XIX. *On the Correction of a Pendulum for the Reduction to a Vacuum: together with Remarks on some anomalies observed in Pendulum experiments.* *By* F. BAILY, *Esq. F.R.S. &c. &c. &c.*

Read May 31, 1832.

THE great importance which has, of late years, been attached to experiments on the pendulum, is evinced not only by the repeated and valuable labours of several of the most distinguished mathematicians and experimentalists of the present age, but also by the numerous scientific voyages that have been undertaken by several of the European Governments, with a view to ascertain and compare the results of different pendulum experiments made in various parts of the globe; and thence to determine the true figure of the earth. These results, or the number of vibrations which are made in a mean solar day, whether made by the same, or by different pendulums, were considered, till within these few years, as strictly comparable with each other by means of certain well known corrections; whereby they were reduced 1° to arcs indefinitely small, 2° to a common standard of temperature, 3° to a vacuum, and lastly to the level of the mean height of the sea.

M. BESSEL, however, has recently proved that the formula for the *reduction to a vacuum* is very defective: and Dr. YOUNG has shown that the formula for the reduction to the level of the sea is, in many cases, too great: whilst Captain SABINE has, in a paper recently published in the Transactions of this Society*, shown that there is reason to suspect the accuracy of the usual formula for the reduction to indefinitely small arcs. This latter gentleman had previously, in another work†, pointed out the discordant results arising from the use of different agate planes with the same knife edge: and had also stated his decided opinion on the powerful effect of certain geological strata in the

* Phil. Trans. for 1831, pages 467—469.

† An Account of Experiments to determine the figure of the earth; 4to, London 1825; pages 190 and 371.

immediate neighbourhood of the pendulum; and has even imagined that the results may be affected by an increase of buildings in the vicinity. But, to whatever cause the observed anomalies may be owing, I must confess that I have myself, during a long course of experiments on various pendulums, at different seasons of the year, and under a variety of circumstances, frequently met with discordancies that have baffled every attempt at explanation by any of the known laws applicable to the subject: and I believe that other persons also, who have had much practice in pendulum experiments, have occasionally met with anomalies for which they have been unable to account satisfactorily. As it is desirable, however, that these difficulties should be cleared up if possible, and as every information connected with so important a subject, founded on such delicate experiments, must add to our means of removing them, I trust I need not apologize for drawing the attention of this Society to the results of some experiments, made with pendulums of various forms and construction, immediately bearing on the discordancies in question.

In fact, till we can construct two pendulums, that will always tell precisely the same tale, cleared of all these discordancies, the important problem of the determination of the length of the seconds pendulum cannot be considered as fully solved: neither can the observations of different experimentalists, in different parts of the globe, with different pendulums, be strictly and directly comparable with each other. It is true that we have two pendulums, in form and construction totally different from each other, whose results have been closely compared: viz. BORDA's pendulum, and KATER's convertible pendulum. But, although the great accordance in those results, by two such different means, evince the talent and skill of the distinguished persons engaged in making the experiments; yet it should now be borne in mind that the reductions to a vacuum were, in both cases, made agreeably to the old formula: and that, since M. BESSEL's important investigations on this subject, which indicate the necessity of revising the computations of all preceding experiments, no rigid comparison of the results has yet been repeated. The amount of the additional correction, for the two respective cases, varies materially, as I shall more fully show in the sequel: so that we are, in fact, at the present moment, totally ignorant whether the results of any two pendulums that have ever yet been constructed, are in strict and reasonable accordance with each other. And until this is practically accomplished, and can be practically repeated, I do not think that the true length of the seconds pendulum can be considered as satisfactorily determined.

Reduction to a vacuum.

M. Bessel has shown, in his very interesting work on the pendulum *, that the usual formula for the reduction to a vacuum, as far as the specific gravity of the *moving body* is concerned, is very defective; and by no means expresses the whole of the correction which ought to be applied: in fact, that a quantity of *air* is also set in motion by, and adheres to, the pendulum (varying according to its form and construction), and thus a *compound pendulum* is in all cases produced, the specific gravity of which will be much less than that of the metal itself. He states (page 32) that "if we denote by m the mass of a "body moving through a fluid, and by m' the mass of the fluid displaced "thereby, the accelerating force acting on the body has, since the time of "Newton, been considered equal to $\frac{m-m'}{m}$. This formula is founded on the "presumption that the moving force, which the body undergoes, and which is "denoted by $m-m'$, is confined to the mass m. But, it must be distributed not "only over the moving body, but on all the particles of the fluid set in motion "by that body; and consequently the denominator of the expression, denoting "the accelerating force, must necessarily be greater than m." M. Bessel then enters into a mathematical investigation of the principles from which the results of his experiments are deduced: and at length comes to the following important conclusion: viz. "that a fluid of very small density, surrounding a "pendulum, has no other influence on the duration of the vibrations than that "it diminishes its gravity and increases the moment of inertia. When the increase in the motion of the fluid is proportional to the arc of vibration of the "pendulum, this increase of the moment of inertia is very nearly constant: in "all other cases it will depend on the magnitude of that arc."

The obvious inference from those experiments and researches is, that the amount of the correction will not only vary according to the *length*, *magnitude*, *weight*, *density* and *figure* of the pendulum; but also that in the case of

* Untersuchungen über die Länge des einfachen Secundenpendels, von F. W. Bessel. Berlin 1828, 4to. This work forms part of the Memoirs of the Royal Academy of Sciences of Berlin for 1826.

the convertible pendulum (except perhaps in that particular instance when it makes the least number of vibrations possible,) the correction will not be the same for the two knife edges: and consequently that a pendulum, which has been made convertible in air, will no longer be so when tried in a vacuum. It becomes therefore of importance to know how far the differently constructed pendulums, made use of by various experimentalists, are affected by this newly discovered principle, in order that their results may be strictly comparable with each other. The amount of the required correction, however, cannot (according to our present knowledge of the subject,) be determined by calculation, but must, in every case, be ascertained by actual experiment. The most direct method of effecting this appears to be, as M. Bessel states (page 37), by swinging the pendulum in a vacuum: although he himself, on account of some doubts which he entertained of this method, but which he has not explained, adopted another and a very different plan.

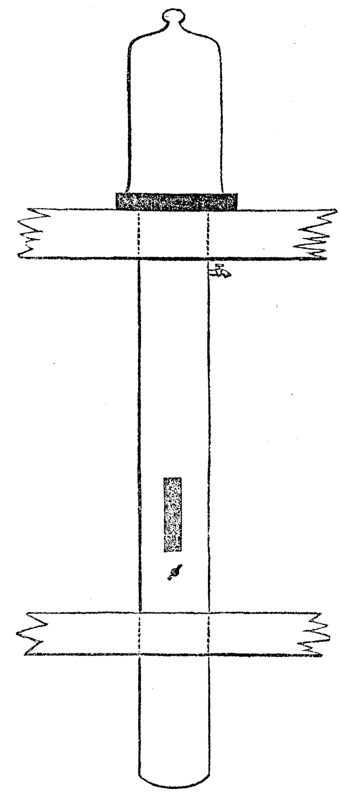
The mode adopted by M. Bessel was of two kinds. The first and principal one was by swinging in air two spheres of equal diameter (about 2·14 inches) but of very different specific gravity, viz. brass and ivory, suspended by a fine steel wire: the other, which was not commenced till the subsequent year, was by swinging the same sphere (brass) first in air and afterwards in water. The result of the experiments, by both these methods, showed that the usual correction for the reduction to a vacuum was much too small; and that the true correction was nearly double what has been generally assumed. The first method gave 1·946*, and the second 1·625, as the *factor* by which the old correction must be multiplied in order to obtain the true correction. These values differ materially from each other; but M. Bessel prefers the former, as his investigations are founded on the theory that the vibrations are made in a medium of very small density†.

Being desirous of ascertaining, by a different process, the true value of the correction for the numerous and various pendulums in my possession, as accurately as experiments of this kind will decide the fact, and considering the

* In a paper, subsequently inserted in the Astronomische Nachrichten, No. 223, M. Bessel has increased this value to 1·956.

† M. Bessel also swung a hollow brass cylinder alternately in air and in water, and has deduced some results which appear to be astounding: but, I shall show in the sequel that the discordancy in the results stated by him, will be removed by the assumption of a different specific gravity of the moving body, from that which he has adopted.

subject to be otherwise of importance in a scientific point of view, I resolved to devote some time to its examination: and, for this purpose, caused a vacuum apparatus to be fitted up at my own house, where I could pursue the subject at leisure. This vacuum apparatus is very different, in its form and construction, from that which is erected at the Royal Observatory at Greenwich, and described by Captain SABINE in the Philosophical Transactions for 1829, page 207. It consists of a brass cylindrical tube about five feet long and six inches and a half in diameter, rounded at the bottom, and soldered at the top to a thick iron frame, on which the agate planes rest. This frame is firmly screwed and fastened to solid mahogany beams which are securely wedged between two fourteen-inch walls in the corner of a room, which is remarkable for preserving an uniformity of temperature, during the day, throughout the different seasons of the year. The upper surface of this iron frame is ground perfectly plane, and is surmounted with a moveable glass top, in the manner described by Captain SABINE. The brass tube has two small openings, or windows (cut on opposite sides) at a proper distance from the top, which are covered with plate glass, for the purpose of observing the arc of vibration and the coincidences with the clock, which is placed behind. The lower part of the tube is secured also by cross beams, in order to prevent any lateral motion in the tube itself, during the vibrations of the pendulum. As the whole of the experiments about to be related, are *comparative* only, it will be unnecessary to enter more minutely into a description of this apparatus; the form and construction of which will be best understood from the annexed sketch. The flexible metallic pipe, communicating with an air pump, enters the tube immediately under the upper beams: and a brass wire, passing through a stuffing-box, for the purpose of setting off the pendulum at any given arc, enters the tube just below the glass window.



I ought however here to mention that the agate planes are not, as in Captain SABINE's experiments, *screwed* to the iron frame, but are attached to another solid frame (of brass) three quarters of an inch thick, and having three foot-screws, for the purpose of levelling the planes. These screws merely *rest* on the iron frame: one of them in a conical hole, another in a groove, and the third on the flat surface of the iron frame; by which means, the same position is always preserved, without any strain on the screws. I believe that this method (which was suggested by Mr. TROUGHTON,) is as secure as where the agate planes are screwed to the frame: and the application of Mr. HARDY's inverted pendulum does not detect the least motion. But this is a question that need not here be discussed; since, as I have just stated, the experiments, about to be adduced, are only comparative. The weight of this brass frame is upwards of seventeen pounds and a quarter troy.

The usual correction of the number of vibrations for the reduction to a vacuum has hitherto been deduced from the relative weights of the air and of the pendulum, by means of the following formula*: viz.

$$+\mathrm{N} \times \frac{1}{2\left(\frac{\mathrm{S}}{\sigma}-1\right)} \times \frac{\beta'}{\beta\,[1+\mu\,(\tau'-t)]} \times \frac{1}{1+\alpha\,(t'-t)}$$

where N denotes the number of vibrations made by the pendulum in a mean solar day, S the specific gravity of the pendulum, σ the specific gravity of air, μ the expansion of mercury, and α the expansion of air for one degree of the thermometer, β' the height of the barometer, τ' the temperature of the mercury, t' the temperature of the air during the experiments, β the height of the barometer, and t the temperature of the air, assumed as standards for the specific gravity.

