"On Gravimeters; with special reference to a Torsion Gravimeter, designed by the late J. Allan Broun, F.R.S." By Major J. Herschel, R.E., F.R.S., Deputy Superintendent of the Survey of India. Received October 31, 1880. Read January 20, 1881.

[Plates 8, 9.]

The present paper consists mainly of two parts, of which the second is the earlier, the first having been afterwards prepared as a historical introduction.

In August last a letter was addressed by Major-General J. T. Walker, R.E., C.B., F.R.S., Surveyor-General of India, to the Director-General of Stores for the Indian Government, enclosing copy of a letter from the late Mr. Broun on the subject of his gravimeter. The object of General Walker's letter was to trace, if possible, the instrument in question, and to secure it for the Indian Government. It was also suggested that I might be able to judge of its efficiency. I also received a communication to the same effect, and in consequence offered my services.

On enquiry it appeared that the gravimeter had become the property of the Royal Society. The loan of it was obtained and I was requested to call and see it. It was then arranged that I should take the instrument and report upon its capabilities at greater leisure than was possible at the India Office.

The gravimeter is an instrument obviously requiring very careful manipulation, and as I was not for some time after it came into my hands in possession of any description of its intended use, the examination was attended with anxiety as well as difficulty. It occupied me for three weeks, during which time I was in constant fear lest some awkwardness should prevent my returning it uninjured. I purposely, therefore, wrote what I learnt about it from day to day; and this will perhaps excuse the length of my report, which was eventually submitted to the authority from whom I received the instrument.

The course of the examination necessarily led to my becoming acquainted with the subject in several ways, but the narrative form and the immediate object of the report precluded any discussion. There appeared nevertheless to be sufficient in the study of the instrument itself, and especially of its dimensions and the weights of its parts, to justify a desire for publication. I accordingly solicited permission to communicate the report to the Royal Society, the owner of the instrument. This having been granted, it then seemed permissible to preface the report by a sketch of some of the circumstances bearing on the question of priority of invention, and other matters in con-

2 o 2

nexion with the theory of the torsion gravimeter, together with remarks upon other forms of gravimeter. This now appears as Part I of the present paper.

So far as I was aware, the theory of the torsion gravimeter had not been published. In considering it, I was led to think that in some respects it involved peculiarities which might be taken advantage of to simplify the design. In anticipation of the present publication, therefore, I communicated to the Society a notice of a design to that effect, which was read on the 9th December, 1880, and appears in the "Proceedings" (vol. 31, pp. 141–146).

Although, therefore, it precedes the present communication in that respect, it will be understood to have grown out of the examination of Broun's instrument, to which I am indebted for the means of judging of its feasibility.

I. *Historical Review of Proposals or Designs for a Gravimeter.*

It is hardly necessary to describe at much length the object of instruments of this class. They aim at determining *statically* that which the differential pendulum has been had recourse to, ever since the days of Graham and of Bouguer, to determine *dynamically*, the variation of gravity at different times or places. The principle indeed is so obvious that when we recognise the fact that hitherto nothing has been effected which can be regarded as establishing a rivalry in practice, we also perceive that the interest of any historical review must centre in what it can teach as to the causes of the absence of any practical competition. For this we must first learn what forms have been proposed.

It would be hard to say to whom should be ascribed the credit of first suggesting the statical method. It would seem as if its theoretical possibility must have long been perceived. But probably no clearer enunciation of the principle is to be found than that which appeared in the first, as in all later, editions of the "Outlines of Astronomy" (Art. 234), first published in 1833.* So far as I know, what was there proposed, divested as it purposely was of everything which could confuse the principle to be illustrated, had never been experimentally tried. To understand it, a single glance at the drawing, representing a weight suspended by a coiled spring, its lower surface grazing the base of the supporting frame, should suffice. The supposed variation of gravity is intended to be balanced by the addition or removal of small subsidiary weights, the proportion of which to the principal weight will measure the relative variation. The only doubts which arise at once in considering such an instrument have reference to the means of exactly determining the contact and to the constancy of the

* Art. 189 in the first four editions.

spring. These evidently are the points towards which later improvements may be expected to be directed.

The elasticity of such a coiled spring partakes, I believe, of two characters. It would depend very much upon the *pitch* of the coil whether the elasticity would be chiefly that of bending or of torsion. If, as I suppose, these are not the same, there is to some extent a change of principle in leaving the coil, and adopting the straight suspending wire, the resistance to torsion of which is substituted (in the next form) for the resistance to extension of the coil as above. With a view to meeting one of the two difficulties mentioned, it might become a question whether the required constancy would be more attainable in the one direction or in the other. The modifications of design which actually occurred, however, do not appear to have arisen from considerations of this nature; but were aimed at securing greater facility, constancy of resistance being assumed.

About the year 1860 two inventors devoted their attention independently to methods of statical gravimetry. In each case we have means of perceiving the growth and modification of their ideas, and it must be regarded as a curious coincidence that they arrived at a nearly identical result. One of them eventually designed, and caused to be constructed, an instrument which is the occasion of this paper. The other, so far as I have been able to learn, did not give practical effect to his conceptions in the shape of a finished instrument; at least he has not published any account of observations.

The remarkable similarity which undoubtedly exists between the gravimeter as made under Mr. Broun's instructions and that which was proposed by M. Babinet, has naturally raised the question of priority. In a letter written by the former to General Walker, Mr. Broun expresses himself in a way to show that he can with difficulty divest himself of the idea that M. Babinet had been led to his design by more or less indirect cognizance of what Mr. Broun had published or communicated orally to mutual acquaintances. As it happens that I have access to correspondence bearing on the subject, I have looked into it somewhat closely, and think it not undesirable to publish some of the letters written by M. Babinet to my father at the time when he was busied about it.

The earliest publication on the subject occurs in the "Proceedings of the Royal Society of Edinburgh" for 1861, in which (pp. 411–412, February 4) Broun describes a machine, in course of construction, for the purpose of measuring small changes of gravity by the balancing of a bifilar disturbance by a *hair spring* placed below the suspended weight.

In February, 1863, appeared Babinet's design,* in which a similar

* "Comptes Rendus," lvi, 1863, pp. 244–248; and "Cosmos," 1863, pp. 177–181.

disturbance is balanced by the *torsion of a single wire.* This was followed immediately by a reclamation by Principal Forbes, on behalf of Broun (absent in India), drawing attention to the prior publication, and saying that the new design is practically identical with that already published. In May of the same year appeared a letter from Broun to the Editors of the "Philosophical Magazine," enclosing translation of a letter from himself to the Perpetual Secretary of the Paris Academy of Sciences, the object of which is likewise to claim priority of invention. He refers to his paper in the Edinburgh "Proceedings" in proof. The whole question turns, I think, on this sentence :—"We found, however, that the spring, like the balance-spring of a watch, employed under the weight, acted badly, and I substituted a simple gold wire in the same year" [1861]. Compare this with the statement in the letter of 1879, where he says "Babinet's proposal was for the same instrument exactly, and it was published more than two years after mine, and after, indeed, I had described the instrument to different persons in Paris." It is clear that Broun did not perceive that there was a real difference between the two forms in which the elasticity of metal was opposed to the force of gravity, and that he had *published* nothing regarding his use of *the torsion wire* at the time of Babinet's design. It was only towards the close of 1861, at the earliest, that Broun introduced the gold wire in place of the spring, and as the letters which I shall give prove Babinet to have been working out the same idea early in 1862, there is no doubt some show of reason in Broun's complaint, when he says (as he does in his letter of 1863) that not only was his instrument seen by many persons in Adie's shop in 1861, but that the gold wire which he substituted was actually recommended and supplied by a Parisian artist. Still I think it impossible to read the following letter, and retain any suspicion that Babinet was conscious of having borrowed the principal part of his design from another. The letter is interesting enough in itself, apart from this question.

"Paris, ce 3 Février, 1862.

"*Sur la Mesure statique de la Pesanteur.*

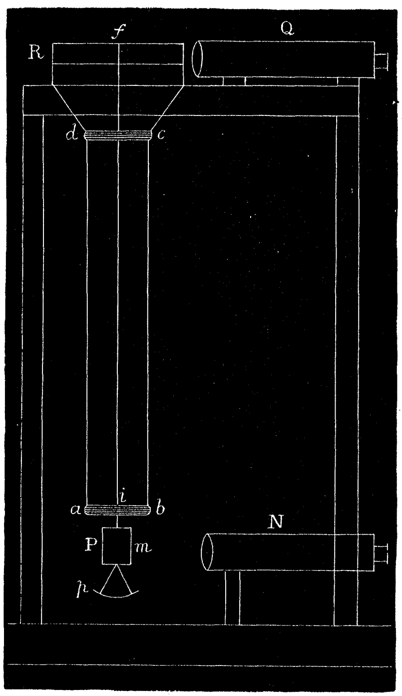
"Monsieur et illustre Confrère,

"C'est une des plus importantes idées qui aient été émises pour la connaissance de notre globe que celle de la mesure statique de la pesanteur. Je crois avoir résolu le problême pratique après d'innombrables essais et bien des années de réflexion. Ce qui distingue surtout ma méthode, c'est le *fractionnement* de la pesanteur, de manière à équilibrer l'effet d'un poids considérable par la torsion d'un fil métallique de force moyenne et restant dans les limites de l'élasticité parfaite. En effet je réduis le poids à $\frac{1}{200}$ de sa valeur au moyen du pendule bifilaire.

"Ayez la bonté d'examiner l'instrument que je propose, de m'indiquer des corrections, et je le ferai construire de suite. Dès que j'aurai votre réponse, je communiquerai ce travail à l'Institut en y mettant pour introduction un précis de ce qui est au No. 23 des 'Outlines of Astronomy.'

"Il n'y a dans mon appareil, ni point de départ obligé, ni mesure d'angles, ni loi supposée pour l'élasticité, ni permanence exigée, ni mesures de longueur, ni inertie de départ à vaincre, ni difficulté d'établissement ou de transport, ni mesures de temps—'it is not laborious, tedious, and expensive;' la température n'a pas le temps de changer pendant la courte durée de l'expérience, ce qui répond à votre condition d'*expédition.*

"L'effet de la température (if any) peut être facilement étudié au préalable et pris en compte.



" *bc* et *ad* sont deux fils métalliques très fins et sensiblement sans torsion (d'ailleurs leur torsion pour un angle constant s'ajouterait sans inconvénient à la force directrice du bifilaire). *bc* et *ad* ont un mètre de longueur ou 1,000 millims., *ab* et *dc* ont 10 millims.: en sorte que la distance de *a* et de *b* au point milieu *i* où s'attache le fil de torsion *fi* est de 5 millims. Soit $ia = ib = a = 5$ millims.

"La tige horizontale inférieure *ab* du bifilaire porte au poids d'environ 1 kilo.=1,000 grms., en y comprenant le poid additionel p qu'on doit ajouter au poids P dans un petit plateau attaché au dessous de P.

