PHILOSOPHICAL TRANSACTIONS.

I. *On the Newtonian Constant of Gravitation.*

By C. V. Boys, *A.R.S.M., F.R.S., Assistant Professor of Physics, Royal College of Science, South Kensington.*

Received May 31,—Read June 7,—Revised October 18, 1894.

[Plates 1, 2.]

Table of Contents.

PART I.

Preliminary.

IN a paper on the CAVENDISH experiment, published in the 'Proc. Roy Soc.,' vol. 46, p. 253, I showed how the famous experiment of CAVENDISH* could be transformed in several particulars, so that greatly increased delicacy and accuracy would be obtainable.

The experiment is so well known that there is no occasion to describe the apparatus which CAVENDISH employed, or the subsequent work of REICH,† BAILY,‡ or CORNU and BAILLE.§ It is sufficient to state that, owing to the extremely small value of the Newtonian constant of gravitation, all these experimenters made use of balls as large as they conveniently could, so as to increase the force of attraction as much as possible, and of a lever as long as they could, so as to increase the effect of the force in producing torsion. However, CORNU realized that if he could keep the period the same by the use of a sufficiently fine torsion wire, and reduce the dimensions of the whole apparatus, the angle of deflection would not be reduced but would remain the same. CORNU also introduced refinements which have made the behaviour of his apparatus far more consistent than that of any which had preceded it.

Soon after I had made and found the value of quartz fibres for producing a very small and constant torsion, I thought that it might be possible to apply them to the CAVENDISH apparatus with advantage, which opinion I found was also held by Professor TYNDALL. Before employing them for this purpose I examined the theory of the apparatus with a view to using them in the most suitable manner.

The sensibility of this kind of apparatus is, if the period is maintained always the same, independent of its linear dimensions; for in two similar instruments, in which all the dimensions of one are n times the corresponding dimensions of the other, the moments of inertia of the beams and their appendages are as $n^5 : 1$, and, therefore, if the period is to be unchanged, the torsional couples must be as $n^5 : 1$ also. The attracting masses, both fixed and movable, are as $n^3 : 1$, and their distances apart as $n : 1$; therefore, the attractions are as n^6/n^2, or as $n^4 : 1$, and these, acting on arms n times as long in one as in the other, produce moments as $n^5 : 1$; that is, in the same proportion as the torsional rigidities, and so the angles of deflection are the same in the two cases.

If, however, the length of the beam only is changed, and the attracting masses are moved until they are opposite to and a fixed distance from the ends of the beam, then the moments of inertia will be altered in the ratio $n^2 : 1$, while the corresponding moments will only change in the ratio $n : 1$, and thus there is an advantage in reducing the length of the beam until one of two things happens, either it is difficult to find a sufficiently fine torsion thread that will safely carry the beam and produce the required period—and this, no doubt, has prevented the use of a beam less than that

* 'Phil. Trans.,' 1798, p. 469.

† 'Comptes Rendus,' 1837, p. 697.

‡ 'Phil. Mag.,' vol. 21, 1842, p. 111.

§ 'Comptes Rendus,' vol. 76, p. 954; vol. 86, pp. 571, 699, 1001.

half a metre in length—or else, when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an increasing effect on the suspended balls at the other end of the beam, that the balance of effect begins to fall short of that which would be due to the reduced dimensions if the opposite ball did not interfere.

I showed, in the paper already referred to, that when the attracting balls have been brought as near to the *equatorial plane*, or plane perpendicular to the length of the beam, as they are to the plane of the beam, so that the line joining them makes an angle of 45° with the beam, that is that the azimuth is 45°, the ultimate sensibility is still further increased by shortening the beam to half the length that would bring the ends opposite the attracting balls. After that the sensibility very slowly begins to fall.

Since, with such small apparatus as the quartz fibre seemed to make practicable, it is easy to provide attracting masses which are very large in proportion to the length of the beam, while with the usual long beam relatively small masses must be made use of, it is clear that much greater deflections can be produced with small than with large apparatus. For instance, to obtain the same effect in the same time in an instrument with a 6-foot beam that I was able to realize in my preliminary apparatus, in which the beam was $\frac{5}{8}$ inch in length, as seen from above, with attracting balls 2 inches in diameter, it would be necessary to provide and deal with a pair of balls each 25 feet in diameter, and weighing 730 tons, instead of about $1\frac{3}{4}$ lb. apiece. There is the further advantage in small apparatus that if, for any reason, the greatest possible effect is desired, attracting balls of gold would not be entirely unattainable.

The use of attracting balls which are themselves very large compared with the beam length makes it convenient to hang the beam in a cylindrical tube, instead of in the long box almost universally employed hitherto. Several advantages follow from this. In the first place, if the beam is hung centrally, neither the gravitational attraction of the tube nor any minute difference of potential between the tube and the beam and its accessories, produce any effect. In the second place, the attracting balls may be carried round outside the tube through a complete circle, and yet be placed but little further from the attracted balls than would be necessary if no intervening tube existed. For this purpose they are conveniently supported by a common metallic structure, symmetrical in form, about the axis of the tube, and able to rotate about this axis also. If, following the usual arrangement, all four balls are on one level, there are obviously two planes, one containing and one normal to the beam, in which the centres of the attracting balls may be placed so as to produce no deflection. At some intermediate position the deflection will be a maximum. The use of this position has the obvious advantage that, besides the fact that this gives the greatest effect, the accuracy with which the angle of azimuth is measured is of little consequence, the geometrical measurements of real importance being the distance between the centres of the large balls, the corresponding distance between the centres of the small balls, and the angle of deflection. This is all the more important since it would be extremely difficult to make a really accurate determination of the azimuth, whereas the other three quantities can be measured, as will appear later, with the highest degree of precision.

It will be evident that, as the size of the attracting balls is increased with the object of increasing the deflection, their action on the opposite suspended balls increases in a very high ratio, so that very soon a practical limit is reached, beyond which any increase of size produces an insignificant effect. For instance, if the distance between the centres of the attracting balls is five times the length of the beam, and they are set at the angle (58° 20′) at which their action is a maximum, the counteracting couple due to the far ball is $\frac{4}{9}$ of that due to the near one, so that the resultant couple is only $\frac{5}{9}$ of that which would be produced if the attraction on the remote end of the beam could be annulled. This, in effect, I practically accomplished by arranging the two sides of the apparatus at very different levels. In this way, if only the exact position of the balls can be determined, or rather their co-ordinates can be ascertained with degrees of accuracy in proportion to their importance, then the arrangement is eminently suitable for the purpose of finding the gravitation constant.

The preliminary apparatus that I made on this principle in 1889 worked so well, even under the unfavourable conditions met with at South Kensington, that I felt satisfied that an instrument built on the same lines, but in which the necessary geometrical measurements could be made, would enable me to make a more accurate measure of the Newtonian constant than had been considered possible hitherto. I even felt satisfied that it could be determined with an accuracy of 1 in 10,000; and this extreme degree of precision I now feel certain may be attained by a skilled experimentalist, if the very small modifications suggested by my recent work, which are described at the end of this paper, are adopted, and if, above all, the experimentalist, whoever he may be, has time and place at his command, and is not driven by necessity to steal as much time for observation from his holidays and nights as his physical strength will allow.

In the design of any apparatus, it is necessary to have some definite idea as to the degree of accuracy which is to be aimed at, so that trouble may not be taken in attaining an absurd degree of precision in one part, while some other part is glaringly in defect. The aim which I made, and which my preliminary experiments showed to be reasonable, was one of 1 in 10,000 in the result; for this purpose the large masses would have to be determined

	to	1	in	10,000;
times	,,	1	,,	20,000;
some lengths	,,	1	,,	20,000 about;
other ,,	,,	1	,,	10,000 ,, ;
an angle	,,	1	,,	10,000.

I gathered from conversation with some physicists of note, whose judgment and experience I fully appreciated, that there was some doubt whether I was doing right in persisting in making small apparatus where absolute determinations were the object, for though I had clearly enough shown that so far as sensibility and constancy of deflection and period were concerned an advantage could so be obtained, it did not at all follow that I should be able to determine the geometry of small apparatus with sufficient accuracy. Of course, as the apparatus is made smaller, this difficulty necessarily increases.

If in the apparatus upon which I have finally decided the angular deflections and squares of the periods can be determined with greater proportionate accuracy than the masses or lengths, or lengths squared, as the case may be, then I have gone too far, and the apparatus is too small, but if, as I expect to satisfactorily prove in this paper, my geometry and weighings (of course, the latter) are well in excess of the deflections and squares of the periods in point of accuracy, then I maintain that I am justified in having acted up to my principles, even though I did so in opposition to the views which I heard expressed.

There is one point referred to (p. 258), but not sufficiently in detail, in my paper already quoted which I should like to develope, more especially as Professor POYNTING* has noticed it, and has, I think, agreed with my conclusion. At the same time I owe to him the discovery of a mistake which I made which led me to attribute too high an importance to the advantage of smallness from this point of view. What follows is the result of a discussion, which I took the opportunity of entering upon while travelling recently with Professor POYNTING. The point is, that the disturbances due to convection are likely to be relatively of less importance in small than in large apparatus, even though the period is maintained the same. As convection disturbances are those which are the last and the most difficult to avoid, and as I feel sure that they set the limit to the accuracy that is obtainable in this experiment, and that discrepancies attributed to silk, or even to quartz fibres, and to other causes, are in many physical investigations simply due to convection, I think that too much attention cannot be given to this part of the subject.

Let there be two pieces of apparatus, precisely similar in all respects, but with the linear dimensions in one n times those in the other, then when the pieces of apparatus are set up, they are subject under the best conditions to infinitesimal variations of temperature from the outside of two kinds; in the first, the surrounding space may not be uniform in temperature, it may be hotter on one side than on the other; in the other, the temperature, whether uniform or not, may slowly change from day to day.

In the first case the instruments may be considered as being placed in a region which would, but for their existence, possess a constant but very small temperature gradient. If an instrument be placed in such a region, the temperature gradient in the instrument will be also constant in certain cases, and will depend simply on the conductivity for heat of the material of which it is made and of the medium in which it is placed, but it will be independent of the linear dimensions. Further, whatever form a pair of similar instruments may have, the gradients at corresponding points in

* 'Phil. Trans.,' 1892, vol. 182, p. 601, and "The Mean Density of the Earth," p. 107.

each will be independent of the linear dimensions, and so the temperature differences of corresponding pairs of points or of the two sides will be proportional to n. In consequence of this difference of temperature the included air will be warmer on one side than on the other and will circulate. The linear velocity of circulation will depend upon the difference of pressure between the ends of the upcast and downcast sides divided by the resistance due to viscosity, *i.e.*, in such cases as we are concerned with, where the pressures and velocities are infinitesimal, and practically all the energy is expended in overcoming viscosity and none in imparting energy of motion to the gas.

The difference of pressure varies as the height multiplied by the difference of temperature, or as $n^2 : 1$. The effect of viscosity is proportional to the length of the channel, and inversely as its area; it varies, therefore, as $n^{-1} : 1$. The velocity of circulation will vary as n^2/n^{-1}, or as $n^3 : 1$. In order to ascertain what disturbing effect this movement may have upon the suspended part of the apparatus, we may either consider that the force depends upon the product of the area into the velocity gradient or rate of shear of the surrounding air, *i.e.*, that it is proportional to $n^2 \times n^2$, or n^4, in which case, since the force is to be multiplied by an arm also n times as long in order to obtain the couple, this becomes n^5; or without considering the velocity at all we may consider the suspension as part of the boundary of the gas receiving its share of the drag which is felt by the surrounding tube. The proportion must be the same in the two cases. The force causing the drag is proportional to the difference in temperature of the air columns multiplied by their area, or to n^4, and, therefore, the drag on the suspension varies as n^4 and the couple produced as n^5, as before. From this it would appear that no gain or loss results from a diminution of size. It must, however, be remembered, that, as apparatus is made larger, the three-fold increase in velocity in the air-current may well bring it up to such a value that its square can no longer be considered inappreciable. When the velocity is sufficient for the effect of impact to be felt, then the couple will follow a law depending upon a higher power of n than the fifth which, with increase of velocity, will approach the eighth power of the linear dimensions.

I do not anticipate with my design, which with its double tube and protecting screens is eminently favourable for the attainment of a uniform temperature in the inside, that the air velocity will ever approach that in which the square becomes appreciable, so that in a suitable underground observing room I should not expect any loss of definiteness to follow a moderate increase of size; nevertheless, I should feel doubtful as to the result if the dimensions were increased inordinately. Practically, however, smallness has a very great advantage, owing to the length of time which must elapse between the carrying out of any operation in which the apparatus is handled or otherwise warmed by manipulation, and its acquiring such a steady state again as to be fit for the observer to make the delicate observations of the movements of the suspended system. I have even considered three days to be necessary for my small apparatus to be ready for observation of deflection and period after making the micrometric observation. This, in fact, corresponds, but not exactly, with the second case mentioned above, where a gradual change of temperature is going on in the surrounding space; those parts of the apparatus that are massive will lag behind in temperature more than the lighter and thinner parts, and, as was pointed out by CAVENDISH, this is especially the case in apparatus for measuring the Newtonian constant of gravitation. The large lead balls are sure to be hotter or cooler than the light rectangular box, and, when hotter, by warming the side of the box near to them they set up a circulation, which, in the apparatus of CAVENDISH, produced an appearance of attraction.

If it is supposed that after all has acquired a uniform temperature a slight change occurs in the surrounding space, then the asymmetrical store of heat will, in the case of a large apparatus, be n^3 times as great as in the other. As before, the conductivity will be n times as great, so that an asymmetrical distribution of temperature will be n times as great, and will last n times as long in the large as in the small apparatus.

Before I come to describe the apparatus which forms the subject of the present paper, I wish to explain why I have employed what may appear objectionable, viz., mixed units. I applied to Mr. CHANEY, at the Standards Office, for his opinion, as to the limit of accuracy with which he could verify certain lengths and masses. The lengths upon which the accuracy of the whole research would depend were to be of the order of 1 inch and 6 inches. If I could, as he considered certain, have them determined more accurately in relation to the standard 1-inch than I could in relation to the centimetre, it would be preferable to have the main dimensions of the apparatus set out in terms of the inch, and for construction in England there were practical advantages in adopting the inch system. On the other hand, the cathetometer that I used (Cambridge Scientific Instrument Co.'s), and the screw micrometer (ELLIOT), both of which were required to make measures of only secondary importance, were divided in centimetres. I have therefore had to make use of both kinds of measures, but have retained the inch as my standard. With respect to the masses, no difficulty could arise in obtaining the necessary accuracy, whether pounds or grammes were used. Having gramme weights I was led to make all the weighings in grammes, except where, owing to an insufficiency, I had to make up with a standard 7 lb. and 4 lb. weight belonging to the South Kensington Museum. These were determined in grammes, and expressed as such. Circumstances have therefore compelled me to carry out my experiments on the inch gramme second system, and this I have done, finally converting the values for G so found into the C.G.S. system, by multiplying by the number of cubic centimetres in a cubic inch. $(2\cdot53995)^3 = 16\cdot3861$.

As the suspended masses take up slightly different positions according as they are attracted by the large balls in one or the other direction, I was most careful in the design to arrange that the apparatus, with the exception of these balls, should be one figure of revolution about the suspending fibre as an axis. With the hope of obtaining very perfectly conducting and uniform cylinders, both for the outer case and for the central tube, I ascertained what sizes of Elmore tube would be obtainable, and thus determined the actual and final dimensions of the apparatus. When this was too far advanced for change to be possible, the Elmore Company informed me that they could not supply the sizes previously settled, and so I had to be content with a piece of thick triblet-drawn brazed copper tube for the centre, and a thick brass casting for the surrounding case. The experiments show that no appreciable disturbance has arisen owing to any want of perfection in the tube. The casting was turned inside and out without being moved from the face plate, and, except in conductivity, is as perfect as pure copper. In order to keep the gravitational symmetry round the axis as perfect as possible, I had holes drilled in the massive base round the levelling screws, so as to remove as much metal as they added. The important dimensions on which I finally decided were :—

Distance from centre to centre of large balls *in plan*, 6 inches or 4 inches.

Distance from centre to centre of small balls *in plan*, 1 inch, about.

Diameter of large balls, $4\frac{1}{4}$ inches or $2\frac{1}{4}$ inches.

Diameter of small balls, ·2 inch and ·25 inch.

Difference of level between upper and lower pairs, 6 inches.

With these settled the rest of the design of the apparatus shown in figs. 1–15 followed naturally enough. I think it most convenient first to describe the apparatus in moderate detail, without going into the reasons why I decided upon each particular, and afterwards to show how the design accomplishes all that is needed for an accurate determination of G, the Newtonian constant of gravitation.

The Apparatus (Plate 1).

Fig. 1 is a vertical section through the centre of the apparatus, the window alone being in elevation; fig. 2 is a sectional plan through *aa*. Taking the structure first, B is a massive brass base, turned on both sides, carried by three levelling screws with lock nuts. C is the outer brass cylindrical casing screwed to the base B and acculately turned as already mentioned. L is a turned brass lid mechanically fitting C, on which it can be made to turn by the action of the train of wheels WWW. The edge of the flange is divided in degrees, and can be read to $\frac{1}{10}^{\circ}$ upon the vernier V, fig. 3. Two tubular pillars PP are fitted into holes diametrically opposite to one another and 6 inches or 4 inches apart, according to the size of ball that is to be used. The heads of these pillars are shown half size in figs. 4, 5, 6, where it will be seen that at angles of 120° there are three radial V's forming a geometrical clamp with either ball support. Also that on just raising the latter and giving it a rotation of 60° it can be let down through the tubular pillar. As seen, the large balls hang from these geometrical clamps by wires, but into these particulars and into the details connected with the construction of the balls I shall enter later. The central tube T is held accurately in its place by a cylindrical fitting and the hollow screw S. This tube, up to the window just above the lid, is made of thick copper; at the window level it is united by the window casting to the upper tube of the same size, which is made of brass, and this carries at its upper end the torsion head surmounted by the bell jar J with a central stop-cock. The torsion head admits of a variation of level of about 2 inches and of horizontal adjustment by means of three screws. The window casting forming the centre of the tube does not touch the lid, there being a space of about $\frac{1}{20}$ of an inch between them. The equality of this all round is an excellent test of the accuracy of this part of the construction. The window is shown half size in figs. 8, 9, and 10. Fig. 8 is a front view, the upper part being in section, fig. 9 is a side view, and fig. 10 a section through *aa*. The thick cylindrical casting is cut through front and back so as to form two flat square faces FF 2 inches in the side each, and over these 2 inches the casting is cut right through, forming a square chamber in which the beam mirror hangs, and certain operations can be carried on. Four milled heads h_1, h_2 are employed in making the transfer of the smaller balls to and from the beam mirror, of which an enlarged view is shown in fig. 7. This operation is performed as follows: the two heads h_1 are fixed to the same cross axle, and when turned through a right angle cause two arms with V notches at their ends to pick up the beam (fig. 7) by its upper cross arms. In this way the beam can be raised or lowered a little or let down so as to hang from its torsion fibre. The small balls hang by quartz fibres from the hooks and eyes seen in fig. 7. When not on the beam these hang by their eyes from the points projecting from the cranked ends of the pins operated by the heads $h_2 h_2$, which can be turned or pushed in or drawn out. By combining the movement of the heads h_1 and h_2 one of the hooks and eyes can be transferred to the V at the end of the upper arm of the beam mirror resting there by its hook. In the same way the other one is transferred. To prevent risk of the tipping of the beam and fracture of the torsion fibre during this operation, a weight is first hung on to the lower central hook of the beam and removed when the double operation is complete. The ends of the mirror have very fine V grooves ground in them, so that the quartz fibres hanging from the hooks may lie in these grooves and so be held definitely in position, both with respect to their distance apart and circumferentially with respect to the mirror. A cylindrical counter-weight K, fig. 7, of known very small moment of inertia, but of exactly the same weight as the small balls with their hooks and fibres, can be hung upon the central hook of the beam, when the balls are removed to the side hooks, so that the fibre may be stretched to the same extent and therefore have the same torsional rigidity when the periods are being taken with or without the small pair of balls.

A series of windows are provided to fit upon and make an air-tight joint with the plane-faces F F. Two are mere squares of plate-glass of the exact size needed. These are used to protect the freely hanging mirror from draughts when observations are made upon it with the cathetometer. The window, fig. 11, is made of brass, electro-gilt, with a small aperture just large enough to allow the telescope T, figs. 18, 19, and all parts of the scale S, figs. 18, 21, to be seen from all parts of the mirror. The outer face of this window is covered with a plate of glass optically worked by HILGER, held in place by soft wax. The top and bottom sectors and the faces F F are smeared with vaseline to make an air-tight joint when the window is in position. The window, fig. 12, is made of brass, electro-gilt; it is similarly fixed in position behind the mirror. A brass tube, lightly filled with cotton-wool, screws into this window on one side. The window shown in vertical section, fig. 13 and in plan, fig. 14, is made of brass with a flat tubular opening with rounded ends. This enters the rectangular chamber and rests against the faces F F, which have been cut away at their lower part sufficiently for this purpose. The inner end of this tube is covered with a naturally cleaved thin film of mica, which enables the two quartz fibres hanging from the freely suspended mirror to be seen by two high-power microscopes whose noses penetrate into the flat tube without allowing them to be blown about by draughts. The use of mica for this purpose is essential. Mr. CUNYNGHAME had previously shown me that the definition of a good telescope, which is absolutely destroyed by window glass held in front of it and impaired by any but perfect optically worked glass, is not affected by a leaf of mica, even though it may be bent or be apparently irregular. In the same way, the apparent position of anything seen by a microscope is altered if a piece of ordinary cover glass is placed between the two at some distance from the object, besides which the definition suffers. A thin leaf of mica in no way affects the definition or the apparent position, and so the distance apart of the fibres measured by the microscopes, as will be described later, is the true distance, which it never would be if cover glass were employed. This distance must be measured with the mirror freely hanging so that it may be the same as it is when the deflections, etc., are being observed.

Resting on the base B, fig. 1, are four india-rubber discs I R, with large central holes, their object being to form a soft cushion for the lead balls M M to rest upon when not suspended or to fall upon in case of accident.

In the same way I have provided a safety catch and recovering device in case the small balls should fall down the central tube. When the mirror is suspended and has been adjusted with its torsion fibre axial, the loss of time that would ensue if the little balls could only be recovered by moving the central tube is so great that some contrivance of the kind is necessary. At first I merely had some cotton-wool at the bottom of the tube, and fished for the little balls with an india-rubber tube let down through one window opening. On sucking air through the end with the mouth, the balls could generally be picked up and drawn out attached to the lower end of the pipe. My present plan is less precarious. W is a piece of wood loosely fitting the tube. On this there is half an inch or so of cotton-wool on which is a disc of wash-leather just fitting the tube. A piece of thread long enough to reach beyond the window is fastened at one end to the piece of wood and at the other end to a small fragment of iron wire. The thread and wire rest upon the wash-leather, and to make sure of this a second cylinder of wood is let down to press all in place. In case of accident to the little balls, a magnetized tuning-fork is let down the tubes by a piece of string, and the iron wire pulled up. It is then easy, by pulling the thread, to bring the wash-leather to the window level and so to pick out the little balls with forceps.

Fig. 15 is a vertical section of the innermost of the series of screens employed to protect the apparatus from variations of temperature. I could not at first believe that these would be required, but each additional protection of the kind has certainly improved the constancy of behaviour of the apparatus, and I have now no doubt as to the necessity for their use. t_1 is a brass tube with inner and outer ledges split into two halves, so as to fit on to the upper part of the window casting shown in chain lines; t_2 is a plain brass tube reaching nearly to the top of the central tube, and t_3 is a third brass tube, with an internal ledge resting on t_1. This is large enough to clear the milled heads h_1, h_2. An opening is made in it large enough to allow the telescope T and all parts of the scale S to be seen from all parts of the mirror. There is also a small hole in the back, through which the tube of the window, fig. 12, can be screwed. The screen tube t_3 is just clear of the lid and the window tube.

To protect the whole instrument from variations in temperature it is completely surrounded by the octagon house, of which a horizontal section is shown in fig. 22. It is double-walled, and is made in two halves of $\frac{3}{8}$-inch pine boards, separated by a space of 1 inch. This is filled with cotton-wool. The top is flat, double, and packed with cotton-wool in the same way. The two halves slide together upon the table on which the instrument is placed, and meet, completely enclosing it, with the exception of a small hole in the centre of the top, through which a cord, the use of which will be described later, can pass; of a narrow slit in the front, through which the scale and telescope may be seen from the mirror; and of two small apertures through one of which the vernier V may be seen by the aid of the small telescope t (figs. 18, 19), the other admitting of the driving wheel D and air tube. The connecting wire between D and the wheelwork above lies in the narrow space between the inner and outer boards and the two styles which separate them.

By way of illustrating the state of steadiness to which I have reduced the air in the central tube, I may give the result of a calculation made in the case of Experiment 8. In that experiment the points of rest would have been disturbed by one unit if the air in the tube had been moving round at the rate of one turn in six weeks, *i.e.*, at such a rate as to blow past the balls at a rate of 1 inch in $13\frac{1}{2}$ days. This follows immediately from the torsional rigidity, decrement, period, and angular value of one division. No uncertainty so great as this appears in the mean deflections obtained during the night.

The Laboratory and Accessories (Plate 2).

The apparatus is set up in the vaults under the Clarendon Laboratory at Oxford, to fit which, in fact, it was specially designed. I cannot sufficiently express my obligation to Professor CLIFTON for giving up to me entirely for four years this very perfect observing room, for not only was I able to make my observations under specially favourable conditions, but I have had the advantage of having at hand the resources of his splendidly equipped laboratories, and of being allowed to make any use of them that I desired. I feel that Professor CLIFTON's kindness in the matter is the greater as I have no claim upon him whatever, and I can only hope that in so far as my work carried out in his rooms may represent progress in practical physics, he may feel justified in having sacrificed to this end his best observing quarters.