If we suppose that the temperature of the mercury in the barometer is the same as that of the air surrounding the pendulum, which may generally be assumed as the case, in experiments of this kind, without the risk of any perceptible error, the above formula may be rendered more convenient as follows: viz.

$$+\mathrm{N} \times \frac{1}{2\left(\frac{\mathrm{S}}{\sigma}-1\right)} \times \frac{\beta'}{\beta} \times \frac{1}{1+(\alpha+\mu)\,(t'-t)} \qquad (1)$$

* See M. MATHIEU's paper on this subject, in the Con. des Tems for 1826, page 288.

But, here it will be proper to remark that S does not denote the specific gravity of the pendulum, as determined in the usual manner when *at rest*, unless the mass be homogeneous: for, in all other cases, where the pendulum consists of several parts, whose specific gravities are different, we must compute the *vibrating* specific gravity of the mass in the following manner. Let d', d'', d''', &c. denote the distance of the centre of gravity of each body respectively from the axis of suspension*: w', w'', w''', &c. the weight (in air) of each body: s', s'', s''', &c. the specific gravity of each body, determined in the usual manner. Then will the required *vibrating* specific gravity of the pendulum be†

$$S = \frac{w' d' + w'' d'' + w''' d''' + \&c.}{\frac{w' d'}{s'} + \frac{w'' d''}{s''} + \frac{w''' d'''}{s'''} + \&c.} \qquad (2)$$

And, it is in this manner that I have deduced what may be called the *vibrating specific gravity*, for all those pendulums, which, in the following experiments, consist of substances of different specific gravities.

With respect to the other quantities involved in the above formula (1) there are two modes which have been pursued for expressing them numerically: viz. one by assuming Sir George Shuckburgh's determination of the relative weights of air and water, as stated in the Philosophical Transactions for 1777; that is, $\sigma = \frac{1}{836}$, $\beta = 29{\cdot}27$, and $t = 53°$: and the other by assuming the more recent determination of MM. Arago and Biot; that is, $\sigma = \frac{1}{770}$, $\beta = 29{\cdot}9218$, and $t = 32°$. The former has been adopted I believe by most English experimentalists; but, as I conceive the latter to be the more accurate determination, I shall adopt it in all the present reductions. They differ from each other about $\frac{1}{93}$th part of the whole correction: the French result being the greatest in amount.

The expansion of mercury is generally assumed equal to ·0001 for each degree of Fahrenheit's thermometer: but the expansion of air is not quite so

* When the body is *below* the axis, d is *plus:* when *above*, it is *minus*.

† I am indebted to Professor Airy for this formula: which, although of considerable importance in all investigations relative to the pendulum, has not, as far as I am aware, been alluded to by any writer on the subject, except Bessel.

well agreed upon. It has generally been taken at $\frac{1}{480}$th of its bulk, or ·002083 for each degree of FAHRENHEIT: but this value applies more particularly to air rendered perfectly *dry* for the purpose of the experiments from which such value has been deduced. The expansion of common atmospheric air, impregnated, as it generally is, with a certain degree of *moisture*, is supposed by M. LAPLACE to be $\frac{1}{450}$th of its bulk, or ·002222 for each degree*. I have assumed this latter value, and consequently make $(\alpha + \mu) = \cdot 0023$. Whence the numerical expression for the formula in question will be

$$+\mathrm{N} \times \frac{1}{2(\mathrm{S} \times 770 - 1)} \times \frac{\beta}{29 \cdot 9218} \times \frac{1}{1 + \cdot 0023\,(t' - 32^\circ)} \qquad (3)$$

If we make $\beta = 1$, and $t' = 32^\circ$, we might readily obtain for each pendulum, a *constant quantity*

$$\mathrm{C} = \mathrm{N} \times \frac{\cdot 0000217016}{\mathrm{S} - \cdot 001299} \qquad (4)$$

for one inch pressure of the atmosphere, at the freezing point of water; whence the value of the correction at any other pressure β, and at any other temperature t, would be denoted by

$$\mathrm{C} \times \frac{\beta}{1 + \cdot 0023\,(t - 32^\circ)} \qquad (5)$$

This is the old correction, which is so far erroneous that no account is taken of the effect of the air set in motion by, and accompanying the pendulum, as if *adhering* thereto; and which is now found to influence the result very materially. This formula, however, will still be of considerable service to us, since not only M. BESSEL's experiments, but also those about to be detailed in this paper, have for their object the determination of the *factor*, by which the quantity C must be multiplied (according to the form and construction of the pendulum,) in order to produce the *true* correction: this being one of the most convenient forms of showing the relative value and amount of this new influence. I have already stated that the mode proposed to be pursued in the following experiments, for the determination of this *factor*, was to swing the pendulum under the full pressure of the atmosphere and also in a highly rare-

* Système du Monde, 5th edition, 1824, page 89. See also BIOT's Traité d'Astronomie Physique, vol. iii. (Mésures Barométriques, page 14.)

fied medium, nearly approaching to a vacuum. Let N′ denote the number of vibrations made by a pendulum in a mean solar day (corrected in the usual manner for the rate of the clock, the arc of vibration and the temperature of the room, *but not for the height of the barometer*), β' the height of the barometer, and t' the height of the thermometer when the pendulum is swung under the full pressure of the atmosphere: and let N″, β'', t'' denote respectively the same quantities, when it is swung in a highly rarefied medium. Then will $\frac{N'' - N'}{\beta' - \beta''}$ express the true correction for one inch pressure of the atmosphere at the temperature t°; where $t^\circ = \frac{1}{2}(t' + t'')$: which, being multiplied by $1 + \cdot 0023\,(t^\circ - 32^\circ)$, will give the *true constant*

$$C' = \frac{N'' - N'}{\beta' - \beta''} \times \left[1 + \cdot 0023\,(t^\circ - 32^\circ)\right] \qquad (6)$$

for the same pressure, and at the freezing point: whence we obtain the following expression for the *true* correction, at any pressure β, and at any temperature t: viz.

$$C' \times \frac{\beta}{1 + \cdot 0023\,(t - 32^\circ)} \qquad (7)$$

agreeably to which formula I have deduced the value of C′ in the experiments about to be detailed.

Now, the value of C′ is always greater than C: and, in order to determine the *factor* by which C must be multiplied in order to produce the true correction, (which factor will vary according to the form and construction of the pendulum,) we must make $C' = n\,C$: whence we obtain, for the factor required,

$$n = \frac{C'}{C} \qquad (8)$$

and it is in this manner that the value of the factor (n) has been deduced in the following experiments. And it may be proper to state that the quantity which is here denoted by n, M. Bessel expresses by $(1 + k)$.

Description of the Pendulums.

The number of pendulums, for which I have deduced the comparative results, by swinging them in a vacuum apparatus, amounts to forty-one *; varying from each other in figure, dimension, weight, specific gravity, length, mode

* This number has been more than *doubled* by the experiments hereafter alluded to, made subsequent to the reading of this paper.

of suspension, or some other influential property: and comprise almost every variety of pendulum that is ever likely to be made the subject of experiment. In order to prevent confusion in occasionally referring to them, I shall here arrange them in numerical order, and class them according to their form.

No. 1, 2, 3, 4, are spheres of platina, lead, brass and ivory; all of the same diameter; which is somewhat less than $1\frac{1}{2}$ inch. The platina sphere (No. 1.), which has been kindly lent to me, for the occasion, by the Astronomer Royal, is of French manufacture, and about 1·44 inch in diameter; which is the same size as that used by M. BIOT in his pendulum experiments, and in fact appears to have been formed from the same model*. It is furnished with a copper *calotte*, and also with a knife edge, attached to a frame, capable of being brought to a state of synchronism with the pendulum with which it is used, by means of a screw, in the manner described by M. BORDA in the Base du Système Métrique, vol. iii. page 338. Its specific gravity I found to be 21·042; and it weighed 8963 grains. The copper calotte weighed 87 grains, and was firmly attached to the platina sphere by means of shell-lac; as the ordinary mode, by greasing the parts, failed in the present experiments. I unfortunately attempted the usual method, in the first instance, not recollecting that the adhesion is caused principally by the pressure of the atmosphere; and that when that pressure is removed the sphere would no longer be supported. This proved to be the case; and the platina sphere, in its fall, received a slight cut against the sides of the vacuum apparatus; but not of sufficient importance to impair its accuracy in any future experiments. It certainly cannot affect the present ones, which are merely comparative. The *vibrating* specific gravity of the mass, including the iron wire to which I shall presently allude, was computed to be 20·745. The leaden sphere (No. 2), the brass sphere (No. 3), and the ivory sphere (No. 4), were ordered to be made of the same size as the platina one: but they are somewhat larger, being 1·46 inch. They have no calotte, but were tapped for the purpose of inserting a brass screw, perforated with a small hole for the insertion of the wire by which they were suspended. The screw weighed $19\frac{1}{2}$ grains: and the same screw has served for all the experiments, where it was required, except for the platina sphere. The wire employed in these and all the subsequent cases, unless otherwise expressed, was of iron about $\frac{1}{70}$th of an inch in diameter; and

* Base du Système Métrique, vol. iv. page 449.

weighed about 11 grains *: its specific gravity I found to be 7·666. I was unwilling to use a finer wire (except with the ivory sphere), for fear of accidents, the issue of which could not be easily remedied in the vacuum apparatus. In each of these experiments the wire was attached, at its upper end, to the shank (1·55 inch long) of the knife edge, on which the vibrations were made; in the manner described by MM. Borda and Biot: and the adjustment of this knife edge apparatus to a state of synchronism with the pendulum was always attended to. The specific gravity of the leaden sphere including the brass screw I found to be 11·250; and they weighed 4648 grains: of the brass sphere and screw, 7·660; and they weighed 3217 grains: and of the ivory sphere and brass screw, 1·864; and they weighed $776\frac{1}{2}$ grains †; but in all the cases where the pendulum has consisted of more than one metal (or even of two pieces of the same kind of metal, but of two different specific gravities,) the *vibrating* specific gravity of the mass has been deduced from the formula (2). The wire, by which the ivory sphere was suspended, was the finest silver wire that would sustain it with safety; and weighed little more than half a grain. As these three spheres are not of precisely the same diameter, I shall designate them as the $1\frac{1}{2}$ inch spheres.

No. 5, 6, 7 are spheres of lead (No. 5), brass (No. 6), and ivory (No. 7), all ordered to be made of the same diameter, viz. 2·06 inches; which was intended to be, and is nearly, the same size as the spheres used by M. Bessel ‡. These spheres were tapped in the manner already described, for the purpose of inserting the screw above mentioned: and the same knife edge and iron wire, as those above described, were used in all the experiments. The specific gravity of the leaden sphere and the brass screw I found to be 11·281; and they

* As a new piece of wire was occasionally found necessary, I have given what I consider the average weight.

† In obtaining the specific gravities of the different substances, alluded to in this paper, I would observe here, once for all, that I used (not distilled water, but) river water that had been filtered and boiled. The values deduced are the results of two, and sometimes three, different weighings on different days; and are sufficiently accurate for the *comparisons* intended. They are all reduced to the freezing point of water, and to 29·9218 inches of barometric pressure.