"Si P est le poids que porte un pendule bifilaire, on sait qu'étant écarté de sa position de repos d'un angle α, il tend à y revenir avec une force—

$$P\frac{a}{l}\sin\alpha\sqrt{1-4\frac{a^2}{l^2}\sin^2\tfrac{1}{2}\alpha}$$

(Si a et l sont du même métal, la chaleur ne trouble point la force directrice du bifilaire, puisqu'elle dépend unique de $\frac{a}{l}$). a étant égal à 5 millims., et l à 1,000 millims., la force directrice du bifilaire n'est qu'une fraction, savoir $\frac{5}{1000}$ ou $\frac{1}{200}$ de sa grandeur naturelle. Du reste on verra tout-à-l'heure qu'il n'est besoin de mesurer ni a ni l.

"Supposons la tige supérieure *dc* du bifilaire déviée de 90°. Alors, le poids P étant de 1,000 grms., la force directrice du bifilaire pour $\alpha=90°$, sera à peu près $\frac{1}{200}$. 1,000 grs.=5 grms. On équilibre cette force directrice horizontale par une torsion de 180° que l'on donne au fil métallique *fi* attaché en haut à la pièce tournante R et en bas au point *i* milieu de *ab*. La tringle supérieure *dc* du bifilaire tient aussi à une autre pièce rotative faisant partie de R et qui permet de tourner *cd* de 90°. Ces deux pièces de R tournent l'une dans l'autre, à l'ordinaire.

"La lunette inférieure, N, a des fils micrométriques qui vont se mirer en *m* sur un petit miroir vertical fixé au poids P. En ramenant l'image de ces fils à la coïncidence avec les fils eux-mêmes, comme dans le pointé nadiral, on ramène la direction *ab* de la partie inférieure du bifilaire à sa position primitive. De même, des miroirs attachés aux parties tournantes en R, et réglés sur la lunette Q, assurent des rotations connues pour le haut du bifilaire *dc* et pour le fil de torsion *fi*. Voici maintenant l'opération complète :—

"L'appareil étant calé par ses niveaux, et bien stable, j'amène le bifilaire dans la position où l'image des fils de la lunette N coïncide exactement avec ses fils. À ce moment un petit miroir vertical fixé latéralement à la pièce qui en R donne le mouvement de rotation au bifilaire, ce petit miroir, dis-je, renvoie la partie inférieure des fils de la lunette Q sur eux-mêmes ; et de plus un autre petit miroir pareil, fixé au-dessus du précédent à la pièce qui donne la rotation au fil métallique *fi*, renvoie dans la partie supérieure du champ de la lunette Q l'image de la moitié supérieure des fils de cette lunette sur ses fils eux-mêmes.

"Tout étant ainsi réglé :—

"Je fais tourner de 90° la pièce qui guide la tige supérieure *dc* du

bifilaire, ce que l'on obtient au moyen d'un second petit miroir vertical fixé latéralement sur cette pièce et à 90° azimuthalement du premier. Ce miroir est amené à remplacer le premier miroir de la même pièce pour le renvoi des rayons dans la lunette Q, et la rotation de *dc* est alors de 90°.

"La pièce tournante à laquelle est fixée l'extrémité supérieure *f* du fil de torsion a aussi un second petit miroir vertical qui fait azimuthalement avec le premier miroir de cette pièce un angle égal à trois fois 90°, et en tordant par son moyen le fil *fi* de trois fois 90° en sens contraire du mouvement donné à *dc* du bifilaire, la torsion de *fi* restera en définitive une torsion de 180° quand sa direction *ab* du bifilaire aura repris sa position primitive.

"Soit P le poids fixe du bifilaire (y compris *a*, *b*, &c.). On ramenera *ab* à sa position primitive au moyen d'un petit poids additionnel *p*. Alors la pesanteur *locale*, agissant sur une masse $P+p$, par l'intermédiaire du bifilaire équilibre la torsion de 180° du fil métallique *fi*. Si dans une autre latitude il faut $P+p'$ pour équilibrer la même torsion, les intensités de la pesanteur dans les deux localités seront entre elles comme $P+p$ est à $P+p'$.

* * * * * * * *

"Pour $\frac{1}{10000}$ (qui est réclamé dans l'article des 'Outlines') voyons l'effet produit par un poids de 0·1 gr., ajouté à un poids de 1,000 grs. Ce poids de 1,000 grs. équivaut dans le bifilaire à 5 grs., et un poids de 0·1 gr. équivaudra à $\frac{0 \cdot 1}{200}=\frac{1}{2000}=0{\cdot}0005$ gr.

"Mais 5 grms. sont équilibrés par une torsion de 90° [180°] du fil *fi*. Donc 1 gr. correspondrait à une torsion de 36°, ou bien de 129,600″. Alors 0·001 répond à une torsion de 129″·6, et $\frac{1}{2000}$ gr. correspond à la moitié de ce nombre; mais comme le mouvement du miroir double le déplacement de l'image, il restera un déplacement de 129″·6 (un peu plus de 2′) pour $\frac{1}{10000}$ de variation dans la pesanteur. Si la lunette N a un peu plus de 30 centims., le déplacement de l'image des fils sera de $\frac{1}{5}$ millim.

"*Nota.*—Il faudra prendre la position de *ab* ramené à sa position primitive en faisant osciller *ab* pour éviter les adhérences de départ.

"Voyez, jugez, et surtout conseillez; votre avis sera suivi. Je finis par ces mots des 'Outlines': 'The great advantages . . . render the attempt well worth making.'

"J'ai l'honneur, &c.,

"BABINET.

"À Sir John Herschel.

"P.S.—Il faut que je me hâte de publier ce que j'ai trouvé, car à 69 ans ce serait vraiment être trop exigeant envers la bonne providence que de lui demander encore indéfiniment du temps pour remplir la

tâche qu'il lui a plu de m'assigner. Vous avez été plus sage, pour le grand bien de la science. Moi, je puis malheureusement me dire,

"'Cras vives! hodiè jam vivere, Posthume, serum est.
Ille sapit quisquis, Posthume, vixit heri.'—MARTIAL."

I have also seen a copy of the reply to this letter. It is needless to say that it was full of approval, and only made some suggestions, which were at once adopted or accepted. There was no mention of any other form of gravimeter having been proposed in England. I need not transcribe from the subsequent letters; until, on the 15th February, a year later, Babinet again writes:—

"M. Le Verrier me presse de terminer notre balance gravimétrique. Je ne veux pas commencer imprudemment avant d'avoir votre ultimatum d'approbation."

Here follows a *résumé* of the principles involved in the design, as to which I need only remark that the elasticity of the suspending wires is not taken into account, nor is the necessary effect of this elasticity upon the balance in any way referred to.

"L'appareil sera établi et essayé à l'Observatoire Impérial de Paris, puis essayé dans les latitudes voisines. Je serai heureux si cette mesure de la gravité et les déplacements nécessaires qu'elles entrainera me fournissent l'occasion de vous voir en Angleterre."

The letter then passes on to other matters—a new plan for measuring aberration by means of gratings—and only returns to the gravimeter at the close.

"J'aurais encore attendu à vous parler de ceci sans la circonstance de notre balance gravimétrique. En cas de succès de cette balance pourriez-vous, vous ou quelqu'un des vôtres, vous charger des stations du Royaume Uni? Il faudrait au moins une mesure de gravité pour chaque degré carré de surface.

"*Nota.*—Sitôt après votre réponse on se mettra à l'œuvre pour l'appareil gravimétrique.

"Recevez, &c.,
"BABINET."

The paper in the "Comptes Rendus" (lvi, pp. 244—248) was read on February 9, 1863, or a week earlier than the date of this last letter, which therefore appears to have resulted from the approval with which the paper was received at the reading. The published account adds little to what the letters tell—on the contrary—which is partly my reason for transcribing them. It is noticeable that there is from first to last no mention of any prior attempt in the same field.

I have endeavoured to ascertain, by enquiry from a gentleman who was associated with Babinet in physical researches of this nature, whether the instrument was ever constructed; but I do not gather from the reply that such was the case. It would seem as if there was too much uncertainty as to the constancy of the force relied upon in torsion.

The original paper communicated by Broun to the Royal Society of Edinburgh follows immediately upon two others by the same author, in one of which the theory of the bifilar magnetometer is considered, and *mention is there made of the elasticity of the suspending wires.* Their effect is considered to be of little importance. The effect of temperature in modifying the elasticity is also alluded to. We cannot therefore suppose Mr. Broun to have disregarded these considerations altogether. It is perhaps the more remarkable that he has left no indication of being alive to the really interesting balance which occurs in his instrument, and which must equally have occurred, and can hardly have escaped his notice, in the former one.

In his letter, above referred to, he speaks of his gravimeter as if the instrument ultimately perfected was substantially the same as the one described in 1861. But, as I have had occasion to point out, it differs in one respect which I cannot but consider important—the difference between a spiral balance spring and a twisted wire. It is to be regretted that we are not in possession of any information as to the reasons which induced him to discard the one in favour of the other. In any case, the change is distinctly a part of the history of the invention. The next step was the construction of the instrument as mentioned in his letter; and then its exhibition and description by himself in the Catalogue of the Loan Collection of Scientific Instruments at the South Kensington Museum, where, he tells us suggestively, it was shown alongside of Mr. Siemens' bathometer. We may now turn our attention to this last.

My knowledge of the bathometer designed by Dr. C. W. Siemens is confined to the account given of it in the "Philosophical Transactions" for 1876, and I should not presume to examine it were it not necessary to do so for the purpose of this review. Dr. Siemens, it is true, regards it, in the introductory passages of that paper, as having a different sphere of action; but, on a careful consideration of the principles—so far as I understand them—of its construction, it appears to me to belong decidedly to the class of instrument, whatever we may call them, typified by the one already mentioned as described in the "Outlines of Astronomy." The special purposes for which it has been designated, and the actual uses to which it has been put, do not of themselves preclude it from being set to the more general purposes and uses of a gravimeter.

Reference is made in the cited paper to another form of instrument

constructed by the author in (or about) the year 1859, which, however, was abandoned, although it enabled him "to predict approximately the depth that would be found on the use of the sounding line." In this instrument the specific gravity of mercury would seem to have been measured by determining the column necessary to balance the pressure of a fixed quantity of confined air. The rise of the surface under a change of gravity was magnified by a lighter supernatant liquid being forced up a narrow tube. The principle, in this case, is analogous to the former typical one only if we regard the compressed air as acting the part of a spring. It is obviously dependent on the possibility of guarding against, or exactly allowing for, the change of elasticity with temperature—in this, too, being analogous to the typical form. It was sought to meet this very serious difficulty by keeping the air at the temperature of melting ice. There is another method—perhaps not less difficult to practise successfully; that, namely, of selecting three (or preferably four) more accessible temperatures, and so experimenting at these that in the long run the actual observations shall supply a constantly accumulating body of evidence as to the requisite factor, as well as two or more actual data in each case from which to infer what is required. But it might turn out after all that the uncertainty must remain greater than the variation to be measured. The possibility of such a result militates strongly against methods involving large corrections. But, perhaps, least of all in cases such as this where the whole instrument may be immersed without injury. It then becomes chiefly a thermometric question.

In the bathometer, on the contrary, which superseded the one just mentioned, "changes of temperature are entirely eliminated from the result," at least in theory. In practice such a thing would perhaps be hardly possible; but in any case residual effects may be regarded as subjects for ultimate consideration. I will now endeavour to condense a description of this instrument, so as to assign it a place in the present category.