The vault is a double one, of which the southern half is shown in plan in fig. 18. This is separated from the northern half by two piers. The entrance is by a door at the east end of the northern half. The two tables, A_1, A_2, which Professor CLIFTON had built for the purpose of the experiment in the positions shown, are of his standard pattern. The top, made of one slab of slate, rests on a large block of freestone, and this is supported by three walls of brick set in cement, forming an H. The instrument surrounded by the octagon house is placed upon the table A_1. On the table A_2 is arranged a large astronomical telescope T, by Cooke, of York, by means of which the scale is read by reflection from the mirror. The great focal length and the perfection of the object glass are necessary to obtain sufficient magnifying power to be able to read with certainty to $\frac{1}{500}$ inch on the scale; the large diameter of 4 inches has the advantage of giving a large field of view, which is almost essential in taking rapid transits. Moreover, telescopes of the same perfection of construction and length are not immediately obtainable of smaller diameter. This telescope is supported by two cast-iron standards, each with its own travelling V, of my own construction, which give absolute steadiness, being geometrically designed. In this way a considerable range of height can be obtained in case it is wanted. The small telescope t, for reading the vernier V, by which the angular position of the lid and of the large balls M M is determined, also stands on the table A_2. Besides the telescopes, a pulley-wheel p_1 rests upon the table, and a driving-wheel d is clamped to it; p_1 is pulled by a stretching weight, so as to keep the blind cord b passing round the other wheels p_2, p_3, and fastened to the go-cart g in a state of tension. The cart is a beautifully executed specimen of part of a "natural philosophy set" of the last century, and was lent me by Mr. G. S. NEWTH. It runs on the wooden framework, which is wedged into the recess at the east end of the room, between a pair of rails made of angle brass. It carries an albo-carbon lamp, so that the flame can be brought behind any division of the great scale S, which may be seen in the telescope T by reflection from the mirror. The flame is turned down very low to avoid heating the room unnecessarily. I generally set it so that the flame is about $\frac{1}{4}$ inch wide and $\frac{1}{2}$ inch high. The driving-wheel D is made with two heavy projections not shown in the figure, to give it considerable moment of inertia, and with the handle at a distance of an inch about from the axis. Any motion given to it by hand is therefore less likely to be subject to jerks than it would be if unweighted. A very light cord rests loosely round this pulley, is supported by an arm of wood projecting from the other end of the table, is supported again on the edge of the other table, and then lightly passes round the little wheel D, figs. 1 and 2. This rests upon the table, and is kept from moving about by a weighted foot. Two pins fastened into the wheel D engage in a hole and slot in the cross-piece y at the bottom of the hanging wire b. Thus when D is turned the motion is communicated to the wheel-work WWW through the light and loose cord, the wheel D, and the cross-arm and wire. The only kind of force between the wheel D and the wire is by the construction a couple, and this, owing to the high ratio of gearing in the wheel-work WWW, need be only very small to give motion to the lid L. The friction due to the great weight of the balls M M, and of the lid, is largely reduced by hooking to the two rods R R screwed into the lid guys joined to a cross-bar above the bell-jar, which there hangs from a single line passing round the centre one of five wheels secured to the arch, the edge of this being exactly above the axis of the instrument. The line then passes over a second wheel close to the west wall of the vault, and carries two weights each exactly equal to one of the balls, and an extra weight to partly balance the weight of the lid. When the handle of the wheel d is turned the lid slowly and almost insensibly creeps round, and no tremor appreciable with ordinary apparatus is communicated to the suspended mirror. Owing to the extreme sensitiveness of the apparatus, and the very great magnifying power, a high period tremor is set up in the mirror, about equal in amount to that caused by ordinary traffic in St. Giles', about a quarter of a mile away. This dies away very rapidly, and I am unable to trace any anomaly to this cause. The corner of the vault, in which the instrument is placed, is screened off from such small variations in temperature as my presence and the small gas flame produce, by two double partitions of felt, $f_1 f_1$, $f_2 f_2$. Furthermore, the vault itself is protected from variations in the temperature of the air, in the long underground passage by which it is approached, by two felt curtains some distance apart.

Slits and holes, no larger than are necessary, are made in the partitions $f_1 f_1$ to allow the scale and telescope T to be seen from the mirror, the vernier V to be seen from the telescope t and the light string to pass through. These partitions are temporarily lifted out of the way when a certain beam l_1, shown in position in fig. 19 but not in fig. 18, is being used. The two beams l_1, l_2 have their upper edges planed true, and are so supported by levelling screws that their upper edges form one level straight edge. These are employed when the distance from the scale to the mirror is being determined in the manner to be described under the heading "The Steel Tape and Accessories." The beam l_2 I leave in position permanently, but as l_1 would be in the way, it is only put up when required.

The Large Scale.

The large scale is etched on a piece of plate-glass 9 feet long, 6 inches wide, and half an inch thick. The divisions are 50ths of an inch, and there are 4800 of them. I made many experiments to find the most suitable kind of scale and thickness of line to suit the mirror which I had to use. I shall return to this point when I explain the advantages that I have gained by the use of the curious form of beam mirror. It is sufficient to state now that the divisions are black upon a clear ground, and that the thickness of the lines is greater than at first anyone would be likely to think suitable, being about $\frac{1}{250}$ of an inch. The method by which the scale is held rigidly and definitely in place but without strain, is illustrated by the isometrical projection in fig. 23, which shows one end only. Z Z are a pair of gun-metal castings screwed to the wood frame which is securely wedged into the recess at the east end of the vault. u is a brass rod passing through a hole in each casting and able to be clamped by a screw at each end. v is a casting with a cylindrical piece turned at each end and exactly the same length between shoulders as u is. This rests at each end upon a levelling screw, and can be clamped by pinching screws. At the other end of the scale, 9 feet away, there is an identical construction. Two plates of glass, the back one of which is a dummy, the front one only being divided, rest upon the cylindrical projections of v, being definitely held in position by the V notch shown, which of course is only at one end. The glass plates rest also against the shoulders and against the ends of u u and are kept in contact by the action of a bent piece of brass at each end which lightly presses them towards one another. The glass plates are therefore geometrically clamped, each resting on seven points, the one in excess of six being introduced to counteract the one degree of freedom which the flexure of so long a plate introduces. The middle of the front plate is silvered at the back up to near the line of divisions, which is $1\frac{1}{2}$ inch from and parallel to the upper edge. The levelling screws enable me to bring the upper edge and therefore the line of divisions truly level, and this is finally tested by observation with the telescope and swinging mirror. The rods u and v are then gently worked in or out as needful until an observer with his eye at the window of the apparatus in the place of the mirror sees the window reflected from the clear glass on the line of divisions. The silvering of the middle of the scale is not absolutely necessary, but it enables one more quickly to recognize the position of objects placed against the lower part of the apparatus and so acts as a finder. When the scale is thus adjusted and has been placed with its divisions on a level with the mirror, a division not far from the middle will be the point at which a perpendicular dropped from the centre of the instrument will cut the scale. The eight pinching screws are then clamped and this division is recorded when its position has been more accurately determined by the use of a small telescope in the place of the eye. The division 2260 was the perpendicular reading in all the experiments made up to the present. The dummy was provided partly to absorb radiation from the flame and so to protect the working scale from heating, but mainly so that I should have a glass plate ready to divide myself without loss of time in case of accident to the working scale. This happily has not occurred.

I calibrated this scale by reference to an American steel scale divided into 50ths of an inch, the uniformity of which I had previously tested. This steel scale became for the purpose of the angular measurements the standard to which everything was referred. For this purpose its absolute value is of no consequence; all that matters is its uniformity. A long board was supported so that its upper surface was everywhere level. Sheet lead strips were rolled until they were just thicker than the steel scale and a double row were laid upon the level board. The glass scale was made to rest upon these with its face downwards and the steel scale was slipped underneath, so that the glass and steel divisions should be superposed. An erecting eyepiece was placed on a stand above the glass and was used as a reading microscope. Every tenth division was observed, and the 480 corrections were entered in a book, and were also plotted out on an enlarged scale, so that an error of $\frac{1}{500}$ inch should be represented by $\frac{1}{10}$ inch. From the irregular curve drawn through all the points the calibration error of every scale reading was afterwards ascertained. In order to determine the circular error, the true distance in scale divisions between the mirror and the scale was measured according to the plan to be described on p. 17. A large number of values of the circular correction were calculated from the expansion for $\tan^{-1} x$ and tabulated in terms of scale divisions. It was necessary to include the term $+\frac{x^5}{5}$ as at the ends of the scale this amounted to half a division, while at 1800 divisions on either side of the perpendicular reading it was one-tenth of a division. The perpendicular reading 22,600 being invariable, these corrections were plotted on the same sheet as the calibration errors, thus the two corrections could be taken out simultaneously for every reading which was thus converted into the reading that would have been obtained if every division subtended the same angle at the mirror. In this way the time and labour that are ordinarily required in finding the angles corresponding to scale divisions and in correcting for calibration are reduced to a few seconds for each, and error is almost impossible.

The Overhead Pullies.

The overhead wheels are eight in number, and are all of the same size. Five are over the instrument, and three close to the west wall. As already stated the edge of the middle one, which has a round groove in it, is exactly over the centre of the apparatus. Those on either side have flat-bottomed grooves, and they can be placed either 6 or 4 inches apart, according as $4\frac{1}{4}$ or $2\frac{1}{4}$-inch balls are to be used. Outside these, and the same distance apart as the screwed pillars R R in the lid, are two round grooved wheels. The central wheel of the set near the west wall is round grooved, and the other two, which can be set either 6 or 4 inches apart, have flat-bottomed grooves. The purposes which these wheels serve are numerous and important. In the first place the middle ones are employed to reduce the friction of the lid, as has already been explained. In one of the cathetometer operations the lead balls and the tops of their supporting pieces have to be observed in order to find the levels of their centres when they are hanging out of sight inside the apparatus. At the same time the lid must be raised, and held out of the way; but it cannot conveniently be removed altogether. To accomplish this, steel bands are passed over the flat-grooved pullies, and are each of them pinned to the ball holder at one end and hooked to an exactly equal counter-weight at the other. The balls can then be raised, and will remain hanging at any level at which they may be left. Two cords are hooked on to the eyes of the pillars R R of the lid, and after passing round the outermost pullies above, converge, and then, becoming single, pass over the central pulley next the west wall. There, a weight exactly equal to the lid, serves to counterbalance it, so that it will remain suspended in a horizontal position at any level. The height is so chosen that one of the ball holders is just above the pillar on its side, while the other is just below the lid on the other. The balls are then at the same level, and their upper portions can be seen just above the edge of the casting C. The balls under these conditions hang quite freely, neither touching the instrument nor being deflected by contact between their wires or steel bands with the lid. The steel is necessary to give definiteness to the positions of the lead balls during the cathetometer measures as if they were to hang from cord the twisting and uncertain and variable stretching would make accurate measurement impossible. The central overhead wheel alone is employed in placing the small balls in position. I used at first, after fixing them to their own fibres and hooks, and measuring the distances when hanging from the point of the hooks to the tops and bottoms of the balls, to get them in through the window, supporting the hook by a bent pin held in one hand and passing the fibre over a bent pin held in the other. The process was one of great delicacy and difficulty, but it answered with gold balls ·2 inch in diameter. It was, however, next to impossible with balls of double the weight, as the fibre would not, under such a strain, bend round a pin, a polished steel rod, or anything that I could think of. I had therefore to adopt the plan with the overhead wheel, which has never failed. A pin, with the point bent at right angles to form a horizontal hook, is tied to a piece of sewing silk, and allowed to hang from the central pulley. A weight equal to the ball is tied to the other end. The pin-hook is inserted in the eye of one of the hooks and eyes from which the gold ball is suspended, and pulled up till the ball is over the tube. It is then let down until the eye is opposite the window, when its hook is made to rest upon the point of a large pin held in one hand; by this means it is transferred to the side hook where it is left hanging by its eye, and ready to be placed upon the arm of the mirror when that is in position.

The Steel Tape and its Accessories.

In order to make an accurate determination of the optical distance between the reflecting surface of the mirror and the foot of the perpendicular upon the scale, I have prepared a steel tape to lie upon the beams L_1 and L_2 already described, and two sliders, one carrying an erecting eyepiece or low power microscope, and the other a sliding brass rod.

The steel tape is one of ordinary construction, half an inch wide, and divided on one side in millims. and on the other in inches and eighths. As the lines on this, as is necessary with etched tapes, are thick and raised above the general surface, I engraved fine lines on the divisions—2 inch; 7 ft. 4 in.; 14 ft. 6 in.; 21 ft. 8 in.; and 21 ft. 9 in. After removing the lead slips which had supported the glass scale while it was being calibrated, I laid this scale face downwards on the steel tape, setting 0 of the glass scale upon the first engraved line at 2 inch. The reading in scale divisions of the fine line at 7 ft. 4 in. was then observed to be 4302·5. The tape was drawn back until 0 of the glass scale was over 7 ft. 4 in., and the reading taken for 14 ft. 6 in.; this was 4302·85. The readings for 21 ft. 8 in. and 21 ft. 9 in., taken in the same manner, were 4302·5 and 4352·5. The temperature was 19°·75 C. The calibration correction for the division 4302 of the glass scale is 3·0. Hence the distance in corrected scale divisions from the engraved lines at 2 inch and 21 ft. 8 in. at 19°·75 C. is 12898·85. The glass scale was calibrated in terms of the steel standard at 14°·5 C.; it had, therefore, relatively contracted at the higher temperature. Taking ·000002 as the differential coefficient of expansion, the distance between the engraved line becomes 12898·98 in terms of the divisions of the standard steel scale at any temperature. The sliders have bases made of plate glass, on each of which is an engraved cross line. One carries on two V's the low power microscope, and this, after the tape is placed in position, is arranged with its cross-line over the engraved line on the 2 inch division. The microscope is then made to slide in its V's until a small cross engraved at the centre of the back of the freely suspended mirror is seen through the front window sharply in focus. The microscope is then clamped to its V's, and the slides moved out of position and again set several times, the relative position of the engraved lines being noted. If these are systematically on one side, the microscope is shifted in its V's until repeated settings bring the engraved lines together. At the other end of the tape a corresponding slider is placed with its engraved cross-line over one of the engraved lines at 21 ft. 8 in. or 21 ft. 9 in., and the brass rod is slid forward until it just touches the scale at the foot of the perpendicular from the mirror. The two sliders are then placed upon the original steel scale, from which the glass scale was calibrated, and moved until the fine lines on the end of the brass rod are seen sharply in focus. The distance between the engraved lines on the plate-glass bases, so determined, added to the distance between the engraved lines at 2 inch and 21 ft. 8 in. or 9 in., as the case may be, is the true optical distance from the reflecting surface of the mirror to the foot of the perpendicular upon the scale. This includes the small correction for the reduction in distance owing to the refractive power of the glass composing the mirror and front window. The divisions on the scale are, of course, placed on the side facing the instrument, so that no refractive correction is needed for the scale.

The Optical Compass.

In order to make the horizontal measures of the distances between the wires from which MM hang, and the quartz fibres which carry *mm*, measures which have to be made with the greatest possible accuracy, I had to design a special instrument which was suggested to me by Professor CLIFTON's optical compass. That is an arrangement by which two microscopes can be made to slide parallel to one another. After being simultaneously focussed on the two marks whose distance asunder is required, the frame to which they are clamped is rotated so as to bring them relatively unchanged in position to view a scale divided by lines microscopically fine. In this way the distance is directly transferred to a scale in terms of which it is known. In my case the chief difficulty was to keep the whole apparatus confined within the horizontal limit of $1\frac{1}{4}$ inches, which was all I had liked to allow myself in the design of the apparatus itself. Into this space I had to get (1) a rotating slide to move on the lid round the axis of the apparatus; (2) a focussing slide to move to and from the plane of the wires and fibres; (3) a pair of traversing slides, each to carry one microscope capable of being separated by a fine adjustment and with a motion parallel to the planes of the fibres and wires. It was essential, moreover, that the slides should be very rigid, and that the focussing slide in its traverse should remain upon the same supports to avoid difference in flexure in case there should be any. The geometrical principle was of course followed, each moving piece resting on five independent small surfaces, and free from mechanical constraint. This instrument is shown in figs. 24 to 29. To avoid confusion, the rotating and focussing slides, with scale and micrometer screw only, are shown in figs. 24, 25, 26, in full lines upon the lid in chain lines, and the focussing and traversing slides in figs. 27, 28, 29. The rotating slide R rests upon the lid by means of two curved V's, v_1v_1, resting in a circular V groove upon the lid, and by the flat surface f bridging the V groove at the back. It can, therefore, rotate upon the lid without shake, but no other motion is possible. This piece is made very stiff by the raised rib r round the triangular part, and by the overhanging ledge which extends over its whole width. The rotating slide also carries a micrometer screw S of 100 threads to the inch, with a head divided into 100 parts, and two parallelizing screws, S_2S_2. On the flat surface before S_2S_2, a glass microscopically divided scale stands upon two little glass feet. Full particulars of this will be given after the description of the optical compass. It merely rests against the parallelizing screws and can be moved bodily to the right by the micrometer screw. No slide of any sort is provided, this simple construction, though perhaps less convenient, being far more perfect than any possible kind of slide.

The focussing slide F rests upon R by means of the two V's, v_2v_2, which fit into a straight V groove in R, and by means of the flat surface f_2, which rests upon a planed surface parallel to the v groove. This focussing slide is stiffened by longitudinal ribs above and below the general level, one on one edge, and the other on the other edge. It also carries a focussing screw S_3 of 50 threads to the inch roughly divided on the head. This merely pushes against the tail rib of R, causing the slide F to retreat from the centre of the apparatus. It can be moved the other way by hand, or by a gentle forward pressure on the screw head when it is being turned backwards. As it is necessary to be able to give a fine focussing movement to this slide in two separate positions, about one inch apart, a focussing block b of the required length is pivotted on R, so that it may either remain out of use as shown in the figures, or may be brought under the focussing screw after the focussing slide has been withdrawn. A turn or two is all that is necessary then for the purpose of focussing in either position.

Two traversing slides T_1, T_2 each rest upon the focussing slide by five small surfaces, of which four in each case are due to the long projecting V's v_3 on the front edge, of which the middle parts are scraped so as not to bear upon the longitudinal V groove in the traversing slide.

The fifth point in each is formed by a small friction wheel w, which lies in a recess in the traversing slide, and runs upon a planed surface on T parallel to its V groove. The reason that I introduced a wheel here is, that while a very small vertical uncertainty is of no consequence, I was thus able to cause the whole of the frictional resistance to traversing to lie in the V itself, which is the more necessary as the distance between the ends of the V is necessarily less than the distance perpendicular to it. This is taken advantage of further, for I have arranged that the force that draws the two traversing slides together is produced by a long, very small helical spring of steel lying in a hole drilled in the V's themselves, thus being in the line of friction and producing no tendency in either to depart from its geometrical bearings. For the same reason I have made the fine adjusting screw which separates them act in the same line. This consists of a fine steel screw S_4, fitting rather loosely in its very short nut, carried by T_2 at one end, and with a fine polished conical point at the other, which rests between a little V carried by T_2 and a vertical surface on T_1. The screw and cone piece, therefore, are free from constraint, but simply push the traversing slides apart in the same line where the friction and the opposition of the spring act. Thus, when the screw is turned forwards, the slides simply separate to a minute extent, but have no tendency to lose their parallelism. Each traversing slide is furnished with three grooves cut away so as to support a microscope lying in any of them at each end only over small surfaces at 45° on either side, thus allowing it two movements, one of rotation, and one fore and aft. The latter is prevented by the use of focussing collars C, C, which slide stiffly on the microscopes and are so adjusted that when the two microscopes are alternately placed in the same groove and pushed up to their focussing collars they will each be in focus upon the same object. The positions of the grooves are such that the microscopes, when in their symmetrical positions, can be brought upon points distant from one another by 1, 4, or 6 inches, with a small margin on either side of a few hundredths allowed by the screw cone. Each microscope is furnished with a cross-wire and an eyepiece divided scale, one or other of which can be used according to the position of the positive eyepiece. If the microscopes are laid in grooves that do not correspond with one another, they may also be focussed upon points $2\frac{1}{2}$, $3\frac{1}{2}$, and 5 inches apart. If the two traversing slides are made to exchange places, for which purpose the screw cone has an extra nut and bearings provided, then distances of 2, 3, and $4\frac{1}{2}$ inches can be measured also, should any of them be required.

Beyond stating now that the optical compass does a great deal more in the investigation than merely measure horizontal distances between vertical wires or fibres, and that the geometrical and rigid construction makes it possible to work to the full limit which optical definition imposes, I shall not at present explain the details and the order of the operations carried out by its aid. They will come more conveniently under the description of the experiment itself (Operation 9, p. 40).

The Small Glass Scale.

This was made for the optical compass by ZEISS. A strip of plate glass $6\frac{1}{2} \times 1 \times \frac{1}{8}$ inch was divided by lines microscopically fine as follows. A line was ruled at every inch from 0 to 6, and at 2·50 and at 3·50 inches. In addition to these, five lines, $\frac{1}{100}$ of an inch apart, were ruled on either side of the divisions 1·00, 2·50, 3·00, 3·50, 5·00, and 6·00. The five on either side of the zero were by inadvertence omitted, and the zero line was, by some obscure accident, ruled at ·04 instead of at its true place. This, however, was of no consequence, as the 6-inch distance was measured by reference to the divisions ·04 and 6·03, 6·04, or 6·05. The 4-inch distance (not yet wanted, however), by reference to 1·00 and 5·00, and one or two contiguous divisions, and the small distance which was to have been 1 inch about, but which is in reality almost exactly ·9 inch by reference to 2·55 and 3·45. When I was in Cardiff at the meeting of the British Association, Professor VIRIAMU JONES allowed me to measure the absolute distances between these divisions and a few on either side upon his measuring machine. This machine is one of WHITWORTH's ten-thousandth machines, but of more than usual stability, and with a bed long enough to take in bars three feet long. It is provided with a set of these bars increasing by inches from 1 to 12 inches, and with a 2-foot and a 3-foot bar. The glass scale was attached to the upper surface of the tail headstock by being simply pressed down edgeways upon two wafers of soft wax, and it was pressed endways against a third on the index. Safety fingers of wire were also attached to the headstock, but not in contact with the scale, so as to prevent it from falling, if it should by accident get displaced. One of the microscopes of the optical compass was allowed to rest in brass V's bolted to a solid iron casting, which rested on the same slate-topped pier as the machine. The microscope was moved in its V's until one end of the scale was in focus; the tail headstock was then traversed on the bed of the machine until the other end was opposite the microscope. The scale was then moved until this end was in focus, but the process, being only carried out by the fingers, was difficult to perform, as besides fixing the scale parallel to the bed as tested by the focus of the microscope, it was necessary also to see that it remained parallel in the vertical plane, and to adjust this by pressing out or adding to the soft wax wafers by which the scale was lightly held. At first this quadruple adjustment, in which the setting of one right generally put the other three out, seemed as if it would require for its successful accomplishment some mechanical contrivance more under control than the fingers. However, by a happy accident, I succeeded in soon getting the scale so that I could detect no want of parallelism either way with the fairly powerful microscope that I was using. The actual distances were determined along a line about $\frac{1}{90}$ of an inch below the upper ends of the short divisions.

The distances which it was necessary to know with the greatest accuracy, were those from 0 to 6, from 1 to 5, and from $2\frac{1}{2}$ to $3\frac{1}{2}$. These were determined as follows. The loose headstock was traversed on the bed, and clamped when the division at one end of the distance to be measured was on the cross-wire of the microscope. A bar was then put in, the feeling piece put in its place, and the micrometer head turned until the feeling piece was just prevented from slipping, when the reading was taken. The headstock was unclamped, moved, and the process repeated until two or three readings had been taken. The bar was then removed, the loose headstock moved until the division at the other end was on the cross-wire, and a new bar of suitable length put in, and the micrometer turned until the feeling piece was again just held. When the three readings had been taken in the second position, the headstock was set back to the first position, the first bar placed in position again, and three new readings taken; then in the same way three more were taken in the second position. The microscope was not touched at all during the process.

In connection with these measures, the following important details may be referred to. I found great difficulty in setting the loose headstock by means of the high-pitched leading screw, especially as the wheel was almost out of reach, and in seeing the cross-wire in the eyepiece projected upon any division of the scale so as to bisect that division exactly. These difficulties were practically removed by placing the microscope so that its cross-wire was slightly inclined to the vertical in which direction the divisions are ruled, and by setting the ruled lines symmetrically between a pair of microscopically fine specks of dust upon the cross-wire which, with the particular inclination then, just lay on either edge of the line. The width of the line itself was found to be $\frac{1}{5000}$ inch. In this way there was no doubt as to the setting of the cross-wire accurately to a tenth of the thickness of the line. I was not, however, always able, owing to the circumstances to which I have already referred, to leave the headstock set and clamped so accurately in position, that I could detect no want of symmetry in the microscope. In a few cases I left it with a + or − error of one-tenth of a division as estimated by the eye, which error I entered in the note-book at the time, and before I knew what the reading of the Whitworth machine would be. These were taken by Mr. HARRISON, the very skilful assistant in the Physical Laboratory at Cardiff, and he did not know what correction I had entered, or indeed, if I had entered any correction. His reading was then entered into the book also. In this way I hoped to avoid that spurious appearance of accuracy that is apt to result from knowing during a process of adjustment when the last setting has been reproduced. The temperature, of course, was frequently taken, but it only rose half a degree during the day, and to avoid as far as possible differences in temperature in the apparatus, the bars that were used were kept during the measurements upon the bed of the machine. As an example, I give the figures of the middle inch exactly as they were entered. The whole numbers represent $\frac{1}{10000}$ths, and the decimals $\frac{1}{100000}$ths of an inch.

Division on Scale.	Bar used.	Reading and correction.	Reading and correction.	Reading and correction.
$2\frac{1}{2}$	9-inch	315·1 − ·2	314·9	315·0
$3\frac{1}{2}$	8-inch	313·3	313·4	313·4
$2\frac{1}{2}$	9-inch	314·4	314·4	314·6
$3\frac{1}{2}$	8-inch	312·9 − ·2	312·8	312·3 + ·2

Temperature 16°·9 C.

The distance between $2\frac{1}{2}$ and $3\frac{1}{2}$ on the scale found by taking the simple mean of the above figures and subtracting, is ·99983 inch, or allowing for the gradual change, due probably to temperature variation, which seems to have been going on at this part of the day (about 1 P.M.) more particularly, the distance is ·99986.

A determination, made early in the afternoon, of the same distance by direct comparison with the 1-inch bar, the 8-inch bar remaining in the machine, gave as the length of the middle inch of the scale ·99979. Since the bars are not guaranteed to be nearer to the truth than $\frac{1}{10000}$th of an inch, the agreement between the 1-inch bar and the difference between the 8- and the 9-inch bars is better than might have been expected. Meanwhile, I may consider for the present that the true length of the middle inch is known with an accuracy of 1 in 10,000 at least. I have taken it to be equal to ·99980.

The interval 1·00 to 5·00 was compared with the difference between an 11-inch and a 7-inch Whitworth standard bar. Assuming the difference to be 4 inches, this interval was found to be 3·99970 inches. In the same manner the interval between zero (really 0·4) and 6·00 was found by comparison with the difference between a 12-inch and 6-inch Whitworth standard bar to be 5·95996 inches.

The distance from 6·00 to each of the divisions up to 6·05 was measured in the Whitworth machine, and also by means of the micrometer screw of the optical compass. The value of the screw was found in terms of the middle inch of the scale, which had been measured most carefully upon the Whitworth machine. The screw measures were found short by ·145 per cent. Allowing for this the distances were found to be :—

Between	By screw corrected.	Total from 6·00.	Corresponding totals measured in Whitworth machine.
6·00 and 6·01	·010145	·010145	·01015
6·01 „ 6·02	·010105	·020250	·02019
6·02 „ 6·03	·009815	·030065	·03002
6·03 „ 6·04	·009915	·039980	·03995
6·04 „ 6·05	·010095	·050075	·05012

Adding the measures of the intervals ·04 to 6·00, and 6·00 to 6·04, the sum is 5·99994 or 5·99991 according to the value taken for the smaller interval. Mr. CHANEY allowed me to measure the distance from ·04 to 6·04, at the Standards Office, by comparison with the intervals 24 to 30 and 30 to 36 in the standard yard measure. The two measures did not differ by an amount that could be detected and the result was found to be 5·99995 at the temperature 59°·7 F. There was no question, therefore, that the 6-inch distance was known correctly to one part in 100,000.