‡ This is the exact size of the engraving of the sphere in M. Bessel's work; where it is stated to be the true size: but on subsequently examining the detail of the experiments I found that the correct size is 2·14 inches. The plate had probably shrunk in its dimensions, since it had been printed.

weighed 13019 grains: the specific gravity of the brass sphere and the screw I found to be 7·995; and they weighed 9302 grains: and the specific gravity of the ivory sphere and the brass screw I found to be 1·747; and they weighed 2066½ grains. I shall designate these three spheres, as the 2-inch spheres.

No. 8, 9 are the same leaden and ivory spheres as No. 5 and 7: but the vibrations, instead of being made on the knife-edge above mentioned, were made by causing the wire to pass over a steel cylinder about one fifteenth of an inch in diameter, in a manner somewhat similar to that adopted by M. BESSEL in some of his experiments. The wire used with the leaden sphere was the same iron wire as in the former experiments: but that used with the ivory sphere was fine silver wire, rather thicker than that used with No. 4, and weighed 2 grains. The experiments made with these spheres, and with this mode of suspension, are not (I fear) entitled to much credit; for reasons which I will presently explain.

No. 10 is a solid brass cylinder 2·06 inches in diameter and 2·06 inches high; in order to correspond in dimensions with the brass sphere. It was tapped with a screw-hole on its flat side; and was supported by the same iron wire and knife edge as above described. Its specific gravity, with the screw, I found to be 8·174; and they weighed 14190 grains.

No. 11 is the same solid brass cylinder, tapped with a screw-hole on its circular side: but, as it was liable to turn on its axis when suspended by the iron wire, I was obliged to support it with a rod, or piece of thick brass wire, 0·185 inch in diameter, and 37½ long; the upper end of which was attached to the knife edge above mentioned, on which the whole vibrated. The rod weighed 2050 grains, and its specific gravity was somewhat greater than that of the cylinder. The computed specific gravity of the whole was 8·202. This pendulum was swung with its cylindrical side opposed to the line of its motion.

No. 12 is the same solid brass cylinder, supported by the same brass rod, and in the same manner as in the preceding case, except that it was now swung with its flat side opposed to the line of its motion.

No. 13 is the same solid brass cylinder, supported by the same brass rod screwed into its flat side (as in No. 10): an experiment made for the purpose of determining the difference in the results, when suspended by the brass rod, and by the iron wire. See the preceding figure, which exhibits this pendulum.

No. 14 is a cylinder of lead, 2·06 inches in diameter, and 4 inches long; tapped with the screw-hole on its flat side, and supported by the same iron wire and knife edge as above mentioned. It should here be remarked, however, that this cylinder was not wholly of lead; since it was formed of a thin brass tube filled with lead: and this tube was made to slide into an outer cylinder of brass, having the dimensions above described, as will be more fully explained in the next article. The specific gravity of the whole I found to be 10·237; and it weighed 34500 grains.

No. 15, 16, 17, 18 are cylindrical tubes of brass, 2·06 inches in diameter on the outside, 4 inches long, and 0·13 inch thick. These, however, are not different tubes, but consist of one and the same cylindrical outer piece; and is in fact the tube into which the leaden cylinder is made to slide, as mentioned in the preceding article. This cylindrical outer piece is capable of being varied in the four following ways, by means of an inner sliding tube. No. 15 is when both the ends are open, with the exception of a narrow cross piece at the top, to which the screw is attached. No. 16 is when the top is still left open, but the bottom closed. No. 17 is when the top is closed, and the bottom left open. And No. 18 is when both ends are closed. In all the cases, the tube was suspended by the same kind of iron wire as that already described; and from the same knife edge. The specific gravity of the metal I found to be 8·453: but here it may be proper to remark (what I shall again advert to, in the sequel,) that when a *hollow* body is swung as a pendulum, we must take into account the quantity of air contained within the moving body (which, in the present case, is computed to be 3050 grains,) and diminish the specific gravity of the metal accordingly*. Proceeding on this principle, I

* Cases of this kind appear to admit of two distinctions: one, where the hollow body is hermetically sealed; the other, where the included air communicates freely with the surrounding atmosphere, and consequently escapes under the action of the air-pump. But, in the case of a cylindrical tube (like that in question) there will be no difference in the result: as, from the similarity of distribution of the masses of metal and of air (at least, in the case of the tube, open at both ends; and ap-

have calculated the specific gravities of each of these hollow pendulums as follows; to which also I have annexed the weights.

No.	Spec. grav.	Grains.
15 =	2·536	8497
16 =	2·623	8922
17 =	2·561	8622
18 =	2·649	9048

After the experiments with these tubes were completed, I caused the inner sliding tube to be filled with lead, as mentioned in the preceding article: and this solid cylinder could be readily put into and taken out of the outer tube, at pleasure. And when the experiments with this solid cylinder were completed, a new top piece was made to the outer tube, which was closely soldered on: a new bottom piece was also made, to screw on and off, which, by the application of an oiled leather to the screw, might at any time be rendered hermetically sealed.

No. 19 is the tube thus hermetically sealed. The inner sliding tube having been taken away, the weight was reduced to 7250 grains: the specific gravity I found to be 2·233*. The hollow portion of the cylinder now contains 3·255 grains of air.

No. 20 is a lens of lead, 2·06 inches in diameter; 1 inch thick in the middle, and having a flat circumference about a quarter of an inch wide. This lens was tapped with a screw-hole on one of its protuberant sides, and was supported by the same iron wire and knife edge as above described: the position of the lens was consequently horizontal. Its specific gravity with the screw I found to be 11·254; and they weighed 6505 grains.

proximately so, in the other cases,) the centre of oscillation of the included air will coincide with that of the metal; and the centre of oscillation of the compound mass will therefore coincide with that of the metal alone.

* When the bottom piece of this tube was *loosely* screwed (so as to admit the free passage of the air under the exhausted receiver,) it might be considered as a pendulum similar to No. 18, with the specific gravity of 2·233: and when the bottom piece was wholly taken away, it might be considered as a pendulum similar to No. 17. Experiments were made with the tube under these circumstances, to which I shall allude in the sequel, fully confirming the results of the former ones. In the latter case, when the bottom piece was taken away, the weight was reduced to 6744 grains; and the specific gravity was computed to be 2·042. The solid sliding cylinder is also adapted to this new state of the tube; but at present I have not made use of it, in this way.

No. 21 is a solid copper cylindrical rod, 0·41 inch in diameter, and 58·8 inches long. This pendulum was invented by Mr. TROUGHTON, and was made by him about 16 years ago, when the Commissioners were appointed by Government for determining the length of the seconds pendulum. It would take up some time to describe the mode in which this pendulum was originally intended to be mounted and swung; and would be irrelevant to the present subject: but, as great part of the apparatus could be dispensed with, on the present occasion (the results being comparative only), I shall merely state that I at first attempted to swing it by suspending it, at one end, with a piece of steel wire, drawn close up to the cylinder mentioned in No. 8 and 9. But I found the discordancies (to which I shall afterwards have occasion to allude,) so enormous, that I was obliged to abandon this mode: and I ultimately fastened it, by means of an adjusting screw, to the knife edge used in the preceding experiments. As I had no means of determining the specific gravity of the rod, I have assumed it as equal to that of the copper bar No. 27; viz. 8·629: its weight is 16810 grains.

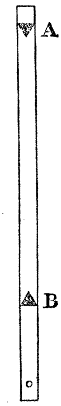
No. 22 is KATER's invariable brass pendulum. Several pendulums of this kind have been made for our own, and for other Governments, and for public bodies; and all from the same model, which is that described by Captain KATER in the Philosophical Transactions for 1819, page 341. I have two now in my possession (numbered 10 and 11,) belonging to the Admiralty; and are those that were taken out by the late lamented Captain FOSTER, in his voyage of experiment. They are formed of a bar of brass 1·8 inch wide, and rather less than $\frac{1}{10}$th of an inch thick. At the top of this bar is a knee piece, also of brass about three tenths of an inch thick, to which a steel knife edge is firmly screwed: and, at about $40\frac{1}{2}$ inches from this knife edge, is fastened a flat circular bob of solid brass, about 6 inches in diameter, and $1\frac{1}{4}$ inch thick, but tapered at the edge. Below this bob the bar is reduced to about $\frac{7}{10}$ths of an inch in width, and is continued about $16\frac{1}{2}$ inches: thus forming what is called a tail piece,—a most unnecessary and inconvenient appendage; since the arc of vibration, which this tail piece was intended to indicate, can be as readily observed by means of the *edge* of the bar above the bob. As my vacuum apparatus was not sufficiently large to receive the bob of this pendulum, I shall deduce the results from the experiments made by Captain

SABINE on two similar pendulums, with the vacuum apparatus at Greenwich; as described by him in the Philosophical Transactions for 1829, page 235. The specific gravity of this pendulum I have assumed equal to 8·4. Captain KATER states that the specific gravity of the first pendulum which he made of this kind was 8·61 (See Philosophical Transactions for 1819, page 354): but this is greater than that of any brass that I have yet found, and greater I believe than what is usually met with; it is even considerably more than the specific gravity of his convertible pendulum, mentioned in the following article, which was formed of nearly similar materials, and which was only 8·248. Captain SABINE, relying on this single experiment of Captain KATER's, has assumed 8·6 as the proper specific gravity for a pendulum of this kind: and as the results therefore which I have deduced from his experiments will not exactly agree with those that he has given, it was necessary here to state the principal cause of the discordancy. I estimate the weight of this pendulum at 90500 grains, from the mean of the weights of two similar pendulums in my possession.

All the pendulums above described can be swung only in one position. I now come to those which are furnished with two or more knife edges; and which are of the kind called *convertible* pendulums. The knife edges of these pendulums (at least, all those hitherto constructed,) are placed at unequal distances from the centre of gravity; and consequently the same pendulum, when swung with that knife edge placed uppermost which is furthest from the centre of gravity, will set in motion a different quantity of air, and, as far as the subject of the present inquiry is concerned, produce a different result, from that which would be produced when the pendulum is swung from the other knife edge. I shall therefore consider these knife edges, which I shall designate respectively A and B, as two separate and independent pendulums: the term A being applied to that knife edge which is the most distant from the centre of gravity, and the term B to the knife edge at the other end of the pendulum.

