamour speaks as though it were generally recognised that one pier of a transit circle rises during one part of the year and falls at another.[1] But if this be so throughout Europe, we must suppose that there is a kind of tide in the solid earth, produced by climatic changes; the rise and fall of the central parts of continents must then amount to something considerable in vertical height, and the changes of level on the easterly and westerly coasts of a continent must be exactly opposite to one another. We are not aware that any comparison of this kind has been undertaken. The idea seems of course exceedingly improbable, but we understand it to be alleged that it is the eastern pier of transit instruments in Europe which rises during the warmer part of the year. Now if this be generally true for Europe, which has no easterly coast, it is not easy to see how the change can be brought about except by a swelling of the whole continent.

We suggest that in the future it will be thought necessary to erect at each station a delicate instrument for the continuous observation of changes of level. Perhaps M. d'Abbadie's pool of mercury might be best for the longer inequalities, and something like our pendulum for the shorter ones; or possibly the pendulum when used in a manner which we intend to try might suffice for all the inequalities.

[1] 'Dans l'opération au moyen de laquelle on vérifie l'horizontalité de l'axe d'une lunette méridienne, il parait qu'on remarque bien un léger mouvement d'exhaussement de l'est pendant une partie de l'année, mais il n'est pas aussi considérable que celui qu'accuse mon niveau, et l'on n'a jamais remarqué, que je sache, une oscillation diurne comme celle qu'a indiquée le niveau dans le pavillon.'—*Comptes Rendus*, June 24, 1878, vol. 86, p. 1525.

At present the errors introduced by unknown inequalities of level are probably nearly eliminated by the number of observations taken; but it could not fail to diminish the probable error of each observation, if a correction were applied for this cause of disturbance from hour to hour, or even from minute to minute. If the changes noted by M. Plantamour are not entirely abnormal in amount, such corrections are certainly sufficient to merit attention.

In our first set of experiments we found that stone piers are exceedingly sensitive to changes of temperature and to small stresses. Might it not be worth while to plate the piers of astronomical instruments with copper, and to swathe them with flannel? We are not aware as to the extent to which care is taken as to the drainage of the soil round the piers, or as to the effect of the weight of the observer's body; but we draw attention to the effect produced by the percolation of water round the basement, and to the impossibility we have found of taking our observations in the same room with the instrument.

In connection with this subject we may notice an experiment which was begun $3\frac{1}{2}$ years ago by my brother Horace. The experiment was undertaken in connection with my father's investigation of the geological activity of earthworms, and the object was to determine the rate at which stones are being buried in the ground in consequence of the excavations of worms.

The experiment is going on at Down, in Kent. The soil is stiff red clay, containing many flints lying over the chalk. There are two stout metal rods, one of iron and the other of copper. The ends were sharpened and they were hammered down vertically into the soil of an old grass field, and they are in contact with one another, or nearly so. When they had penetrated 8 feet 6 inches it was found very difficult to force them deeper, and it is probable that the ends are resting on a flint. The ends were then cut off about three inches above the ground.

A stone was obtained like a small grindstone, with a circular hole in the middle. This stone was laid on the ground with the two metal rods appearing through the hole. Three brass V grooves are leaded into the upper surface of the stone, and a moveable tripod-stand with three rounded legs can be placed on the stone, and is, of course, geometrically fixed by the nature of its contact with the V's. An arrangement with a micrometer screw enables the observer to take contact measurements of the position of the upper surface of the stone with regard to the rods. The stone has always continued to fall, but during the first few months the rate of fall was probably influenced by the decaying of the grass underneath it. The general falling of the stone can only be gathered from observations taken at many months apart, for it is found to be in a state of continual vertical oscillation.

The measurements are so delicate that the raising of the stone produced by one or two cans full of water poured on the ground can easily be perceived. Between September 7 and 19, 1880, there was heavy rain, and the stone stood 1·91 mm. higher at the latter date than at the former. The effect of frost and the wet season combined is still more marked, for on January 23, 1881, the stone was 4·12 mm. higher than it had been on September 7, 1880.

The prolonged drought of the present summer has had a great effect, for between May 8 and June 29 the stone sank through 5·79 mm. The opposite effects of drought and frost are well shown by the fact that on

January 23 the stone stood 8·62 mm. higher than on June 29, 1881. The observations are uncorrected for the effect of temperature on the metal rods, but the fact that the readings from the two rods of different metals always agree very closely *inter se*, shows that such a correction would amount to very little.

The changes produced in the height of the stone are, of course, entirely due to superficial causes; but the amounts of the oscillations are certainly surprising, and although the basements of astronomical instruments may be very deep, they cannot entirely escape from similar oscillations.