There are two spiral steel springs which, in the drawing of the smaller of two instruments, are about 16 inches long and rather more than one in diameter. The lower ends are attached to a cross-bar which is to bear the intended weight. This weight is that of a column of mercury resting upon a certain surface to be presently described. There is an arrangement for maintaining the height of the column constant. I am not sure that I understand how this works; but supposing it effective, the weight upon the cross-bar must depend upon the area pressed by the mercury; and if this also is constant we have in effect the same principle as in the gravimeter depicted in the "Outlines."

The mercury is contained in a vertical steel tube having cup-shaped

enlargements above and below. The lower end is closed by a flexible corrugated steel plate, which has a solid central disk, by which the pressure on the plate is transferred to the cross-bar. When this plate is horizontal, the mercury presses equally on every part; but it is clear that the whole of the pressure is not borne by the cross-bar. In fact, it would only by trial be possible to ascertain what weight is borne by the springs. Any change in the position of the disk must alter the curvature of the plate, and with it the proportion of the weight borne by the springs and by the rim of the cup—irrespective of any change in the height of the column. The diameter of the steel tube would seem to be adjusted so as to cause a proper change in the level of the upper surface, when the bulk of the mercury is affected by change of temperature. I am not quite sure that, in presence of these several causes of variation, the principle of constancy of weight on the springs can be said to be retained.

Alterations of position of the disk are read by means of a micrometer below the cross-bar. It seems clear that the desired indications must be obtained from the readings of this contact micrometer, and some curiosity may be experienced as to the manner of interpreting its readings. On this point the author says:—"It would be difficult to determine the actual scale of the instrument *à priori;* and I therefore adopted the easier and safer method of relying for its final adjustment upon the result of actual working."

If it were possible to ascertain, with some approach to certainty, the weight borne by the springs in some particular position of the diaphragm and cross-bar (to be called the zero position) the instrument might be used, on the principle of the simple gravimeter, by ascertaining *what weight* upon the cross-bar would be necessary, under a diminished gravity, to bring it again to this zero position—temperature being the same. This, however, is not what is intended in this form of instrument.

The subject of compensation for temperature receives considerable attention. This, indeed, is very necessary, when we remember the sensitiveness of springs to heat. The author gives, as the result of experiment upon these springs, a factor of variation which appears to be $\frac{1}{4000}$ for each degree Centigrade. The importance of temperature in such case will be better understood if we consider that at this rate it would need but 21° C. to effect as great a change in the elastic force, as transference from equator to pole would effect in gravity. Change of temperature has here, it would seem, an effect twelve times as great as it has in the case of the pendulum. Certainly there is room for compensation; and Dr. Siemens obtains it, to some extent, by apportioning the diameter of the vertical tube to the cups. It is not necessary, for the present purpose, to consider closely how this exercises a compensating effect. It is estimated, by experiment, that

the factor is reduced to $\frac{1}{8000000}$. It seems highly probable, however, that the residual effect would no longer be a simple function of the change of temperature, and might be quite incalculable.

It is also necessary to consider that the masses of metal concerned would probably, except under controlling conditions, cause considerable uncertainty in this effect.

Enough has been said to show that the bathometer, although a gravimeter in principle, cannot be regarded as likely to prove of service in measuring small changes of gravity under different climates. Its efficacy as an instrument for measuring sea-depths need not be considered here. It may have peculiar properties in that connexion which I have not understood; for indeed I may confess a certain hesitation in believing that any instrument can do that, by determining the change of attraction, except under conditions favouring an empirically deduced scale.

There are rumours of other designs for measuring small changes of gravity, of quite recent invention, of which I can only say at present that it is very much to be hoped that they will serve their intended purpose, and that we shall soon be in possession of experimental evidence to that effect.

II. *On the Torsion Gravimeter, constructed on the Design of the late J. Allan Broun, by Dr. C. S. Müller, of Stuttgart.*

Before entering upon any description of this instrument it may be well to explain that the title of this paper is intended to take cognizance of the fact that the designer did not experiment with it or improve upon it after it left the maker's hands, and that consequently some portion of the merit, whatever it may prove to be, is due to the appreciation of the design by the latter. Conversely, it is due to the designer to recognise that failure of any kind may to some extent be attributable to misapprehension on the part of the maker. At the same time, it must be said that the workmanship is of a high order, and betokens a more than ordinary attention as well as great skill and delicacy. Should the instrument justify its existence by its ultimate utility, it cannot be denied that it will owe its success, in a high degree, to the intelligence of the constructor as well as to the genius of the inventor.

There are two ways of describing a new form of instrument. We may either approach it from outside, and learn its functions by considering its parts, or we may study it with a prior knowledge of its intention. The latter method is, perhaps, the best for one to adopt who wishes to describe an instrument of his own designing; for he cannot fail to indicate truly what design he had. But for one whose only knowledge of the functions of an instrument are inferred from what he sees, it might be dangerous to presume a full knowledge of

the design. I shall, therefore, describe the instrument as I see it, with direct reference to what I conceive to be the intention.*

The gravimeter stands, when set up for use, on a *tripod;* but it might equally be stood upon a table with a bay cut out of the side two or three inches deep. In either case it would stand on three adjusting *foot-screws* carrying a thick brass *foundation plate.* Below this plate projects what I will call the *well* and a *cathetometer.* Above it stands the *chamber*, consisting of two brass plate sides and glass plate front and back, besides various clamping and manipulating appliances. On the top of the chamber are two cross *levels*, and from the centre rises a hollow *shaft.*

Measuring from the surface of the foundation plate, the depth of the well is about $8\frac{1}{4}$ inches (26 centims.), and the height of the chamber $5\frac{1}{2}$ inches (16 centims.). The shaft stands about $18\frac{1}{2}$ inches (47 centims.) above the plate, or 13 inches above the roof of the chamber. The latter is about $2\frac{1}{4}$ inches wide and $2\frac{3}{8}$ inches deep (say 6 centims. each way). This general description will suffice to give an idea of the framework. More minute descriptions will follow as they become necessary.

Within the chamber are suspended by wires, one below the other, two weights, which for distinction I will call the *major weight* and the *minor weight.* The major weight consists of a brass cubical *block*, from which rise two lateral rectangular *pillars*, crossed at top by a somewhat slighter *bar.* The block is perforated vertically by a large cylindrical hollow. This major weight is suspended by two parallel wires which traverse the shaft; being just visible at its lower end, where they enter two small screws on the cross-bar. There are means of adjusting both their length and distance apart, above and below.

The principle here is that of the bifilar torsion balance: if the block be turned through any angle less than 180°, its tendency to return is (mainly) due to the weight of the mass suspended. This, however, is not that of the major weight only; for from the under side of the cross-bar hangs a single wire, which, descending through the hollow in the block, is attached to and supports the minor weight; hence the mass suspended by the double wire is the sum of the masses of the two weights.

The minor weight consists of a light frame, which I will call the *head*, and a long thin glass rod or *plunger*, which descends into the well.

Suppose the major weight turned through an angle θ. When the disturbance has subsided and oscillations have been quelled, it will, of course, be found that the minor weight has turned through a like

* Since this report was written, two plates have been drawn to illustrate the description which follows. Necessarily there is no direct reference to them in the text.

angle. The double wires are in a state of torsion, but the single wire hangs freely, helping by its tension to increase the tension on the double wires caused by the major weight, but not itself in a state of torsion. Now, suppose the minor weight turned round: it will immediately begin to exert through the resistance to torsion, or elasticity, of the single wire, a force which will tend to relieve or oppose, according to the direction of the new application, the external force which keeps the major weight detorted. Suppose the new force applied in the *same* direction, so as to *relieve* the former. As the torsion of the single thread increases—with the increase of the angle through which the lower weight is turned—a point is at length reached when it exactly relieves the whole of the external force applied to the upper. Suppose this to occur when it has turned through an angle ϕ, *i.e.*, through an angle $\theta+\phi$, from the initial position. Then it is clear that the force which turned the upper end of the single wire through an angle θ, has been found equal to that which turns its lower end through an angle ϕ, relatively, and $\theta+\phi$, absolutely. We have now to consider what these forces are.

Before doing so, it would be advisable to recognise the means provided for observing these angles.

In some way, which there are no means of exactly discovering, the designer or constructor has ascertained that by a certain apportionment of lengths, weights, and thicknesses, the proportion of θ to ϕ (which of course is a variable one) can be made $1:3$ when $\theta=90°$. The result of this is, that when $\theta+\phi$ amounts to one complete revolution, θ alone is one quarter of a revolution. [In this position the resistance of a bifilar suspension is a maximum. I do not know that it has any strong advantage except what may turn on that. It is only necessary to allude to the fact, to take occasion to add that it is certainly of no importance, either practically or theoretically, whether θ is exactly or only approximately, equal to 90°. We shall see eventually that another consideration (perhaps not considered by the inventor) entirely overrides the one mentioned. This by way of parenthesis.]

The head of the minor weight carries a small flat *mirror*, which faces in the position of rest, as also after one revolution, a horizontal *collimator*. The block of the major weight carries *three similar mirrors*, of which one is parallel to the former (or may be made so) when in a state of ease, and the others become so (under proper conditions) when the major weight has been turned to the right or left through one quarter of a revolution. These mirrors face the upper half of the object-glass of the collimator. A *fiducial mark* in the focus of the latter is seen by reflection from the mirrors when they are perpendicular, or nearly so, to the line of sight. It will be necessary to return to this in describing the intended observation.

To understand thoroughly the conditions of equilibrium we must now study in detail the counteracting forces. We have seen that the weight supported by the double wire is the sum of the two weights described as the major and the minor weights. One-half of this sum is, of course, borne by each of the wires, which are further twisted individually through a quarter of a circle. Let R be the length of these wires, and $2r$ their distance apart. Consider one only: the upper end being fixed, the lower will describe a curve which will be the intersection of a sphere by a vertical cylinder, the ordinate from which to a plane drawn horizontally through the lowest point may be shown to be equal to

$$\frac{r}{\mathrm{R}} \cdot r \cdot \operatorname{versin}\theta \left\{ 1 + \tfrac{1}{2}\frac{r^2}{\mathrm{R}^2} \cdot \operatorname{versin}\theta - \&c. \right\}.$$

As $r : \mathrm{R}$ is a small fraction, less than $\frac{1}{100}$, it will be a question whether the second and further terms may not be neglected; for the present we may be content with the first, as an approximation only is wanted here. This ordinate is the height through which the weights rise as they are turned round. Let this be called h. A little consideration of the meaning of $\frac{h}{r \operatorname{versin}\theta} = \frac{r}{\mathrm{R}}$, *a constant*, shows that in this case the restriction to the first term makes the path of the lower end of the wire a circle lying in an inclined plane,* and the force tending to cause rotation (irrespective of the torsion of the parallel wires) is that of a body having the joint weight of the two, whose path of descent is to be along this circle. The gradient along this path (which is the tangent of the inclination) is $\frac{dh}{r \cdot d\theta} = \frac{r}{\mathrm{R}} \sin\theta$, which is zero at $0°$ and $180°$, and a maximum at $90°$. Let $\mathrm{A}+\mathrm{B}=\mathrm{P}$ be the weights, which are augmented for adjustment by a small weight, p. Then the force, depending on gravity, which tends to turn the system, resolved horizontally, is $(\mathrm{P}+p) \tan \angle$ inclination $= (\mathrm{P}+p)\frac{r}{\mathrm{R}} \sin\theta$.