No. 23, 24 are the two knife edges A and B, of KATER's convertible pendulum, described by him in the Philosophical Transactions for 1818, page 37: the first of these letters designates the pendulum when the great weight is below; and the other, when the great weight is above. This pendulum, having been successively altered by Captain SABINE, furnishes us with four separate and independent results, according to its form when it was swung: 1°. with the wooden tail pieces 17 inches long with which it was originally furnished: 2°. with those wooden tail pieces reduced to the length of 6·4 inches: 3°. with brass tail pieces 7 inches long, instead of the wooden ones: and 4°. without any tail pieces whatever, and moreover deprived of the small sliding weight. In this last case, it was reduced to nearly the same figure and dimensions as the invariable pendulum (No. 22) just described, but without its tail piece. As my vacuum apparatus was not sufficiently large (as already mentioned,) for a pendulum of this kind, I have deduced the results from the experiments made by Captain SABINE with the same pendulum, in the several states above alluded to, as detailed by him in the Philosophical Transactions for 1829, page 331, &c., and for 1831, page 459, &c. With respect to the specific gravities, I must take that of the first case, which was the original construction of the pendulum, as equal to 7·373; which is the value stated by Captain KATER in the Philosophical Transactions for 1819, page 415. But this is the specific gravity of the body when *at rest*, deduced in the usual manner, and not the *vibrating* specific gravity of the mass deduced from formula (2) above given: and as the weights and distances of the several parts from the axis of vibration are not stated, and are now completely destroyed by the alterations in the pendulum, I have no means of ascertaining how far the results might be affected by this view of the subject. As to the second case, where the wooden tail pieces were reduced to 6·4 inches, I have computed the specific gravity (on the assumption that 7·373 in the former case was correct,) as equal to 7·909. With respect to the remaining two cases, as the pendulum here consists wholly of brass, I have computed the specific gravity from the data given by Captain KATER in the Philosophical Transactions for 1818, page 63, and make it equal to 8·248. Captain KATER's result is 8·469; but I apprehend there must be some error in his computation. The weight of the pendulum is somewhere about 66900 grains: but there appears to be some confusion in the weighings. In the Philosophical Transactions for 1818, page 63, the brass parts alone are stated to weigh 9·57 pounds; which, on the presumption that these are avoirdupois pounds, will be equal to 66990 grains troy. But, in the Philosophical Transactions for 1819, page 415, the weight of the whole pendulum, including the wooden tail pieces (which would probably weigh 500 or 600 grains), is stated to be only 66904 grains.

No. 25, 26 are the two knife edges A and B of a convertible pendulum, formed of a plain brass bar, 2 inches wide, $\frac{3}{8}$ths of an inch thick, and 62·2 inches long. The form and construction of this pendulum will be best seen from the annexed sketch, which is taken from the description given of the two following ones in the Philosophical Magazine for August 1828, page 137. At 5 inches from one end of the bar is placed one of the knife edges (A), fastened to knee pieces in the usual manner; and at 39·4 inches therefrom is placed the other knife edge (B). The adjustment to synchronism is coarsely effected by filing away from the requisite end: and ultimately to great exactness by means of a small screw inserted at the end B, reduced to a weight proper for such purpose. Its specific gravity, as obtained from a piece of brass, said to be from the same casting, I found to be 8·034: its weight is 121406 grains.

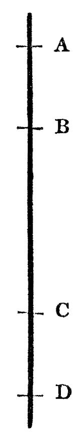


No. 27, 28 are the two knife edges A and B of a copper bar, similar to the last, except that it is $\frac{1}{2}$ an inch thick, and 62·5 inches long. Its specific gravity, as obtained in the manner described in the preceding pendulum, I found to be 8·629: its weight is 155750 grains.

No. 29, 30 are the two knife edges A and B of an iron bar, similar to the copper one, except that it is 62·1 inches long. Its specific gravity, as obtained in the manner described above, I found to be 7·686: its weight is 140547 grains.

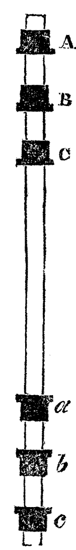
These two last-mentioned pendulums have been already described by me in the Philosophical Magazine as above stated. They belong to the Royal Astronomical Society, and are the same that were taken out by Captain FOSTER in his late scientific voyage.

No. 31, 32, 33, 34 are the four knife edges A, B, C, D, of a brass bar similar to the three last-mentioned ones, except that it is $\frac{3}{4}$ths of an inch thick, and 62 inches long. The position of the knife edges will be best understood from the annexed diagram, which is taken from the Philosophical Magazine for February 1829, page 97, where this pendulum is more fully described. It may be sufficient here to



state that the knife edges A and C are rendered synchronous, or nearly so; and that B and D are also rendered synchronous, or nearly so. It follows therefore that each pair (when properly reduced,) should give the same result for the length of the simple pendulum. But the discordancies which they exhibit have been already described by me in the work just quoted, and have given rise to three separate papers on the subject by Captain Everest, Mr. Gompertz and Mr. Lubbock *. The specific gravity of the pendulum, deduced from a piece of metal said to be from the same casting, I found to be 8·060: its weight is 231437 grains.

No. 35, 36, 37, 38 are four of the knife edges, or rather *planes*, A, C, *a*, *c*, of a brass cylindrical tube, or rather tubes, for it is formed of 7 different tubes drawn closely one within the other; so that their joint thickness, which is very firm and compact, and appears as one solid body, is about 0·13 inch. The diameter is $1\frac{1}{2}$ inch on the outside, and it is 56 inches long: the ends are not closed. The specific gravity of the metal I found, by weighing a piece of the tube itself, to be equal to 8·406: but as the included air must be taken into account, the diminished specific gravity of the moving body (deduced agreeably to what is stated in page 411,) will be 3·034. Its weight is 81047 grains. This pendulum is of a totally different construction from any hitherto made: for instead of being fitted up with steel knife edges that vibrate on agate planes, it is furnished with steel planes that vibrate on a pair of agate knife edges which is common to all the planes. The mode of suspension therefore is, in this case, reversed. The pendulum has six planes: but, as two of them (B and *b*) have not yet been used, I shall confine my remarks to the four above enumerated. At the distance of 4 inches from each end of the tube is placed one of the planes, fastened to a brass collar, firmly fixed to the tube. At 12 inches distance from each of these, towards the centre, is placed another plane; thus forming four in the whole.



* In this last-mentioned paper, which is inserted in the Phil. Trans. for 1830, page 201, Mr. Lubbock has shown the effect on the number of vibrations of a given pendulum, corresponding to given deviations in the position of the knife edges. And the result is, that no error of any considerable (or even appretiable) magnitude can arise from such causes, when the artist uses even the most ordinary precaution in fixing the knife edges in their proper position. The discordancies, I believe, arise from irregularities in the knife edge or planes; as I shall more particularly allude to, in the sequel.

The two planes (B and *b*) not here enumerated, lie between the other pairs; as will be best seen in the preceding figure. The vibrations on the planes A and *a*, are rendered synchronous, or nearly so; and also on the planes C and *c*. The length between each synchronous pair of planes is, as nearly as possible, equal to the standard yard.

This completes the list of pendulums hitherto proposed or adopted for the purpose of any physical inquiry, and it embraces almost every variety that has been suggested. I took advantage however of the favourable opportunity that was presented for trying the effect of the pressure of the atmosphere on a few *clock* pendulums. In these cases the pendulum was suspended by a spring, in the same manner as when it is attached to the clock. I shall not stop to inquire whether the arcs, on these occasions, diminished in a geometric ratio; because as the experiments were carried on nearly under the same circumstances in each case, the comparative results will be but little affected by such a consideration.

No. 39 is a mercurial pendulum, such as is now generally attached to astronomical clocks. The pendulum actually employed by me on this occasion, was one that Mr. HARDY was about to attach to an excellent clock which he had just made for HIS ROYAL HIGHNESS the President of this Society; and is the first that has ever been submitted to so rigid a test. It is constructed in the usual manner, and similar to one described by me on a former occasion *, except that the rod and sides of the stirrup are half an inch wide, which I consider an improvement. The whole is rivetted together in a very firm manner, and finished in a very superior style. The height of the mercury in the glass cylinder, when I swung it, was 6·8 inches. The vibrations were made, as I have already observed, on its own spring, and not on a knife edge. The weight of the mercury was 82960 grains, the weight of the glass cylinder was 6463 grains, and the weight of the steel parts was 13565 grains. The specific gravity of the glass I found to be 3·300; and I have assumed that of mercury to be 13·586, and of steel to be 7·800: the *vibrating* specific gravity, therefore, of the mass, deduced agreeably to the formula (2), I find to be 10·591.

No. 40 is another clock pendulum formed of a cylindrical rod of deal, about $\frac{3}{8}$ths of an inch in diameter, passing (at its lower end) through a cylinder of lead

* Memoirs of the Astronomical Society of London, vol. i. p. 409.

1·8 inch in diameter, and 13·5 inches long; in the manner described by me in the paper just alluded to. The specific gravity of the lead I have assumed as equal to 11·300: but on account of the cylindrical hole made in it, and the wooden rod inserted therein, I estimate the *vibrating* specific gravity of the mass as equal to 11·113 only.

No. 41 is the same cylinder of lead attached to a flat rod of deal, 1 inch in width, and about 0·14 inch thick in the middle of its width, but bevilled to a thin edge. The cylindrical hole was (as in the preceding case,) completely filled with the rod, which was designedly constructed in that form at its lower end, in order to exclude the air which would otherwise remain in the cylinder and thus alter its specific gravity. The *vibrating* specific gravity of the mass is therefore the same as the preceding: and it was also suspended by the same spring. It was swung with its thin edge opposed to the line of motion. The weight of the leaden cylinder is 93844 grains.

Results of the Experiments.

Having thus given a description of the several pendulums employed in the following experiments, I shall now proceed to state the results obtained from each of them respectively: dividing them into different sets according to the form and construction of the pendulum. And here I would remark that the number annexed to each result denotes the number of the experiment, as given in numerical order in the *Appendix* to this paper; where all the necessary information for obtaining the result, is given in detail: this mode of reference being considered preferable to an interruption of the narrative in this part of the paper. I would also previously observe that, in conducting these comparative experiments, I have always made them *in pairs*, on the *same day*, and *immediately* succeeding each other; whereby any discordancy arising from an alteration of temperature of the room, or the rate of the clock, is in a great measure avoided: and, in continuing any series, the order of proceeding has been alternately reversed, which is an additional check against any error arising from a *progressive* (but unperceived and consequently unrecorded) variation in the rate of the clock, or the temperature of the room. Thus, when four experiments have been successively made (which is the smallest number employed,) I have swung the pendulum first in free air; then, after pumping out the air and the lapse of a given interval (for the equalization of the temperature which is always disturbed by this process,) but without touching any part of the apparatus, I have taken the second series in vacuo: these two sets form one comparison. On the conclusion of this series (everything remaining the same, and no part of the apparatus being in any way disturbed or handled,) I have immediately taken the third series in vacuo: then, after letting in the air, and suffering everything to remain undisturbed as before for a given time, I have taken the fourth series in free air; which is compared with the experiment immediately preceding, and thus forms a second comparison. These four experiments, thus compared, give two results, which are in general sufficient for the determination of the quantity sought. But, I have frequently repeated the process and taken four other consecutive sets: in which case I have usually taken off the glass top, and turned the knife edge, end for end, for a reason which I shall more particularly allude to in the sequel; and have then conducted the new series precisely in the same manner as the former ones. The pendulum has generally been set off, as nearly as possible, to the same arc of vibration, and continued for nearly the same length of time. In short, I have endeavoured, as much as was in my power, to make each *pair* of experiments which are compared together, as nearly as possible under the *same* circumstances, in order to avoid the chance of any error or discordancy arising from any unforeseen cause*.

First set.—*Results with the* $1\frac{1}{2}$*-inch Spheres.*

1) Platina.		2) Lead.		3) Brass.		4) Ivory.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
1—2	1·873	17—18	1·896	9—10	1·819	25—26	1·879
3—4	1·883	19—20	1·909	11—12	1·817	27—28	1·864
5—6	1·866	21—22	1·840	13—14	1·849	29—30	1·858
7—8	1·904	23—24	1·840	15—16	1·849	31—32	1·886
Mean =	1·881	Mean =	1·871	Mean =	1·834	Mean =	1·872

* These remarks apply more especially to the experiments recently made for the express purpose of this inquiry. Other experiments, made prior to the present year, without reference to this subject, are taken from my observation books, in the order in which they were made.