In his address to the mathematical section at the meeting of the British Association at Glasgow in 1876, Sir William Thomson tells us[1] that Peters, Maxwell, Nyrén, and Newcomb[2] have examined the observations at Pulkova, Greenwich, and Washington, in order to discover whether there is not an inequality in the latitude of the observatories having a period of about 306 days. Such an inequality must exist on account of the motion in that period of the instantaneous axis of rotation of the earth round the axis of maximum moment of inertia. The inequality was detected in the results, but the probable error was very large, and the epochs deduced by the several investigators do not agree *inter se*. It remains, therefore, quite uncertain whether the detection of the inequality is a reality or not. But now we ask whether it is not an essential first step in such an enquiry to make an elaborate investigation by a very delicate instrument of the systematic changes of vertical at each station of observation?

We will next attempt to analyse the merits and demerits of the various methods which have been employed for detecting small changes in the vertical.

The most sensitive instrument is probably the horizontal pendulum of Professor Zöllner, and its refinement might be almost indefinitely increased by the addition of the bifilar suspension of a mirror as a means of exhibiting the displacements of the pendulum-bob. If this were done it might be possible to construct the instrument on a very small scale and yet to retain a very high degree of sensitiveness. We are inclined to think, however, that the variation of the torsional elasticity of the suspending springs under varying temperature presents an objection to the instrument which it would be very difficult to remove. The state of stress under which the instrument is of necessity permanently retained seems likely to be prejudicial.

Next in order of sensitiveness is probably our own pendulum, embodying the suggestion of Sir William Thomson. We are scarcely in a position as yet to feel sure as to its merits, but it certainly seems to be capable of all the requisite refinement. We shall give below the ideas which our experience, up to the present time, suggest as to improvements and future observations.

Although we know none of the details of M. Bouquet de la Grye's pendulum actuating a lever, it may be presumed to be susceptible of considerable delicacy, and it would be likely to possess the enormous

[1] *B. A. Report* for 1876, p. 10. For 'Nysen' read 'Nyrén.'

[2] Peters' paper is in *Bull. St. Pet. Acad.*, 1844, p. 305, and *Ast. Nach.* vol. 22, 1845, p. 71, 103, 119. Nyrén's paper is in *Mém. St. Pet. Acad.* vol. 19, 1873, No. 13. With regard to Maxwell, see Thomson and Tait's *Nat. Phil.* 2nd edit. part 1, vol. 1. An interesting letter from Newcomb is quoted in Sir W. Thomson's address.

advantage of giving an automatic record of its behaviour. On the other hand the lever must introduce a very unfavourable element in the friction between solids.

M. d'Abbadie's method of observation by means of the pool of mercury seems on the whole to be the best which has been employed hitherto. But it has faults which leave ample fields for the use of other instruments. The construction of a well of the requisite depth must necessarily be very expensive, and when the structure is made of a sufficient size to give the required degree of accuracy, it is difficult to ensure the relative immobility of the cross wires and the bottom of the well.

Levels are exceedingly good from the point of view of cheapness and transportability, but the observations must always be open to some doubt on account of the possibility of the sticking of the bubble from the effects of capillarity. The justice of this criticism is confirmed by the fact that M. Plantamour found that two levels only two meters apart did not give perfectly accordant results. Levels are moreover, perhaps, scarcely sensitive enough for an examination of the smaller oscillations of level. Dr. Siemens' form of level possesses ample sensibility, but is probably open to the same objections on the score of capillarity.

In the case of our own experiments we think that the immersion of the whole instrument in water from top to bottom has proved an excellent precaution against the effects of change of temperature, and our experience leads us to think that much of the agitation of the pendulum in the earlier set of experiments was due to small variations of temperature against which we are now guarded.

The sensitiveness of the instrument leaves nothing to be desired, and were such a thing as a firm foundation attainable, we could measure the horizontal component of the lunar attraction to a considerable degree of accuracy. We believe that this is the first instrument in which the viscosity of fluids has been used as a means of eliminating the effects of local tremors. In this respect we have been successful, for we find that jumping or stamping in the room itself produces no agitation of the pendulum, or at least none of which we can feel quite sure. We are inclined to try the effect of fluids of greater viscosity, such as glycerine, syrup of sugar, or paraffin oil. But along with such fluids we shall almost inevitably introduce air-bubbles, which it may be hard to get rid of. If a fluid of great viscosity were used, we should then only observe the oscillations of level of periods extending over perhaps a quarter to half a minute. The oscillations of shorter periods are, however, so inextricably mixed up with those produced by carriages and railway trains, that nothing would be lost by this.