Neglecting for the present the torsion of the parallel wires, we see that this force is resisted by the torsion of the single wire, as to which we must have regard to two propositions:—

(1.) Torsion is independent of tension;

(2.) Torsion varies directly as the angle of twist.

We may, therefore, express the torsion by $\rho_1\phi$ (at the distance unity), remarking that ϕ is mechanically increased until it balances the force tending to turn the system as above described.

Now, since $(\mathrm{P}+p)\frac{r}{\mathrm{R}} \sin\theta$ attains a maximum when $\theta = 90°$, if at

* This demands an elliptical cylinder.

this point it were balanced by $\rho_1\phi$, any increase of torsion caused by increase of ϕ, however small, would be answered by no corresponding augmentation of the opposing force. The upper system would obey, θ would increase, but the resistance would diminish instead of increasing. In short, $90°$ would represent a position of *unstable* equilibrium.

This state of things is prevented by the torsion of the parallel wires, which act in aid of the weight and *defer* the condition of unstable equilibrium to a point some degrees beyond $90°$. The perception of this may or may not have been present to the designer; but it may obviously be made use of advantageously as follows :—Let $2\rho_2\theta$ denote the torsion of the two parallel wires when turned through an angle θ. Then the forces which balance are $(\mathrm{P}+p)\frac{r}{\mathrm{R}}.\sin\theta+2\rho_2\theta$, and $\rho_1\phi$; and the utility of the instrument, depending in the first place on the absolute constancy of $\rho_1\phi$, depends also on the possibility of exactly equalising to this the other force by varying either θ or $\mathrm{P}+p$. It is in this respect exactly analogous to a balance, the desired relation of θ to ϕ being equivalent to the demand for horizontality in the latter. The sensibility of such an equipoise will be measured by the smallness of the change in $\mathrm{P}+p$ requisite to produce, or to correspond to, a given small change of θ.

Let $$(\mathrm{P}+p)\frac{r}{\mathrm{R}}.\sin\theta+2\rho_2\theta=\mathrm{F},$$

then $$\frac{d\mathrm{F}}{dp}=\frac{r}{\mathrm{R}}.\sin\theta,\qquad \frac{d\mathrm{F}}{d\theta}=(\mathrm{P}+p)\frac{r}{\mathrm{R}}.\cos\theta+2\rho_2,$$

and $$\delta p=\frac{\mathrm{R}}{r}\operatorname{cosec}.\theta\left((\mathrm{P}+p)\frac{r}{\mathrm{R}}.\cos\theta+2\rho_2\right)\delta\theta.$$

When $\theta=0°$, δp is infinite; *i.e.*, no change of weight will affect θ. Similarly when $\theta=180°$. But when $\theta=90°$, $\delta p=2\rho_2\frac{\mathrm{R}}{r}\delta\theta$. At this point no relation would subsist between δp and $\delta\theta$ but for ρ_2; *i.e.*, if the equilibrium were independent of the torsion of the parallel wires. As it is, the equilibrium is a compound one, and is disturbed by a change of weight, *the effect of which is inversely proportional to the torsion of the parallel wires*, in apparent contradiction to the first of the above propositions regarding torsion. If the torsion of these wires be doubled, the addition of the same small weight δp will cause a change of θ to one-half the amount. The sensibility of the instrument is therefore proportional (within limits) to the fineness of these wires; but this must not be carried so far as to bring the position of unstable equilibrium too near to $90°$, or else the stability of the balance will become too delicate and observation too difficult.

To determine the position of unstable equilibrium, consider the general statical equation

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}\cdot\sin\theta+2\rho_2\theta=\rho_1\phi.$$

When the left-hand side is a maximum its differential with respect to θ equals zero, therefore

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}\cos\theta_0+2\rho_2=0,$$

whence
$$\cos\theta_0=-\frac{\mathrm{R}}{r}\cdot\frac{2\rho_2}{\mathrm{P}+p}.$$

Now, as in the intended position of rest, $\theta=\frac{\pi}{2}$, $\phi=\frac{3\pi}{2}$, we have

$$(\mathrm{P}+p)\frac{r}{\mathrm{R}}=(3\rho_1-2\rho_2)\frac{\pi}{2}=2\rho_2\left(\frac{3}{2}\cdot\frac{\rho_1}{\rho_2}-1\right)\frac{\pi}{2};$$

Combining this with the former, it appears that

$$\sec\theta_0=-\left(\frac{3}{2}\cdot\frac{\rho_1}{\rho_2}-1\right)\frac{\pi}{2}.$$

The double wires and the single wire are no doubt of the same material, but the former are thinner. Their lengths are as $4:1$. Suppose their thicknesses as $2:3$. Then $\rho_1:\rho_2::4\times3^2:2^2::9:1$, which gives

$$\sec\theta_0=-12{\cdot}5\,\frac{\pi}{2}=-19{\cdot}64,$$

and $\therefore$
$$\theta_0=92^\circ\ 55'.$$

That is to say, the position of unstable equilibrium lies less than 3° beyond the intended position of rest. [This is closer than I should have thought, but it is borne out by the fact that I have not been able (with the existing adjustments) to reach the intended position of rest *both* ways, *i.e.*, turning both to the right hand and to the left hand, before reaching the point of unstable equilibrium. This is easily accounted for by supposing the primary position to be a balanced one, a slight deviation from parallelism of the mirrors being induced by a slight initial torsion of the parallel wires.] To return:—

The angle through which the *lower* end of the single torsion wire is turned, by the stop acting on the minor weight, is $\theta+\phi$. As this angle approaches 360°, θ, which is the angle through which the *upper* end and the major weight are turned, approaches, or should approach, 90°. As this stage is approached a very small increase of $\theta+\phi$ induces a larger and larger increase of θ, according to the proximity to the value θ_0. And the above expression for sec θ_0 shows that this depends on the ratio of $\rho_1:\rho_2$. By varying the strength of the double

wires the position of unstable equilibrium may be placed anywhere from $\theta_0=90°$ to $\theta_0=180°$; and the nearer it is to 90°, *i.e.*, the weaker they are, the more sensitive will the instrument be at the position $\theta=90°$. This suggests, I think, that the double wires should be coarse and inelastic rather than the reverse.

The ratio $r:\mathrm{R}$ is as nearly as I can judge by measurement 1 : 138, and $(\mathrm{P}+p)$ would be, at the equator, 3,725 grs.; so that $(\mathrm{P}+p)\frac{r}{\mathrm{R}}=$ 27 grs. very nearly. Now,

$$\rho_1\cdot\pi=\frac{2}{3}(\mathrm{P}+p)\frac{r}{\mathrm{R}}\cdot\frac{1}{1-\frac{2}{3}\cdot\frac{\rho_2}{\rho_1}}$$

$$=18\left(1+\frac{2}{3}\cdot\frac{\rho_2}{\rho_1}+\dots\right)\text{ grs.}$$

This is the force, exerted at a distance $r=\cdot09$ inch, which is requisite to turn the single wire, of length 3 inches, through the angle π. If $\frac{\rho_2}{\rho_1}$ be taken as above, equal to $\frac{1}{9}$, $\rho_1\pi=19\frac{1}{3}$ grs., at ·09 inch; and $\rho_2\pi=2\frac{1}{7}$ grs. nearly.

We can now form some estimate of the effect of a small change of weight. We have seen that $\delta p=2\rho_2\cdot\frac{\mathrm{R}}{r}\cdot\delta\theta$. Substituting numerical values, as above, we find $\delta p=138\times4\frac{2}{7}\times\frac{\delta\theta}{\pi}=591\frac{\delta\theta}{\pi}$ nearly.

The collimator contains a scale, exactly $\frac{1}{4}$th of an inch long, divided into 30 parts. The focal distance being 8·5 inches, the angular value of the scale is $\tan^{-1}\frac{1}{34}$ or 1° 41′; and of each part, 3′ 22″, or $\frac{1}{3200}\pi$ approximately. Putting one of these parts equal to $2\delta\theta$ (doubling on account of the reflection) we find $\delta p=\frac{591}{6400}=\cdot092$ gr., the change of weight which will deflect the mirrors *relatively* one division. It follows that the 30 divisions of the scale correspond to a change of weight of about 2·76 grs.

But we must remember that this relation depends on an assumption, viz., that $\rho_1:\rho_2::9:1$. To provide for a more accurate estimate being hereafter obtainable, let $\frac{2\rho_2}{3\rho_1}=k$. Then $\rho_1\pi=\frac{18}{1-k}$, $\rho_2\pi=\frac{27k}{1-k}$, $\delta p=7452\frac{k}{1-k}\frac{\delta\theta}{\pi}$; and in the case of the scale-division $\delta p=1\cdot16\frac{k}{1-k}$ grs., or very nearly $35\frac{k}{1-k}$ for the whole scale.

Let us now consider what may be expected as the actual consequence of a change of gravity. The most practical evidence of this is the effect on the rate of a pendulum. A pendulum which would beat seconds at the equator would gain 225 seconds, or beats rather, at the

poles. The corresponding increase of gravity is in double this proportion, *i.e.*, 450 on 86,400, or 1 on 192, nearly. Weights, as measured by a constant force such as the torsion of a wire is supposed to be, are affected in like proportion but inversely. Now the weight of the mass suspended by the double wires is 3,725 grs.; and the change of weight, if one may so express it, from pole to equator, will be $\frac{3725}{192}$, or 19·4 grs.

We have seen that the range of the scale in the collimator will measure a deflection due to a change of weight of 2·76 grs., or thereabouts. It follows that the extreme change of gravity which can be looked for would cause a deflection about seven times as great as what the scale will measure.

This is provided for by the auxiliary weights, of which there are five, weighing 4·4 grs. each, and by the contrivance of the glycerine well, which will now be described. Before quitting this part of the subject, however, I should point out that one division of the scale seems to correspond to a change of pendulum rate of $\frac{\cdot 092}{19\cdot 4} \times 225 = 1\cdot 07$ second *per diem*.

The buoyancy of the glycerine in the well depends on the volume of liquid displaced by the plunger, and therefore on the diameter of the latter, which can only be got at awkwardly, in one place. To ascertain this diameter I cut a slit in a piece of zinc plate, rather wider at the mouth than at the inner end, and used it as a gauge, marking the place where the narrowing width fitted the plunger. A piece of copper wire, ·05 in diameter, was found to fit it at the same spot; perhaps ·048 would be more correct.

The specific gravity of glycerine is 1·26; hence a cubic inch weighs 349·4 grs. With these data we find that one inch rise of glycerine will buoy 0·628 gr. The cathetometer scale being divided metrically, this corresponds to 0·0247 gr. per millimetre. The micrometer reads to hundredths of a millimetre.

The riders weigh, as nearly as I can determine, 4·40 grs. each. Hence one rider has the same effect as 178 millims. of rise; but the scale only runs up to 150 millims., so that additional riders will be requisite.