The results of all these pendulums agree very well together, except the brass one: and seem to show that in pendulums of equal length and of similar construction, the factor for this additional correction depends on the *form* and *magnitude* of the moving body; and is not affected by its weight or specific gravity. The mean of the whole makes $n = 1{\cdot}864$*. I am unable to account for the discordancy of the brass sphere from the others; unless it be in the determination of the specific gravity, which is certainly less than that of any brass I have yet examined: it being only 7·660 from a mean of three different weighings on three different days, and agreeing very well with each other. If the specific gravity be assumed equal to 7·8 or 7·9 (which is still small,) the result of this pendulum would agree with the others: but I could never make it exceed 7·67†.

Second set.—*Results with the 2-inch Spheres.*

On the Knife edge.						On the Cylinder.			
5) Lead.		6) Brass.		7) Ivory.		8) Lead.		9) Ivory.	
Exp.	n	Exp.	n	Exp.	n	Exp.	n	Exp.	n
49—50	1·764	33—34	1·736	41—42	1·752	61—62	1·811	57—58	1·760
51—52	1·732	35—36	1·732	43—44	1·759	63—64	1·682	59—60	1·722
53—54	1·717	37—38	1·770	45—46	1·762				
55—56	1·739	39—40	1·767	47—48	1·748				
Mean =	1·738	Mean =	1·751	Mean =	1·755	Mean =	1·746	Mean =	1·741

* There are some singular coincidences and discordancies in these results which though slight are worthy of notice. For instance, in the experiments with the lead sphere, No. 17—20 are almost identical; and so likewise are No. 21—24, yet differing from the former. Also in the experiments with the brass sphere, No. 9—12 are almost identical; and so likewise are No. 13—16, yet differing from the former. These and other cases of a like kind are trifling anomalies for which I cannot give any satisfactory explanation.

† Some persons have supposed that if the ball be *greased*, the results might be affected: and if so, the present discordancy may have arisen from some accidental circumstance of this kind. It is probable also that the results may vary according to the state of moisture or dryness of the atmosphere; or from some other unknown cause. On these points, there is certainly a wide field of inquiry open, but on which, at present, I have not leisure to enter. The true cause however, of the present discordancy, I suspect to arise from some internal cavities in the sphere (indicated by the smallness of its specific gravity,) which are connected with the screw-hole, and thus suffer the escape of the included air when submitted to the action of the air pump. This contingency cannot be allowed for, in the computation; although it may be appreciable in the result.

If we reject the two results from the cylinder, (which I shall show, in the sequel, cannot be depended upon,) we shall have the mean of the rest equal to 1·748: thus confirming the remark just made, that the factor for this additional correction, in pendulums of equal length and of similar construction, seems to depend on the form and magnitude of the moving body, and is not affected by its weight or density. This result certainly does not accord with that deduced by M. BESSEL, from his experiments with brass and ivory balls of nearly the same size as the present ones; which result I have already stated to be 1·946*. M. BESSEL's experiments appear to have been conducted with very great care, and with all that accuracy and all those powerful talents for which he is so highly distinguished. At the same time however I would remark that I have carefully revised all my own experiments, and have not been able to discover any source of error: in fact, the general result is corroborated by the uniformity in the results of the experiments with the other pendulums. The subject therefore is still open for further elucidation. In all M. BESSEL's experiments, he used wires of two different lengths; one being about the length of the seconds pendulum, and the other differing from it the exact length of the French toise: or, in round numbers, about 39 inches and 116 inches. The value of the factor which he has deduced, appears to be that which he considers common to both: but it perhaps may be a question whether pendulums, differing so much in their lengths, give precisely the same value for the factor.

Third set.—*Results with the* 2-*inch solid Brass Cylinder.*

Flat sides horizontal.				Flat sides vertical.			
10) Suspended by an iron wire.		13) Suspended by a brass rod.		11) Round side opposed to the line of motion.		12) Flat sides opposed to the line of motion.	
Exp.	n	Exp.	n	Exp.	n	Exp.	n
65—66	1·839	77—78	1·905	69—70	1·912	73—74	1·954
67—68	1·880	79—80	1·940	71—72	1·928	75—76	1·946
Mean =	1·860	Mean =	1·922	Mean =	1·920	Mean =	1·950

* See the note, in page 402.

The difference between the results of the pendulums 10 and 13 will show the effect produced by the substitution of the brass rod for the iron wire*. The results of the pendulums 11 and 13 are, as might have been anticipated, nearly equal. The comparison of the results of the pendulums 11 and 12 will show the difference produced, according to the manner in which the cylinder is swung. The whole appear very consistent with the assumption that in pendulums of equal length and of similar construction, the factor for the additional correction depends on the form and magnitude of the moving body.

Fourth set.—*Results with the* 4-*inch Cylinder.*

Solid.		Hollow.									
14) Filled with Lead.		15) Both ends open.		16) Top open, bottom closed.		17) Top closed, bottom open.		18) Both ends closed.		19) Hermetically sealed.	
Exp.	n	Exp.	n	Exp.	n	Exp.	n	Exp.	n	Exp.	n
97— 98	2·011	85—86	1·921	89—90	1·937	93—94	1·983	81—82	1·995	101—102	2·055
99—100	2·052	87—88	1·929	91—92	1·943	95—96	1·968	83—84	2·006	103—104	2·085
Mean =	2·032	Mean=	1·925	Mean=	1·940	Mean=	1·975†	Mean=	2·000†	Mean=	2·070†

It appears from these last experiments that the effect of the circumambient air on the moving pendulum is the same whether a portion of the pendulum be solid or hollow; provided we take into account (in the case of hollow bodies,) the diminution of the specific gravity of the pendulum, by reason of

* It might reasonably be inferred, from this insulated comparison, that the thicker the suspending rod, or wire, the greater would be the value of n. But, it will be seen from some *additional* experiments, made since this paper was read and which will be given in the sequel, that this is not always the case: and that the present results can be satisfactorily accounted for, on a totally different assumption.

† The experiments with the top closed and bottom open, and with both ends closed (similar to those of pendulums No. 17 and 18), were repeated after the inner sliding tube had been taken away, and a new top to the outer cylinder had been soldered on, as mentioned in page 412: and the results were as follow:

Top closed, and bottom open.	Both ends closed.
1·977	2·111
1·963	2·094
Mean = 1·970	Mean = 2·102

which agree very well with the preceding results.

I will also take occasion here to observe that, having reason to suspect the escape of the air from the

the included air: and there is little or no difference whether the hollow body be hermetically sealed, or whether the ends be loosely closed, and a free communication left between the internal and external air: due regard being had, in all these cases, to the correct determination of the *vibrating* specific gravity of the body. When both ends of the cylinder are left open, the effect of the air appears to be the least, as in the pendulum 15; and it is increased when either the top or bottom pieces are replaced, as in pendulums 16 and 17: which seems to show that some slight modification of the results is caused by leaving the ends *open* to the circumambient air. I would observe that, with the exception of No. 14 and 19, the specific gravities of the cylinders could not be practically determined, but were computed only; and from assumptions relative to the contents of the cylinder, which could not be completely verified. But they are probably very near the truth; and the comparative results cannot be materially affected by any error that is likely to have occurred. The repetition of three of the experiments, as stated in the preceding note, after the cylinder had been altered, and its contents subjected to a new computation, shows the degree of accordance that may be attained in these experiments.

If the results of the experiments with these hollow cylinders be compared with those made by M. Bessel, with a hollow brass cylinder of a somewhat similar form, vibrating in air and in water, there will be found a very considerable and remarkable difference; inasmuch as he makes the value of n equal to 9·100 *. But, on examining the steps of the process by which he deduces this value, it will be easy to discover the source of this apparent discordancy. The specific gravity of the brass, of which the cylinder was formed, is stated to have been 8·3; but by reason of the included air, the specific gra-

interior of the cylinder, in the experiment with pendulum 19, when the vacuum tube was exhausted, I repeated the experiment, and found the following result:

2·076
2·160

Mean = 2·118

But, here also, from some appearances round the screw of the bottom piece, I had again reason to suspect the escape of some of the air from the interior of the cylinder; which may perhaps account for the slight discordancies apparent in the partial results. The whole however are very satisfactory.

* See his work, page 67. He makes the value of k, from two experiments, equal to 7·99 and 8·21; mean = 8·100: to which, unity must be added, in order to obtain the value of n. The diameter of M. Bessel's cylinder was 2·84 inches, and its height 3·20 inches.

vity of the moving mass was reduced to 2·079: and this is the value which M. Bessel employs, in deducing the results from the first set of experiments, where he swings the cylinder (closed) first in air and afterwards in water: which result gives $n = 1{\cdot}754$. In the second set of experiments, he takes away the bottom piece of the cylinder, and having swung it first in air (where the diminished specific gravity was nearly the same as before), then immerses it in water, whereby, he says "the specific gravity of the brass, about 8·3, "is restored." With this assumed specific gravity, the value given by M. Bessel is certainly correct. But if we suppose that the specific gravity of the moving mass is not restored to the specific gravity of the metal by suffering the tube to be filled with water; and that the pendulum can be considered in no other light than as consisting of a cylinder filled with water, instead of a cylinder filled with air; (the specific gravity of which, instead of being 8·3, will probably not be so much as 2·8;) the value of the result will be materially altered. In fact, if the specific gravity were only 2·5, the value of n would be only 1·85: which differs very little from the value deduced by M. Bessel from the experiments with the *closed* cylinder. Now, I find (from the data furnished by M. Bessel,) that the specific gravity of the cylinder when filled with water and with the bottom piece annexed, is about 2·8: but it is evident that, when the bottom piece is taken away, the specific gravity will not be so much; and by the assumption of such diminished specific gravity, the discordancy noticed by M. Bessel, would be considerably reduced, if not wholly eliminated *.

Fifth set.—*Results with the 2-inch Leaden Lens.*

No. 20.

Exp.	n
105—106	1·614
107—108	1·546
Mean =	1·580

* M. Bessel remarks that in this experiment there was a more than usual motion of the water, arising from a portion of the fluid flowing out of the cylinder to supply the vacuum caused by the motion of the cylinder; and the reverse. But the effect of this on the general result would, I apprehend, be very slight. In my experiments with hollow cylinders, above detailed, we observe but a trifling difference when the ends of the cylinder are left *open*.

The whole of the experiments with the preceding 20 pendulums were made for the purpose of determining the additional correction due to bodies suspended by a fine wire, or by a very thin rod: this being one of the forms in which pendulums are constructed for the purposes of physical inquiry. In the present experiments, the pendulums were all nearly of the same length, or about 39 inches; and the results tend to show that the value of n, in pendulums of equal length and of similar construction, depends entirely on the external form and magnitude of the pendulum, and is uninfluenced by its weight or specific gravity. But, whether any portion of this result (and, if any, how much of it,) is to be attributed to the wire or suspending rod; or whether it is caused wholly by the sphere or cylinder; or whether the effect would be greater or less with longer or shorter pendulums; or in what ratio they would be affected by such alterations,—must be left to be determined by future experiments, undertaken with a view to such special investigations *.

I come now to pendulums of a totally different construction.

Sixth set.—*Results with the Copper Cylindrical Rod* 0·41 *inch in diameter, and* 58·8 *inches long.*

No. 21.