The distances from 2·50 to 2·55 and from 3·45 to 3·50 were measured by the micrometer screw of the optical compass, and their sum was found to be (employing the corrected value of the screw) ·100125, so that the distance between the divisions 2·55 and 3·45, which are those actually used in all the measures of the horizontal distances between the fibres, is ·89967. There can be little doubt that this is correct to one part in 10,000 and, as a small error in the working length of the beam produces an error of about the same magnitude in the result, the value of G is not likely to be affected seriously by uncertainty in the value of this inch. I did not attempt to determine this inch at the Standards Office, as I found that, owing to the coarseness of the lines on the standard bars and the imperfect optical means, so small a distance could not be measured with a high degree of proportionate accuracy. Should the rest of the experiment ever be carried out with such perfection that a possible doubt of one in 10,000 on this measure becomes of importance (and I see no reason why it should not), then I should have to rely upon a measurement made at the International Bureau at

Sèvres, but up to the present I am quite content with that made upon the Whitworth machine.

The Clock.

The clock, the position of which is indicated in fig. 18, is a Frodsham regulator, which was lent to me by the late Professor PRITCHARD, who took a great interest in the experiment. The present owner has kindly allowed me to retain it until the work is finished. The clock is placed so that it can be seen from the observing stool at the telescope, and is illuminated when necessary by a small incandescent lamp. It is employed to mark time upon a smoked drum, upon which also are marks made by the action of a key at the telescope. I finally determined to employ the chronographic method after seeing Professor CORNU's apparatus. To the lower end of the pendulum I screwed a platinum wire flattened and filed to a rounded edge at its end, the edge being in the plane of oscillation. This passes through a horizontal line of mercury, standing up by its capillarity above a transverse groove in a piece of wood. The end of the groove opens into a large well filled with mercury, so as to retain the purity and the level. The wood on either side of the groove is cut away to an edge, so that mercury dust carried over by the platinum cannot accumulate and give trouble. The wood is so placed that when the pendulum is moving through a small swing of a quarter of an inch only, the time marker actuated by the contact ticks regularly. With the full excursion the alternate marked seconds are then indistinguishable in length. I soldered two platinum wires to the second hand and brought an insulated elastic platinum point over the seconds dial and under the minute hand, so that the second hand should make contact twice a minute. I so bent the wires that at the minute contact should be made again immediately after the pendulum had broken contact, and retained till the end of the first second, while at the half-minute it was made again after the thirtieth second and immediately broken. The minutes and the half-minutes were in this way clearly and differently marked (fig. C, p. 48), and it was thus unnecessary to count more than 15 seconds on the charts. The time markers and the drum to carry the smoked paper were made by the Cambridge Scientific Instrument Company, and are of their well-known pattern. I took out the long heavy wires of the time markers, which, I understand, are made to the order of the physiologists, and replaced them by very small and light styles of copper foil tapering to a point. The two are arranged close together so that the tracing points are not more than $\frac{1}{100}$ of an inch apart. The sheets of paper, 12×19 inches, are most readily smoked when stretched on the drum by pouring a little benzine into the india-rubber pipe which supplies a fishtail burner. The very smoky gas flame which results rapidly produces a deep and uniform coat of soot. The sheets when finished are passed through a bath of very dilute shellac varnish, the strength being such that the smoke does not rub off, but may have numbers, &c., readily scratched upon it with a pointed style. The drum is driven through worm gearing by a P 1 electric motor, by CUTTRISS, of Leeds, the current being supplied by a couple of E.P.S. cells. Mr. F. J. SMITH has kindly allowed me to charge them when required at his laboratory. The same cells are connected up to the two time-marking circuits, and to a small electric lamp placed in the octagon house to illuminate the vernier and divisions close by. This is lighted by raising, and so making the upper contact of the key, which on depression is employed to make the signal marks from the telescope. Owing to the self-indication of the small electro-magnet of the time marker, a considerable spark would be formed at the mercury break in the clock, to the destruction of the contact, if it were not for two electrolytic cells in series, charged with battery acid and with platinum electrodes, which are employed as a bridge across the clock break. This sets up an electromotive force of polarization which prevents any current from passing when the contact is kept broken, but its resistance is so small that the high electromotive force set up at the break is able readily to fall through them, thus practically abolishing the spark. I found this greatly superior in every way to a non-inductive resistance. There is one point connected with this break which I believe to be worth recording. To ensure good contact I amalgamated the platinum point with sodium amalgam, but immediately found that the contact lasted longer, and was more irregular than before. However, I left the point amalgamated for a fortnight, during which it gave more trouble by drawing the mercury out of the trough. I then unamalgamated it by holding it over a candle flame, until I concluded that the mercury was all gone. I did not make it very hot. Since that time the contact has never failed, which it occasionally used to do before. I attribute the improvement to an atomic roughening produced by the penetration of the mercury. Before the point was unamalgamated, the pendulum, as I afterwards discovered, made a second contact with a pool of mercury drawn out of the trough and electrically insulated, which contact mechanically disturbed its period. For this reason Experiment 4 is incomplete, as its time observations are untrustworthy. I took, however, the rigidity of the fibre from Experiment 5, and so completed the calculation.

The Large Balls and their Supports.

One of the difficulties in the preparation of apparatus for measuring the mutual gravitation of comparatively small bodies is met with in making the large balls. CAVENDISH employed large balls of cast lead 1 foot in diameter. Professor POYNTING made the balls in his apparatus of an alloy of lead and antimony, for the sake of the extra hardness, which would make it easier to turn them accurately to form and would render them less liable to subsequent deformation. Though special precautions were taken to avoid cavities, and to obtain homogeneity, the large one was found to act differently in different directions, and he localized a cavity by observations in this way. Afterwards he found the centre of gravity was to one side of the centre by an amount corresponding very nearly with that which he had deduced from his gravitational observations. I do not feel satisfied myself that this affords any proof of the existence of an actual cavity, as a gradual variation of density, such as might easily occur in an alloy, might have produced the same effect in each case. For this reason, when a much higher degree of accuracy is being sought for, I consider that alloys are fatal to success. Professor CORNU has without any doubt avoided any uncertainty as to cavities or uniformity of density, or probably truth of form, by employing mercury aspirated from one pair of spherical hollow cast-iron moulds to another pair so placed as to reverse the attraction. By this means it seems to me everything may be known with more than abundant accuracy, except the actual position of the centres of the spheres, or what comes to the same thing, their actual distances from the centres of the attracted masses. As I explained in my first paper, in my arrangement, the difficult geometrical measurements are almost all made of secondary importance. A small uncertainty in the levels is, as in previous arrangements, of secondary importance, as in this sense they are at a position of maximum effect. A small uncertainty of the angle of azimuth does not matter, for this also is at a position of maximum effect. If there is a small eccentricity of position of the gold with respect to the lead balls, either in the plane of the lead balls, or across that plane, again the effect is infinitesimal, for the departure is from a minimum of effect in the first case, and a maximum in the second. The only measures of serious importance, on the accuracy of which the result directly depends, is the distance *in plan* from the centre of one lead ball to the centre of the other, and the corresponding distance in the case of the gold balls. In the first there must not be an uncertainty of $\frac{1}{3000}$ of an inch, or in the second of $\frac{1}{10,000}$ of an inch. I do not think it would be possible on Professor CORNU's plan to obtain a knowledge of the positions of the centres of the mercury spheres, especially when one is six inches above the other, with anything like this degree of accuracy, and therefore, though with the large apparatus he used, and the proportionally lower degree of accuracy that was sufficient, the plan is most excellent, and answers perfectly, it would not be suitable in the present case. There is a second objection depending upon the magnetic quality of the iron moulds. For though to ordinary tests the beam and gold balls are not affected by magnetism, I have felt that in measurements of forces of such supreme delicacy it is safest to avoid introducing magnetic materials, lest any systematic disturbance should be introduced. I have however, satisfied myself by experiment since putting up the apparatus, that a magnetic force much greater than that due to the earth produces no effect.

The plan that I have adopted seems to me to be free from the objections that I have urged, to be easily carried out, and to be specially adapted to the purpose of exact measurement of the distance in plan from the centre of one ball to the centre of the other.

Mr. MUNRO, who has special experience in accurate spherical work, made the cast-iron mould shown in figs. 16 and 17. The internal hemispheres are turned out so truly that the steel disc used as a template would audibly rattle when placed in either alone, but could not be got in at all when a single strip of cigarette paper was inserted on one side only. The two halves can be screwed together by means of six steel bolts as shown. A $\frac{1}{4}$-inch hole is bored in the centre of one hemisphere and one of $\frac{3}{4}$-inch in the centre of the other. Into the latter an accurately fitting steel plunger was inserted, and when pressed down to the head, was turned at the inner end, so as to complete the sphere. A small hole is drilled on the equator, enlarged almost immediately to a greater size. Into this a brass plug can be pushed. Before being used, the mould is warmed, and the internal surface smoked with a gas flame. Into the $\frac{1}{4}$-inch hole the brass ball-holder *e* is inserted. A number of these were made by Mr. COLEBROOK, of the utmost possible accuracy of the form shown, this being a $\frac{1}{2}$-inch sphere, surmounted by a $\frac{1}{4} \times \frac{1}{4}$-inch cylinder with a shoulder of $\frac{1}{64}$ inch, of such a depth that when pressed home the $\frac{1}{2}$-inch sphere should be tangential to the $4\frac{1}{4}$-inch sphere. Though these ball-holders were made to measure only, their weights were closely alike, being 10·70 grams for each before cutting the slot and drilling the cross hole, and 10·28 afterwards. Since the whole effect of the gravitation of these ball-holders and of the $4\frac{1}{4}$-inch lead balls, all in their ultimate positions, is $\frac{1}{7320}$ in excess of what it would be on the supposition that the whole mass is concentrated at the centres of the lead balls, any doubt as to the amount of the correction, which cannot be so much as one part in a hundred, leaves an uncertainty in the result of about one part in a million, and is of no consequence. I should state here that this correction includes the very small pieces of brass fastened to the lower end of the wires, which with their pins were made to occupy the same volume as the material removed in the slot and cross hole. The brass ball-holder, before being inserted in the smoked mould, was tinned on the spherical surface, and then wiped to remove superfluous metal. The mould was then put together, and the steel bolts, after being well rubbed with blacklead, screwed up as tightly as possible. The mould was then slowly heated over a Fletcher gas burner, until a piece of lead lying upon it began to melt. The brass plug was then inserted in the side hole, and pure skimmed lead was gently poured in through the neck from an earthen pot until it was full. The mould was then lifted on to a cold block of iron, but a large blow-pipe was kept playing on the top of it, the effect of which was that the metal slowly solidified from below upwards. The progress could be followed by inserting a fine carbon rod, or more evidently by watching the contraction of the metal in the neck. It was necessary to add lead from time to time to keep the neck full, and in the case of the $4\frac{1}{4}$-inch ball the amount required would have filled about 3 inches of the neck had it been so long. In this way perfectly sound castings, free from vacuous cavities, which always form when the metal solidifies on the surface first, are easily obtained; but to make the metal free from pores, and to close up any such cavities should they by any possibility exist, the moulds were placed in the hydraulic press immediately the metal in the neck had become solid, and after removing the brass plug, the steel plunger was forced down upon its shoulder. The solid metal was thus under great pressure made to flow, and a quantity of wire was forced out of the small side hole. Under these circumstances cavities are impossible, and since pure metal was employed, variations of density were out of the question. It may be worth mentioning here that of all metals in commerce, lead may be obtained of a greater degree of purity than any other. As soon as the pressing had been completed, the mould was removed, and allowed to cool. On being opened the lead ball was found perfect in form, and, so far as it is possible to judge, perfect in every respect, or at any rate so perfect that any departure from such a state cannot produce a disturbance in its gravitative power which is comparable with the limits of accuracy with which the attractions can be observed. I have made four balls of the $4\frac{1}{4}$-inch size, numbered 1, 2, 3, and 4, but I have at present used only numbers 1 and 2. Besides these I have made four of the smaller size of $2\frac{1}{4}$ inches in a mould of the same construction and numbered them numbers 6, 7, 8, and 9. To avoid risk of injury, the balls are kept in pairs in four well-made mahogany boxes, with two velvet-lined hemispherical hollows, in each half of each of the boxes. I have made lifters also to raise them by their brass lugs.

I weighed these balls on August 18, 1891, on the large Oertling balance in the South Kensington Museum. Through the kindness of Mr. CHANEY I was able to find the true value of all the weights employed, by comparison with the standards at the Standards Office. The weights of the two lead balls made use of, with their included brass ball-holders are, taking due account of the corrections :—

No. 1	7407·47 grams.
,, 2	7408·16 ,,

The lead balls are suspended from the geometrical clamps at the tops of the lid pillars by phosphor-bronze wires, which I drew myself down to the smallest size that I considered safe. This was found by measure to be ·0232 inch in diameter. As it had to carry 16·33 lbs., the stress would be one of between 15 and 16 tons to the square inch only, or about one-third of what I had found the wire able to carry. I could not silver-solder the wire into the upper and lower connecting pieces, as the strength was destroyed by annealing, and I found that soft solder allowed it slowly to creep out even when it was soldered into a hole which it nicely fitted, and $\frac{1}{4}$ inch long. I overcame this minor difficulty by dipping the ends of the wires into copper solution and thickening them by electro-depositing copper until they were just too large to enter the enlarged holes prepared for them. I then drew them through holes in a draw plate down to the right size. They were then sweated into their places, and the end of the wire at the upper end bent over at the bottom of the slot where it just protruded, hammered down and again sweated ; while at the lower end a small transverse hole was drilled on each side so as just to touch the side of the wire, and pins driven in, and the whole sweated together. This was done on September 2nd, 1892, and though the wires have carried the balls ever since, they have not broken, stretched, or drawn out. The wire, as it left the draw plate, was free from kinks, and was not allowed to be bent afterwards. As it is stretched so severely, I have assumed that the centre of gravity of the suspended ball is vertically below the axis of the wire, measured at a point nearly 2 inches below its point of support. The actual horizontal distance between the axes of the wires at this level can be determined with the optical compass, with an accuracy of $\frac{1}{10,000}$ inch; and therefore I maintain that this, the most important of the geometrical determinations, is known with abundant accuracy.

The Small Balls or Cylinders.

Owing to the small size of the attracted masses, I was able to make them of pure gold, a metal which possesses all the advantages of lead, besides increased density and freedom from oxidation or corrosion. As the inside of the central tube of copper in which the gold balls move is polished and electro-gilt, and as they move about an axis which coincides with the axis of this tube, any attractions or disturbances due to difference of electrical potential after contact must be of the smallest order possible. The method of making the gold balls is somewhat similar to that followed in the case of the lead balls, the difference in procedure depending upon the nature of the metal and the reduced size. Mr. COLEBROOK made for me pairs of hardened steel bars, with ends ground out and polished to true spherical surfaces, each of them just under a hemisphere in extent of surface. These were made in pairs for spheres of diameter, ·2, ·25, and ·3 inch. The quantity of gold necessary for making a ball, plus a small excess for waste, was placed in a hollow in a piece of Bath brick or of prepared charcoal, and heated with an oxy-hydrogen jet with just enough oxygen to melt the gold until it had run down to a clean button. When this was allowed to cool by itself, the surface was drawn in by the contracting centre, and a cavity and dimple were formed. When, however, the flame was gradually withdrawn, or reduced in size, so that the metal solidified from below upwards, a solid button with a perfectly smooth surface was easily obtainable. The button so formed was placed in one of the hemispherical moulds, held in a vice, and covered with the other, which was then given a smart blow with a hammer. The gold was thus compressed to an almost spherical form with an equatorial rib. On being turned over through a right angle, the rib was compressed into the gold, leaving a projecting head on each side, and these were finally compressed into the gold after a further twist through a right angle. The gold was then annealed, and beaten in the moulds with gradually reduced blows, being turned between each blow. When the ball was made practically perfect, it was weighed, and brought down by a very fine file to within 1 milligram. of the ultimate weight. It was then annealed and gently beaten again with rapid light blows, being turned between each, until a highly polished perfect sphere was the result. Rubbing with wash-leather and a little rouge brought it to the required weight, when it was finally gently beaten in the mould. These balls were made in pairs, identical in weight, so far as I could determine with the balance. The smallest weighed 1·2983 grams each. The point of a fine needle, held in a special tool in which the ball was placed, was forced a short way into the gold, and removed, after which the ball was replaced in the dies and compressed again, which process forced down the very small elevation round the little hole, and left a much smaller hole than could be made direct. In order to fasten these balls to their respective fibres, a pin was dipped in shellac varnish and rapidly passed across the end of the fibre. After one or two trials, a semi-microscopic bead of varnish, formed by capillarity, was left just above the end of the fibre. This was then placed in the little hole in the ball, and the latter was placed in a conical hole in a brass blank, which had been warmed in the flame of a spirit lamp. Under the influence of the warmth the little bead slid down, and instantly flashed into vapour as it touched the gold ball. If necessary, a small quantity more varnish could be applied upon the point of a very fine needle. The ball thus attached formed the lower part of a perfect Borda pendulum, there being nothing visible outside the spherical surface. So perfect is this mode of support that when a gold ball is hung up by its fibre, and set in torsional vibration, the image of the window seen reflected on the spherical surface was not seen to move or quiver when examined by a strong lens. The upper end of the fibre was fastened with shellac to the tail of the hook and eye, seen in fig. 7, the length of course being adjusted so that the gold and lead ball would hang at the same level.

There is no question that this is the most perfect method of holding the gold balls, but when I came to the larger size of ·25 inch, weighing 2·6501 grams, the risk of fracture due to an accidental roll of the ball was increased, and in one case, after a week spent in making all the preliminary measurements, one of the balls drew off, owing to imperfectly dried varnish, and it and its companion and the mirror were all precipitated down the central tube and the torsion fibre was lost. To reduce the risk, I therefore arranged another process which is practically as good and is much safer. A piece of No. 40 copper wire, $\frac{1}{5}$ inch long, weighing ·00084 gram, was inserted into the hole, and soldered in its place with a scarcely visible amount of solder, the wire and solder weighing exactly ·001 gram. A calculation of the total attraction of ball and wire, on the supposition that the wire as well as the ball acts upon the centre of the lead ball as if it were concentrated at the centre of the gold ball, shows the error to be only $\frac{1}{2500000}$ of the whole, it is therefore of no consequence. To the side of this wire the quartz fibre was easily fastened with shellac varnish, the amount of shellac used being ·0001 gram, or even less. I have not made any balls of the largest size, but in the one experiment in which larger masses were employed, have made instead cylinders of gold, $\frac{1}{4}$ inch in diameter and ·2587 inch long. These were prepared in a similar manner. Mr. COLEBROOK made for me a hardened steel cylindrical mould, the inside being lapped out to size and polished. The end was made plane and truly perpendicular to the cylindrical hollow. A polished steel plane was kept pressed against this face by screw pressure, and a steel plunger, accurately fitting the mould, with a polished plane perpendicular end completed the tool. The required quantity of gold, plus a small quantity for excess, was melted into a button as before, and placed into the mould. The plunger was beaten with a heavy hammer, under the blows of which the solid gold flowed as freely as the lead in the other case, penetrating the fissure between the cylinder and the bottom slab. A second plunger, made of brass, with an exactly central, fine needle point, was then pressed with light blows upon the gold to make a central hole, into which to solder a supporting wire, as in the case of the gold balls. Mr. EDSER, of the Royal College of Science, was kind enough to calculate for me, by means of spherical harmonics, the very small difference between the attraction of the cylinder upon a point at the centre of the lead ball, and that which would be exerted if the whole of the mass of the cylinder were concentrated at its centre. The correction is for a greater variation of distance and of inclination from the equatorial plane than can have been met with $-\cdot 00030$ of the whole, and this correction is accordingly applied in the one experiment (9) in which the cylinders were employed.

I have not at present referred to the attractions between the suspending wires and the gold balls, and between the suspending quartz fibres and the lead balls. The attractions of the fibres and wires for one another are, of course, infinitesimal to the second degree. Calculation shows that in experiments 4 to 8 and 10 to 12, the attraction of the lead balls for the fibres is to their attraction for the gold balls as 1 is to 204,500, and in the same direction, while the attraction of the gold balls for the wires is to their attraction for the lead balls as 1 is to 115,000, and in the opposite direction. The reversal of the direction is surprising, but it is due to the fact that the most effective part of the fibres is absent in the case of the wires, owing to the large diameter of the lead balls, and, therefore, the action of the long wire on the upper gold ball, which is on the other side, is the preponderating influence. The difference of these two corrections is $\frac{1}{263000}$ of the main effect, and in the opposite direction. I should add, that though the absolute masses of the small balls are known with an accuracy of 1 in 10,000, this is in no way necessary. So long as they are practically alike, it does not matter whether their mass is known or not, as it is ultimately eliminated. I have, however, introduced into the numerical work the actual masses in order to obtain the constants of the fibres and of the mirror, and the viscosity of the air absolutely.

The Beam Mirror and its Attachments.

One of the most important parts of the whole apparatus, and certainly the most difficult to arrive at in perfection, is the combination of beam and mirror which shall support the gold balls definitely in position, and shall, in its capacity as mirror, make it possible to determine their position with the greatest possible accuracy. In my paper on the radio-micrometer,* I dwelt on the importance of the proper proportioning of the mirror to the rest of the suspended body. Then I only considered its moment of inertia and weight. In the present case these remain important, and there is besides the resistance to motion due to the viscosity of the air, which unfortunately is of the most serious moment. If the usual round mirror is employed, the definition, as is well known, is directly proportional to its diameter—that is, if its figure is perfect. The advantage of a large mirror is somewhat counterbalanced by its weight, which tends to break the fibre, so that lighter balls or a coarser fibre are necessary; by its moment of inertia, which increases the already prolonged period; and by the resistance which it meets with in moving through a viscous atmosphere. As it is important to keep the mirror or mirror and balls swinging as long as possible, in order to determine their periods accurately, a high decrement is most objectionable. By making the mirror in the form of a long bar, I have succeeded in partly reconciling the incompatible conditions, for not only may the weight and moment of inertia be reduced to less than half of that which would be due to a disc of the same diameter, but the definition is decidedly better, as I have proved by experiment, and as is shown by optical theory. I should have said that the definition in a direction parallel to the length of the bar is better, that at right angles being obviously not so good. For the purpose of reading the divisions of a horizontal scale, vertical definition is of consequence only in so far as it is sufficient for the purpose of reading the figures attached. These I had made large with this object. I have already stated that the scale is formed by black lines on a luminous ground. By this means I am able to obtain a degree of reading power which might seem beyond that which a mirror of the size used ought to give. The mirror will form a spurious image of a luminous point of an oblong form, the long dimensions being vertical, and bearing the same relation to the short as the length of the mirror, which is horizontal, bears to its breadth, which is vertical. If the mirror could be made indefinitely long, the image would shrink to a vertical line, and horizontal definition would be perfect. Owing to the limited length of the mirror (·9 inch, about) the width of the spurious image subtends an angle of about 5″, and this is the limiting separating power in the horizontal direction. Now, if the scale were made by very fine white or luminous lines upon a dark ground, each line would be seen as a band with shaded edges 5″ wide; but, as the angular distance which I have been able to employ from one division to the next is only 14″, the lines would have appeared half as broad as the spaces. If the lines on the real scale were of sensible thickness, then the proportion of apparent line to space would have been higher. If, on the other hand, the ground is luminous, the lines are dark and are made rather coarse; the effect of the spreading of the light is to pare off the dark edges, and leave the appearance of a finer line, which, though it is not sharp, is symmetrical, and, as a recurring phenomenon, allows of definite observation to one-tenth of a division, *i.e.*, in the actual case to 1·4″. This corresponds to a movement

* 'Phil. Trans.,' Jan., 1889.

of the mirror of ·7″, and of the balls of $\frac{1}{750,000}$ inch. To this degree of accuracy there is no difficulty in observing; in fact, I might have made the divisions smaller, and still have been able to read to tenths. Of course this perfection is only possible with a very perfect mirror, and for this I applied to Mr. HILGER, who took special pains in preparing several as thin as he dare. Of these, one only, when tested while still round with the large astronomical telescope upon an artificial star, was perfect in its definition, and formed the spurious disc and diffraction rings equally in all directions. This mirror was the one employed in all the experiments up to date. A second one was nearly as good. One made of quartz showed the diffraction rings strongly in three directions only, 120° apart. In making the test I was careful to notice in which direction the two images, one due to the front surface and the other to the silvered back surface, were separated, owing to the inevitable want of perfect parallelism between the faces. I then cut them in such a direction that the displacement should be vertical, so as to avoid the confusion caused by the superposition of the dim reflection of the scale upon the image under observation. The two good mirrors I slit with a fine steel disc and diamond dust, so as to leave a central bar $\frac{1}{4}$ inch wide. This gives abundant light, and defines well enough to enable the figures to be read. I found by the use of screens that a bar $\frac{1}{8}$ inch wide, though it gave enough light, so destroyed the vertical definition that the figures could not be read, and the long and short lines were not so clearly distinguished. I then, with a very sharp-edged brass disc and washed emery, ground in the thickness of each mirror at each end a vertical V, seen in the plan (fig. 7). The bottom was so fine and sharp that a quartz fibre $\frac{1}{1000}$ inch in diameter would rest definitely in its place. The mirror was cemented with three spots of shellac varnish to the gilt copper support O. This was so formed that the balls could hang by their eye-hooks from the notches at the end of the arms, with the fibres resting in the vertical V grooves of the mirror. The beam mirror was carried by a quartz fibre, fastened to the point of O by shellac in all experiments up to No. 3, and soldered to an intermediate tag in the later ones. The details of the soldering process are given in the 'Phil. Mag.' for May, 1894. From the lower central hook of the beam mirror the silver counterweight K may be suspended when the gold balls are removed. This was turned by Mr. COLEBROOK out of pure silver, which I had prepared by casting and hammering. It is truly cylindrical, and, with the triangular wire hook to which it is soldered, weighs exactly the same as the corresponding pair of balls with their fibres and hooks. Its diameter was measured in several places with a screw micrometer. The weight of the cylinder, of the little hook, and of the solder used were separately determined, and the very small radius of gyration of the hook estimated from its dimensions and form. The period of the mirror was observed (*a*) with the balls on, (*b*) with the counterweight on, and (*c*) alone. From (*a*) and (*b*) the unknown moment of inertia of the beam could be determined, and from *a*, *b*, and *c* the effect of the stretching upon the rigidity of the fibre could be ascertained. The suppositions made are rather numerous, and are best discussed here, In the first place it is supposed that the mirror does not change its axis when swung alone or with the balls or counterweight suspended, for, if it does, its unknown moment of inertia will not be a constant, as assumed. Secondly it is supposed that when the gold balls are suspended from the beam mirror, they are rigidly connected with it, or that the mobility of suspension or torsion is so minute as to be equivalent to a rigid suspension. Thirdly, that the counterweight, when suspended from the beam mirror, is rigidly connected with it, and that it rotates about its geometrical axis. It is obvious that however carefully the parts have been made, the suppositions cannot be rigidly true. It is necessary, therefore, to find what order of error is introduced by any possible or observed want of perfection.