I found by trial that 100 millims. rise of the glycerine buoyed about 2 grs. It was not a careful observation, but agrees fairly with the above calculation; so that it seems pretty certain that the riders are too heavy.

The total weight being 3,725 grs., the variation of gravity from pole to equator (being as 193 : 192 nearly) will require an augmentation of 19·4 grs. This corresponds to 225 seconds, therefore 1 second corresponds to 0·0867 gr., or 3·5 millims. of glycerine, if the above is correct.

We must look for accuracy which may be represented by $\pm 0{\cdot}1$ second, or the instrument is not worth testing severely. This corresponds to 0·35 millim. of glycerine, or 35 divisions of the micrometer.

The foregoing description and investigation represent what I had learnt about the gravimeter by inspection, measurement, calculation, and reflection, before meeting with any instruction from the designer. In his letter to General Walker, Mr. Broun mentions having seen and corrected the proof of the description which was to appear in a new edition of the Catalogue of Instruments exhibited in the Special Loan Collection. Not expecting that a catalogue description would be otherwise than brief, and confined to principal features such as I could not fail to perceive unaided, I did not wait until I could procure it, to study the instrument. Some delay also occurred before I received, through the courtesy of the Secretary of the Science and Art Department, the extract in question. It seems right to mention this, in explanation of the independence which will be remarked in what I have said. Whereas, in what follows, I acknowledge the said description as an additional source of information. Mr. Broun's description is accordingly inserted here; and I shall then add some comments upon points which seem to need further elucidation or notice.

"**421d. Gravimeter.** An instrument for the measurement of the variations of the earth's attractive force, invented by J. A. Broun, F.R.S., and constructed from his drawings by Dr. C. Müller, of Stuttgart. *J. Allan Broun, F.R.S.*

"The instrument consists of a weight suspended by two gold wires; a single wire fixed to the top of the weight and passing through its centre carries a cylindrical lever; when the lever is turned through 360° at the normal (say southern) station, the torsion of the single wire thus produced carries the weight round through an angle of 90°. The forces then in equilibrium are, the torsion force of the single wire and the attraction of the earth on the weight, which, as the two wires are no longer vertical, has been slightly raised and seeks to attain its lowest point.

"On proceeding from a southern to a more northerly station the earth's attraction increases; the amount of this increase may be measured in two ways:—

"1st. The lever will require to be turned through *more* than 360° in order to carry the weight to the height due to turning it through 90°. (Had the station been more southerly the lever would be turned through *less* than 360°.) The difference of the angle from 360° measures the increase (or diminution) of weight.

"2nd. By removing a small portion of the weight, equal to that due to the increased attraction of the earth, the weight can be turned through exactly 90° by rotating the lever through 360°, as at the normal station. (On proceeding south weight has to be added.)

"The following are the instrumental arrangements in order to make these observations:—

"The weight has on each of three sides, at its base, a vertical mirror (silvered, not quicksilvered); the middle mirror makes an angle of exactly 90° with the other two. The lever also carries a vertical mirror, which, when there is no torsion in the suspension wire is immediately below and in the same vertical plane with the middle mirror of the weight. A telescope, having a glass scale at the focus of the eye-piece, is adjusted so that images of the scale can be seen (one higher than the other) reflected from the middle mirror of the weight and the lever mirror. When both of these mirrors are exactly in the same plane, the middle division on the scale seen directly with the eye-piece, coincides with the same division in the two reflected images.

"By a wheel and pinion (with endless screw and clamp for delicate movement) placed below the instrument, a polished agate point can be made to act on a similar agate point fixed to the lever, so as to turn the latter through any angle. When turned through 360° the middle scale division again agrees with the image from the lever mirror. If the image reflected from one of the side mirrors of the weight does not agree also, the lever is turned through a greater (or lesser) angle than 360°, till this agreement is obtained; the difference of the angle through which the lever has been turned from 360° is obtained from the scale reading, as seen on the lever mirror.

"The following apparatus is employed for very small increases or diminutions of the weight. Suspended to and vertically below the lever is a carefully *calibrated* glass wire (1 millim. diameter), which enters a glass tube fixed below the instrument. At the lower end of this tube is a cistern containing a liquid (distilled water, or as at present, chemically pure glycerine). This liquid can be forced into the glass tube by a screw and piston (as in some barometer cisterns). The liquid is then raised till such a diminution of weight is produced by the immersion of the glass wire as to bring the mirror of the weight through exactly 90°, when the lever is turned through 360°. The length of glass wire immersed is read, by a micrometer microscope and scale, to a thousandth of a millimeter.

"Though finely polished agate points have been employed for

turning the lever so as to diminish the friction, there is an additional apparatus to ensure that vertical friction has no effect on the observation at last. The lever contains a magnet; and two bar magnets, with rack-work adjustments for height, are placed one on each side of the instrument, so that by a pinion and rack movement they can be approached to the lever magnet till their force is exactly equal to the torsion force of the single wire, and the agate points are no longer in contact.

"The instrument is made to serve for latitudes differing about 10° or 15°, but an auxiliary apparatus carries five platinum rings, which can be lowered upon the weight, so as to make the instrument serve from the equator to the poles, and to any height in the atmosphere.

"There are special appliances for portability, by one of which the weight is fixed; another fixes the lever; so that strain is removed from the suspension wires, and the suspended parts cannot be shaken from their places. Levels, a thermometer, and other details fit the instrument for the most accurate observations. The suspension wires are fixed at their ends in a special manner, so that the fixed points cannot vary. All the suspended apparatus is electro-gilt."

(1.) We learn from this that the suspending wires are of gold, and that suspended parts are electro-gilt. I observe that there is a tendency to spottiness, resembling mould, on some parts of the gilded surface.

(2.) The part which I have called the "minor weight" is here designated as a "cylindrical lever." The latter term is but remotely descriptive.

(3.) The torsion of the double wires *is not alluded to* in describing the forces which balance each other. This confirms my doubt whether the very important part played by this torsion was recognised. It is an essential feature, without which the position of maximum gravity-action could not be chosen; and in making the adjustments it is impossible to disregard it.

(4.) There is no necessity for the two side mirrors on the major weight to be inclined at "exactly 90°" to the middle one. Indeed, it is scarcely possible to tell exactly what their inclination is. It is *about* 90°; and that is all that can be said or desired.

(5.) Of the two terms "telescope" and "collimator," the latter describes more correctly the function of the appliance by which the angular positions of the mirrors are observed. The scale is in the focus of the *object-glass* of this collimator, rather than in that of its *eye-piece*, though, of course, the latter is also true. The divisions and value of this scale are not mentioned. If my estimate is right, which

makes one division correspond to $1^s \cdot 07$ *per diem*, it is clear that this is a weak point, especially as the collimator is optically indifferent.

(6.) This "telescope" or collimator is said to be "adjusted" so as to perform certain functions. Unfortunately it has no means of adjustment except in a vertical plane. There is no horizontal motion; nor has the object-glass a power of focal adjustment by rack and pinion. It is *very* difficult to get a good sight of the reflected scale.

(7.) The only dimension stated in the whole description is the diameter of the "plunger" (1 millim.). Neither are the weights of the suspended masses stated. A precise knowledge of the joint weight is necessary for the calculation, even of *differential* results. By precise I mean to within 4 or 5 grs.

(8.) The plunger is described as a "*calibrated* glass wire." I imagine this to mean that the glass rod was specially made, and tested at every point. I do not see any advantage in its being of glass.

(9.) "The length of glass wire immersed"—that is to say, the length of liquid displaced—"is read . . . . to a thousandth of a millimeter." The graduation of the micrometer head enables hundredths of a millimeter to be read. The thousandths are by estimation. But this probably far exceeds the power of observation of the surface of a liquid, such as glycerine, in a glass tube.

(10.) "The finely polished agate points" appear to be of ruby-coloured glass—if ready fusion be any test.

(11.) The magnetic holder. I imagine this is better removed. It would be nearly impossible to guarantee its action being *entirely horizontal.* And the presence of a magnet as part of a mass, the weight of which is under examination, is inadmissible.

Should these comments give the impression that I wish to cavil at the description, I must reply that it is necessary for the present purpose. The question before us is not whether the instrument is ingenious; but whether it can be used for the intended purpose, in preference to other existing instruments whose use and powers are well known.

It may be said that that question can best be answered *by trying it.* Unfortunately this is not the case. It will already have become apparent that it is one of a class of instruments in which the observation is nothing but the last of a series of elaborate and difficult adjustments—adjustments which require patience and skill and no little time, all of which would be thrown away if the ultimate observation should prove abortive. I will now proceed to indicate more exactly what these adjustments seem to be.

The principle on which the instrument is designed involves, as a primary consideration, the angular rotation and ultimate angular position of two bodies. The angles θ and ϕ of the theory above

explained, although unimportant in absolute magnitude, are all-important *relatively*. The prime defect of the instrument is to be seen in the insufficient optical means of noting and *recording* this relation. This might be remedied without altering the instrument in any way, by changing the collimator; so I will not dwell further on that, but pass on to the means of controlling and *adjusting* this angular position. Inasmuch as *torsion of wires* is in question, it is obvious that *the way in which their ends are held and turned* is a detail of extreme importance. Mr. Broun has alluded to this. He says they "are fixed at their ends in a special manner, so that the fixed points cannot vary." It is unfortunate that one cannot learn certainly by inspection, what the attachment is. It is probably by the pinch of a split screw, for the wires appear to pass through the axes of the holding screws. Whatever the method is, it ought to be unimpeachable. But not only should the holding be secure, it should also be easily manageable. It is of little avail to attach a fine wire to a delicate screw which can only be manipulated with caution by a steady hand, for the adjustments depend mainly on these ends of wires being turned accurately through very small angles. I regard it as a capital error of construction that the grasp of the ends of these suspension wires is made as *small* instead of as *large* as possible. This is an opinion based on wearisome experience no less than on common sense, for I have spent many hours in endeavouring to obtain the adjustment in question, with no other result than this experience and the discovery of the cause of repeated failure—as I will now explain.

The *first* adjustment required would seem to be to make the minor weight hang so that its mirror shall be parallel to the middle mirror of the major weight. To secure this the holding of the single suspending wire must be turned, either above or below. It is difficult, if not impossible, to get at the lower holding. But the upper one offers no difficulty except what is to be expected from the smallness of the parts. The necessary adjustment was at length made, approximately.

The *second* adjustment consists in so managing the torsion of the double wires, that when the minor weight (with its mirror) is turned through 360° *either way*, the major weight shall present its right or its left hand mirror *equally* short of or beyond the ultimate position. The observation in that case will consist in ascertaining what alteration of weight will bring about exact conjunction, *in either case*.

The difficulty, which I have already commented on, of giving any precise amount of rotation to the holding screws, is in this matter also so great as to make exact adjustment quite fortuitous. This will explain why I made the same adjustment *several times in succession* (on each occasion with sorely tried temper and patience) before

becoming aware that there was something wrong. I traced this at last distinctly to a *want of permanence* of the *first* adjustment. I had spent from first to last not less than ten hours on these adjustments alone, which may give some idea of their uncertainty.