In connection with this point Mr. Christie writes to me, that 'In the old times of Greenwich Fair, some twenty years ago, when crowds of people used to run down the hill, I find the observers could not take reflection observations for two or three hours after the crowd had been turned out. . . . . We do not have anything like such crowds now, even on Bank holidays, and I have not heard lately of any interference with the observations.' If the observers attributed the agitation of the mercury to the true cause, the elasticity of the soil must be far more perfect than is generally supposed. It would be surprising to find a mass of glass or steel continuing to vibrate for as long as two hours after the disturbance was removed. May it not be suspected that times of agitation, such as those noted by M. d'Abbadie, happened to coincide on two or three occasions with Greenwich Fair?

As the sensitiveness of our present instrument is very great, although the sensitising process has never been pushed as far as possible, we think that it will be advantageous to construct an instrument on half, or even less than half, the present scale. The heavy weights which we now have to employ will thus be reduced to one-eighth of the present amount. The erection of the instrument may thus be made an easy matter, and an easily portable and inexpensive instrument may be obtained.

Our present form of instrument has several serious flaws. The image is continually travelling off the scale, the gearing both internal and external to the room for observing is necessarily complex and troublesome to erect, and lastly we have not yet succeeded in an accurate determination of the value of the scale.

We are in hopes of being able to overcome all these objections. We propose to have a fixed light, which may be cast into the room from the outside. This will free us from the obviously objectionable plan of having a gas-flame in the room, and at the same time will abolish the gearing for traversing the lamp on the scale. We should then abolish the disturbing pendulum and thus greatly simplify the instrument. The readings would be taken by the elevation or depression of the back-leg, until the image of the fixed light was brought to the cross wire of the observing telescope.

The ease with which the image may be governed with our present arrangements leads us to be hopeful of the proposed plan. The use of the back-leg will, of course, give all the displacements in absolute measure.

The only gearings which it will be necessary to bring outside the room will be those for sensitising and for working the back-leg. The sensitising gearing when once in order will not have to be touched again.

The objections to this plan are, that it is necessary to bring one of the supports of the instrument under very slight stresses, and that it will not be possible to take readings at small intervals of time, especially if a more viscous fluid be used.

Our intention is to proceed with our observations with the present instrument for some time longer, and to note whether the general behaviour of the pendulum has any intimate connection with the meteorological conditions. We intend to observe whether there is a connection between the degree of agitation of the pendulum and the occurrence of magnetic storms. M. Zöllner has thrown out a suggestion for this sort of observation, but we find no notice of his having acted on it.[1]

We shall also test how far the operation by means of the back-leg may be made to satisfy our expectations.

We have no hope of being able to observe the lunar attraction in the present site of observation, but we think it possible that we may devise a portable instrument, which shall be amply sensitive enough for such a purpose, if the bottom of a deep mine should be found to give a sufficiently invariable support for the instrument.

The reader will understand that it is not easy to do justice to an

[1] *Phil. Mag.* Dec. 1872, p. 497.

incomplete apparatus, or to give a very satisfactory account of experiments still in progress; but as it is now two years since the Committee was appointed, we have thought it best to give to the British Association such an account as we can of our progress.

Second Report of the Committee, consisting of Captain ABNEY, Professor W. G. ADAMS, *and* Professor G. CAREY FOSTER, *appointed to carry out an Investigation for the purpose of fixing a Standard of White Light.*

SINCE the last meeting of the Association but little progress has been made in the investigations. Though several series of experiments have been made, no definite conclusion on the subject has been arrived at by your Committee. Owing to the accidental omission to present a report to the last meeting, the recommendation embodied in the communication which was printed in the last annual volume could not be carried out. (See 'Reports Brit. Assoc.' 1880, p. 119.)

Final Report of a Committee, consisting of Professor A. S. HERSCHEL, Professor W. E. AYRTON, Professor P. M. DUNCAN, Professor G. A. LEBOUR, Mr. J. T. DUNN, *and* Professor J. PERRY, *on Experiments to determine the Thermal Conductivities of certain Rocks, showing especially the Geological Aspects of the Investigation.*

IN bringing to a close the series of Reports which it has submitted during the past series of years since 1874, the Committee has endeavoured to collect and to compare together the several exact and well-deduced results from observations arrived at hitherto by various independent experimenters and investigators in the subject of its inquiry, so as to show at once the present position of the experimental research and the most essential points in which it requires further extensions and improvements.

The method pursued by Professor Everett in his work on 'Units and Physical Constants,' of expressing all the well-determined data of physical experiments in terms of the centimètre, the gramme, and the second as a common system of units, is adopted in forming the general list of absolute and relative thermal conductivities by different observers, which the Committee has met with and collected together in the simple order and arrangement of a classified Table of thermal properties of rocks presented with this Report. Many of the data presented in the Table are therefore already furnished in the uniform and well-authenticated form required, in Professor Everett's work. But the result of the present comparison has afforded the Committee such positive grounds of confidence in the general accuracy of the values found by its long-continued series of experiments, that it has been enabled by that means to assign absolute values to the relative ones of several important lists of thermal