It is evident that the axis of rotation of the mirror must always pass through the point at which the quartz fibre leaves it, and that, when it is unloaded, its axis passes through its centre of gravity. As the construction is intended to be symmetrical with respect to this axis, and is so, as far as observation enables one to judge, and as such an axis is an axis of maximum or minimum moment of inertia, the uncertainty in the moment of inertia, due to a small angular displacement, is proportional to the square of the angle, and is altogether beyond the region of experimental certainty. When, however, the balls or counterweight are placed in position, if the construction is not truly symmetrical, the beam mirror will now rotate about an axis which does not pass through its centre of gravity. In an experiment made for the purpose, this displacement, when the larger balls were suspended, was found to be ·0063 inch, an amount considerably larger than I had expected. In this case the moment of inertia, added to the whole combination on this account, is, weight of beam $\times \cdot 0063^2 = \cdot 844 \times \cdot 0063^2 = \cdot 0000335$ inch^2 gram. The corresponding increase, when the counterweight was added, was $\cdot 844 \times 0012^2 = \cdot 0000012$ inch^2 gram. Applying these small differences to the observations of Experiment 12, if I may so far anticipate, the torsional rigidity of the suspending fibre is changed from ·0012577 to ·001257736, so that if this had been overlooked, the error introduced would have been 1 in 35,000. It was not, as a matter of fact, observed until after the conclusion of Experiment 12, and then I placed one of the microscopes so as to see the edge of the lower hook of the beam, and measured the unexpectedly large deviations in the plane of the mirror; those perpendicular to the plane I had always known to be practically inappreciable by the very small change in the position of the observing telescope needed to again see the scale reflected centrally. This insensible correction which tends to make G seem greater, can only be applied to Experiments 10, 11, and 12, as the fibre met with an accident after Experiment 9, and was re-fastened to the beam. As it cannot be applied to the others, and is far smaller than the uncertainty of the experiment, it is not taken account of in the table.

The rigid attachment of the gold balls to the mirror might seem to be purely imaginary, seeing that they hang from fibres 5 and 11 inches long, and so can lag behind when the mirror is subject to angular acceleration, that they must fly out owing to their centrifugal force when this acceleration has given rise to an angular velocity, and must be pulled by the gravitation of the lead balls so as to be further apart than supposed. Moreover, since each ball can rotate separately on its own fibre with a period of its own of as much as 6·720 and 9·055 seconds in Experiments 4 to 8 and 10 to 12, they must, in their rotation about their own axes, lag behind the rotation of the mirror when it is being accelerated. The magnitude of these several errors is infinitesimal, or greatly below the limiting accuracy aimed at, and, in all cases, may be calculated and allowed for if necessary. Thus, in Experiment 8, the lower ball in the extreme case of an amplitude of 10,000 divisions, or 100,000 units (I make $\frac{1}{10}$ division the unit, to avoid decimals), is at the middle of the swing thrown out by centrifugal force $\frac{1}{25000000}$ inch, and the upper one about half as much. The linear acceleration on the balls, due to the action of the torsion fibre, is the same as that due to a pendulum nearly six miles long, or, more exactly, 364,335 inches, so that the actual acceleration produced by the fibre in the case of the lower ball is about 1 in 33,000 more than is supposed, and, on the upper ball, about 1 in 70,000. The amount by which the lower ball is pulled outwards by the gravitation of the lead ball next to it, even when that is in its neutral position, where its actual attraction is a maximum, is less than a ten-millionth of an inch. The rotational mobility of the gold balls, however, in Experiments 4 to 8 and 10 to 12, was more than I had intended, and, as I felt that it was important to know precisely what effect this would have upon the result, I referred the problem to Professor GREENHILL, who very kindly explained to me exactly how to evaluate it, and, with Professor MINCHIN, went through the great labour of obtaining and solving the resulting cubic equation. The rigidity of the fibre, in this the worst case, should be diminished by less than 1 in 7850. An increase in the thickness of the two suspending fibres of a few ten-thousandths of an inch, such as I made use of in Experiment 9, would reduce this to the order of 1 in 100,000, or even less, and the complex calculation of this correction would no longer be necessary.

Finally, it is assumed that the counterweight, when it has replaced the gold ball, is also rigidly connected with the mirror and acts axially. With respect to the rigidity of the attachment, it is unnecessary to do more than state that the friction on the suspending hook must be many thousand times greater than the greatest couple ever developed by the torsion fibre, and that, with regard to its axiality, the same remarks that were made with respect to the beam mirror apply with even greater force. There is only one point about which, in consequence of the microscopic examination after Experiment 12, I am not altogether satisfied. It seemed as if the hook suspension was not quite pendularly free so that the counterweight could rest hanging from the beam mirror at a very small angle to its natural position of verticality. This was not observable on the counterweight itself, but only by microscopic examination of the beam. Though the beam hook rarely varied in position by so much as ·001 inch, I was able, by using great care in trying to make it rest in extreme positions, to introduce an uncertainty of position nearly four times as great. I cannot think that any serious discrepancy can have resulted from this, as the uncertainty of moment of inertia would be only a very small fraction of that found in the case of the beam mirror when the balls were suspended. This fault, such as it is, however, I intend to remove in my next experiments, from which I hope, also, to almost entirely exclude the small eccentricities already referred to. For this purpose I may either file the hook rather wider, or, as I think preferable, hang the counterweight by a loop of silk, which will in no way constrain its hanging, but which will be rigid with respect to torsion. I wish, also, to make use of a slightly-silvered torsion fibre, so as to reduce the effect of electrical disturbances if they should be set up by the movements of the air. The square of the corrected period, with the counterweight on, differed more in Experiments 5 and 8 than it should have done, being 1702·35 in Experiment 5, and 1698·73 in Experiment 8. In order to see to what extent this discrepancy may have affected the result, I have calculated the rigidity of the torsion fibre in these two cases with the observations of this quantity reversed, that is, 1698·73 instead of 1702·35, and *vice versâ*. The effect upon the result is 1 part in 10,600, so that if all the error is in one observation only, the other one is on this account alone less than $\frac{1}{10000}$ in error. As any sticking of the counterweight would tend to increase the moment of inertia, and hence the period, I am inclined to look with greater favour on the smaller observation, but the difference, in any case, is considerably less than the actual differences found in the final results.

In connection with the beam mirror, it is convenient here to describe the means provided for keeping it under control from the telescope without entering the shielded corner of the vault.

Below the apparatus, figs. 1 and 2, may be seen a tube terminating in a bent glass pipe which enters the hollow screw *s*. This is joined to a narrow piece of composition piping, which is carried across the interval between the two tables by the wooden bar which serves to protect the light driving cord, and, at the eyepiece of the telescope T, terminates in a mouthpiece. Where observations of deflection and period are being made, air may be drawn through the tube causing a very gentle indraught through the tube of the window, fig. 12. This acting on one end of the mirror produces upon it a couple which may be employed to bring it to rest or to get up a swing of large amplitude. The extreme precision and delicacy of this process is best explained by considering an electrical analogue. The window tube, fig. 12, acts as a moderate resistance, the open space between the glass tube and the large hole in the screw as a short circuit or very low resistance, and the long tube across the room as a high resistance again. The electromotive force is the suction of the mouth. Owing to the high resistance of the long tube, but a feeble effect is felt at the glass end, and this is practically entirely satisfied by the low resistance leak. The available electromotive force acting upon the resistance of the window tube is therefore very small, and in consequence the current acting on the needle is similarly minute. So delicate is this that it is quite easy by pinching the tube properly with the fingers to bring the movements of the mirror, when the counterweight but not the balls are suspended, down to one unit, corresponding to $\frac{1}{750000}$ inch movement of the gold balls if they were in their place.

Conclusion of Part I.

The apparatus and optical compass were made by the Cambridge Scientific Instrument Company. I cannot lose this opportunity of expressing my great indebtedness to Mr. PYE, who, in the absence of Mr. DARWIN through illness, entered into every detail with the greatest care and faithfully carried out all the directions as to modes of construction upon which, after consultation with him, I finally decided.

As already mentioned, Mr. COLEBROOK constructed the special tools and the very numerous extraneous apparatus. He also made all the special windows. Mr. MUNRO made the tools for compressing the lead balls, and being his work they are of course accurate in the highest degree. Mr. STANLEY undertook the large scale, but though the execution of the etching is excellent, the accuracy is not good. This, however, matters little, for the errors are eliminated by the calibration.

The actual work of making the gold balls, the lead balls, the finish of the beam mirror, the quartz fibre work, the gilding and polishing of the inside of the central tube, and a great deal of the general fittings I did myself, either alone or with the help of Mr. CHAPMAN and Mr. COLEBROOK of the Physical Laboratory.

The apparatus, which belongs to the Science Collection of the South Kensington Museum, will, I hope, on the completion of the experiments, be set up in a special place in the Museum so that it may be seen in action by anyone interested. I intend to leave also permanently in the Museum a series of photographs of the apparatus as it appears *in situ* when each one of the operations is being carried out. Besides this, the note books and all the calculations will be left there permanently so that reference may be made to them in case any point is insufficiently explained.

PART II.

The Mode of Procedure.

The actual method of carrying out the experiment, though in the main obvious to any one who has read the first part of this paper, is nevertheless in some particulars by no means evident. As, moreover, a careful explanation of the several operations and the purposes which they serve will make the mere numerical details of the actual experiments, which form the third part of the paper, more intelligible, I shall describe them in order, under distinguishing numbers.

The operations, as described, are fourteen in number, but they are of very different importance, and the time required for carrying them out differs also very much. Some few need only to be performed on the first putting together of the apparatus.

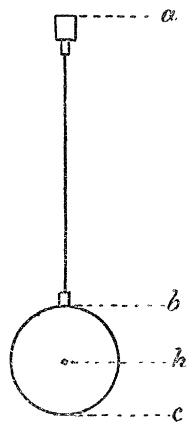
Operation 1.

After placing the instrument in the selected spot, with its centre tube vertically below the edge of the central overhead pulley, it is to be levelled accurately by placing a spirit level on the lid, and adjusting the levelling screws, until the bubble occupies the same position in the tube when the lid, carrying with it the level, is slowly made to revolve. Fix also the scale and large telescope in position.

Operation 2.

Hang up the lead balls by their wires and upper supporting pieces, pinning the latter to the thickened lower ends of the two steel bands. These are carried over the flat-rimmed pullies, and carry at their other ends counter-weights, so that the balls will remain suspended at any level. If the central tube is not in position the three readings, a, b, and c, fig. A, are made with the cathetometer, and thus the true distance from a to the centre of the lead ball, when the wire is stretched by the weight of the latter, is known. If, after one experiment, it is desired to make this measure, but not to move the central tube, since it is impossible to remove the lead balls completely, they

Fig. A.

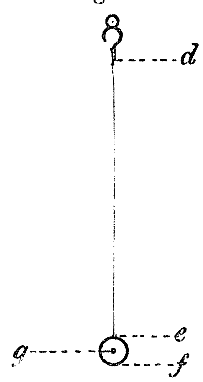


must be left suspended at such a height that the b of each is visible, This can be accomplished without taking away the lid and lid pillars, since they can be left suspended by their counterweight at such a height that the a of the (ultimately) upper ball is below the lid, while the a of the (ultimately) lower ball is above its lid pillar. Since all hang freely without touching one another, the a's and b's can be determined, and on adding the known radius of the lead balls, the distances ah are known.

Operation 3.

Hang the gold balls by their eye hooks upon a pin driven into any temporary stand. Measure with the cathetometer the three positions d, e, and f of each, fig. B. From these the distances dg can be determined when the fibres are stretched by the weight of the balls. Also find their individual torsion periods by the method of coincidences,

Fig. B.



watching them with the cathetometer telescope, and listening to the clock. This latter is needed for the purpose of calculating the very small correction (p. 35) which should be evanescent. The cathetometer measures can be made with an accuracy of two or three-hundredths of a millim., and this, as will appear when the figures are examined, is more than abundantly accurate.

Operation 4.

Put the lead balls in the apparatus, fix the central tube in place, with its front window facing the scale, let the gold balls down the central tube, and hang them on the side hooks, place the lid in position; having marked one side of the lead balls in some distinguishing manner (a very small spot of shellac varnish is what I used), suspend them with these marks pointing in some definite direction, *e.g.*, inwards; let down the mirror by its torsion fibre, and adjust the torsion head so that it hangs approximately centrally, and at a convenient height.

Operation 5.

Find, as already described, the division at which a perpendicular from the axis of the tube falls upon the scale. Twist round the central tube, if necessary, until the window reflects a light placed behind this division into the large telescope.

Operation 6.

Find the optical distance in terms of the corrected scale division from the silvered surface of the mirror to the foot of the perpendicular upon the scale, and thus find the angular measure corresponding to any observed deflection.

The details of this process are sufficiently described under the heading *The Steel Tape and its Accessories* (p. 17).

Operation 7.

Make cathetometer readings of a, a and d, d. Subtract from these the corresponding quantities found in operations 2 and 3, *ah* and *dg*, and thus find the levels of the centres of the gold and of the lead balls. If the gold balls are on the whole high or low, lower or raise the torsion rod the necessary amount and re-measure *a*, *a* and *d*, *d*.

Operation 8.

If the mirror rests so that the foot of the perpendicular is about the position of rest as seen by reflection in the telescope, when the lead balls are, so far as it is possible to judge, in the same plane as the gold balls, all is ready for the next operation; otherwise it will be necessary to turn the support of the torsion rod, which will not raise or lower it, until observation at the telescope shows that this is the case. The mirror will then be symmetrically placed with respect to the window.

Operation 9.

This is a long operation carried out with the optical compass, the object being to find the horizontal distances between the axes of the wires and between the axes of the fibres, to centre the torsion fibre with respect to the axis of the instrument, and to find the corresponding readings of the vernier of the lid and of the glass scale seen by reflection in the telescope when the plane of the wires and fibres is identical. Owing to the great length of this operation, the description of it is divided into sections.

Section a.—Remove the two pillars RR from the lid, and the first spur-wheel of the driving train W, which is made to simply lift off. Put the optical compass in its place on the lid so that the line of traverse is apparently parallel to the line joining the wires.

Focus the positive eye-piece of each microscope upon its cross-wire or scale, and then shift the focussing collars *c* of each, so that when the microscopes change places in the same groove and are pressed up to their collars, they are each in focus on the same object.

Section b.—Using one microscope in any one groove of one of the traversing slides T_1 or T_2, focus alternately on the edges of the two wires, using the focussing screw S_3 to move both the same way, and turning the base of the optical compass upon the lid to move them opposite ways. When both are found in exact focus the line of traverse is parallel to the plane joining the axes of the wires.

Section c.—Measure this distance. To do this the two traversing slides are placed together, and the fine steel spring passed through the hole in their V's. The spring is then stretched and prevented from contracting by a pin passed through it at one end. The two slides T_1, T_2 then are pulled together by the spring, but are separated more or less by the adjusting screw cone S_4. The two microscopes are laid in the outer grooves (or the middle pair in case a 4-inch distance has to be measured), and the slides moved until each is directed upon one side of the corresponding wire. The final exact adjustments are easy of execution, for the focussing screw S_3 supplies a slow fore and aft motion, while the corresponding slow lateral movement is given by the screw cone S_4. In order to allow one only of the traversing slides to move, the fingers of one hand are made to rest upon the slide which is to be kept still, and thus its friction on the base increased, the other one then only moves. When both microscopes are focussed upon (say) the right or apparent left sides of the wires, and their cross-wires are directed upon them also, the focussing slide is withdrawn about an inch, and the focussing block *b* put into its place. The slide is then pushed forward until the focussing screw rests against the focussing block. The small glass scale is then placed so as to rest against the two parallelizing screws S_2 S_2, and against the micrometer screw S_1, and S_2 S_2 are moved until the scale is in focus in both microscopes at the same time. S_1 is then turned until the intended zero of the scale ·04 is on the cross-wire of the left microscope and its head is read : it is then turned forward a fraction of a turn until a division at the other end of the scale 6·03 or 6·04 is on the cross-wire at that end. The amount of movement indicated by the head of the screw S_1, added to the tabulated distance from ·04 to 6·03 or 6·04, is the distance from the right side of one wire to the right side of the other. If the eye-piece micrometer is used instead of the cross-wire, then ·04 is brought in the central division of the left-hand microscope, and the micrometer readings of the divisions 6·03 and 6·04, or 6·04 and 6·05, are taken in the right microscope. Knowing the tabulated length of either of these divisions and the number of eye-piece divisions corresponding to it, the amount to be added to the tabulated distance between ·04 and the lower of the two observed, is readily found. The distance between the left sides of the wires is found in the same way, and the mean of the two is taken for the distance between their axes. The thicknesses of the wires are also found by subtracting the readings of the apparent right from the readings of the apparent left sides, remembering that there are two whole turns of the screw. After each operation the focussing block is moved round, and the focussing slide moved up so as to bring the wires into view. If they are not exactly on the cross-wire as before, the measure is rejected and a new one taken, but this is rarely the case.

The example taken from Experiment 8 does not show the confirmatory observations made with the eye-piece micrometer, for at that time the micrometer scales had not been made. As I still rely upon the screw measure of the fractions and only take the eye-piece micrometer readings to satisfy myself that the screw observations have been correctly worked, this example will serve as well as a later one.

MEASURES of Wires with Optical Compass.

Side.	Wire.	Scale.	Head.	Correction.
Apparent right . .	Right	6·03	641	+·000025
,, ,, . .	Left	·04 (+4)	029	
,, left . .	Right (605+2) . .	6·03	955	
,, ,, . .	Left	·04 (+4)	357	

Apparent left side.	Apparent right side.	Thickness.	
		Left.	Right.
6·03955 ·05357	6·03641 ·05029	·02357 ·00029	·02955 ·00641
5·98598 5·98612	5·98612	·02328	·02314
Mean . . 5·98605 centre to centre. + ·000025 correction.			
5·986075 corrected value.			

The (+ 4) after ·04 indicates that when the right microscope was exactly on the division 6·03, the left one was, by eye estimation, at ·044, or $\frac{4}{10}$ of a division above ·04. Since also the screw-head reading has passed 1000, or a complete turn, and has risen in the two cases to 029 and 357, ·05 is the quantity that must be written in the subtraction below to find the distances from side to side of the wires.

(6·05 + 2) means that 6·05 was the division seen when the scale was put in its former place and that two whole turns of the screw were needed to bring 6·03 into view again. It was because there was only one division at the supposed zero ·04, that the scale had to be pushed forward so as to bring 6·03 into view again, and ·04 and 6·03 were used to measure the apparent left (but really right) sides of the wires.

Section d.—Place the two microscopes in the centre pair of grooves and bring them both in succession to focus on the same wire, adjust the focussing collars, if necessary, so that when pushed up to their collars they are each in exact focus. Having the proper back window, figs. 13, 14, in position, withdraw the microscope and move the traversing slides till the microscope will be able to slide forward and view the fibres. Push both up to their collars and move laterally until the fibres are found. If, as is probable, they are out of focus, ascertain by moving either microscope by hand in its grooves whether the error of focus is in the same direction for both. If it is not, slowly turn the lid, using a lever bearing upon the pin of the first wheel of the train and entering the nearest tooth on the edge of the lid. This does not affect the parallelism of the line of traverse with the plane of the wires, for the optical compass is carried round with the lid. When the fibres seem about equally out of focus and in the same direction, bring them towards or away from the microscopes, as may be necessary, by the adjusting screws of the torsion head. However carefully this may be done, the suspended beam and gold balls are sure to be set swinging slightly as a pendulum. This is easily overcome by resting a wax taper or very flexible piece of wood against the central tube, and while watching the motion in the microscope bearing very lightly upon it in time with the oscillations. There is no difficulty in reducing the motion to the $\frac{1}{10000}$ inch in a very short time. If a torsional swing has begun, there is nothing to be done except to wait until the amplitude is reduced to a very small amount. It is impossible to obtain real quiet in this sense, as owing to gravitation the observer's body and the gold balls will attract one another unequally. After one or two trials the two fibres will be found simultaneously in focus, at any rate, if not continuously owing to the small torsional oscillations, yet between the elongations at successive half periods. I prefer to leave the apparatus at this stage and to return after an hour, when the mirror is more quiet, to verify the accuracy of focus. If this is correct since the focussing screw has not been touched, the plane of the fibres coincides with the plane of the wires. If more than a very small focussing correction is made, Operation 8 will have to be repeated afterwards.

Section e.—Read the great scale in the telescope and take the vernier reading of the lid. Then, since the wires and fibres are in the same plane, the lid reading and corresponding scale reading are known for one position, and, since the angular value of a scale division is known, also for all positions.

Section f.—Measure the distance between the fibres and their thicknesses, using exactly the same procedure as described under Section *c*. I may mention here that as the beam mirror is only ·9 instead of 1 inch as intended, the traversing slides as made would not come near enough together for both fibres to be seen at the same time. I therefore made use of the traversing slides in their other position, *i.e.*, with T_1 to the right of T_2 instead of to the left, after having had the right side of T_2 reduced by slot drilling so as to allow the now inner grooves to come $\frac{1}{8}$ inch nearer to one another. The change from one position to the other is made in a moment.

EXAMPLE.—Measures of Fibres with Optical Compass.

Edge.	Fibre.	Scale.	Head.	Correction.
Apparent right . .	Right . .	3·45	439	—·00033
" " . .	Left . .	2·55 (+ 7)	078	
" left . .	Right . .	3·45	545	
" " . .	Left . .	2·55 (+ 7)	180	

Apparent left edge.	Apparent right edge.	Thickness.	
		Left.	Right.
3·45545 2·56180	3·45439 2·56078	180 078	545 439
·89365 ·89361	·89361	·00102	·00106
Mean . . ·89363 centre to centre. — ·00033 correction.			
·89330 corrected value.			

Section g.—Set one microscope to see one fibre and the other in the proper groove in the traversing slide to see one wire. Measure, as in Section *c*, the distance from one edge of one fibre to one edge of one wire. Then move the traversing slide so as to measure the corresponding interval on the other side. Knowing the two intervals, the thicknesses of the wires and of the fibres, and the distance between the axes of the wires (2 R), and between the axes of the fibres (2 r), it is at once known to what extent the pair of fibres are eccentric with respect to the pair of wires. Without touching the back adjusting screw of the torsion head, screw the other two until one of the fibres has moved the right amount as measured by the eyepiece micrometer. If a movement of more than a few thousandths of an inch is necessary, it is best to re-measure the right and left intervals, and again adjust. When this is done the two fibres are in the same plane as the wires and exactly half-way between them.

If now the construction were perfect the torsion fibre would be in the axis of the instrument, and the lead balls would move centrally round it, but if either the two radii of movement of the gold balls r, r or the two radii of movement of the lead balls R, R differ from one another, or if the two lead balls hang from points which are not diametrically opposed to one another, then the lead balls will not rotate about the torsion fibre as an axis. If, however, the optical compass is removed and the lid is turned round 180°, and the measurement repeated, then half the movement necessary to bring the fibres into focus as measured by the head of the focussing screw is the diametrical inaccuracy of the lead balls, and half the inequality of the right and left intervals is their eccentricity with respect to their mechanical axis measured in their own plane. The torsion head may now be moved half of each of these amounts separately, and then the vertical line half way between the pair of fibres, which by construction is necessarily the same as the torsion fibre within less than $\frac{1}{1000}$ inch, is also in the mechanical axis about which the lead balls actually turn.

EXAMPLES.—Plane of Fibres and Wires.

Focus for wires, ·05 on focussing screw.
,, ,, fibres, ·25 ,, ,,

Therefore the fibres are behind (away from microscope) the wires by $\frac{\cdot 2}{50} = \cdot 004$ inch.

Eccentricity of Pair of Fibres.—After setting the pair of microscopes to one interval and sliding to the other, the left appears in the eye-piece to be smaller than the right by about ·007 inch.

By measurement it is found to be ·00792 less, or the pair of quartz fibres are ·00396 out of centre.

The necessary observations are :—

5·98607	Centre to centre, wires.
·89330	Centre to centre, fibres.
·02314	Thickness right wire.
6·90251	
·00102	Thickness left fibre.
2)6·90149	
3·45074	Mean of right and left intervals.
3·45470	Right edge left fibre to right side right wire.
·00396	Excess of right interval above mean.
·00792	Right interval greater than left.

Operation 10.

Prepare for observations of deflection and period. Remove the optical compass and replace the windows by fig. 12 at the back and fig. 11 in front. Place the tubular screens, fig. 15, in position. Screw in the pillars R, R, and arrange the guys, &c.

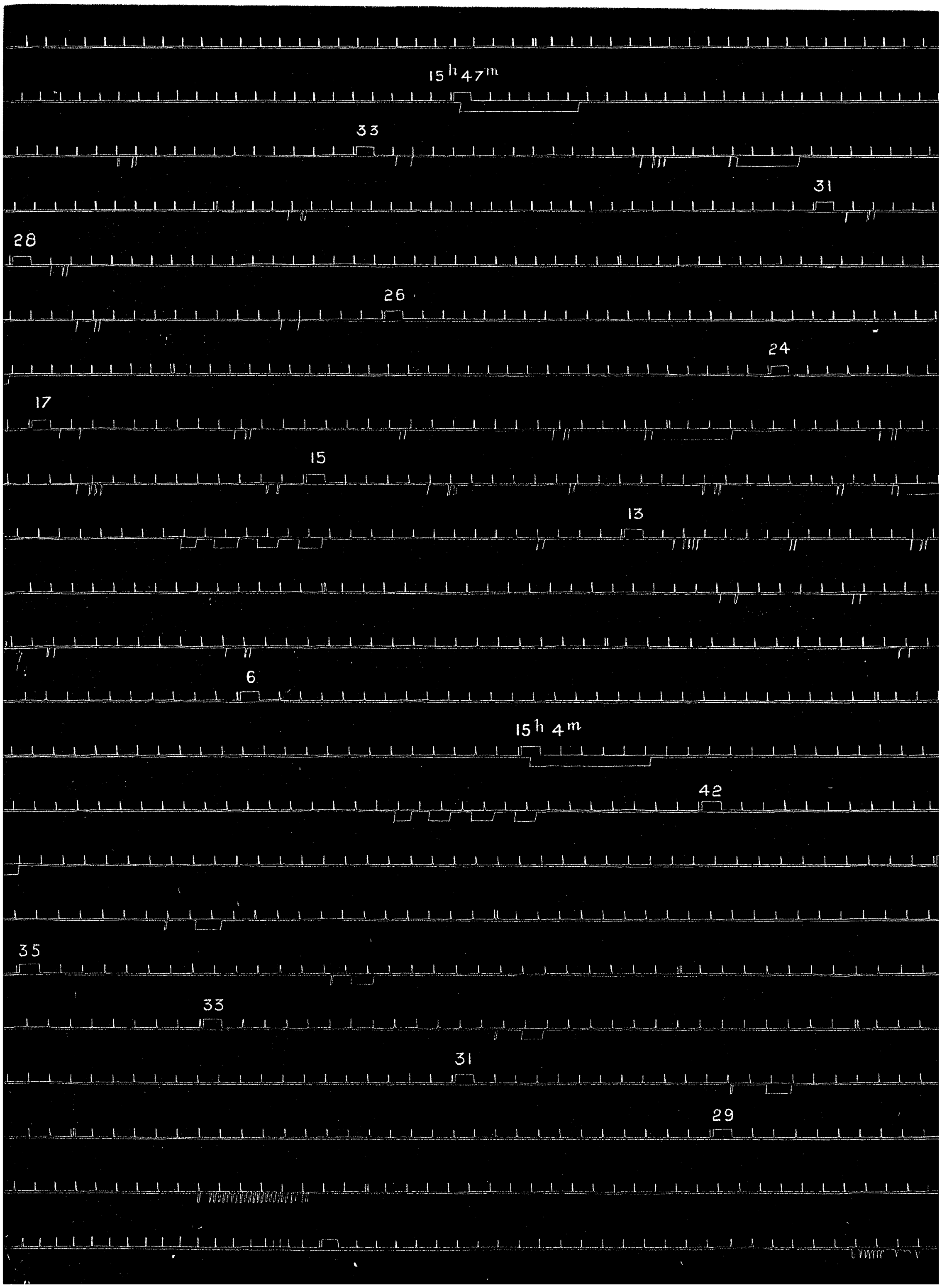
and counterweight so as to reduce the weight of the lids and balls and therefore their friction to a small amount. Place the first of the train of wheels W in place. See that the string operating the driving gear is in place, and that the india-rubber tube under the apparatus is connected to the composition tube going to the telescope. Place the two halves of the octagon house in position, and fill up the open gap where it overhangs the table at the back with a duster. See that the little electric lamp inside the house is properly placed to illuminate the vernier. Remove all superfluous apparatus from the table. Place the felt screen in position, and, when all is proved to be in working order, leave, if possible, for three days to acquire a uniform temperature.