It was now necessary to ascertain what was the source of the instability. It must be clearly understood that there is not any part of the whole design which is of greater importance than the attainment of permanence in this part. If, after disturbance, the lower weight does not return to the *same* position, relatively to the upper, *with absolute exactness*, I see no chance of obtaining anything which can be called a result. It is literally a *sine quâ non.*

The instability might be due to one of two causes—I see no third alternative. Either the holding was insecure; or the wire was strained beyond what its elasticity would bear. I tried various plans to test this. At first it seemed clear that the holding was in fault. Then I fancied that the wire was strained. I mention this vacillation purposely, because my final conclusion, which condemns the holding after all, though more hopeful, *might* be wrong, and as it challenges the "special manner" noticed in Mr. Broun's description, it will be best not to be too certain. The following test, however, seems conclusive.

I prepared two needles of deal, about 2 inches long and as thin and light as seemed necessary. These were split at one end, and thrust upon the taut wire, so as to stand out from it horizontally. One was placed close to the upper holding, but free from contact; the other about 0·3 inch lower. The upper weight was clamped, and the lower then turned through two entire revolutions. [I did not scruple to overdo it, having ascertained from collateral experiments with other wire that a wire will bear being turned twenty or thirty times to every inch, without any other ill effect than a permanent twist.] The result was that the upper index turned through a few (8 or 10) degrees, and the lower through 70° or 80°—the latter being sensibly in due proportion to its distance: as to the former, I could not say exactly where the holding point might be. Now, the test would be in the positions to which they would return. If the wire was strained, the lower index would not return to conjunction with the upper: if the holding had failed, the *two* would not return to the starting point. The event proved the latter alternative. The lower index returned to conjunction with the upper; but both failed to return to the original position. I repeated the experiment, giving the weight only a single turn. The result was the same, in less degree: the holding had again failed, *i.e.*, had allowed the wire to turn in its socket *still further.* The screws were all firm, on trial.

The above test is so easy, and useful, that a description of it needs no apology. Still, it is rather by way of proof that I give it; for a

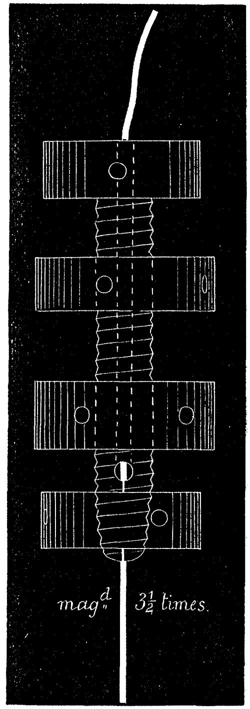
bare assertion of belief in the insecurity of the holding might fairly be challenged.

The above result could in no way be passed by. It was necessary to run any needful risk to ascertain further whether the evil was accidental and capable of being remedied without some decisive alteration. I therefore loosened the fastening and drew out the wire. The only result of this was to show that the wire was held *solely* by the pinch of a split screw. This is the "special manner" alluded to in the description, and I have no hesitation in saying that it is bad, because it relies on the almost microscopic accuracy of cutting of the parts. Assuming that no play is required in respect of length, there can be no objection to doubling back the wire before clamping it—and that in a large screw, split boldly for half an inch or more. No slip could possibly happen in that case, even with very slight clamping. This is the hold I have used (in the experiments alluded to), the clamp being a cleft in wood, pinched *ad libitum*.

Under the circumstances I replaced the wire, and screwed the holding nut nearly to the utmost the screw would bear; and then repeated the test—with the same result. The hold by friction (of brass upon gold) is, at any rate in this particular case, insecure. Nor do I see any means of remedy short of partial reconstruction, which should, of course, extend also to the other five holding points.

Should any doubt remain as to the slipping I add the following. Wishing to see whether it was due to the split being too coarse, I decided on extricating the holder and examining it under a microscope; as also to see if the wire showed any abrasion or destruction. To get the holder out involves a great deal of dismemberment; but the choice lay between this and leaving the question in some doubt. I succeeded in doing it—it is needless to describe how. The first result was the possibility of recognising clearly the nature of the holder, of which I give here an enlarged view. It is a screw about $\frac{3}{4}$ inch long, with capstan head, and three capstan nuts; of which two hold it on the beam, and the third pinches the split point. The split is quite flat-sided. The shank is hollow, as far as to the end of the slit, where there is a cross tunnel. By way of trial I pinched a piece of silver wire in the way the suspension wire is pinched, and twisted it. It was thinner than the latter. I think the pinch held it, for it soon broke. This was against the theory of slipping. Fortunately, I had saved a fragment of the gold wire (about $\frac{1}{2}$ inch) which had projected at the free end and had been broken off by the insertion of the pin (I suppose). This piece I inserted and pinched firmly, as firmly as I dared without spoiling the screw. The projecting end I bent into a crook, which I grasped with tweezers. I then turned the screw. Would the wire strain, or break, or slip? That was the question. The event justified previous experience. It slipped freely.

I could not pinch it tight enough to hold. Subsequent examination showed no sign upon the gold wire.



This proves conclusively that, under these circumstances, the pinch of a round wire is an insecure hold against torsion.*

I cannot but regret this result, for the sake of the exquisite workmanship; but there is no help for it, the wires must be held in some other way before anything can be done to test the instrument as a gravimeter. I find, for instance, that a slight flattening of the wire meets the slipping difficulty perfectly; but a flattened wire strains (*i.e.*, takes up a twisted or strained condition) very readily, and it is doubtful whether any alteration of the cylindrical form, even for a *very* small part of the length, can be permitted without risk.

My examination having reached this stage, I found it necessary to decide whether to stop here or run some risk of injuring the apparent perfection of the instrument, in the endeavour to obtain the first requisite, a firm hold of the ends of the torsion wire. I defer, for the

* I have since tried a steel wire pinched between lead sheets in a vice. No pinch seems sufficient to prevent the slipping, if the wire is straight, but a slight crook suffices.

present, all reflections in order to proceed with my narrative. I decided to flatten the wire slightly at the place where it passes the split. Whether I did so insufficiently or not at the right place it is impossible now to say. The result was slipping, as before. I then determined to have recourse to the loop—by which I mean giving to the wire at the place where it is pinched a sharp bend back upon itself. To do this the whole instrument had again to be dismembered. I cannot give an adequate idea of the anxiety attending a step of this kind, in the case of an unfamiliar instrument of delicate construction. My anxiety would have been greater perhaps, but hardly my care, had I known, what I now learnt, that the gold wires were nearly as brittle as untempered steel. In dismounting the major weight (without which the lower attachment of the single wire could not be reached) one of the double suspending wires snapped off short at the fastening. I did not recognise the cause until on attempting to double back the single wire to give it the crook it also snapped short. These mishaps were experienced without much cost, for the new plan demanded but a fifth of an inch of wire; which the other end, in each case, could easily spare. But before trying to bend it again I took the precaution to anneal the end. At length, after a deal of trouble, I had succeeded in fastening in this way both ends of the single wire, and of the broken one of the pair. I ought to have done the same with the other as well, but courage was wanting to go further in this direction than accident had rendered necessary. The torsion on so long and thin a wire (the pair are much thinner than the single one) turned through only one quarter of a revolution, would probably be so slight as not to exceed the holding power of the existing attachment.

I should say here, that before putting the parts together again, I weighed them, and measured the wire-lengths. The weighing is elsewhere recounted. The lengths are 12·6 and 3·08 inches respectively. I also took the opportunity to measure exactly the diameter of the glass plunger, having reason to doubt the correctness of the measurement assigned in Mr. Broun's description. The result justified my suspicion.

The end of all this is now at hand. In due time the instrument was once more in a condition to recommence the adjustments. Warned by previous experience I wasted no time over perfecting the first; but, noting the actual position of the lower mirror when at rest, I turned it through two revolutions and allowed it to return. *It failed to reach its former place.*

I tightened the holding nuts to the utmost which the metal would bear; with no better result than to reduce the slipping, but not to prevent it. The index test seemed to exonerate the upper holding, but there was either slipping or straining—and that to a variable extent—on every trial, whichever way the lower weight was turned.

At this point, I decided on abandoning the investigation, until I should receive further instructions. Considering that the instrument has been entrusted to me to experiment *with* rather than *upon*, I have already dared more perhaps than I ought to have—certainly more than most persons would have felt justified in doing. I presume, perhaps, in thinking that a rather long experience of instruments of precision will be accepted as my excuse for having gone so far; in the earnest endeavour to ascertain whether an instrument of such exceeding beauty (as to workmanship) would prove as valuable as it looked, and as, I must say, the principle of its construction leads one to expect. In pursuing this endeavour I have done some slight injury to it—which can be easily remedied if necessary. This I admit: but *per contra* I have ascertained a good deal without which it would be useless, besides gaining some experience which should avail in perfecting it, and making it (or another similar in principle) useful for its intended purpose. Finally, if any further apology is necessary, I will add this—that in no case could the instrument have been actually and *efficiently* employed for that purpose without material alterations.

I cannot too often repeat, that both as regards the design and its execution, the instrument deserves high praise. Nevertheless it is a failure. I have felt this all along, and I ought not to conclude this paper without pointing out what I conceive to be its chief defects. Of the defect which has brought this trial to a premature conclusion —the insufficient hold of the ends of the wires—it is only necessary to say that some plan should be discovered of putting this beyond question. This is the *first* consideration. I do not regard this as an error either of design or of construction such as can be complained of. But I do regard as such that which gives the instrument, as actually constructed, its beauty, viz., the minuteness and delicacy of its parts. There is a wealth of adjustment which is not necessary, and their details are all on far too small a scale. I suppose that the major weight consists of not less than 100 parts. Of these probably 80 could be set apart whose total weight would not reach 200 grs. out of the 3,100 which the whole weighs. It is clear that, supposing the total to be restricted to that, a more generous distribution to the smaller parts might have been made, without any disadvantage; but on the contrary, a great gain in handiness. I fear it is useless to add that a large proportion of these smaller parts are of the kind which instrument makers delight to show their wonderful skill in producing —as nature does flowers. Of course, one cannot tell how far these multifarious adjustments may not owe their presence to a conscientious endeavour on the part of the maker to give effect to his instructions: but that must not prevent my saying that they are, to a large extent, redundant, unnecessary, if not useless.

I regard it as a mistake that the whole instrument is on so small a

scale. We should not be far wrong in estimating the power of such an instrument, not *in proportion* to its size, but in an even higher ratio. Even in scale-balances there is an advantage in size; though there it is the *absolute* weight put into the scale which is measured, whereas in the gravimeter we have a balance whose delicacy is measured by the *relative* minuteness of the weight added. The torsion gravimeter relies on the perfect obedience of the bifilar suspension, due to the absence of friction. Until it can be practically shown that this obedience is, in practice, *not* perfect, it is an abuse of the leading principle to refrain from drawing upon the resource it offers. This is what is done when high constructive art and skill are exerted to keep down the weight and size of parts instead of the contrary.

The minor weight is turned as one turns the hand of a clock with the finger. I cannot imagine why this one-sided action is preferred—for I suppose it is preferred—to the obvious two-fingered action, which comes into play in so many common practices where one wishes to avoid displacing the central axis of motion. The effect is to give a wobbling swing to the whole hanging system, increasing the risk of jar and strain, besides displacing the centre, and thereby altering the normal direction of the pull of the lower weight as well as the verticality of the mirrors. [It is true that magnets are provided to relieve this, but I hardly suppose any one desiring to make accurate observations would allow them to remain as preferable to the two-fingered stop.] I have already remarked on the so-called "finely-polished agate points," which I have been obliged to replace by steel ones, because being very thin and of glass they soon got broken off. I do not recognise any objection to their being of metal.