Exp.	n
109—110	2·952
111—112	2·913
Mean =	2·932

The factor arising from this pendulum is the greatest of any I have yet found: it exceeds all the preceding ones deduced from spheres and cylinders suspended by wires or fine rods; and also the massy bar pendulum No. 31—34, which is 2 inches wide, $\frac{3}{4}$ of an inch thick, and 62 inches long.

* Since this paper was read before the Society, I have made several experiments to determine some of the points here alluded to; which, by permission of the Council, are added to this paper, and will be given in the sequel. They tend to open a new view of the subject; inasmuch as they show that, in pendulums suspended in the manner mentioned in the text, the value of the factor n is affected not only by the magnitude of the sphere or cylinder, but also by the magnitude and length of the rod or wire.

Seventh set.—*Results with* KATER's *Invariable and Convertible Pendulums.*

	Invariable.		Convertible.				
	22)		23) Knife edge A or heaviest end below.		24) Knife edge B or heaviest end above.		
	Exp.	n	Exp.	n	Exp.	n	
No. 12.	I.	1·588	Ph. Trans. 1829	2·144	Ph. Trans. 1829	2·204	with wooden tail pieces.
	II.	1·589					
No. 13.	III.	1·570		1·840		2·205	with do. reduced.
	IV.	1·574					
	V.	1·615		1·853		2·012	with brass tail pieces.
	VI.	1·606					
			1831	1·811	1831	2·109	without any tail pieces.
				1·910		2·161	
				1·905			
	Mean =	1·590	Mean =	1·875	Mean =	2·135	

The mean result of the invariable pendulum differs from that deduced by Captain SABINE, who makes $n = 1·655$. This difference arises from two causes: in the first place, he adopts Sir GEORGE SHUCKBURGH's determination of the relative weights of air and water; whereas I have preferred, in all these reductions, the more recent determinations of MM. ARAGO and BIOT: and secondly, (which is the principal cause of the difference,) he has assumed the specific gravity of the pendulum equal to 8·600; whereas I do not consider that it can be correctly assumed greater than 8·400, as I have already stated in page 414. Captain SABINE made use of two different pendulums, marked No. 12 and 13; and the results of each accord very well together.

With respect to the convertible pendulum, it is clear that the first determination of the values of n (viz. 2·144 and 2·204,) must be used with all those experiments made by Captain KATER for determining the length of the seconds pendulum, and inserted in the Philosophical Transactions for 1818: subject however to the proper correction for the *vibrating* specific gravity. The last three for the knife edge A, and the last two for the knife edge B, can be applied only to the pendulum as it now exists, deprived altogether of the tail pieces, and its sliding weight.

The detail of the experiments with the invariable pendulum will be found in the Philosophical Transactions for 1829; and of the convertible pendulum, in the same work for 1829 and 1831.

Eighth set.—*Results with a Brass Bar, 2 inches wide, $\frac{3}{8}$ inch thick, and 62·2 inches long.*

25) Knife edge A.		26) Knife edge B.	
Exp.	*n*	Exp.	*n*
113—114	1·872	115—116	2·027
119—121	1·819	117—118	2·007
122—124	1·844	126—128	1·975
124—125	1·865	128—130	1·956
136—139	1·848	131—133	1·945
139—141	1·838	133—135	1·896
Mean =	1·848	Mean =	1·968

Ninth set.—*Results with Copper and Iron Bars, 2 inches wide and $\frac{1}{2}$ inch thick.*

Copper, 62·5 inches long.				Iron, 62·1 inches long.			
27) Knife edge A.		28) Knife edge B.		29) Knife edge A.		30) Knife edge B.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
	1·896		1·998		1·935		2·098
	1·915		1·994		1·926		2·019
	1·856		1·978		1·975		2·078
	1·899		1·994		1·945		2·061
Mean=	1·891	Mean =	1·991	Mean=	1·945	Mean=	2·064

As these two bars are both of the same thickness, it would appear that the shorter pendulum gives the greatest value of *n*: but the discordance probably arises from some error in the assumed specific gravity of the metals; since, as I have already observed, it was not deduced from the pendulum itself. I have not here given the references to these experiments, as the details of them will form the subject of a Report to be laid before Government, which I am about to draw up, relative to the pendulums employed by the late Captain Foster, in his voyage of experiment.

Tenth set.—*Results with a Brass Bar,* 2 *inches wide,* $\frac{3}{4}$ *inch thick, and* 62 *inches long.*

31) Knife edge A.		32) Knife edge B.		33) Knife edge C.		34) Knife edge D.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
142—143	2·061	145—146	2·071	151—152	2·098	148—149	2·090
143—144	2·057	146—147	2·061	152—153	2·064	149—150	2·046
154—155	2·054	157—159	2·053	164—165	2·111	161—162	2·104
155—156	2·114	159—160	2·127	165—166	2·124	162—163	2·109
Mean =	2·071	Mean =	2·078	Mean =	2·099	Mean =	2·087

If we take the mean of the two knife edges A and D (which are situated at the ends of the bar, and in which positions of the pendulum the heaviest weight is below the axis of suspension,) the value of n will be 2·079; and the mean of the other two knife edges B and C, in the reversed positions of the pendulum, will make n equal to 2·088: which two values will be the correct mean for this pendulum. But the difference in these values is so trifling, that the general mean ($n = 2·083$) may be assumed for all the knife edges, without the risk of any material error.

Eleventh set.—*Results with a Brass Tube,* $1\frac{1}{2}$ *inch in diameter, and* 56·2 *inches long.*

35) Plane A.		36) Plane C.		37) Plane *a*.		38) Plane *c*.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
169—170	2·318	171—172	2·269	173—174	2·243	167—168	2·293
175—178	2·318	179—180	2·247	181—184	2·291	185—188	2·341
Mean =	2·318	Mean =	2·258	Mean =	2·267	Mean =	2·317

If, as in the case of the preceding bar, we take the mean of the two planes A and *c*, which are situated at the ends of the tube, the values of n will be identical with each other, or 2·318: and the mean of the other two planes, in the reversed positions of the pendulum, will make n equal to 2·262. So that, with this pendulum the value of n, when the heaviest end is above the axis of suspension, is *less* than it is when the pendulum is in the reversed position:

contrary to what takes place with all the preceding convertible pendulums; and contrary to the theory on this subject recently developed by some excellent mathematicians.

Twelfth set.—*Results with Clock pendulums, suspended by springs.*

39) Mercurial.		Leaden cylindrical bob.			
		40) Cylindrical rod.		41) Flat rod.	
Exp.	*n*	Exp.	*n*	Exp.	*n*
189—190	2·441	201—202	2·562	197—198	2·794
191—192	2·316	203—204	2·616	199—200	2·860
193—194	2·350				
195—196	2·267				
Mean =	2·343	Mean =	2·589	Mean =	2·827

Besides these clock pendulums there is another kind, not here enumerated, consisting of a lenticular shaped bob, of some heavy metal, suspended either by a single rod, or by several compensation rods; in which latter case, it is called the gridiron pendulum. As my vacuum apparatus was not sufficiently large to receive a pendulum of this kind, I cannot throw any light on the probable value of n in these cases. But, as the bob of such a pendulum is not much unlike the convertible pendulum of Captain KATER, when deprived of its tail pieces, (see the description of the pendulum No. 23 in this arrangement,) we may form some estimate of the probable value of n, when the pendulum is suspended by a single rod. In the case of the gridiron pendulum however, it may be a matter of doubt whether the air between the vertical rods may not diminish their specific gravity, when considered as a *vibrating* body. With respect to the leaden cylinders attached to the wooden rods, it will be seen from the experiments with the pendulums No. 40 and 41, that in the case of the thin flat rod, the factor is greater than with the cylindrical rod: contrary perhaps to what might have been anticipated*.

* This is confirmed by a repetition of the experiment with the same cylinder, and with rods of the same form as the present ones, but of different materials. The difference was, as in the present case, about ·200.

General view of the preceding Results.

Having thus given the detail of the several experiments, I shall bring the mean results of the whole into one general view, in the following Table: where I shall first give the value of the old correction, for the reduction to a vacuum, for each pendulum, on the assumption that the barometer stands at 30 inches, that the thermometer is at 32°, and that the number of vibrations in a mean solar day is in each case exactly 86400; then the value of n, or the factor by which such correction must be multiplied in order to obtain the new correction; which new correction, as deduced from the preceding experiments, is given in the next column. To which I have added, in the last column, the weight of air adhering to and dragged by the pendulum in consequence of the air put in motion thereby, when vibrating in the mean state of the atmosphere above mentioned: or rather the quantity of air which, if applied to the *centre of gyration* of the pendulum, would produce the retardation shown by the experiment. This view of the subject was suggested by Professor AIRY; who, at the same time, favoured me with the following investigation and formula for the computation of the *weight of adhesive air* required.

" Let N denote the number of vibrations made by a pendulum, in a mean " solar day, when swung in air: and let ν be the additional number which it " makes when swung in vacuo. Also let w be the weight of the pendulum, " in grains troy; S its vibrating specific gravity, and σ the specific gravity of " the air. Now, since the force of gravity diminishes in the ratio of $(N+\nu)^2$ " to N^2, or in the ratio nearly of $\left(1+\frac{2\nu}{N}\right)$ to 1, it follows that when the pen- " dulum vibrates in air, it is as if, retaining the inertia of its weight w, it " had the gravity of only $w \times \frac{N^2}{(N+\nu)^2} = w\left(1-\frac{2\nu}{N}\right)$ nearly: or, as if it had " lost the weight $w \times \frac{2\nu}{N}$. But, the weight which it has really lost from the " displacement of a quantity of air is $w \times \frac{\sigma}{S}$. Consequently the portion " which is not accounted for by the mere displacement of the air, is

$$w\left(\frac{2\nu}{N}-\frac{\sigma}{S}\right) \qquad (9)$$

"and which may be considered as the additional weight gained by the pen-"dulum, (or rather, the addition to its inertia) when moving in air, supposed "to be *applied to the centre of gyration* *." This is the value given in the last column of the following Table†. Its weight is expressed in grains troy; and the air is supposed to be reduced to the temperature of 32°, and to the pressure of 29·9218 inches: and its specific gravity is assumed equal to ·001299.

* "The inertia of the whole pendulum, in resisting angular motion, is the same as if it were "collected at the *centre of gyration*. The immediate result of the experiment and formula above "given is, that the inertia of the whole pendulum ought to be increased in the proportion of 1 to "$\left(1 + \frac{2\nu}{N} - \frac{\sigma}{S}\right)$: or that, instead of supposing the inertia w applied at the centre of gyration, the "inertia $w\left(1 + \frac{2\nu}{N} - \frac{\sigma}{S}\right)$ ought to be applied there. The addition to the inertia is therefore "$w\left(\frac{2\nu}{N} - \frac{\sigma}{S}\right)$, applied where that of the whole pendulum may be supposed to be applied; that is, "at the *centre of gyration*."

† In all the computations, however, instead of using the approximate value $\frac{2\nu}{N}$, I have taken the correct value $(N + \nu)^2 - N^2$. The difference is unimportant, unless the specific gravity of the pendulum be very small.

A comparison of the Old and New Corrections, for the reduction to a vacuum: with the Factor by which the former must be multiplied, in order to produce the latter: also the Weight of adhesive air, dragged by the pendulum.