The angle through which the lead balls must be turned in order to produce the maximum deflection I had found in a preliminary calculation, completed before the apparatus was made, in which 2 R was made 6 inches, $2r$ 1 inch, and the difference of levels 6 inches, to be $61^\circ 15'$, and the effect of an error of $15'$ at this position to be 1 part in 280,000. With the actual apparatus, in which $2r$ is less than 1 inch, I found by experiment the angle of greatest effect to be 65°, which is that adopted. Knowing the vernier reading corresponding to the observed scale reading when there is no deflection observable with the optical compass, *i.e.*, when the lead and gold balls are in the same plane, or in the neutral position, and taking this provisionally as a position of no deflection, move the lid in one direction through an angle of 65° by turning the handle of the wheel d by the telescope round 115 times. The exact setting of the lid is finally accomplished by lighting the electric lamp in the octagon house, and observing the vernier with the small telescope t. Three or more elongations of the apparently moving scale are now read in the large telescope, and then the wheel d is turned 125 times back, and, when the mirror is approaching its position of rest, the remaining 105 turns are given to the handle, which leaves the balls 65° on the other side of the neutral point, and the mirror oscillating through 50 divisions of the scale, or even less. The vernier reading must be correctly set by the use of the electric lamp, little telescope, and handle d, as before. Three or more elongations are again taken. The elongations are corrected, and from the corrected elongations the positions of rest, when the lead balls are in their + and in their − positions, are calculated. If the supposed neutral position had been accurately found, its scale reading would be exactly half way between the + and − position of rest. If, as is probable, it is not quite accurate, then, since the variation of the position of rest of the mirror is hardly observable when the lid is moved even so much as 1° from its position of maximum effect, while such an uncertainty of position is not possible in the provisional setting, all that has to be done is to bring the lid into such a position that the mirror is at rest exactly half way between its extreme + and − positions already observed. It is not sufficient to move the lead balls through an angle corresponding to the error, because, though at the neutral position the smallest variation produces the greatest effect upon the gold balls, they do not follow it absolutely. I always take two observations with the telescope of the position of rest of the gold balls when the vernier readings differ by about 2° at the neutral position, and thus, knowing how many scale-divisions deflection are produced at this position by a movement of the lead balls of 1°, I am able at once to find the vernier reading of the true neutral position (N). Then, when the vernier reading is made N + 65° or N − 65°, the deflections in the + and − directions are found to be the same within $\frac{1}{10}$ per cent., from which it is evident that the + and − positions have been set truly, with a superabundant degree of accuracy. This preliminary determination I generally make the night before the deflections and periods are determined, which in Oxford is best done on Sunday night between midnight and 6 A.M. The daytime, of course, is out of the question, owing to the rattling traffic on the stones in St. Giles', about a quarter of a mile away; and all nights except Sunday night the railway people are engaged making up trains and shunting, which is more continuous and disturbing to the steadiness of the ground than a passing train. Even these come through at intervals on a Sunday night, and this limits the accuracy with which the periods can be observed. All having been prepared for the Sunday night during the previous week, the room is shut up all day, and at midnight or a little later the actual observations of deflection and period are begun.

For a long night's work without accidents, I am able to take two or three sets of observations alternately at each of the + and − positions, with one at the neutral position; one period at each of the + and − positions, lasting about 45 minutes, and occasionally one at the neutral position, lasting about 15 minutes, and finally two or three sets alternately at each of the + and − positions, followed rarely by one, or even two, periods of 30 to 45 minutes. For each set of observations at the + and − position, I observe six consecutive elongations, and sometimes eight if I am disturbed by the trains. I do not begin them until the apparatus has quieted down from any very small tremor which the rotation of the lid may have set up.

In observing the period, the point of rest is known from previous observation. A conspicuous pointer tapering to a point, which can be inserted into any division, or if between can be read to a tenth of a division, is placed at the point of rest. Air is gently drawn from the mouthpiece for a quarter period and then stopped. According to the speed at which the pointer flashes past I determine whether or not to draw air again. If I do, I begin about a quarter period after the transit, and continue until the next transit, or a quarter period longer still, according to the velocity of the transit. It is most important not to begin or leave off drawing the air suddenly, lest a quick period movement, of which there are five independent of one another, should be set up in the mirror. I begin very gently, gradually increase and gradually leave off, the result of which is a beautifully steady motion of the mobile system, extending far beyond the limits of the scale. I then start the drum and make a dozen marks with the key in rapid succession, to indicate the beginning of an observation. At each transit, the key is pressed suddenly and then

Fig. C.



held down for one second, which produces a "transit mark," the purpose of which is to indicate that the previous dash was made at a transit of the pointer. Immediately afterwards I note in the book an arrow showing the direction of motion. Every transit is thus marked during the first stage, which lasts 10 or 15 minutes. In order to know the actual time of any of the minute marks, I hold the key down once at a minute for six seconds and thus produce "time mark." This also is noted in its proper place in the book with the time. To still further make sure of the place in the book which corresponds to any place on the smoked drum, I occasionally make a "castle mark," that is, hold the key down during alternate seconds four or five times, and note that also in the book. The proper sequence of time marks, castle marks, and transit marks, is sufficient to make it evident afterwards to which arrow in the book any particular transit mark belongs. During this stage it is not possible to take the elongations as they are off the scale. I then leave the apparatus to itself for about 20 minutes after first stopping the drum. On my return I start the drum, secure another time mark, and every transit, as before, with a castle mark somewhere for distinction, but now the elongations being observable I note them in the book at the same time. Following the practice of Professor CORNU, but on an extended scale, I take the transits of the chief divisions at first of every 1000 divisions, then of every 100, and after a time of every 10. These are distinguished on the drum by four rapidly repeated dashes after a 1000, three after a 500, two after a 100, one after a 50, and none after a 10. I cannot take single divisions as they pass by so rapidly. I have not used these marks except in rare instances, but they are available for reduction in case time for the very tedious calculation could be found. Fig. C is a full size reproduction of a portion of the sheet of October 1st, 1893, after I had gone over it and scratched in the actual times. The different classes of marks are all to be seen. I do not, as a rule, find it possible to put in arrows while writing elongations, and marking transits of divisions as well as of the pointer. Their existence is understood between elongations. In addition, I generally place a letter or word to distinguish good observations of transits from bad, thus: $\rightarrow$ g., $\leftarrow$ v.g., $\rightarrow$ bad, or $\rightarrow$ $\cdot 04^s$ late. I do not think the v.g. observations can be more than ·01 second in error, or the g. more than ·02 or ·03 second. Those unmarked might be perhaps as much as ·1 second, though they may also be good, but those marked bad would probably be more. If it were not for the high period tremor set up by the trains, which prevent good observations when the amplitude is less than about 40 divisions, I should expect to obtain a higher degree of accuracy in the periods than are actually obtainable. I sometimes take observations of deflection and period on more than one night.

Operation II.

Transfer the gold balls to the side hooks, and leave for a day, if possible, to quiet. Find the deflection, if any, produced upon the mirror alone, by moving the lid and lead balls from the + to the − positions. Find also with large amplitude, the exact period with and without the counterweight, using the drum and following all the details given in operation 10. At this stage it is convenient to set up the cathetometer and measure the stretching of the torsion fibres by observation on the bottom hook of the mirror. This is not necessary for the purpose of finding G, but it is of interest as bearing upon the elastic properties of quartz fibres.

Operation 12.

Turn the lid round to the neutral position. Place the steel bands on the flat-rimmed pullies. Pin to the ball holders, and hang on at the other end the counterweight. Raise the balls about $\frac{1}{8}$ inch, turn them individually through 60° and let the geometrical clamps down through the triangular orifices of the lid pillars, until the balls rest on the india-rubber rings on the base. Raise the lid, leaving it balanced in the air by its counterpoise, and after removing the two counterweights and the holding pins, take away the steel bands. Let down the lid again. Partly balance it as before. Put screens and octagon house in position as before, and after a day or two take deflections, if any, when the lid and lid pillars are moved between the + and − positions. Three sets of six elongations at least should be taken at each position.

Operation 13.

Re-suspend the gold balls in the same position as before, and find the deflection of the mirror, if any produced, by moving the lid and lid pillars from the + to the − positions.

Operation 14.

Take the focussing collar off one microscope of the optical compass, and slide it on to the nose end of the other, so as to raise it high enough to see the side of the bottom hook of the beam mirror. Remove the front lens of the object-glass, which reduces the magnifying power to rather less than one-half. Set the optical compass so that the vertical tangent to the curve of the lower hook is on the zero of the micrometer scale in the eye-piece, that is when the mirror alone is freely suspended. Hang on the counter-weight and take the scale reading. Hang on the gold balls instead of the counter-weight and take the scale reading again. The object of this is to find to what extent, if any, the axis of rotation of the beam changes when the balls or the counterweight are suspended (see p. 34).

Part III.

The calculation of the results from the figures obtained by observation is divided into four sections:—(1.) The deflections and periods; (2.) The geometry of the apparatus; (3.) The dynamics of the moving system; and (4.) The combination of these resulting in the determination of G, the Newtonian constant of gravitation, and indirectly of Δ, the mean density of the earth. In the preparation of this part I have been greatly helped by Mr. S. G. Starling, of the Royal College of Science, who has carried out the laborious numerical calculations.

1. *The Deflections and Periods.*

The treatment of the figures obtained during the observations of deflection will be best explained by an example. I give two consecutive sets, one the worst obtained the whole night, and the other a particularly good one, but others obtained that night were practically as good.

Sept. 17, 1893.

A = 150°·9.		13h. 0m.		15°·15 C.		
24907	−24	24883				
			716		390	24493
24193	−26	24167		·835		
			598		326	24493
24789	−24	24765		·834		
			499		271·5	24493·5
24292	−26	24266		·838		
			418		226	24492
24709*†	−25	24684		·848		
			354·5		192·3	(24491·7)
24355†	−25·5	24329·5		·842		
			298·5			
24654†	−26	24628				24492·9

A = 20°·9.		13h. 15m.		15°·15 C.		
19888	−19	19869				
			1702		926·4	20795·4
21591	−20	21571		·8373		
			1425		775·3	20795·7
20165	−19	20146		·8379		
			1194		649·5	20795·5
21359	−19	21340		·8384		
			1001		544·5	20795·5
20358	−19	20339		·8382		
			839			
21197	−19	21178				20795·5

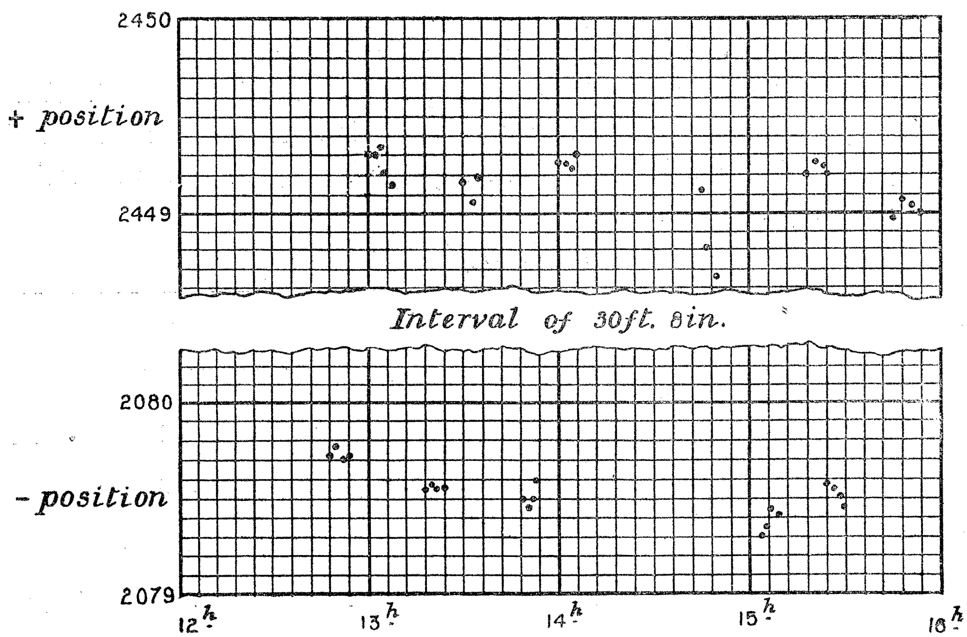
* A short period oscillation or tremor, set up probably by a passing train.

† A pendular oscillation, set up probably by a passing train.

A represents the lid reading differing from 85·9, the neutral position, by the azimuth 65° of the lead balls. The actual time of the beginning of a set is next marked and then the temperature of a thermometer set up close to the apparatus. This is illuminated and read by the same means that are provided for reading the lid vernier.

The first column of figures are observed elongations expressed in tenths of scale-divisions. The second column contains the scale corrections, in which are included

Fig. D.



both the calibration and the circular errors. The third column are the corrected elongations, the fourth the differences of these or amplitudes, the fifth the ratio of these or the decrements, in the sixth are the quotients of each amplitude by 1 + the following decrement. The last column contains the algebraical sums of the corresponding numbers in the third and sixth columns. They represent the points of rest during the course of the oscillation. Each series gives a mean point of rest of which a dozen or more may be obtained in one night. I prefer to arrange all the individual points of rest as well as the mean points in their proper places in a diagram, of which abscissæ represent the hour and the ordinates the points of rest, but on a very large vertical scale. The one of these for the evening's work chosen for illustration is shown in fig. D, from which it will be seen that a gap of 368 inches has been cut out to save room. The series of mean points of rest are shown in the following table :—

Time.	Position.		
	Neutral.	Positive.	Negative.
h. m.			
12 32	22649·9		
12 46			20797·3
13 0		24492·9	
13 15			20795·5
13 30		24491·3	
13 43			20795·1
13 57		24492·5	
Oscillation of large amplitude for period.			
15 2			20793·7
15 15		24492·2	
15 21 (?)			20795·2
15 42		24490·2	

The deflections P are obtained by taking these in groups of consecutive threes, to allow as far as possible for steady creeping of the zero. It will be seen that in the present instances there is nothing very definite that can be attributed to this.

The four deflections obtained in this way before the time interval are—

3696·5
3696·6
3696·0
3696·8

The two after are—

(3697·7)
3696·0.

As the mean point of rest 20793·7 was taken immediately after the oscillations of large amplitude produced by the air draught and was definitely slightly disturbed, I have not included the resulting deflection 3697·7 in the series from which the true deflection on that particular night was determined. The agreement is exceedingly close, so that the mean of the five values $P = 3696{\cdot}4$ may be taken with considerable confidence.

The observations on this night were rather more consistent than usual, owing, as I believe, to the very unusual quiet noticed at the time.

I only took one observation of period on this night, recorded as follows :—

A = 150°·9. Pointer at 24520 Drew air once.
− 27
24493 No time correction worth making.

Times of Transit.

h.	m.	s.				Amplitudes.	Points of rest.
14	20	36·34		→ g., time mark, 14h. 21m.			
	22	13·1		← g.			
	23	49·7	+ ·02	→ ·02 early.			
	25	26·58	− ·06	← ·06 late.			
	27	3·01		→ v.g. Castle mark.			
	28	39·68		←			
				Interval.			
	41	32·7		← Time mark, 14h. 42m. Castle mark.			
	43	9·42		→ v.g.			
	(44	46·1)		← Pendular oscillation.		(Intermediate figures not printed.)	
				26915 − 26	All bad with pendular disturbance		24491·2
	46	22·87				4406	
				22505 − 22			24488
	47	59·42				3690	
				26199 − 26			24486·6
	(49	36·1)		Bad transit		3101	
				23106 − 24			
	51	12·48				2601	
				25698 − 25 Castle mark.			
	52	49·35		Transit.			

If the pointer had been found to have been definitely out of place, but, of course, by a small amount, I should have corrected the observed times of transit by a series of alternate + and − quantities, calculated from the amplitude, period, and error of position. In the present case, owing to disturbance, the point of rest showed an uncertainty of nearly half a division, and I could not be sure from these observations that the pointer was not placed that much in error. On the other hand, after subtracting the time of each transit from that above, the series of observed half periods show a small, fairly regular, increasing and alternating second difference, which is in itself a sign that the pointer was very slightly on one side of the true position of rest. If no account is taken of this, the half period deduced from the first and last observation is found to be

96·650 seconds ;

from the first observation marked *g* to the last marked *g* it is

96·649 seconds ;

and if the times of transit are corrected by one quarter of their second difference, so as to approximate to the times of transit of the point of rest, the half period over the whole series becomes

96·645 seconds.

The object of examining so many transits is not so much with the idea of applying methods of least squares or of otherwise equalising errors, but mainly to see that the oscillation is going on regularly and that no sudden disturbance has arisen which, if it were undetected at the time, might become lost and yet leave the result in error by an observable amount. I find that in the present instance I did not try to improve upon the observations by arithmetical manipulation, and that 96·650 was taken as the observed half-period. Two small quantities had to be subtracted from this, one a correction of — ·0034 due to a gaining rate of two seconds a day of the clock, and one of — ·1508 on account of the damping effect of atmospheric resistance. The true whole period for no damping then becomes 192·992 seconds, and its square 37245·9 is the quantity which is finally made use of in the dynamical calculation. It is recorded as T_B^2, the square of the time with the balls on. In a similar manner T_C^2 is found when the counterweight is on, and T_0^2 when the mirror alone is swinging.

Under this heading the deflections produced in Operations 11, 12, 13 must be considered. The deflection in Operation 10 is due to four possible attractions:—

(1.) Lead balls, &c., on gold balls.
(2.) Lid and permanent fixtures, &c., on gold balls.
(3.) Lead balls, &c., on beam mirror.
(4.) Lid and permanent fixtures, &c., on beam mirror.

The deflection, if any, of Operation 11, is due to (3) and (4) above. Similarly the deflection produced by Operation 12 is due to (4) alone, and of Operation 13 to (2) and (4). Knowing, therefore (1) + (2) + (3) + (4); (3) + (4); (4); and (2) + (4); (1), (2), (3) and (4) are separately determined.

I have on two occasions since Experiment 3 was completed (which was of a semi-provisional nature) carried out the deflection observations of Operation 11. On September 1, 1893, 1·5 units or ·15 division was obtained. There was a very slight + creep. On September 11, with no creep and very consistent behaviour, ·5 unit or ·05 division was obtained. I do not know whether I should take ·5 or 1 unit. The difference is beyond what can be observed with any certainty.

I find that the lid was turned 180° between the two experiments, but this could not make any difference. I have taken 1 unit as the deflection in Experiments 4 to 12, and have calculated what it should be in Experiment 3.

Most careful observations on September 2 and 3, 1893, failed to show any deflection under Operations 12 and 13; certainly not 1, and not, apparently, ·1 unit. From this it will be evident that since

$$
\begin{aligned}
(3) + (4) &= 1, \\
(4) &= 0, \\
(2) + (4) &= 0,
\end{aligned}
$$

$(2) = 0$, $(3) = 1$, and $(4) = 0$. Therefore 1 unit must be subtracted from the observed deflection of Operation 10 in all experiments after No. 3. It is for this reason that the numbers under P in Table I., p. 63, differ very slightly from the observed deflections.

2. *The Geometry of the Apparatus.*

In this part of the calculation I find the exact relative positions of the several gravitating bodies, from which the couple twisting the fibre may be found in terms of G. Thus, the couple = QG. As before, the process followed will be most readily explained by giving an example.

Experiment 8.

$\Theta = 65° - 22' = 64° \, 38'$
$R_H = R_L \quad = 2·99304$
$r_H = r_L \quad = ·446650$
$\log r \quad = \bar{1}·6499673$
$p = R \sin \Theta = 2·704465$
$b = R \cos \Theta = 1·282247$
$b - r \quad = ·835597$
$b + r \quad = 1·728897$

$\sin 64° \, 38' = ·9035847$
$\cos 64° \, 38' = ·4284095$

$p^2 = 7·314131$

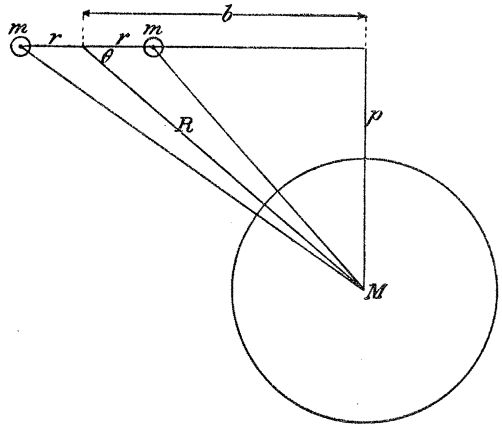
$(b - r)^2 = ·698222$
$(b + r)^2 = 2·989085$

h_L = ·0321	h_L^2 = ·001029	h_H = ·0516	h_H^2 = ·002660
H_L = 6·048339	H_L^2 = 36·582411	H_H = 6·02885	H_H^2 = 36·347032

	Low.	High.		Low.	High.
p^2	7·314131	7·314131	p^2	7·314131	7·314131
$(b - r)^2$	·698222	·698222	$(b + r)^2$	2·989085	2·989085
h^2	·001029	·002660	H^2	36·582411	36·347032
D^2	8·013382	8·015013		46·885627	46·650248
log D^2	·9038157	·9039042		1·6710397	1·6688539
log D	·4519078	·4519521		·8355198	·8344269
log D^3	1·3557235	1·3558563		2·5065595	2·5032808
log p	·4320814	·4320814		·4320814	·4320814
log p/D^3	$\bar{1}$·0763579	$\bar{1}$·0762251		$\bar{3}$·9255219	$\bar{3}$·9288006
log M	3·8696230	3·8696634		3·8696230	3·8696634
log m	·4234097	·4234163		·4234163	·4234097
log r	$\bar{1}$·6499673	$\bar{1}$·6499673		$\bar{1}$·6499673	$\bar{1}$·6499673
log *couple*	3·0193579	3·0192721		1·8685285	1·8718410
Couple	1045·580	1045·371		73·880	74·446
	1045·371			74·446	
	2090·951			148·326	
	148·326				
	Q = 1942·625				

In order to determine the moments of the attractions of the large balls M, M upon the small ones m, m, the distances represented in figure E by p and r, and the true distances D between M, M and m, m are required, also the masses M, M

Fig. E.



and m, m — that of displaced air. Then the moments are for any one attraction out of the four possible,

$$\frac{\mathrm{GM}mpr}{\mathrm{D}^3}.$$

These are most readily obtained from the observations in the manner set forth on the last page, where every figure made use of is set down. Single multiplications are performed on the arithmometer more quickly than by logarithms, hence natural sines and cosines are employed. Continued multiplications are more conveniently performed by logarithms, and the change from D^2 to D^3 can only be so effected.

The angle, θ, is the amount through which the lead balls are turned from the neutral position — the angle through which the gold balls are deflected.

R, the radius of motion of either lead ball, is half the distance between the axes of the wires, and they are taken in the calculation as being identical, *i.e.*, there is supposed to be no eccentricity. In Experiment 5 I calculated the result both on this supposition and more laboriously giving the true and slightly different values to $\mathrm{R_H}$ and $\mathrm{R_L}$, the radius of motion of the upper and of the lower ball respectively. The difference in the result only amounted to 3 parts in 2,000,000, and so, as I explained on p. 26, a small eccentricity, if it exists, is of no consequence.

r, the radius of motion of either gold ball, is half the distance between the axes of the fibres. The next few lines explain themselves; they simply give intermediate quantities required for the solution of the different triangles. h_{L} and h_{H} are the differences of levels of the centres of the lower gold and lead balls and upper gold and lead balls respectively. $\mathrm{H_L}$ is the difference of level of the lower lead and upper gold ball and $\mathrm{H_H}$ the other great difference of level. The four couples obtained are those due to the attraction of the two pairs at the same level and of the two pairs at different levels. The latter are in the opposite direction to the former, and are therefore subtracted. The result, 1942·625, when multiplied by G, is the actual moment produced upon the torsion fibre by the action of the balls upon one another upon the supposition that the balls are all spheres, and act as if they were concentrated in their centres. The brass ball holders, as already explained, cause the forces to be actually more than they appear to be, on the assumption that the lead balls act as if they were concentrated in their centres, so that $1 + \cdot0001366$ is the factor by which the couple must be multiplied to correct for the brass ball holders. The corresponding correction for the gold ball holder is only $\frac{1}{2500000}$ of the whole. One small correction, too small to matter, but which I have calculated with some labour, is the stability due to the gravitation of the table. This introduces a restraining couple upon the balls alone in Experiment 8 of $\frac{1}{2400000}$ of the whole. Finally, there is the correction already mentioned on p. 31 of $\frac{-1}{263,000}$ on account of the attraction of the gold ball for the wires and the lead ball for the fibres. Combining all these, the actual couple developed is found to be equal in Experiment 8 to $1942{\cdot}882$ $\frac{\text{inch}^2\text{ gramme}}{\text{second}^2}$ units.

3. *The Dynamics of the Moving System.*

As before, I shall take my example from Experiment 8.

Moment of inertia of counterweight No. 3 or C . . $= \cdot0163120$

T_B^2	$37245{\cdot}9$	$T_B^2 - T_C^2$ $35547{\cdot}17$
T_C^2	$1698{\cdot}73$	

T_0^2 not taken.

Moment of inertia added to beam when balls are placed in position, called B.

$$B\left\{\begin{array}{lll} \text{balls translated } 5{\cdot}302804 \times \cdot44665^2 & \ldots\ldots & = 1{\cdot}0578893 \\ + \text{ hooks translated } 0{\cdot}1190 \times \cdot379^2 & \ldots\ldots & = \cdot0017093 \\ + \text{ balls rotated } \cdot4 \times 5{\cdot}300252 \times \cdot126134^2 & \ldots\ldots & = \cdot0337303 \\ + \text{ hooks rotated } \cdot012 \times \cdot025^2 & \ldots\ldots & = \cdot0000075 \end{array}\right.$$

$$\begin{array}{lll} B & \ldots\ldots & = 1{\cdot}0933364 \\ C & \ldots\ldots & = \cdot016312 \\ \hline B - C & \ldots & = 1{\cdot}077024 \end{array}$$

$$U = \frac{BT_C^2 - CT_B^2}{T_B^2 - T_C^2} = \cdot0351565,$$

$$S = \frac{4\pi^2 (B - C)}{T_B^2 - T_C^2} = \cdot001196133.$$

The moment of inertia of the counterweight is directly obtained from its dimensions. The moment of inertia added to the beam requires more explanation. When the balls are hung on to the beam in the manner already described, they add to its moment of inertia, both on account of their distance from the axis, and on account of their own moments of inertia, about their own axes. Besides the balls the small wire hooks and the quartz fibres produce their own effects. These are found in the four lines bracketed B. In the first line the mass of the balls is made up as follows:—

Mass of gold balls + wire holders, corrected for buoyancy as against brass weights, but not absolutely, + $\frac{1}{2}$ mass of displaced air	5·302204
+ mass of quartz fibres	·00060
	5·302804

this is multiplied by the square of the radius r.