A tripod stand is furnished with the instrument, and I always used it; but it is very unsuitable, for the following reason. A portable tripod almost necessarily requires a large splay; this involves risk—even in the hands of a surveyor habituated to three-legged stands—and risk of a kind which, in my opinion, is fatal in the case of such an instrument as this. This is why I notice the stand. I doubt if anything like the necessary permanence of condition could be looked for in a fine wire which, when supporting such a weight as 600 grs., had to sustain a jolt, or such a shock as would be caused by a slight kick to one of the legs of the stand. I have no proof of this. It is one of the tests I intended to inflict on the instrument. In the absence of anything but a strong doubt, I can do no better than set it down here for future trial.

The last point which I shall dwell upon is one which has already been noticed, the effect of torsion of the double wires in *deferring* the position of unstable equilibrium beyond that of maximum gravity resistance. As already pointed out, the possibility of choosing 90° as the place of rest turns on the alliance of this torsion with the force of

gravity in opposing the torsion of the single wire. If Mr. Broun was aware of this, it is strange that he gives no hint of it in mentioning the forces in equilibrium; anyhow, the third force is there, and might, I think, be taken advantage of. I found by trial that the torsion of the single wire might be increased so cautiously as to cause the major weight to stand nearly stationary at the position of unstable equilibrium. If the angle of position of the lower weight were read off (by vernier or otherwise) when this happened, both right hand and left hand, I imagine a delicate measure of the variation of gravity would be obtained, without the need of any appliances for varying the weight, or of a collimator. It would be foreign to the purpose of this paper to pursue this design further here; it is enough to have indicated it in connexion with the principle involved in Broun's design. I will only add that the two principles here indicated, viz., the balancing of gravity by torsion, and the determination of the condition of equilibrium when unstable, are both involved in a simple bifilar suspension; and I see reason to think that this form of gravimeter, from its extreme simplicity and great adaptability, is worth consideration.*

It now only remains to offer some apology for the length of this report, and for its discursiveness. It will have been quite apparent to any one who has had the patience to read it, that it has been written from day to day, as the examination proceeded; and now that the latter has to be closed without anything of the nature of an "observation" having been possible, it may be that there is a certain advantage in letting the facts, so brought forward, tell their own tale. Much, no doubt, could well be spared, but at the risk of laming the narrative. . . . . At the same time, I may say that I am quite willing to renew the attack, and do what can be done to reach a more promising conclusion, and to make the instrument efficient; provided I am so instructed. In that case, however, the sanction of the Royal Society, to whom it belongs, will be also

* If $(P+p)\frac{r}{R}\sin\theta = 2\rho\phi$ be the general statical equation of a simple bifilar system, the equilibrium is unstable when $\sec\theta = -\frac{P+p}{2\rho}\cdot\frac{r}{R}$, in which position $\phi_0 = -\tan\theta_0$. From this it is at once apparent, since ϕ must be positive, that θ_0 must lie between 90° and 180°. It must also be greater than 90°, or ϕ_0 would be infinite; but beyond this I do not see any theoretical restriction. I think it would be advisable to make ϕ_0 as large—several revolutions—as the wires will bear without injury. All that is necessary is that they be provided with means of turning their upper ends, until the weight reaches its position of unstable equilibrium (which should happen when θ is little more than 90°), and with means of recording the angle through which they have been turned so as to reach this condition. The applicability of such an arrangement, as a sensitive gravimeter, will depend entirely on the adaptation; which, I believe, would be found quite feasible. The equations here given are sufficient to determine suitable proportions.

necessary; as alterations may be necessary which one would otherwise have no right to make.

I append a separate account of the weighings of the major and minor weights. Supposing the instrument ultimately brought into use, it would become necessary to know the total weight of the suspended parts with some accuracy. This need should be kept in view, if alterations are made.

Account of the Weighing.

I have weighed the suspended parts on three occasions, the circumstances differing in each case somewhat. In the first two weighings, the cross piece at the top of the shaft having been removed, the whole of the swinging parts were suspended from one arm of a balance—with exception of three mirrors absent for repair at the time. This suspension involved certain additions and subtractions by way of allowance for parts not present or redundant. These were either determined or estimated for. I have since been able to correct all the estimations, and the first weighing makes the sum of the two parts A and B equal to $3{,}721+69+14-35-18-22=3{,}729$ grs.

In the second weighing—being uncertain of the accuracy of the weights used—I prepared two lead blocks representing approximately the masses A and B; and, by more directly counterpoising the redundant parts, and reducing to a minimum all corrections, I obtained a mass A equal to 3,109 grs. and a mass B equal to 615 grs., the sum of which or 3,724 grs. balanced, or would have balanced (for I could not eliminate two small pieces which had to be allowed for) the whole of the suspended parts. The separation into two parts provided for the removal of the lesser when the minor weight was supported. I was quite aware that this was a doubtful partition, owing to the impossibility of *exactly supporting* the lower weight without imparting a thrust through the single wire. But there was no way of obviating this without a separation of the two parts of the mechanism. The *total* weight was free from suspicion.

When later events led me to take the whole apparatus to pieces, I took care before building it up again to weigh the two principal parts separately—as well as to take some measurements which seemed important. The result showed that this thrust had been much stronger than I supposed, having transferred 9 or 10 grs. from the lower to the upper estimate. I now found, directly and without any allowances or reductions—the large mass being in one scale, and the major weight *as it would hang* in the other, that the former required paring down. The parings weighed 9 grs. Conversely the smaller mass required an addition of 10 grs. to balance the minor weight. Having made this transfer, I now have the two leaden masses, repre-

senting (each within 1 gr., I think) the major and minor weights, as they actually hang: they weigh (according to my scale weights) 3,100 and 624 grs. respectively. The sum agrees with the second weighing to a grain—the exact agreement being unintentional.

These weighings do not include the platinum wire riders.

I may add here an account of the measurement of the glass plunger—described by Mr. Broun as "calibrated" to 1 millim. My rough (?) measurement had indicated, as above said, ·048 inch as its diameter. The cathetometer microscope seemed to show it about 120 divisions, or 1·2 millims. $=$ ·049 inch, but I did not trust this. I hung a small weight by a fibre of raw silk and wound up 100 turns of it on the glass rod. The length absorbed was 14·9 inches. I estimate the diameter of the silk, which was coarse, at ·001; but it is very uneven. This would give $\frac{0 \cdot 149}{\pi} - \cdot 001 = \cdot 046$ inch. Not satisfied, I repeated this with some fine silver wire, annealing it first. This gave similarly $\frac{0 \cdot 154}{\pi} - \cdot 0036 = \cdot 0454$ inch. The diameter of the wire was got by measuring the length covered by the close coil of 100 turns. The true diameter of the glass rod is rather larger than this last, as the outer part of the wire would stretch more than the inner would compress. It may be taken as ·046 inch at the place chosen, which was unfortunately near the top. As 1 millim. is ·0394 inch, Mr. Broun's statement on this point must be rejected.

I cannot imagine on what ground this plunger has been made of glass. Surely it cannot be contended that a metal wire would be of uneven diameter, in a way that a glass one would not be? On the other hand the risk of injury is considerable, and such a rod would be impossible to replace in foreign parts.

DESCRIPTION OF PLATES 8 AND 9.

As has been mentioned in the note to p. 519 the plates were prepared subsequent to the submission of the Report, which they are intended to illustrate.

Plate 8 is a general oblique view of the gravimeter on its tripod stand. The chief parts seen are—the *shaft* through which the parallel wires descend:—the *chamber* through the glass front of which is seen obscurely what is represented very faithfully in Plate 9:—the collimator, or observing telescope, to which a somewhat undue prominence is given in the drawing, by the effect of foreshortening:—the *table* and foot-screws:—the *tripod* stand:—the *well* and glycerine reservoir, with the cathetometer on the left.

There is hardly any part of this plate which requires more special explanation than will be found in the foregoing pages, if we except perhaps the arrangement of nuts and screws on the outside of the chamber; and these will be readily understood by consulting the other plate, where it is seen that they serve to govern the positions of the two stages inside by which the hanging part (the major weight of the Report) is held so as to maintain a fixed position, with slackened supporting wires, when

packed. There is a different arrangement for holding the minor weight, which the engraver has failed in representing so well as the rest.

Plate 9 shows also a variety of milled screw-heads, each of which has of course a purpose; but as neither of them, except one by which the lower weight is turned round, has any part in the observations such as they are described above, but only in ultimate manipulations which it is unnecessary to dwell upon here, they may be regarded as ornaments. It will be noticed that the collimator is removed, and one of the side supports is supposed broken off, to discover the minor weight; which last, with its mirror and magnet bar, is seen turned through 30° or 40° into an oblique position. At the same level and outside the chamber are seen arms, one of which resembles a cross. These are the guides for two magnets which have been removed. Their intended purpose is mentioned by Broun in his description.

The pillar alongside the shaft is a case for a thermometer, the bulb of which is within the chamber.

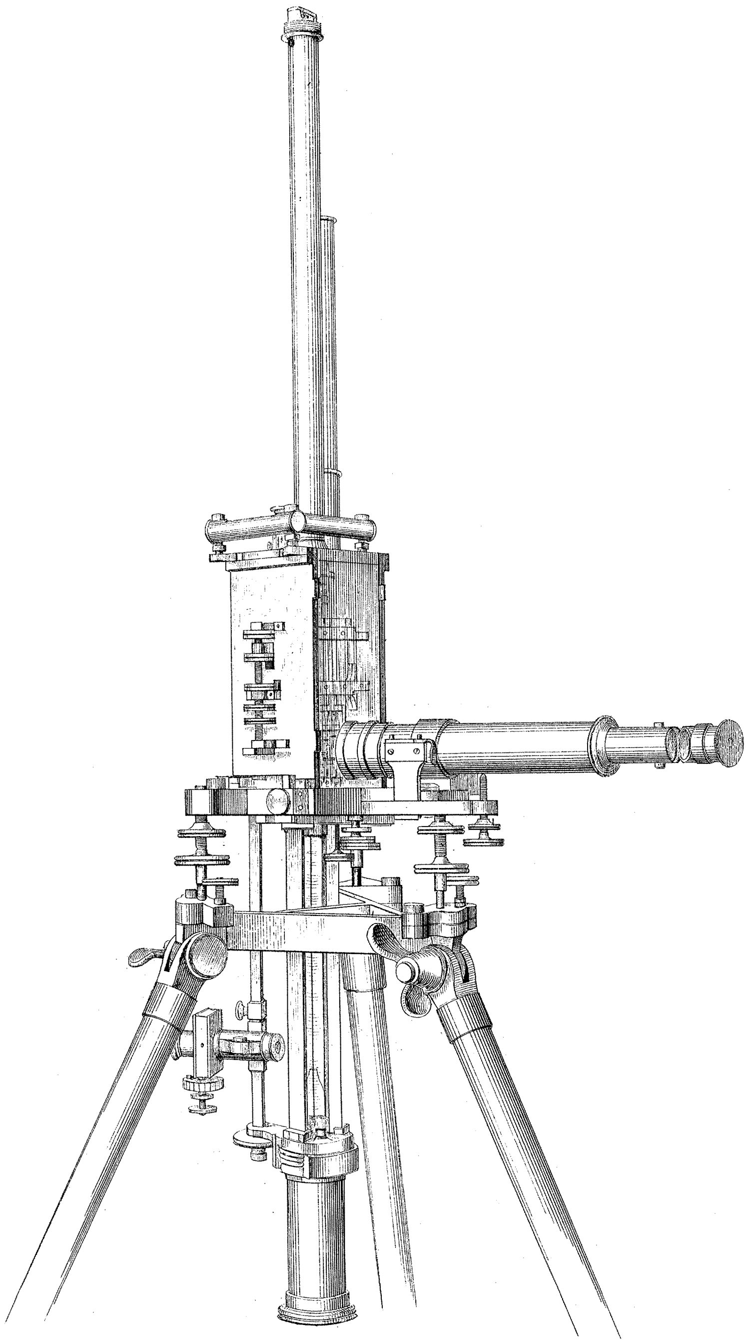
"On the Coefficients of Expansion of the Di-iodide of Lead, PbI_2, and of an Alloy of Iodide of Lead with Iodide of Silver, $PbI_2.AgI$." By G. F. Rodwell, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor A. W. Williamson, For. Sec. R.S. Received March 10. Read March 31, 1881.