Pendulums.	No.	Old correction.	Factor n	New correction.	Weight of adhesive air.
		"		"	Grains.
Spheres, $1\frac{1}{2}$-inch diameter .... Platina	1	2·709	1·881	5·104	0·496
Spheres, $1\frac{1}{2}$-inch diameter .... Lead	2	5·003	1·871	9·362	0·468
Spheres, $1\frac{1}{2}$-inch diameter .... Brass	3	7·343	1·834	13·467	0·457
Spheres, $1\frac{1}{2}$-inch diameter .... Ivory	4	30·080	1·872	56·310	0·472
Spheres, 2-inch diameter, on knife edge, Lead	5	4·988	1·738	8·668	1·115
Spheres, 2-inch diameter, on knife edge, Brass	6	7·032	1·751	12·317	1·140
Spheres, 2-inch diameter, on knife edge, Ivory	7	32·143	1·755	56·420	1·164
Spheres, 2-inch diameter, on cylinder .. Lead	8	4·988	1·746	doubtful	
Spheres, 2-inch diameter, on cylinder .. Ivory	9	32·143	1·741	cases.	
2-inch Brass cylinder, with wire, flat sides horizontal	10	6·882	1·860	12·800	1·945
2-inch Brass cylinder, with rod, flat sides vertical*	11	6·859	1·920	13·169	2·378
2-inch Brass cylinder, with rod, flat sides vertical†	12	6·859	1·950	13·377	2·451
2-inch Brass cylinder, with rod, flat sides horizontal....	13	6·859	1·922	13·188	2·382
4-inch Brass cylinder, solid, filled with lead	14	5·448	2·032	11·070	4·558
4-inch Brass cylinder, hollow, both ends open	15	22·172	1·925	42·686	4·045
4-inch Brass cylinder, hollow, top open, bottom closed....	16	21·437	1·940	41·582	4·165
4-inch Brass cylinder, hollow, top closed, bottom open....	17	21·955	1·975	43·378	4·283
4-inch Brass cylinder, hollow, both ends closed	18	21·227	2·000	42·468	4·454
4-inch Brass cylinder, hollow, hermetically sealed	19	25·191	2·070	52·150	4·532
Lens, one inch thick, Lead	20	5·000	1·580	7·900	0·438
Long cylindrical rod, Copper	21	6·519	2·932	19·117	4·904
KATER's Invariable, Brass	22	6·697	1·590	10·649	8·339
KATER's Convertible, with the wooden tail pieces ‡ .... knife edge A	23	7·630	2·144	16·356	
KATER's Convertible, with the wooden tail pieces ‡ .... knife edge B	24	7·630	2·204	16·815	
Long Bars 2 inches wide, $\frac{3}{8}$ inch thick, Brass, knife edge A	25	7·002	1·848	12·938	16·705
Long Bars 2 inches wide, $\frac{3}{8}$ inch thick, Brass, knife edge B	26	7·002	1·968	13·780	19·049
Long Bars 2 inches wide, $\frac{1}{2}$ inch thick, Copper, knife edge A	27	6·519	1·891	12·330	20·986
Long Bars 2 inches wide, $\frac{1}{2}$ inch thick, Copper, knife edge B	28	6·519	1·991	12·980	23·276
Long Bars 2 inches wide, $\frac{1}{2}$ inch thick, Iron, knife edge A	29	7·319	1·945	14·237	22·455
Long Bars 2 inches wide, $\frac{1}{2}$ inch thick, Iron, knife edge B	30	7·319	2·064	15·107	25·435
Long Bars 2 inches wide, $\frac{3}{4}$ inch thick, Brass, knife edge A	31	6·980	2·071	14·460	40·594
Long Bars 2 inches wide, $\frac{3}{4}$ inch thick, Brass, knife edge B	32	6·980	2·078	14·569	
Long Bars 2 inches wide, $\frac{3}{4}$ inch thick, Brass, knife edge C	33	6·980	2·099	14·506	
Long Bars 2 inches wide, $\frac{3}{4}$ inch thick, Brass, knife edge D	34	6·980	2·087	14·612	
Long Brass tube .... plane A	35	18·546	2·318	42·990	45·937
Long Brass tube .... plane C	36	18·546	2·258	41·874	43·563
Long Brass tube .... plane *a*	37	18·546	2·267	42·048	44·195
Long Brass tube .... plane *c*	38	18·546	2·317	42·974	45·900
Clock pendulums on springs, Mercurial	39	5·312	2·343	12·448	17·003
Clock pendulums on springs, Leaden bob, cylindrical rod........	40	5·190	2·589	13·104	17·462
Clock pendulums on springs, Leaden bob, flat rod..............	41	5·190	2·827	14·312	20·120

* Cylindrical side opposed to the line of motion. † Flat sides opposed to the line of motion.

‡ For the other cases of Captain KATER's convertible pendulum, see page 427.

It appears from this Table that, in the case of spheres, whose diameters are rather less than $1\frac{1}{2}$ inch (which is about the size of that used by M. BORDA, and by M. BIOT, in their experiments on the length of the seconds pendulum), suspended by a fine wire, the value of n may in pendulums of such length be assumed equal to 1·86: but that, if the diameter of the sphere be increased to about 2 inches, as in M. BESSEL's experiments, the value of n will be diminished to 1·75. I regret that my vacuum apparatus is so constructed that it will not admit of my making experiments on either larger or smaller spheres or on longer or shorter pendulums: otherwise I should have pursued this inquiry further, in order to discover the law by which the results of pendulums so constructed are governed*. It will be seen likewise, from a comparison of the pendulums No. 10 and 13, that the *size* of the suspending wire, or rod, has a perceptible (although in those particular cases, not a very material) effect on the results: increasing the value of n, as the size of the wire increases. The value of n is affected also by the *form* of the rod, as may be seen by a comparison of No. 40 and 41, to which I shall again presently allude.

The solid cylinder, 2 inches long, gives the value of n equal to 1·86†; another, of the same diameter, and double the length, gives 2·03; and the cylindrical tube, 56 inches long, gives only about 2·3: whilst the small cylindrical rod, not much more than 4 tenths of an inch in diameter, gives upwards of 2·9. Other apparent anomalies will present themselves, on a more minute examination and comparison of the values given in the Table; which can only be cleared up by future experiments.

It appears also from this Table that the additional number of vibrations to be applied to the results from experiments with a platina sphere, similar to that made use of by M. BIOT‡, will be 2·395: whereas the additional number to be

* I have made some alterations in my pendulum apparatus, since this paper was read, which has enabled me to extend the scale of my experiments; as I shall subsequently state more at length.

† Since this paper was read before the Society, I have seen the account of M. BESSEL's additional experiments on the pendulum, in the Ast. Nach. No. 223. From those experiments, M. BESSEL deduces the value of n, for a cylinder very similar to that mentioned in the text, equal to 1·755. In this experiment the length of the wire was nearly the same as mine. But, for his *long* pendulum, he makes the value of n equal to 1·952. He has also slightly increased the value of n as adduced from his former experiments; making it equal to 1·956, instead of 1·946, as already mentioned in page 402.

‡ Although this is the value to be applied to the pendulum used by M. BIOT, it does not follow that it would be correct to apply the same value to that used by M. BORDA (which was a *two seconds* pendulum), unless it should be found that the factor is the same for long and short pendulums of this construction.

applied to the results from the experiments with Captain KATER's convertible pendulum (knife edge A) will, on the assumption that the specific gravity as taken by him is correct, be 8·726 (See page 415). So that these two pendulums, which were considered to be nearly in accordance when the old correction was applied for the reduction to a vacuum, will now differ 6·331 vibrations in a mean solar day, from each other: or $\frac{1}{173}$th of an inch in the length of the seconds pendulum. In each of these computations the pendulum is assumed as making exactly 86400 vibrations in a mean solar day.

It appears, from the general Table of comparisons above given, that the long cylindrical copper rod (No. 21) is the most affected by this newly discovered principle; even more so than any of the spheres or cylinders suspended by wires, or than the thick brass bar (No. 31) which presents a flat surface of $\frac{3}{4}$ of an inch in width, to the line of motion. We find also that the small spheres are more sensibly affected than the larger ones; which agrees with what M. DU BUAT observed in the experiments made by him, to which I shall presently allude. But the relation between the results of the other pendulums, does not appear to me, at present to be satisfactorily accounted for, or to be referable to any known principle; and, in order to determine the effect which is produced in the results, they must in all cases be made the subject of actual experiment. We may however draw this inference from the whole, that we cannot strictly compare the results of any invariable pendulums, that have been swung in various parts of the globe, without subjecting them (or their prototypes) to this rigid test. The English pendulums have generally been made after one fashion, which is that of No. 22 in the above enumeration: but I have seen some French ones, of a different form, where the bob has been much thicker, and suspended by a cylindrical rod; and which would probably give a very different value of n, if subjected to actual experiment. The rods of the pendulums taken out by MM. FREYCINET and DUPERREY, were cylindrical and about $\frac{1}{2}$ an inch in diameter: and it may be a matter of doubt whether the results with those pendulums are strictly comparable with the results obtained by pendulums of Captain KATER's construction. I have already shown, in the experiments with the pendulums No. 40 and 41, that a similar difference in the form of the rod only (the bob continuing the same) causes a difference in the result, amounting to upwards of 1·2 vibration in a day: and there may probably be other sources of discordancy which can be ascertained only by actual experiment. I fear therefore that, in deducing the true figure of the earth from pendulum experiments hitherto made, we can compare together only those experiments which are made with precisely the same kind of pendulums.

If we examine the new correction for the Mercurial clock pendulum, which is the pendulum now generally adopted for astronomical purposes, we shall find that a difference of one inch pressure of the atmosphere should produce an alteration, in the daily rate of the clock, equal to $0^s,414$; which is more than double the quantity hitherto assumed as depending on the change of the barometer; and which therefore can no longer be overlooked by the astronomer. In order to obviate this effect of a variation in the atmospheric pressure on the rate of the clock at the Observatory at Armagh, Dr. ROBINSON has recently attached a syphon barometer to the rod of the mercurial pendulum, so placed that the variations in the height of the column of mercury in the barometer may exactly compensate the effect produced by the change of atmospheric pressure. Mr. DAVIES GILBERT, in the Supplement to a paper inserted in the Quarterly Journal, vol. xv. has shown that the same compensating effect may be produced by a proper selection of the arc of vibration: since the effect produced by the difference of density in the atmosphere will, in such case, be exactly counterbalanced by the effect arising from the difference in the arc of vibration caused by such difference of density. And proceeding agreeably to the formula which he has there given for finding the value of such arc, and on the assumption of the accuracy of the new correction above mentioned, I find that the value of the required arc should be $2° 45'$ on each side of the vertical line, or a total arc of $5\frac{1}{2}$ degrees. I believe that the semi arc of vibration, in astronomical clocks, is seldom more than 2 degrees; which produces only one half of the compensating effect above alluded to: so that (assuming Mr. GILBERT's theory to be correct,) there still remains an effect on the daily rate of the mercurial clock, by a difference of one inch pressure of the atmosphere, of more than $\frac{2}{10}$ths of a second; which corresponds with the recent determinations of Dr. ROBINSON from observations made expressly for that purpose *. The attention of astronomers will probably in future be more particularly directed to this subject.