In the second line the radius of the support of the hook is obtained by direct measurement of the beam itself. It is relatively unimportant. In the third line the mass of the balls does not include the ball-holders or fibres or (perhaps wrongly) that of any surrounding air. The radius of the ball is found by a screw micrometer. The fourth line needs no explanation; it is infinitesimal.

U is found from the formula placed next to it. This is the moment of inertia of the mirror. It is not required in the calculation, but is found for the purpose of comparison. It should be constant.

S represents the stiffness of the torsion fibre, *i.e.*, the couple that must be applied in order to twist it through unit angle ($57^{\circ}{\cdot}296$).

If the unknown moment of inertia had been eliminated by the usual method, that is by supposing the torsion constant while the mirror was made to swing either *with* or *without* a known added moment of inertia (in this case, that due to the balls) then T_B^2 and T_0^2 would have been required. Taking T_0^2 from the previous experiment when it was found to be 1168·00,

$$\text{U becomes } \frac{BT_0^2}{T_B^2 - T_0^2} = {\cdot}035396$$

and

$$\text{S becomes } \frac{4\pi^2 B}{T_B^2 - T_0^2} = {\cdot}001196375.$$

Since the torsion is not the same when T_B^2 and T_0^2 are being found, as in one case the fibre is much more strongly stretched than in the other, and is therefore longer and thinner, and is not necessarily made of a material having the same rigidity, the above figures are spurious. They differ from the true figures found in the previous page, but U, which is eliminated differs far more than S, which is made use of. The reason of this is made clear if a correction, θ in terms of S, is included in the expressions for U and S. Since the fibre becomes stiffer when the balls are taken off, it is simpler to consider this correction as a stiffening of the fibre when it is unloaded instead of the reverse when it is loaded. The expressions are now :—

$$U = \frac{BT_0^2 (1 + \theta/S)}{T_B^2 - T_0^2 (1 + \theta/S)},$$

$$S = \frac{4\pi^2 B}{T_B^2 - T_0^2 (1 + \theta/S)}.$$

It now appears that in both, the denominator is affected to the same extent, which is very small since T_0^2 is small compared with T_B^2. On the other hand the numerator of S is not affected, while the correction applies to the whole numerator of U.

The very great effect of this upon the absolute value of S is well shown in Experiment 9, where the additional weight was 7·975 instead of 5·314 grms. In consequence of the extra amount of stretching S fell from ·001196 to ·001147, or nearly 5 per cent. The actual elongation of the torsion fibre in the two cases was ·0394 and ·0677 inch. The whole length of the fibre was 17 inches, so the amount of stretch was ·232 and ·398 per cent. Even if the volume of the fibre remained constant the diminution of torsional rigidity could not be accounted for with a material of constant rigidity. This point is perhaps worth considering in connection with Poisson's ratio and the theory of elasticity, more especially in consequence of the great hardness, freedom from structure and possible elongation without permanent deformation or change which are met with in a quartz fibre. I must, however, defer its discussion for the present or leave it to some one more competent than myself.

4. *The Combination of the preceding Three Results.*

The method of combining the results given in the first three sections of this part is simple enough. From the first section, the deflection in scale divisions when the lead balls are moved from the + to − positions is obtained. Let this be called P. The second section gives Q the numerical coefficient of G; thus, Q G is the couple exerted upon the fibre. From the third section, the actual couple S that would be needed to twist the torsion fibre through an angle of 1 unit (57°·3) is obtained without any reference to G, and D being the actual distance in units or tenths of a scale division from the scale to the mirror measured as explained on p. 17, it follows

$$G = PS/4QD.$$

The 4 in the denominator is due to the doubling of the angle by reflection and to the doubling of the deflection by moving the lead balls from the + to the − positions.

G is thus obtained in inch^3/gramme second^2 units; to convert it into centimetre^3/gramme second^2 units it is merely necessary to multiply by 16·3861, the number of cubic centimetres in a cubic inch.

To obtain from this the density of the earth I might have recalculated the attraction of the earth treated as a rotating ellipsoid composed of similar shells of equal density as given in Professor POYNTING's paper, but since it is obvious that G Δ is a constant, and is, taking POYNTING's figures, equal to $36{\cdot}7970 \times 10^{-8}$, it is merely necessary to divide this figure by G to find Δ.

Again, taking Experiment 8 to furnish an example, these operations are as follows :—

$$G = \frac{PS}{4QD} = \frac{3695{\cdot}4^{*} \times {\cdot}00119598^{\dagger}}{4 \times 1942{\cdot}882 \times 139965} = 4{\cdot}06312 \times 10^{-9}$$

in inch^3/gramme second^2 units.

Multiply by 16·3861, then $G = 6{\cdot}6579 \times 10^{-8}$ in C.G.S. units, and $\Delta = 5{\cdot}5268$.

The more important quantities of the whole series of experiments are exhibited in Table I, which, as the heading shows, is constructed on the Inch, Gramme, Second system, in conformity with the actual measurements. The supplementary table is a repetition of the most important quantities in C.G.S. units. Here below the constant of gravitation G is to be found the series of values of Δ the mean density of the earth. Appended are CORNU's and POYNTING's values, CORNU's G being obtained from his Δ in the same way that I obtained my Δ from my G.

An examination of the table shows that I have employed a fair variety of conditions, the lead balls alone being unchanged throughout the series. Three pairs of small masses were made use of. The lead balls were practically unchanged in distance, though, after Experiment 7, they were brought nearer together by $\frac{1}{50}$ inch about. The effect of this on the deflection P and the couple Q is at once evident in Experiment 8. Three fibres were employed, though, as already mentioned, the rigidity was very different in Experiment 9 owing to the great longitudinal strain. The different torsional rigidities are tabulated under S.

The periods are tabulated under their squares, *i.e.*, after correction for damping. T_B^2 is with the gold balls or cylinder suspended from the mirror and with the lead balls at a + or − position, where, by producing a maximum couple, they do not affect the period. T_N^2 is the corresponding period with the lead balls in their neutral position where they accelerate the period. I might, following REICH, have independently calculated G from the acceleration produced in this way, but these periods were not determined with the same care as the others, and in any case, the difference is too small for an equally accurate result to have been obtained. T_C^2 and T_0^2 are the square of the periods with counterweights and with nothing on the mirror.

The pairs of quantities tabulated under H_L^H and h_L^H are the four differences of levels between the lead and gold balls. Thus 6·0296 in Experiment 5 is the

* 3695·4 = 3696·4 − 1. See p. 56.

† ·00119598 = ·00119613 (1 − 1/7850). See p. 35.

TABLE I.—Inch, Gramme, Seconds.

Exp.	3	4	5	6	7	8	9	10	11	12
M No. 1	7406·67 for all									
M No. 2	7407·36									
m No. 3	1·2983	2·6511	for all but Nos. 3 and 9	..	..	..	3·9816			
m No. 4	1·2983	2·6511		..	..	..	3·9831			
R	3·00400	..	3·00421	3·00420	3·00312	2·99304	2·99310	2·99273	Hydrogen experiment	2·99269
r	·446411	..	·446663	·44665	Same as	No. 6	·44694	·44665		·44669
H_L^H	6·0680	..	6·0296	6·0272	6·0272	6·02885	5·97019	6·0005		6·02927
			6·0139	6·0084	6·0084	6·04834	5·96113	6·0312		6·00132
h_L^H	·01	..	·0059	·00275	·00275	·0516	·03780	·0315		·0027
			·0216	·0161	·0161	·0321	·02874	·0008		·0307
T_B^2	58519·35	..	37227·0	..	..	37245·9	58105·6	35441·9		35430·9
T_N^2	57593·76	..	36839·4	Same as	No. 8	..	57254·9			
T_C^2	4219·022	..	1702·35			1698·73	1975·625	1619·322		1617·81
T_O^2	3665·092	..	1168·00	..	..	..	1167·971			
P	5637·3	..	3667·6	3667·7	3664·0	3695·4	5775·5	3515·4		3520·5
Q	943·973	..	1925·768	1925·544	1927·169	1942·882	2914·202	1942·509		1942·804
S	·000380515	..	·00119681	Same as	No. 8	·00119598	·00114703	·00125697		·00125755
G $10^{-9}\times$	4·05887	..	4·07121	4·06899	4·06144	4·06312	4·06036	4·06310		4·07025
Centimetre, Gramme, Seconds.										
Q	6089·89	..	12423·8	12422·3	12432·8	12534·2	18800·5	12531·8		12533·7
S	·00245483	..	·00772104	Same as	No. 8	·00771568	·00739988	·00810915		·00811289
G $10^{-8}\times$	6·6645	6·6702	6·6711	6·6675	6·6551	6·6579	6·6533	6·6578		6·6695
Δ	5·5213	5·5167	5·5159	5·5189	5·5291	5·5268	5·5306	5·5269		5·5172

	CORNU.		POYNTING.	
	Summer mean.	Winter mean.	Set I., mean.	Set II., mean.
G $10^{-8}\times$	6·6181	6·6903	6·6661	6·7393
Δ	5·56	5·50	5·52	5·46

difference of level of the upper lead and lower gold ball, while 6·0139 is the corresponding difference between the other pair. Similarly ·0059 is the difference of level of the upper lead and upper gold balls, and ·0216 the difference of level of the lower pair of balls.

In order to eliminate as far as possible any systematic errors that might arise for instance from imperfections of the copper central tube, which of course is very near the gold balls, or from want of perfection in the lead balls, though nothing comparable with the error of experiment need be feared on this account, I made some change in the circumstances after every experiment. Thus, after Experiments Nos. 1 and 2 had been carried out, which were merely preliminary so that I might learn something of the behaviour of the apparatus, I arranged the balls in Experiment 3 as follows:—

Lead ball,	No. 1,	Wall side,	High level,
,, ,,	,, 2,	Arch ,,	Low ,,
Gold ,,	,, 3,	Wall ,,	High ,,
,, ,,	,, 4,	Arch ,,	Low ,,

Neutral reading of lid 267°.

The arch side is the right as seen from the telescope, or the left as seen from the back when the optical compass is in use. This expression depending upon the structure of the cellar avoids ambiguity.

The corresponding arrangement in the whole series of experiments with the dates at which they were carried out is given in Table II.

As would be expected, I had not at first all the details so complete as at the end of the series of experiments. Thus, in Experiment 3, the air drawing arrangement for steadying the mirror or starting it into motion, had not been thought of. At that time, for want of a better arrangement, I had to enter the corner and withdraw the back window, fig. 12, so as to allow the accidental movement of the air to start a swing of great amplitude. As I have already indicated, the decrement is, in spite of all I can do to prevent it, inconveniently high, so that periods extending over forty-five minutes cannot be taken unless the mirror has an oscillation of large amplitude at starting, far larger than two or three reversals of the lead balls in time with the oscillation would set up. The plan was essentially bad owing to temperature, disturbance, and tremor, nevertheless the observations made at the time were fairly consistent, thus two periods in the + position on October 16th, gave 241·93 and 241·90. The next day I put up the felt screens and two periods were 241·9(4) in the + position, and 241·88 in the negative. I then introduced a different method of producing a swing of great amplitude, viz., in the air tube then screwed into the back window, fig. 12, I fitted a glass stop-cock which could be turned on or off without touching the metal windows or screens. I did this hoping that a clockwork fan belonging to a lamp would produce a gentle draught upon the end of the mirror when set in action opposite a

TABLE II.

No. of experiment.	Lead balls.		Position of shellac spots.	Gold balls.		Neutral lid reading.	Date.	Notes as to additions, alterations, or improvements.
	Arch side.	Wall side.		Arch side.	Wall side.			
3	No. 2 low	No. 1 high	Inwards (?)	No. 4 low	No. 3 high	267°	1892. Oct. 1 to Oct. 30	During this experiment put up the underfelt and concentric metal screens
				(Gold balls of double the weight.)				
4	No. 2 low	No. 1 high	Inwards	No. 4 low	No. 3 high	267	1893. Aug. 15 to Sept. 3	First used soldered fibres Amalgamated platinum pendulum point Wafting abolished; replaced by drawing air Unamalgamated platinum pendulum point
5	No. 1 high	No. 2 low	Inwards	No. 3 high	No. 4 low	86·5	Sept. 4 to Sept. 11	Earthquake (Sept. 9)
6	No. 2 low	No. 1 high	Inwards	No. 4 low	No. 3 high	265·9	Sept. 12 to Sept. 14	
7	No. 2 low	No. 1 high	Outwards	No. 4 low	No. 3 high	265·9	Sept. 15	
8	No. 1 low	No. 2 high	Inwards	No. 4 low	No. 3 high	265·9	Sept. 16 to Sept. 18	
				(Cylinders.)				Put up octagon house
9	No. 1 low	No. 2 high	Inwards	No. 3 low	No. 1 high	86	Sept. 27 to Oct. 3	
10	No. 1 low	No. 2 high	Inwards	No. 4 low	No. 3 high	85·25	1894. Jan. 1 to Jan. 13	First put up the magnetic arrangement for recovering lost gold balls, &c.
11	No. 1 low	No. 2 high	Inwards	No. 4 low	No. 3 high	85·25	Jan. 14	(Hydrogen experiment)
12	No. 2 high	No. 1 low	Inwards	No. 3 high	No. 4 low	265·2	Jan. 17 to Jan. 21	

funnel-shaped end to the pipe. This did not work, and I was therefore compelled at the time to produce the draught by some ready means; gentle movement of a sheet of paper at a distance from the instrument produced the desired motion, but my going near the instrument at all was essentially bad. The periods obtained after this were:—

Oct. 29 . . .	241·99	+	position.
,, ,, . . .	241·91	—	,,
,, 30 . . .	241·88	—	,,
,, ,, . . .	241·93	+	,,

I noticed slight differences between periods taken with the lead balls in the + and — positions with my preliminary apparatus, but, though I am not able to explain it, I am glad to say that, with the more perfect setting and screening now in use, it has practically disappeared. The two periods taken with the counterweight gave identical results, 64·954 seconds.

The soldered fibres used in all the later experiments seem a definite improvement, as the creeping of the zero, which was never very troublesome, almost entirely ceased.

The traffic and the trains are not the only causes of disturbance. Wind, by pressing upon the building and neighbouring trees, of course shakes the ground; but on Sept. 9–10, a particularly quiet night, I had to leave, owing to a sudden disturbance producing a pendular motion of 15 divisions, or 150 units, and for some time there was no quiet. As the motion was clearly produced by a lurch of the whole instrument and table carrying it, and was greater in amount than any traffic in the busiest part of the day had ever produced, and was moreover free from the high period tremor characteristic of human disturbance, I at once set it down to an earthquake. I was marking transits of every 10 divisions at the time. The moment of the last mark was 15h. 44m. 14·3s., allowing for the error of the clock as determined at the Observatory. The next mark was due in 3 seconds, but I was, of course, unable to record it. In the 'Standard' of Sept. 12th there was an account of a violent earthquake at Jassy, which was felt also at Bucharest, at six o'clock in the morning. I have not ascertained the exact time at which the earthquake was felt in Roumania, or the amount to allow for difference of longitude, but these, no doubt, can be supplied from Vienna.*

Experiment 11 was a purely comparative one. Everything being left as in Experiment 10, a tube was connected with the stop-cock in the bell-jar J, and with a hydrogen bottle and drying bottle, so that dry hydrogen could be fed in to displace the air in the central tube. The object was to see if any advantage would be derived from the smaller viscosity of hydrogen; but, though the resistance fell so as to change

* Mr. CHARLES DAVISON informs me that the shock was recorded at Bucharest at 3h. 40m. 35s., A.M., but that the epicentrum must have been some distance from there. The time interval between Bucharest and Oxford appears very small, the usual rate of travel being 3 km. a second, or a little more.

the decrement from ·842 to ·937, which in itself was a great advantage, so much difficulty seemed inherent in the method that I determined not to prosecute it. The values found this night for T_B^2 were

In air	35404
In hydrogen . . .	$3540\dot{1}$

The deflections owing to disturbance could not be so accurately determined as usual; they were

In air	3520
In hydrogen . . .	3523

The only observation of real interest in connection with the hydrogen experiment was the effect of the gas upon the mirror. The mirror was bent to a small extent, causing the image of the divisions to practically disappear. A movement of the eye-piece outwards of about $\frac{5}{8}$ inch was needed to make them appear sharp again. On letting the gas escape the focus went back to its old place, and this was repeated without variation three or four times. I imagine that the glass became convex under the influence of the hydrogen in consequence of the glass being penetrated by the quickly-moving molecules, and so becoming expanded in the front or unprotected side, while the silver and lacquer at the back prevented this action, much as paper or lace will protect glass from the cutting action of a sand-blast. The bending may have been produced by a contraction of the lacquer or silver, but this seems hardly conceivable. An interesting line of inquiry is suggested by this experiment, but I have not been able to do more at present.

An examination of the results shows that they hover about two values, experiments 3, 4, 5, 6 and 12 being about 5·520 for Δ, and the remainder about 5·528. It is impossible to trace any connection between the arrangement of the apparatus, &c., and this small irregularity. It is necessary, therefore, to review the deflections and periods of each and the conditions, whether of disturbance or quiet, under which they were carried out. No. 3 has already been discussed, and the wonder is that it should agree so well with the others when the imperfect conditions are borne in mind, and when it is remembered that the torsion fibre in this experiment had only one-third of the rigidity of those used later, while the gold balls were only half as heavy.

As already mentioned, the periods in Experiment 4 were lost, so that an absolute result could not be calculated. The values of $2r$ in the two cases only differed by ·00002 inch, an amount which is probably beyond the certainty of measurement, and therefore the results for 4 are merely obtained from those from Experiment 5, by multiplying or dividing by the ratio of the deflections.

Deflections and periods for Experiment 4 were taken over several days. The deflections were :—

	Aug. 21.	Aug. 23.	Aug. 25.	Aug. 26.	Aug. 27.	Aug. 29.
	3669·0 3669·6 .	3667·7 3668 3666·7 3667·8	3665·8 (3664·9) 3665·7	3669·7	3668·3	3668·3 3669·0 3669·4
Temperature at end of night	17°·41 C.	17°·04 C.	16°·58 C.	16°·56 C.	16°·59 C.	16°·24 C.

The mean of all but the one in brackets, which depends on a single value only, is 3668·1. The deflection for Experiment 5 is 3668·6, practically an identical quantity, so that the G and Δ of the two are almost the same. The two results should properly be considered as one. Both unfortunately depend upon unsatisfactory period observations which, when squared, varied from 37200 to 37240. On this account the results from Experiments 4 and 5, which are higher than any of the others for G and lower for Δ, should have had a bad mark put against them. In Experiment 6 the deflections were nearly as consistent as those in Experiment 4, while the periods were now nearly the same in the + and − positions, being, when squared, 37245 in the − and 37247 in the + positions. The conditions of this experiment seemed decidedly favourable, and I see no reason inherent in the observations that could lead me to doubt the accuracy of the results.

In Experiment 7 the only change was twisting the lead balls so that the sides that were inwards should be outwards. The sudden change in the result for Δ from 5·5189 to 5·5291 might seem to be due to some irregularity of density in the lead balls. But the extraordinary agreement between this result and the three following, where every kind of change was made in the conditions, including the turning of the balls again, and the change from high to low and low to high, and where, moreover, the extra steadiness of temperature due to the screening of the octagon house was introduced, shows that this argument will not hold. I cannot account for this small difference. In Experiment 8 all the conditions were most perfect. The figures for this have already been given, and so need not be repeated. I may, however, compare the squares of the periods of Experiments 6, 7, and 8, throughout which the beam mirror and gold balls were never touched, to show how much better the agreement was at this time than before.

Experiment.	Date.	− position.	+ position.
6	Sept. 14	37245	37247·5
7	„ 15	..	37242
8	„ 17	..	37245·9

The last figure of this series was taken in determining S. I may mention that a change of $\frac{1}{50}$ inch in 2R was made in this experiment, the effect of which is at once evident both in P the deflection, and in Q the geometrical factor.

In Experiment 9, the conditions were completely changed by the substitution of gold cylinders for gold balls. As already mentioned, the torsional rigidity of the fibre was altered 5 per cent. by the great tensile strain, yet with every quantity redetermined the only change in the result was about 1 part in 1500. The old conditions were realised again in Experiment 10, except that I had taken the fibre to London, re-soldered the broken ends and waited three months, but the result only differed from that of Experiment 8, by 1 part in 60,000. Perfect conditions were met with again, the mean deflections for the two nights, January 6 and January 7, being 3516·5 and 3516·3. The last experiment on January 21 was disturbed, and the points of rest varied several units in the course of the night. I was compelled, moreover, on leaving the apparatus at 17^h, to take off the gold balls, replace them by the counter-weights, and after about three hours' sleep, to return again and take the counter-weight period. This was far too soon for the temperature to have settled after disturbance, and in addition to this cause of error, I had only 10 minutes in which to take the period, and had then to hurry off with the drum record still wet, in order to be in London where I had to lecture at noon. I cannot, therefore, look upon this experiment with the same confidence as Nos. 7 and 10, and so with the exception of Experiment 6, all those that give the lower value for Δ have something against them. Under these circumstances, I cannot do otherwise than look to Experiments 7, 8, 9, and 10 as being the most likely to give a true value. Moreover, as Nos. 8 and 10 were both made under most favourable yet very different conditions, their closely agreeing figures carry more weight than the other two. I therefore conclude that $\Delta = 5{\cdot}5270$ and $G = 6{\cdot}6576 \times 10^{-8}$. The fifth figure in such case is, of course, a purely arithmetical phenomenon, but I do not think that the fourth figure can be more than 1, or at the outside 2 in error.

I had hoped to have made a greater number of experiments under more widely differing conditions, but the strain which they entail is too severe, for not only have I had to give up holidays for the last three years, but to leave London on Saturdays and occasionally to sit up all Saturday and Sunday nights at the end of a week's work. The conditions, therefore, are too difficult for such an extended series as I should like to make to be possible, and I must after one more effort, leave the problem to others who have leisure, and what is of far greater consequence, a quiet country place undisturbed by road and railway traffic, and who possess the knowledge and manipulative skill which the experiment requires.

Conclusion.

I think it might be useful, now that the autumn has passed and I have been unable to make a new series of observations, if I were to state my views as to any change of detail that might conduce to greater accuracy. I am still convinced that G may be determined with an accuracy of 1 in 10,000 by means of apparatus such as I have described.

In the general design I am unable to suggest any improvement. The weakest spot is caused by the resistance of the air making time work difficult, especially where visible shaking interferes with the usefulness of oscillations of small amplitude. I doubt if a practical gain is to be obtained by the use of hydrogen, and I am sure that a high vacuum is out of the question. The only remedy, therefore, is to employ larger suspended balls, and with them a longer beam. For the same argument that shows that with any length of beam the limit of sensibility imposed by the strength of the fibre is increased by reducing the weight of the suspended balls, for the rigidity of a fibre varies nearly as the square of its strength, shows also that if the fibres are not far from their breaking weight, the size of the balls cannot be increased without reducing the sensibility. But increase of size would be advantageous because, while the forces depend upon the cube of the diameter, the resistance to movement depends upon the square. The result is a less serious decrement with larger balls. Now, in order to employ these and yet maintain the period with the necessarily stronger fibre, a longer beam must be employed. Of course, unless the diameter of the large attracting balls are increased in the same proportion the angle of deflection will fall. I do not think that the beam needs to be lengthened to more than about two inches. If this length were adopted it would be better to aim at 5 centims., for since this is half a decimetre, a more accurate determination of the length could be obtained by reference to the standard decimetre at Sèvres, than would be possible if it were not very nearly an exact submultiple. Whether or not the lead balls and the whole apparatus should be doubled in size is a mere question of cost. The expenses would run up very rapidly with very moderate increase of sensibility. I should feel disposed to be content with lead balls about six inches in diameter, but I would certainly have an Elmore tube for the centre one T, and, by preference, for the large cylinder C. The slight diminution of angular deflection which would result from this change, would be more than compensated by the doubled optical definition, but there might be some difficulty in obtaining a thin rectangular mirror 5 × 1 centims., in which no optical defect could be detected.

It may appear that I am reversing all my arguments and practice in now advocating an increase of size, but it must be remembered that the object is not so much to increase the sensibility, or even to be able to make better geometrical determinations, for both of these, in my apparatus, exceed the square of the periods in the degree of accuracy with which they are known. The object is solely to be less influenced by the viscosity of the air, but this would not have limited the accuracy of my periods so seriously if I had not been disturbed by trains.

I should have had less confidence in this doubling of the size, if my supposition that the disturbing moments, due to convection, were proportional to the seventh power of the linear dimensions, had been correct. Since it varies only as the fifth power, and the quietness of the air is so great in my apparatus (p. 11), there can be no objection on this account to the double size; but I would strongly urge that in such a case, a room more uniform in temperature than the one at Oxford should be employed. It would also be well to lay non-connecting mats on those parts of instruments on which the hands are apt to rest when the balls are being transferred, or other manipulative operations are being carried out, so as to reduce, as far as possible, access of heat, and hence the interval that must elapse before observations of deflections or periods can be undertaken. I do not think any ready-made room is likely to be found available. A disused adit, at a great distance from existent mining operations, would be perfect, if it could be made use of. The instrument could then be walled up in a room to itself, and the heat from the observer and the travelling lamp excluded far more perfectly than in my case. An adit would be convenient also, in that it would allow of the use of a greater distance from the scale to the mirror than could be obtained in an ordinary room. This should not be less than 20 metres.

I should recommend a slight change in the upper end of the lid pillars with the object of giving the ball holder two adjustments, one radially, and one at right angles to a radius, so that eccentricities observed by the microscope of the optical compass could be corrected.

I also think more pains should be taken with the beam mirror to insure its rotating about its own centre of gravity, both when the gold balls and when the counterweight are suspended. This would remove any doubt as to its constancy of movement of inertia when made to oscillate under the three conditions, and would at the same time make observable eccentricity of the gold balls impossible.

Finally, I have suffered much from the great loss of time that results from the accidental fall of a gold ball down the central tube. It can only be replaced after lifting out the torsion head, torsion fibre, and beam mirror, so that all the centering adjustments are lost, besides which, there is the serious risk of breaking the torsion fibre whenever this operation is carried out. I would make the lower end of the central tube funnel-shaped inside, and employ a much larger holding down screw, with a central hole more than large enough to allow the gold ball to escape through it. This could, of course, be plugged at other times. In order to put the gold balls into their places without removing the torsion fibre, I would have a large hole in the torsion head behind (away from the big telescope) the torsion rod, and this would be easy with the larger central tube. Then if a special overhead wheel were placed with its edge vertically over the centre of this hole, the gold ball could be let down as described in the paper and transferred, as usual, on to one of the side hooks by the use of a simple tool made of a bent pin.

In every other respect the apparatus behaves so perfectly, and the operations are conducted with such facility, that I am unable to offer any other useful suggestion.

Description of Plates 1 and 2.

PLATE 1.