In former communications which I have had the honour of submitting to the Royal Society, I have given determinations of the coefficients of expansion by heat of the chloride and bromide of silver and the iodide of mercury between 0° C. and the fusing point; also determinations of the coefficients of expansion and contraction of the iodide of silver, and of certain chlorobromiodides of silver. (" Proc. Roy. Soc.," vol. 25, pp. 280–303, and vol. 28, p. 284.)

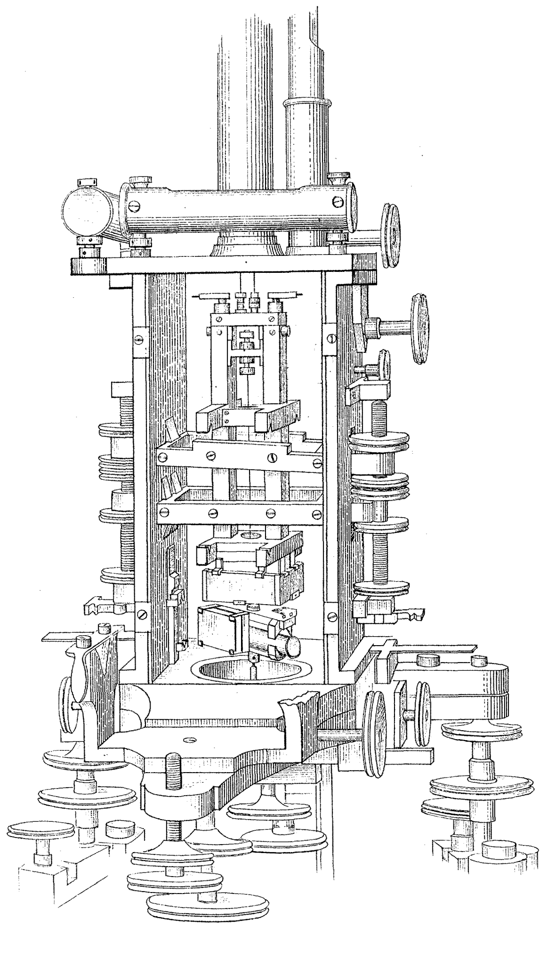
The iodide of lead, and an alloy of iodide of lead with iodide of silver, were thought to be very suitable substances for a continuation of these experiments. The following pages describe the results obtained.

The experimental method was precisely similar to that before described, but the expansion apparatus was rendered more delicate by several notable changes suggested during the course of the former experiments. It is unnecessary to describe this apparatus again (for description *vide* " Proc. Roy. Soc.," vol. 25, p. 281–2), but it may be remembered that a homogeneous rod of the substance under examination is connected with a series of levers which multiply 5,382 times, while the value of the movements is estimated by a micrometer screw reading to $\frac{1}{500000}$ of an inch. The following alterations were made mainly with a view of reducing the resistance by diminishing friction, and thus adding to the sensibility of the apparatus :—

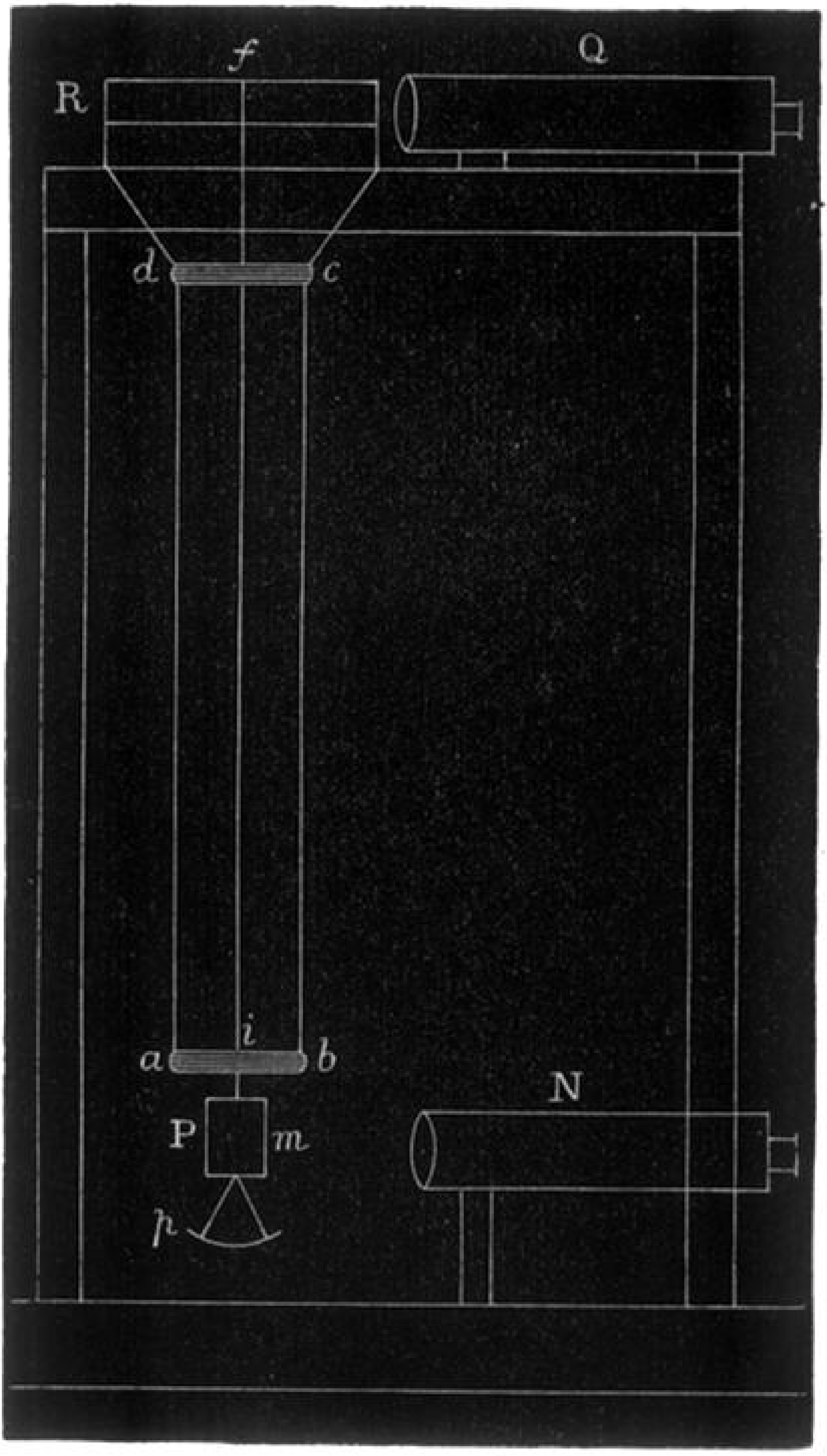
1. The wooden base N (fig. 1) was replaced by a massive stone



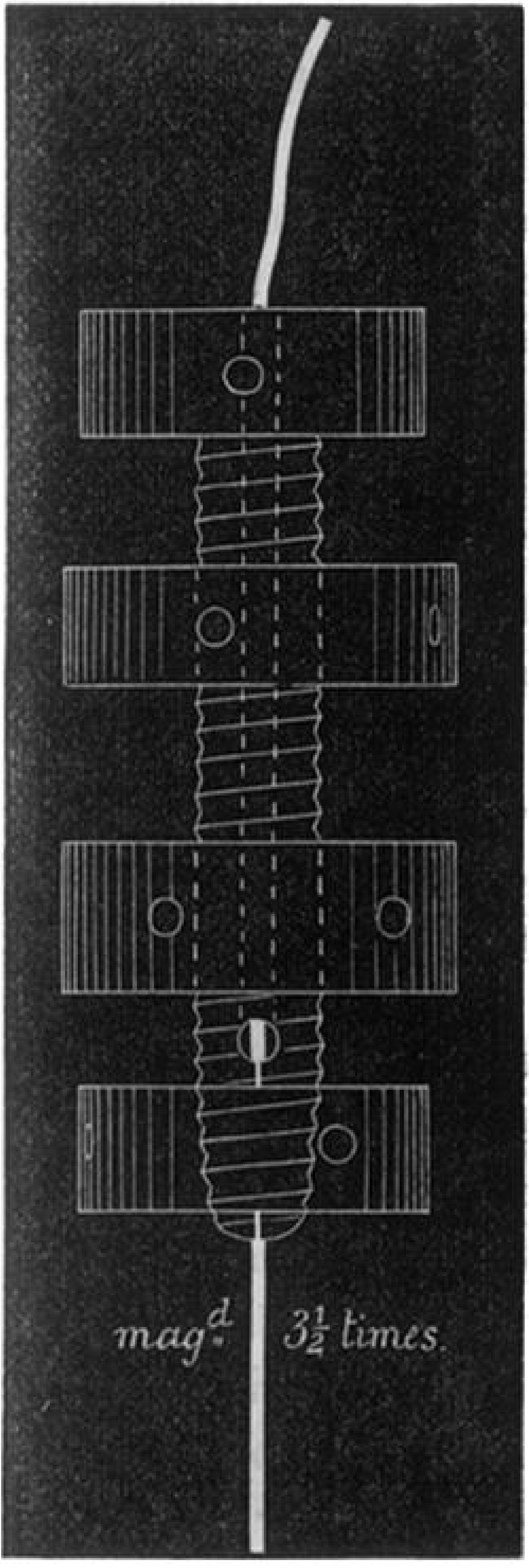
W. H. Wesley del. W. Rhein sc. West Newman & Co. imp.



W. H. Wesley del. W. Rhein sc. West, Newman & Co. imp.



f
Q
R
d
c
i
a
b
N
P
m
p


magd. 3½ times.