Fig. 1 is a vertical section, and fig. 2 a sectional plan of the apparatus. Fig. 3 is a front view of a portion of it. Figs. 4, 5, 6 show details of the lid, pillars, and ball-holders on a larger scale. Fig. 7 represents the beam-mirror, counterweight, and eye-hooks full size. Figs. 8 to 10 show the window apertures in the central tube, beam raiser, &c., on a larger scale. Figs. 11 and 12 are the front and back windows used when deflections and periods are being observed. Figs. 13 and 14 show the back window for use with the optical compass. Fig. 15 represents the tubular screens; figs. 16 and 17, the mould for pressing the $4\frac{1}{4}$-inch lead balls.

PLATE 2.

Figs. 18, 19, 20, and 21 are plan, side, and end elevations of the cellar with the apparatus in position. Fig. 22 is a horizontal section of the octagon house, the position of the apparatus being shown in dotted lines. Fig. 23 is an isometric projection of the geometrical clamps holding the scale and dummy. Figs. 24, 25, 26 are plan, front and side views, of the rotating and focussing slides and scale of the optical compass in position on the lid of the instrument, which is represented in chain lines. Figs. 27, 28, 29 are plan, front and side views, of the traversing slides and microscopes of the optical compass in position on the focussing slide, which is represented in chain lines.

Scale of Figs. Plates 1 and 2.

Scale	$\frac{1}{4}$	$\frac{1}{3}$	$\frac{1}{2}$	1	$\frac{1}{4}$ inch to foot.
Figure . . .	15 16 17 22 23	1 2 3 24 25 26 27 28 29	4 5 6 8 9 10 11 12 13 14	7	18 19 20 21

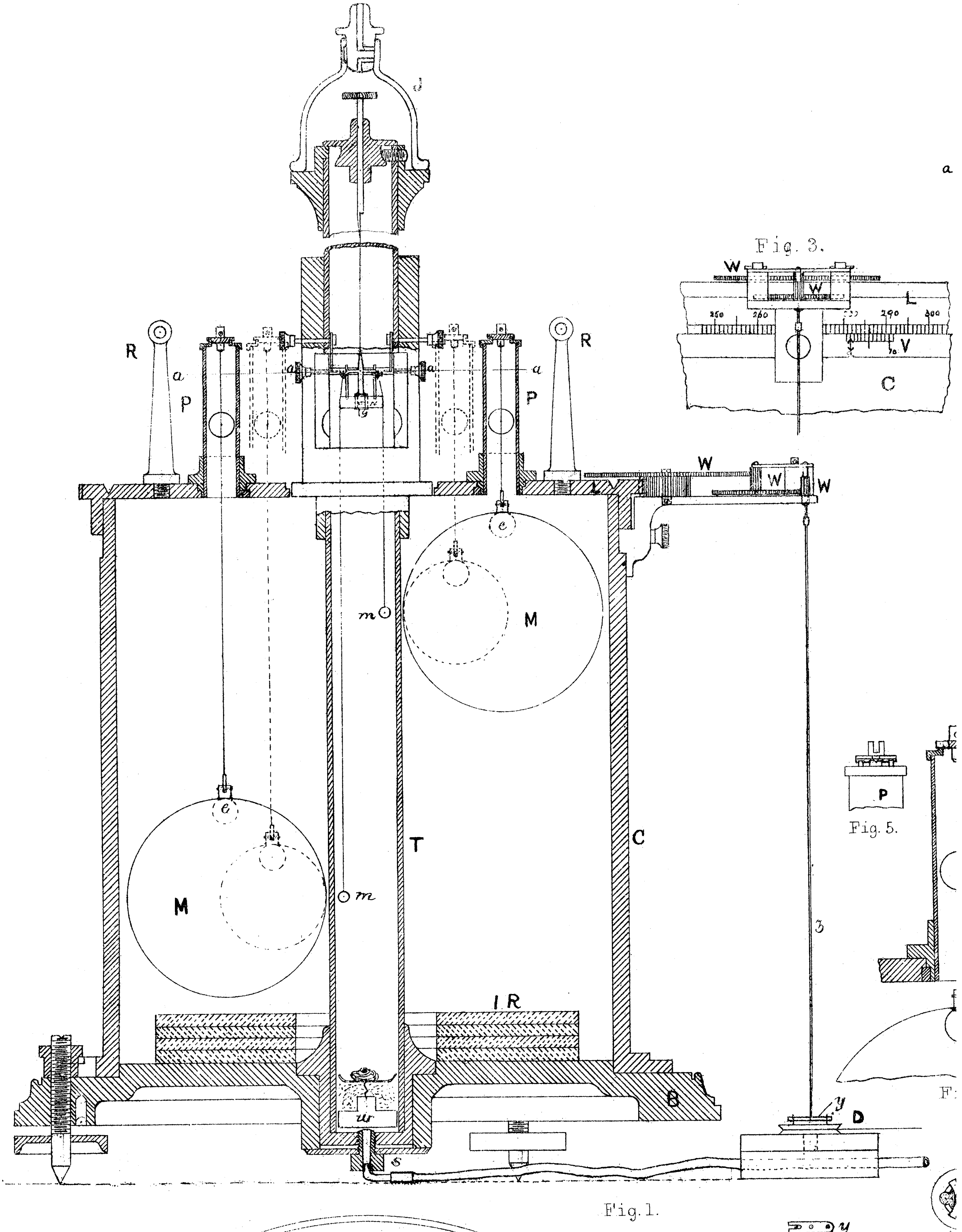
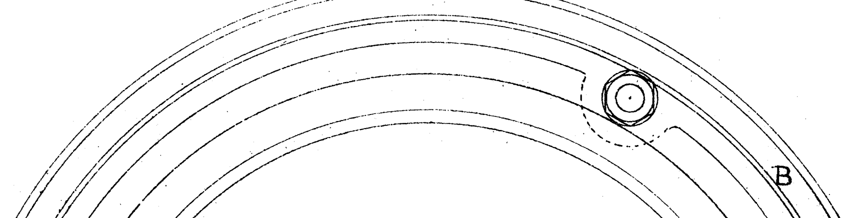
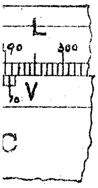


Fig. 3.
Fig. 5.
Fig. 1.



Boys.



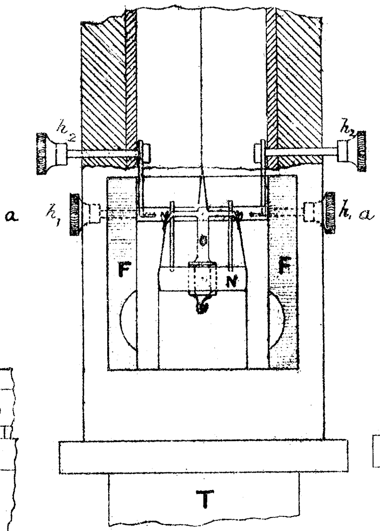


Fig. 8.

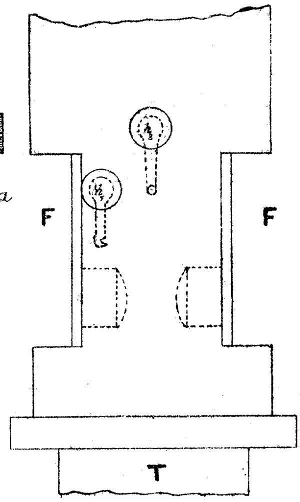


Fig. 9.

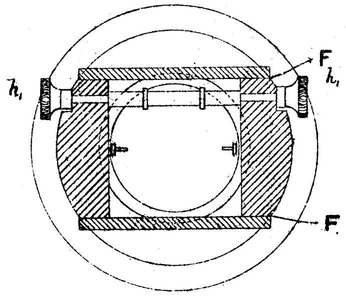


Fig. 10.

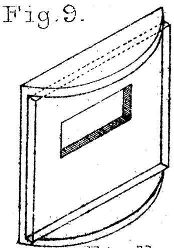


Fig. 11.

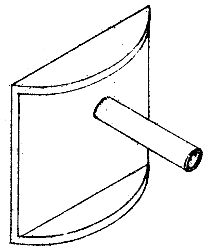


Fig. 12.


5.

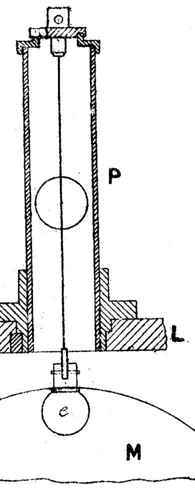


Fig. 4.

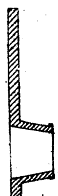


Fig. 13.

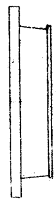
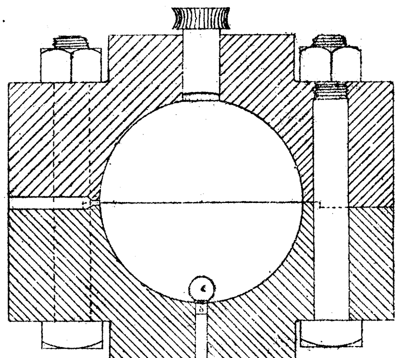
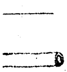


Fig. 14.


Section on a.a.a. Fig. 16.



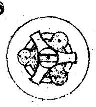
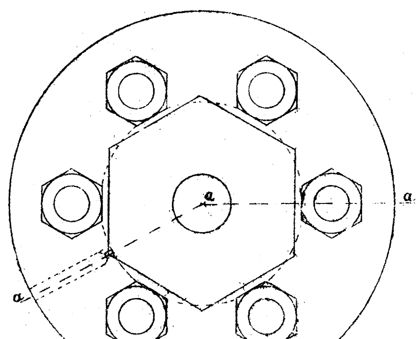
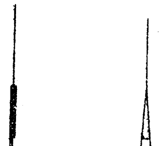


Fig. 6.





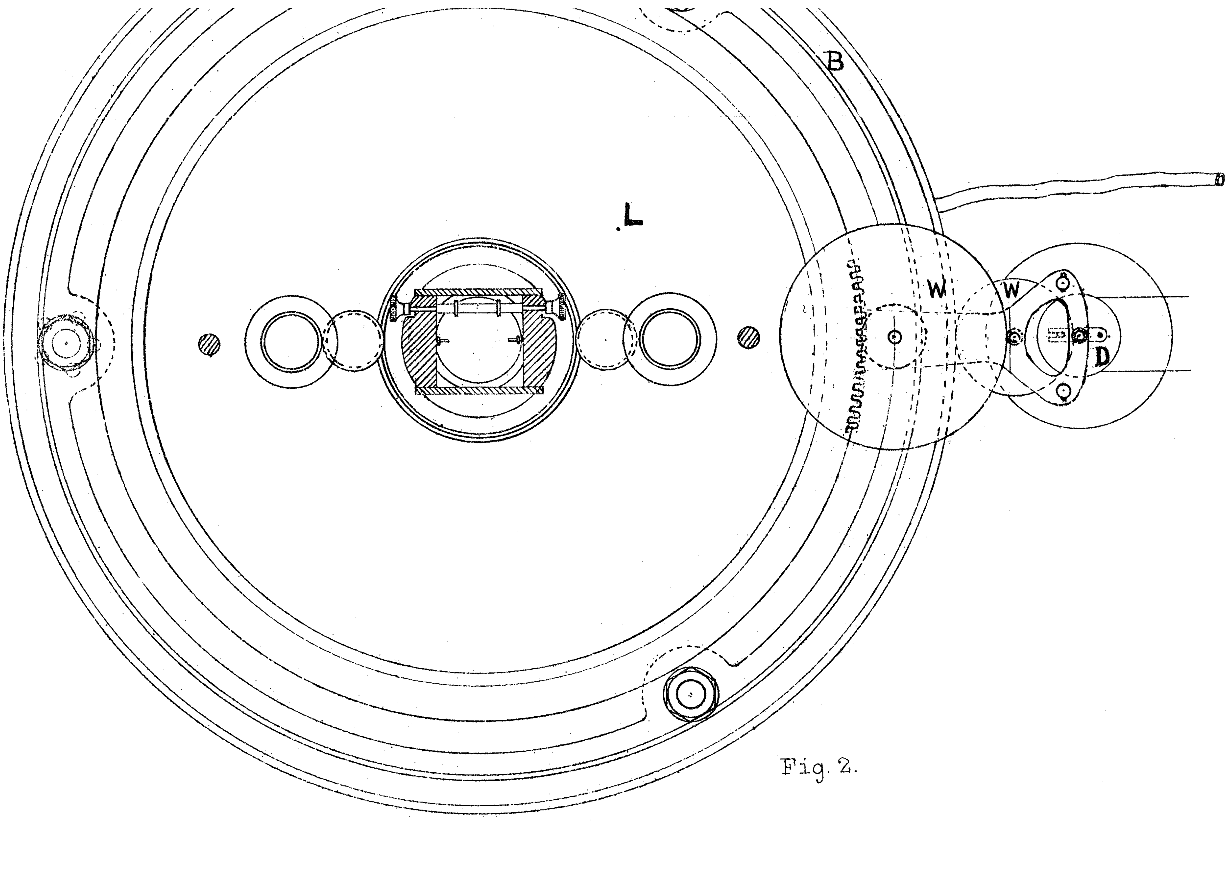


Fig. 2.

West, Newman photo lith.

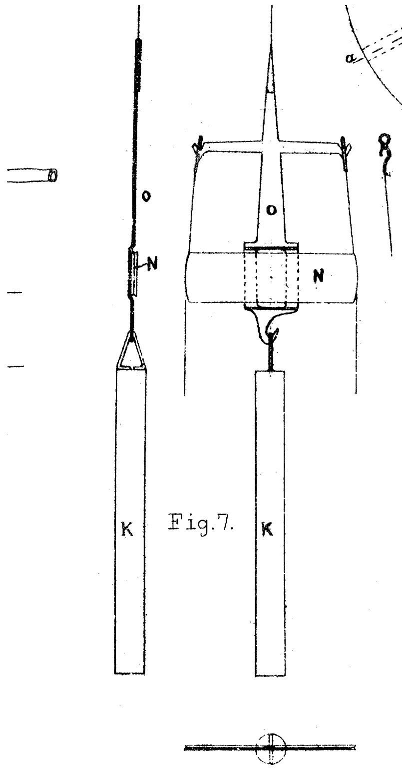


Fig. 7.

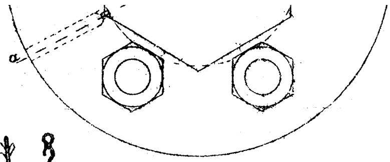


Fig. 17.

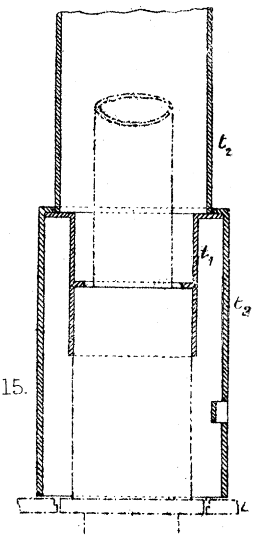


Fig. 15.

Phil. Trans. 1895. A. *Plate* 1.

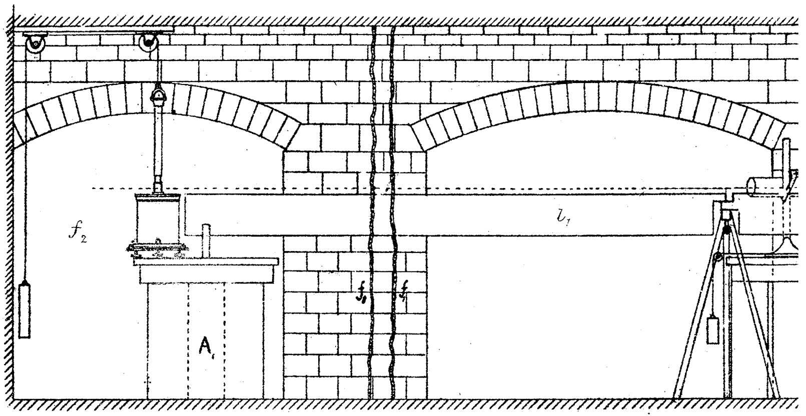
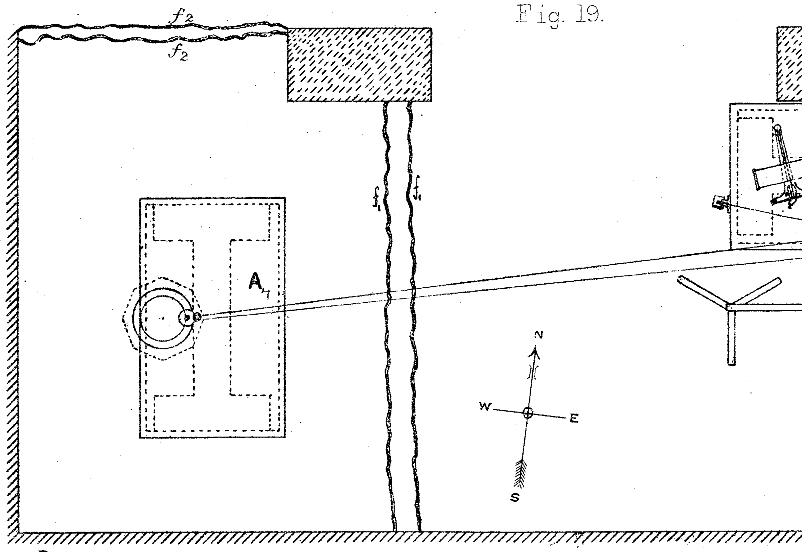


Fig. 19.



Ins. 12 9 6 3 0 1 2 3 4 5 6 7 8 feet

Scale of Feet.

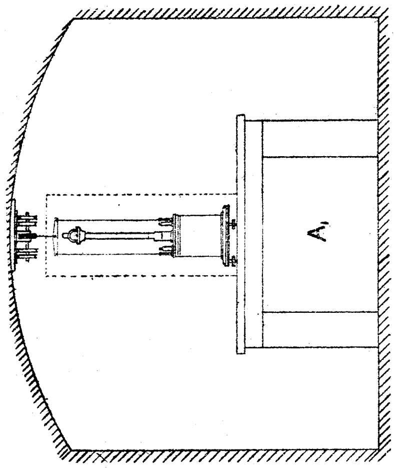
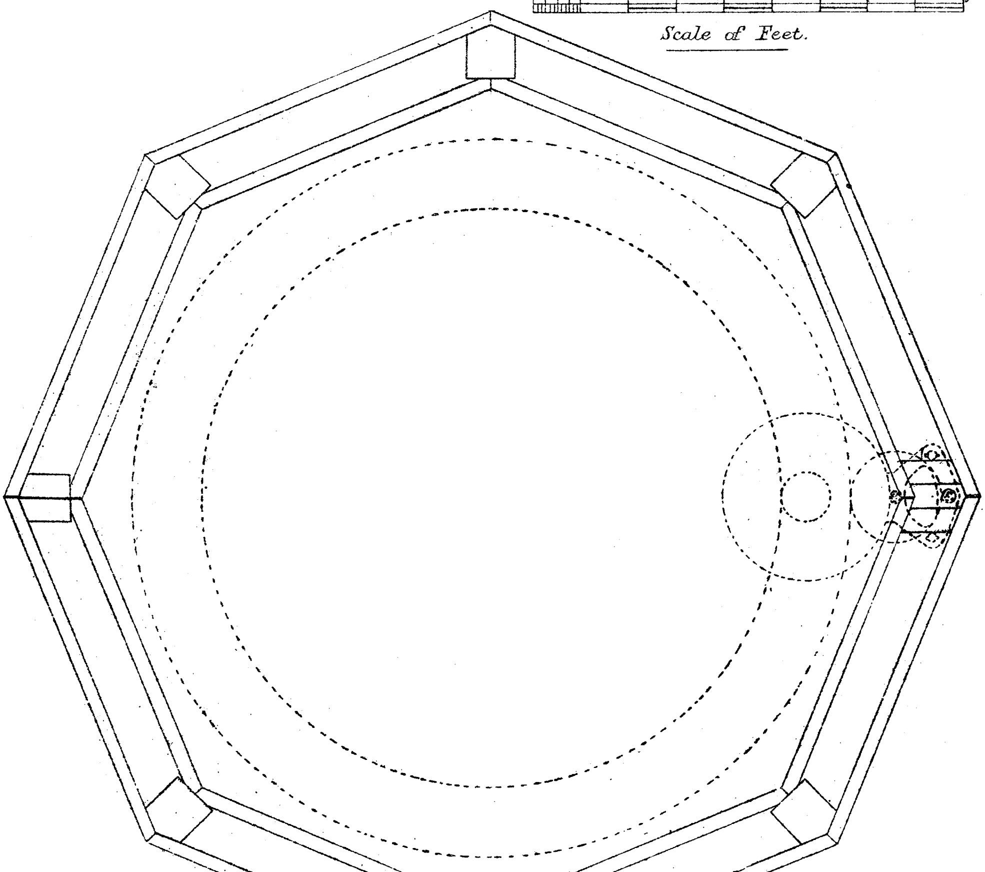


Fig. 20.



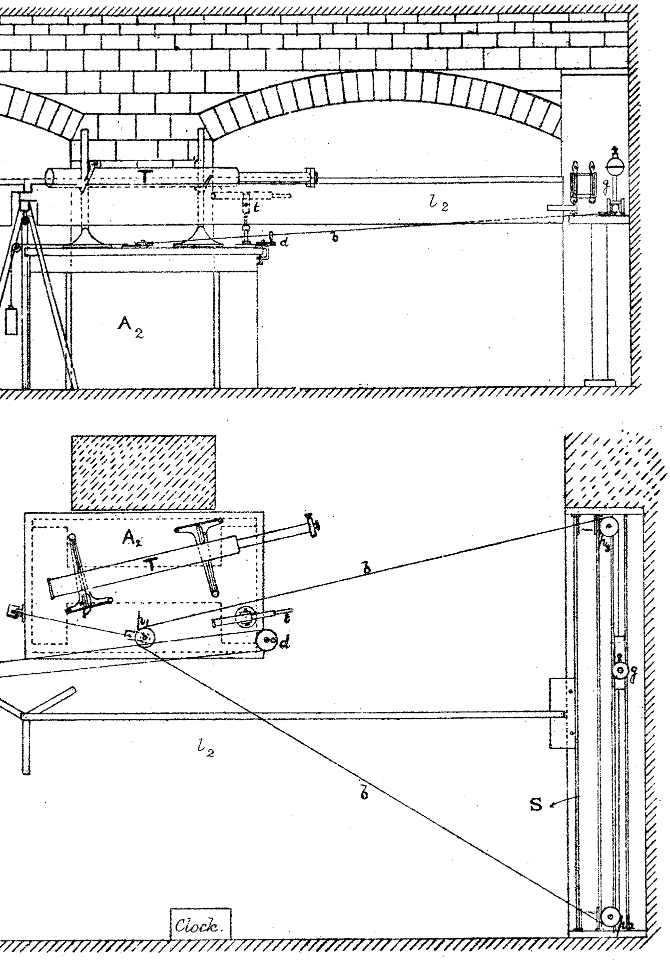


Fig.18.

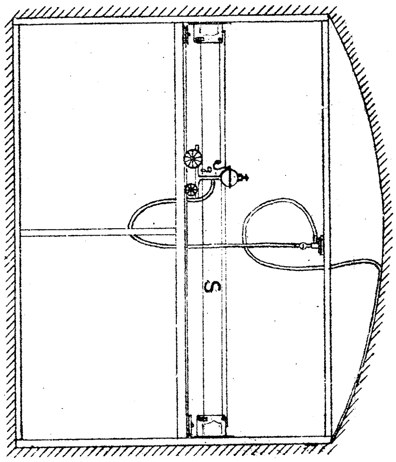


Fig.21.

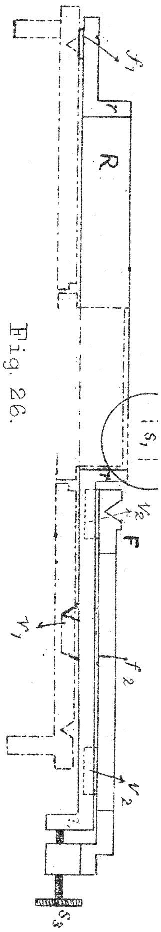


Fig.26.

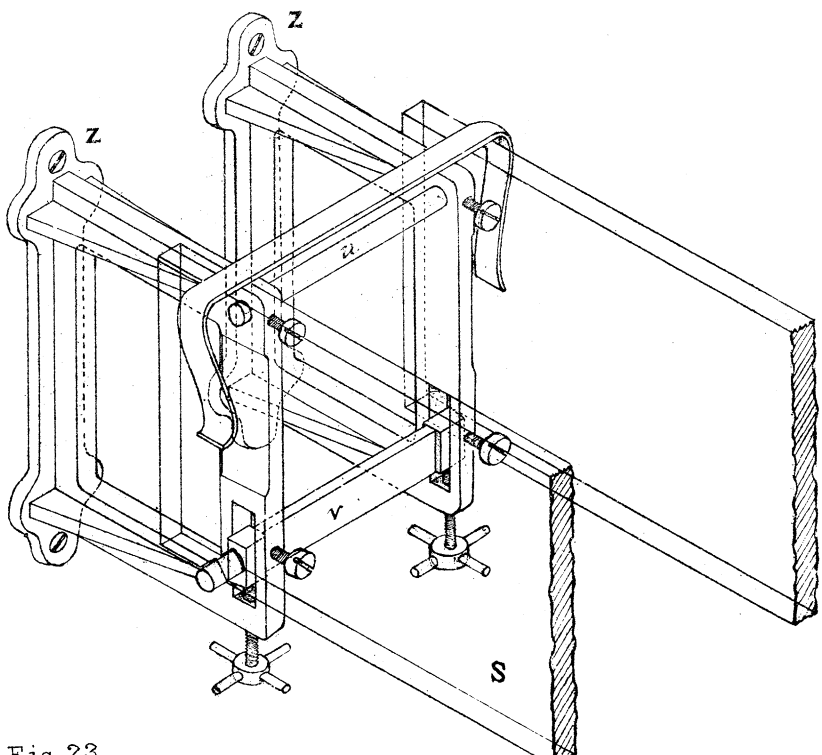


Fig.23.

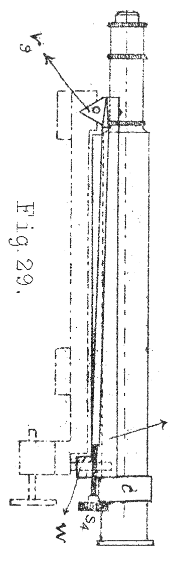
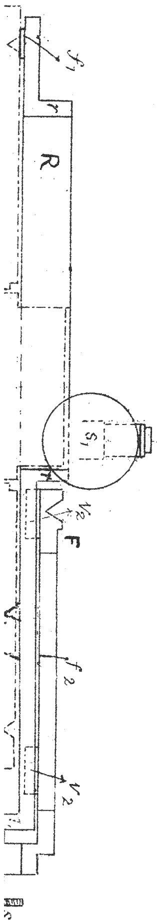


Fig.29.



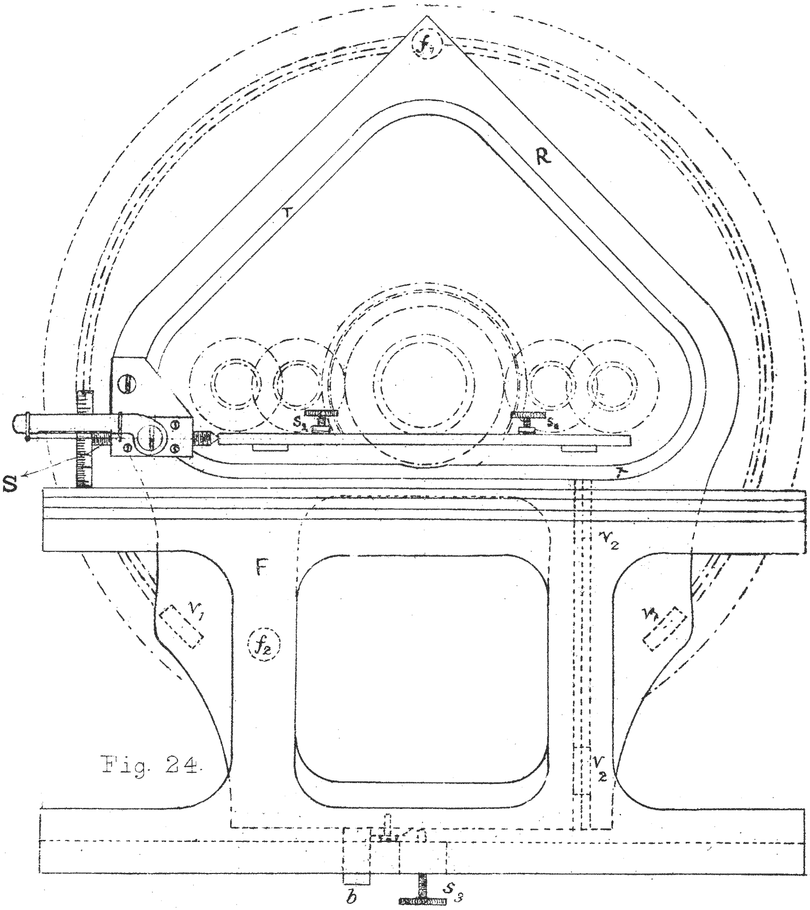


Fig. 24.

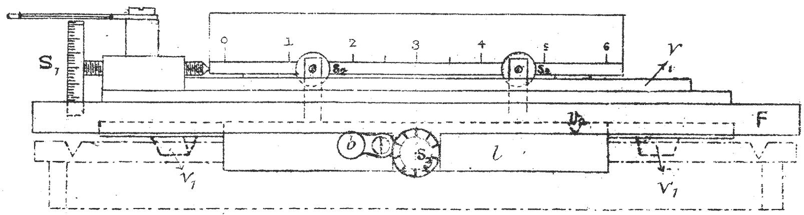
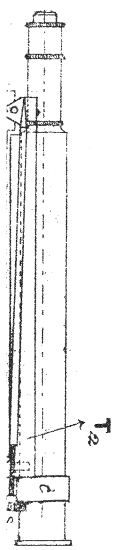


Fig. 25.



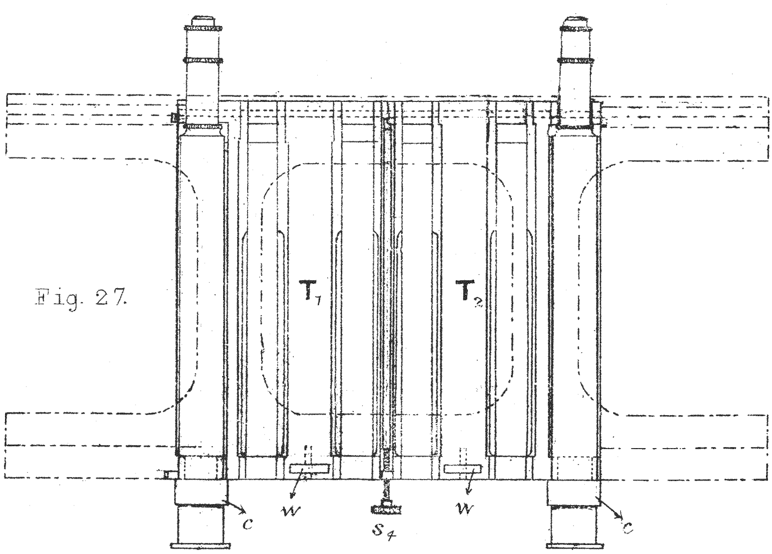


Fig. 27.

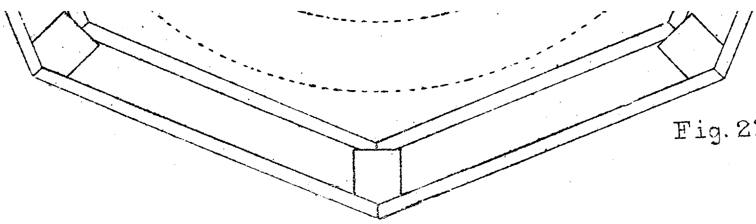


Fig. 22.

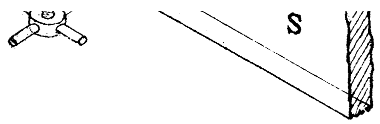
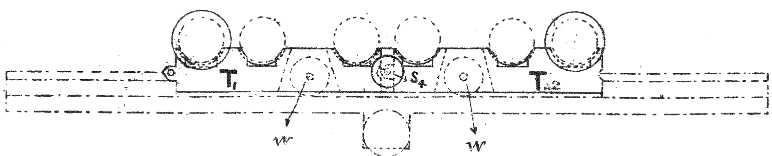


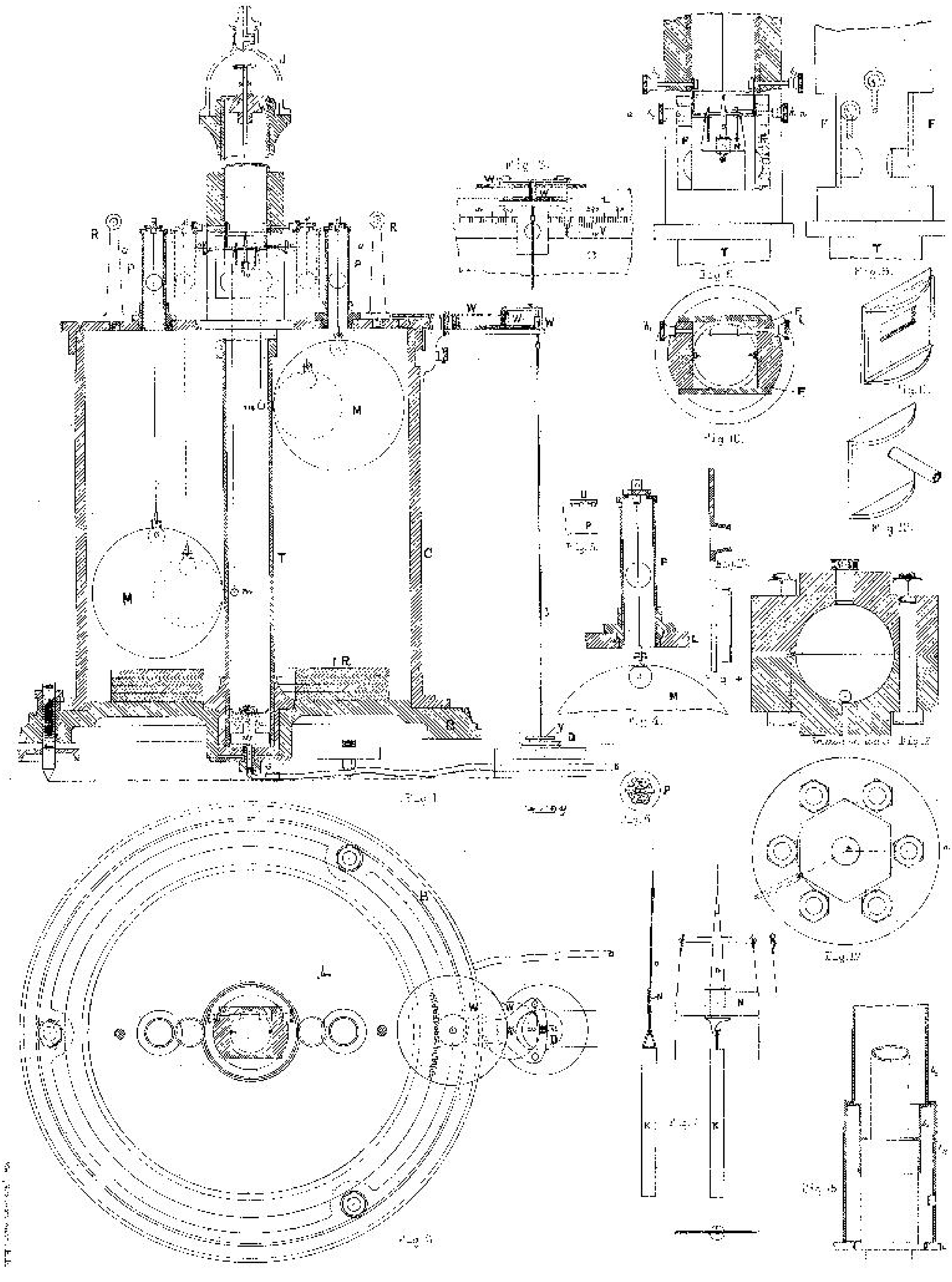
Fig. 23.

Fig. 28.



West, Newman photo. lith.

Boys.



Phil. Trans. 1895. A. Plate 1.

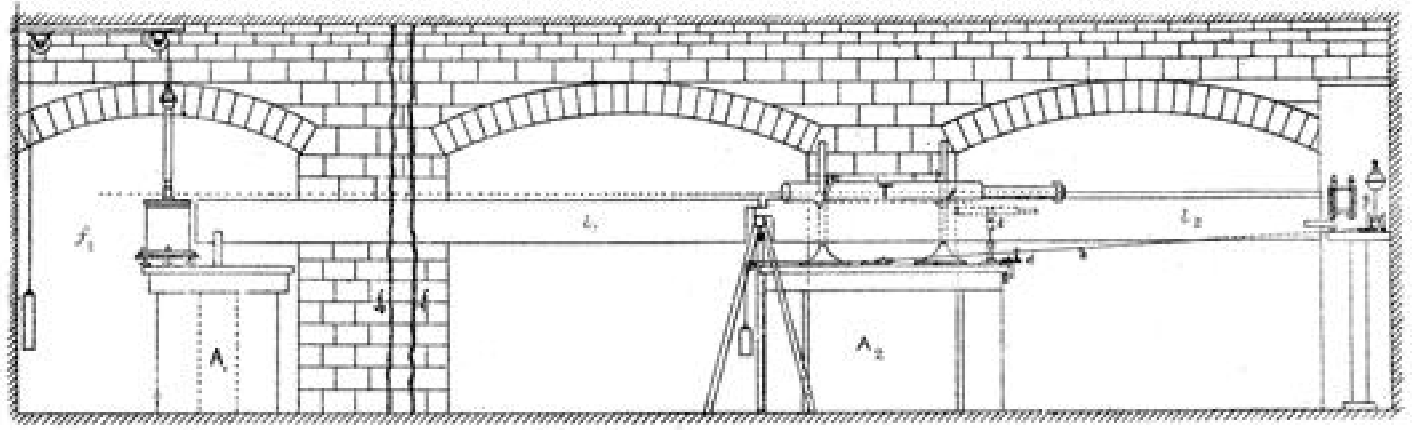


Fig. 19.

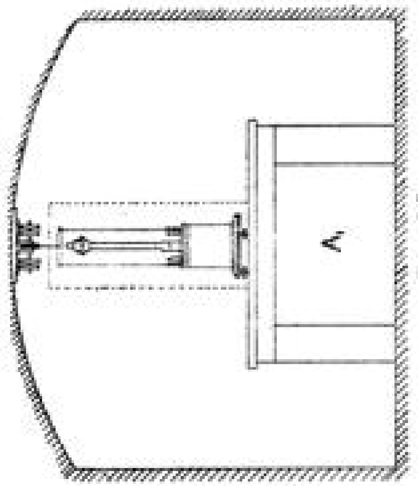


Fig. 20.

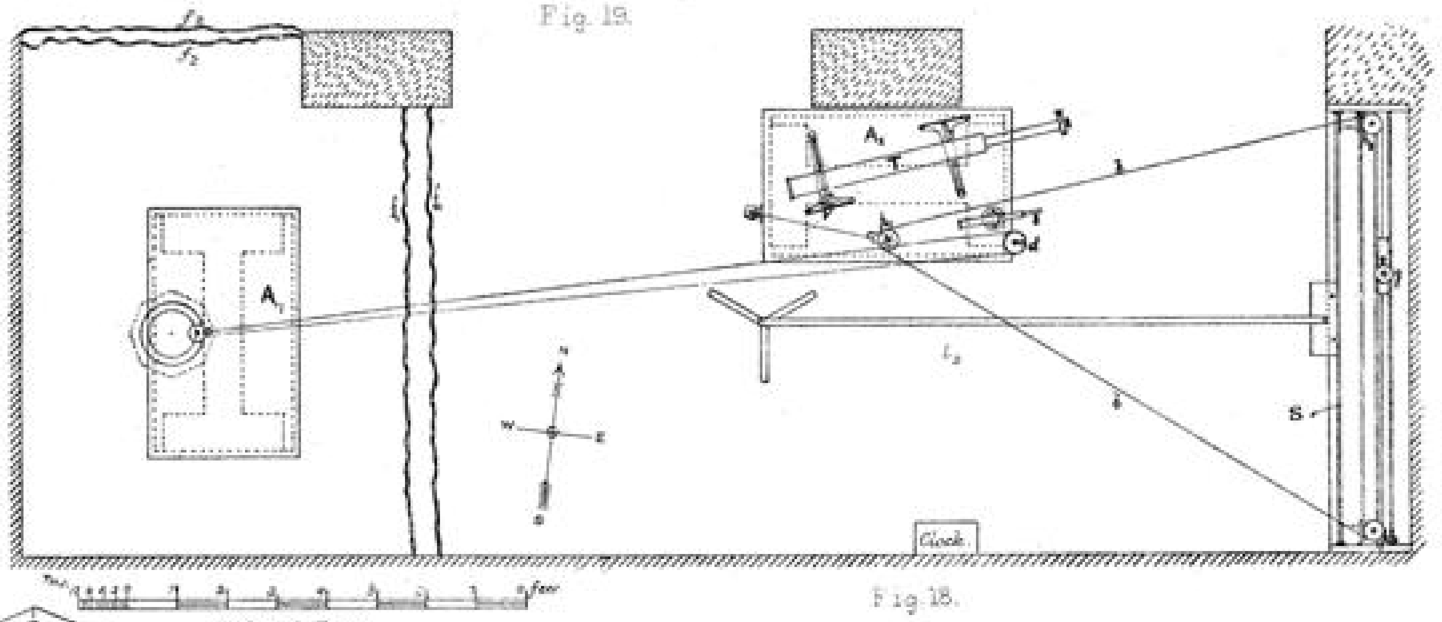


Fig. 18.

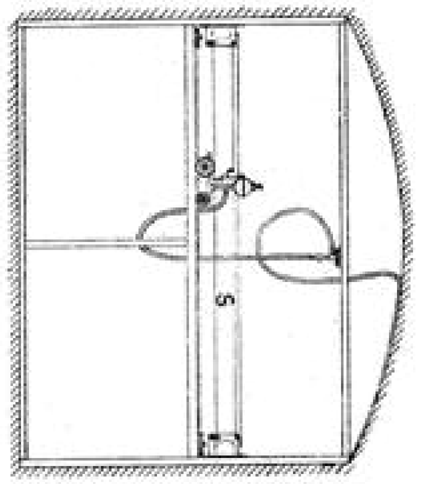


Fig. 21.

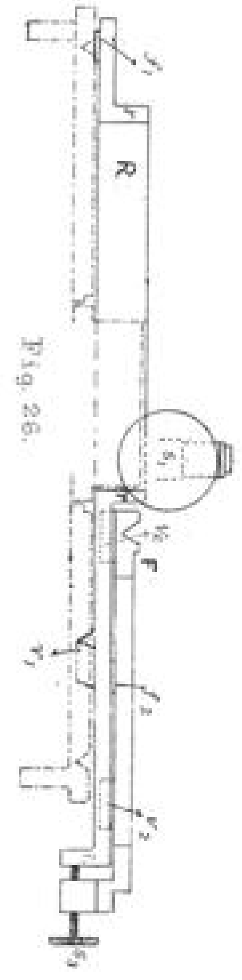


Fig. 26.

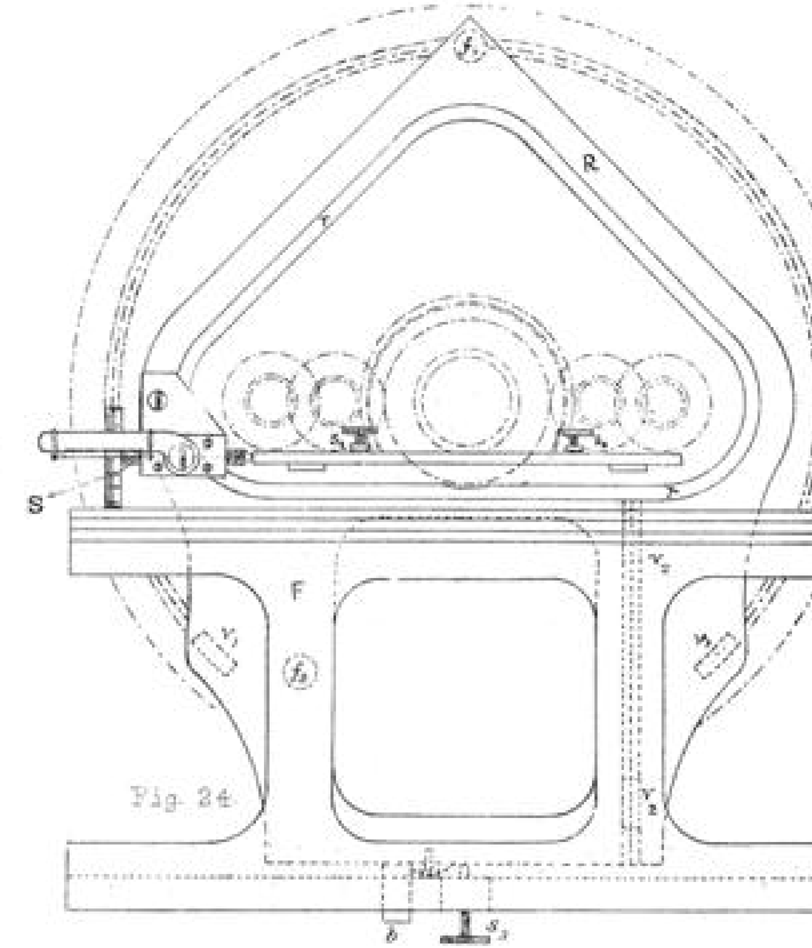


Fig. 24.

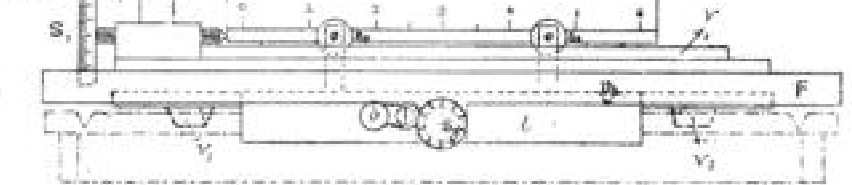


Fig. 25.

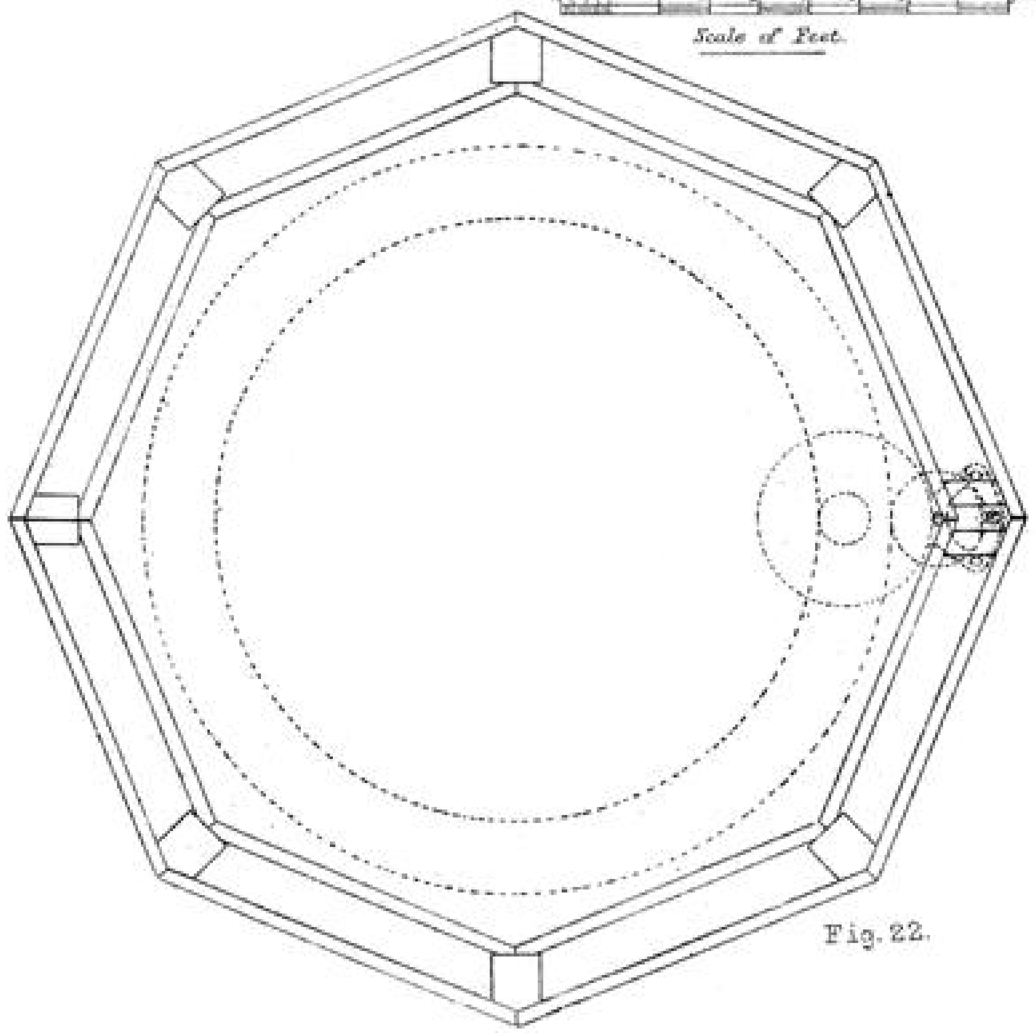


Fig. 22.

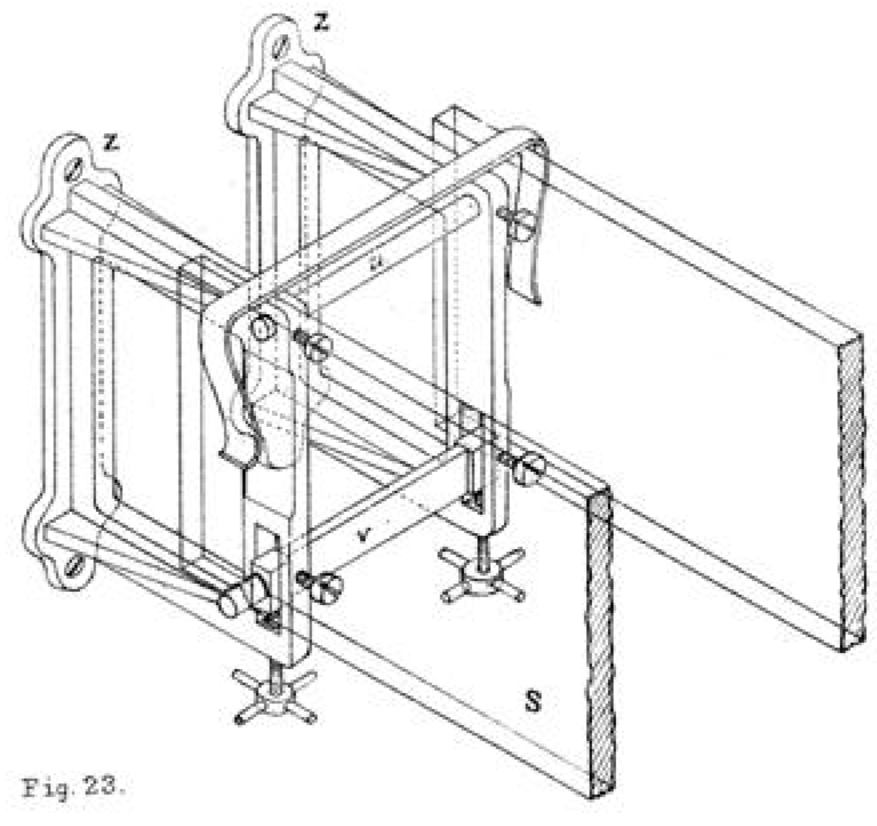


Fig. 23.

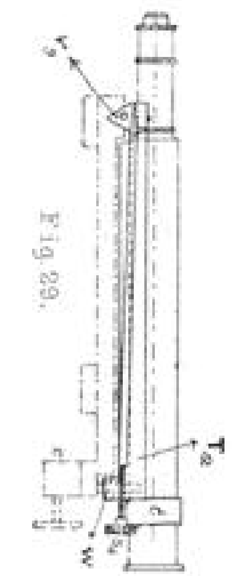


Fig. 29.

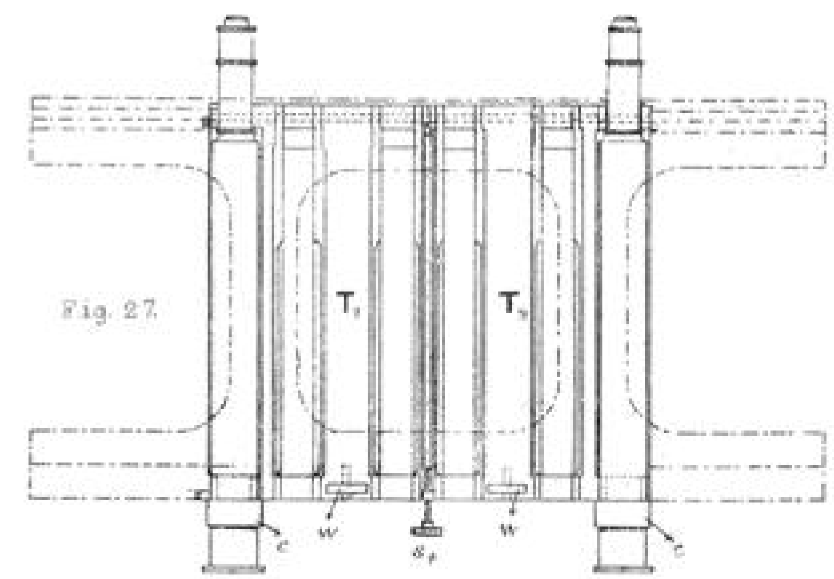


Fig. 27.

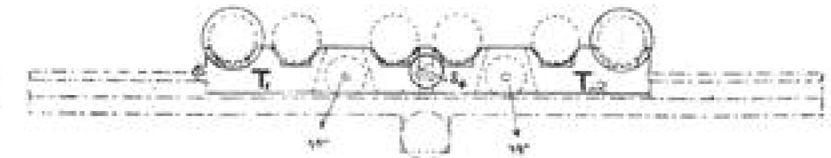


Fig. 28.

West, Newman photo.lith.