The values in the last column of the Table, denoting the weight of air

* See the Memoirs of the Royal Astronomical Society, vol. v. p. 125.

adhering to the pendulum (supposed to be applied *to the centre of gyration*) follow a very different march from the values of the factor n; and lead to a more satisfactory explanation of the effect of the air on the motion of the pendulum. For, it evidently appears that the weight of air, *dragged* by a pendulum in motion, depends principally on the magnitude of the moving body; the influence of which however seems to be affected by other circumstances at present unknown: so that the exact law of the variation of this influence is not sufficiently apparent from the examples adduced: and further experiments are requisite to clear up this difficult but important point *.

Difference in the two ends of a convertible pendulum.

If we examine the results of the several convertible pendulums given in the above Table, we shall find that the factor n is not the same for the two knife edges. This has been already noticed by M. Bessel, in his work so frequently alluded to, and also by M. Poisson in his recent paper in the Con. des Tems for 1834; both of whom seem to consider that the factor ought to be greater when the heaviest end is above the axis of suspension, than in the reversed position of the pendulum. This, however, does not appear to be universally the case, as will be seen by the following Table; where I have given the factors for the two knife edges of the several convertible pendulums used in the preceding experiments: together with the ratio between those factors, assuming as unity the factor for the knife edge A, or that position of the pendulum when the greatest weight is below the axis of suspension.

Factors for the correction of a convertible pendulum, for the reduction to a vacuum: with the ratio between the corrections for the two knife edges.

No.	Pendulums.	Knife edges. A.	Knife edges. B.	Ratio.
23	Kater's, with wooden tail pieces	2·144	2·204	1·028
25	Brass bar, $\frac{3}{8}$ inch thick	1·847	1·968	1·066
27	Copper bar, $\frac{1}{2}$ inch thick	1·891	1·991	1·053
29	Iron bar, $\frac{1}{2}$ inch thick	1·945	2·064	1·061
31	Brass bar, $\frac{3}{4}$ inch thick	2·079	2·085	1·003
35	Brass tube, $\frac{1}{2}$ inch diameter	2·318	2·262	0·976

* The *Additional Experiments* which I have made on this subject, subsequent to the reading of this paper before the Society, will be given in page 438, &c.

From these comparisons it appears that although, in the cases of the first four pendulums, the correction for the knife edge B exceeds that of the knife edge A, yet in the case of the thick brass bar (No. 31), the corrections for the two knife edges are nearly equal; and in the case of the brass tube (No. 35), the correction for the knife edge B is smaller than that for the knife edge A; contrary to what takes place in the other pendulums, and contrary to the assumed theory on the subject *.

M. Bessel has suggested, as one of the modes of rendering the two knife edges, of a convertible pendulum, synchronous, to make the figure symmetrical, but the mass not so: which may be effected by making one part of the pendulum *hollow*. In such case, however, we must consider the hollow portion of the pendulum as a substance of a different specific gravity, and compute its effect on the vibrating mass accordingly. The results also, in such a case, will probably differ according as the hollow portion is hermetically sealed, or communicates freely with the circumambient air.

Additional Experiments.

Since this paper was read before the Society, I have made a number of *additional* experiments on other pendulums of different forms and construction, and have varied and combined some of the preceding pendulums in several new modes; with a view to clear up the anomalies apparent on the face of the preceding experiments, and to throw some light on the manner in which the air operates on the pendulum when in motion and affects the time of its vibrations. As the Council have given me permission to annex the substance of these experiments to the present paper, I shall briefly state the results obtained; together with such explanation relative thereto, as may be requisite for understanding the mode pursued, and the consequences deduced: but, I have not considered it necessary to encroach on this indulgence by giving the full detail of those experiments; which, however, it may be proper to state have been conducted on the same principles and with the same regard to accuracy as those already given in this paper. Indeed, it will be seen that there is less

* Probably the position of the two additional knife edges, with their knee-pieces, in the bar No. 31, and of the four additional planes, with their collars, in the tube No. 35, may have some influence in producing this apparent discordancy.

occasion for entering so minutely into the particulars of these experiments, since it will be found that the most material inferences deduced therefrom, do not depend on nice shades of difference in the results of the experiments, but that the cases are marked by broader lines of distinction; where the probable errors of observation and of computation would not make any appreciable difference in the results, or in the consequences to be deduced from them. Moreover, it will be seen that there is a regular march in the results of the several sets of experiments, which confirms the general accuracy of the whole: and it may be proper to state, once for all, that every value adduced is the result of at least *four* different experiments.

I believe it has generally been considered, by persons who have paid attention to this subject, that, in all funipendulous bodies in motion, the principal effect of the air, in adding to the inertia, is exerted on the *body* attached to the wire by which it is suspended; and that the *wire* itself (which is generally the finest that can be used with safety,) has little or no influence in producing any alteration in the time of vibration: and consequently all their experiments and investigations have been conducted under this view of the subject. This, probably, is not far from the truth in the most usual cases which occur, and have been considered: but, as it is desirable that the direct effect of the air on each portion of the pendulum should be separately and distinctly ascertained, as accurately as possible for all cases that are likely to occur, I instituted some new experiments with a view to determine this point.

In the pursuit of this inquiry I have found the suggestion and recommendation of Professor AIRY, "to ascertain the weight of air adhering to each pen- "dulum of experiment," of very essential service: as it has enabled me not only to mark the direct influence of the atmosphere on the pendulum much more accurately and distinctly than by merely deducing the value of the factor n: but likewise to distinguish its influence on the several parts of the pendulum. In many of the following experiments the march of the values, indicating such influence, appears at first sight very complicated and anomalous: for, in some of them, (see the 14th set,) the weight of adhesive air seems to be *less* when the spheres are attached than when the bare rod is used; and in others, (see the 19th set,) the weight of adhesive air dragged by a thin disc, appears to increase in a most extraordinary manner, merely by changing its position on the rod. But, I have been favoured by Professor AIRY with the following investigation and remarks on this subject, which will clear up these and other seeming discordancies.

" It appears that the phenomena, to which you allude, may generally be ex-
" plained by supposing a quantity of air, depending on the figure of the body,
" to adhere to it whilst it is moving, and to add to its inertia without altering
" its gravitation. In the experiments on bodies of a simple shape, the quan-
" tity of air is found, whose inertia, supposing it to adhere to the centre of
" gyration, would account for the retardation of the pendulum (see page 431).
" If then a compound body C consist of two parts A and B, (the distances of their
" centres of gyration from the axis of motion being respectively c, a, b,) and if
" the air adhering to the centres of gyration of A and B respectively were α
" and β; then the compound pendulum C must be supposed loaded with the
" inertia of α at the distance a, and of β at the distance b. The effect of these
" would be the same as if the inertia of $\frac{\alpha a^2 + \beta b^2}{c^2}$ were applied at the distance
" c. If then we find, as the result of experiment with the compound pendulum
" C, that it has (from the action of the air) the inertia γ adhering to its centre
" of gyration, we obtain the equation $\frac{\alpha a^2 + \beta b^2}{c^2} = \gamma$. Whence, the inertia due
" to B alone, or

$$\beta = \gamma \left(\frac{c}{b}\right)^2 - \alpha \left(\frac{a}{b}\right)^2 \qquad (10)$$

" will be the correct measure of the adhesive air *dragged by that body alone.*"

We thus obtain a method of exhibiting separately the effect of the air on a sphere, cylinder, or other body (B) fastened to a rod (A) at any distance from the point of suspension. In the subsequent Tables therefore, I have annexed another collateral column, indicating in each case the effect of the air, or the increase of inertia, due to the suspended body alone, (without regard to the rod,) deduced agreeably to the above formula. I shall now proceed to the detail of the experiments; commencing with those which determine the effect due to the rods alone.

It will be seen that, amongst the preceding experiments, there are some made on a long brass cylindrical tube (No. 35—38), and on a long copper cylindrical rod (No. 21): and that the former, which is $1\frac{1}{2}$ inch in diameter, gave a less value for the factor *n* than the latter which is little more than 4 tenths of an inch in diameter. Conceiving therefore that I might be enabled to determine the law by which such values were governed, I was induced to try other cylindrical rods, supported in the same manner as the copper one above mentioned, and of nearly the same length, but much smaller in diameter. I accordingly procured a brass rod, or wire, only 0·185 inch in diameter: in fact, it was a piece of the same kind of wire as that which was used with the solid brass cylinder No. 11, mentioned in the preceding part of this paper, page 410. I also caused one to be made about the same length, still smaller in diameter: but, as brass was not exactly suitable to such purpose, when so small, I procured one of steel, only 0·072 inch in diameter *. The length of the brass rod was 56·4 inches, it weighed 3106 grains and its specific gravity I found to be 8·444. The length of the steel rod was also 56·4 inches, its weight (including a small brass screw attached to the end) was 433 grains, and its specific gravity I found to be 7·687. Each of these, when in use, was screwed into the shank of the knife edge apparatus, which was 1·55 inch long, as already described in page 409. The results are contained in the following short Table: where I have continued the numbers of the pendulums from the preceding Table in page 433, for the sake of a convenient reference: No. 42 being No. 21 in the former list.

Thirteenth set.—*Results with plain cylindrical rods.*

Pendulum rods.	No.	*n*	Weight of adhesive air.
Copper, 58·8 inches long, 0·410 inch diameter	42	2·932	4·904
Brass, 56·4 inches long, 0·185 inch diameter	43	4·083 †	1·484
Steel, 56·4 inches long, 0·072 inch diameter	44	7·530	0·479

Now, here we find a regular increase in the value of *n*, as the diameter of the rod is diminished: and the inference is that, with a much smaller wire (such as is generally used in experiments with the pendulum,) the value of *n*

* This was just 5 times the thickness of the iron wire, used in the preceding experiments, with the pendulums No. 1—20.

† I ought not to omit stating that this is the mean of eight different experiments, made with two different rods, at two different periods. Four of them (viz. two double ones,) were made with the wire here alluded to, on June 14th, and the others on August 2nd, with another piece of exactly the

would be considerably increased. But, to what limit this might extend I had no means of ascertaining, since the above steel wire was the finest that I could operate with: for, on account of its small weight, a pendulum of this kind soon comes to rest: and in order to guard against any probable error arising from this source, I took the mean of three consecutive sets of experiments, in determining each separate result. It also appears from these experiments that the quantity of adhesive air decreases as the diameter of the rod diminishes. For, a rod, about 59 inches long, and whose diameter is about 4 tenths of an inch, drags with it nearly 5 grains of air: whilst another rod of nearly the same length, and little more than one sixth of the diameter drags with it scarcely half a grain. But, although the thicker rod drags more air than the smaller one, yet the effect on the latter is much more considerable than on the former. For the 4·904 grains of air added to the weight of the copper rod, would reduce the specific gravity of the vibrating mass from 8·629 to 2·939 only: whilst the 0·479 grain of air added to the weight of the steel rod, would reduce the specific gravity of the vibrating mass from 7·687 to 1·024. And these are the respective specific gravities which if used in the computations for the reduction to a vacuum, would cause n to vanish *.

Having thus ascertained the fact that the influence of the air is greater upon small rods than upon large ones (increasing considerably as the diameter of the rod diminishes), I next tried what effect would be produced by affixing various bodies to the ends of these rods. For this purpose I made use of the two brass spheres No. 3 and No. 6, already described in the preceding part

same kind of wire, and having precisely the same specific gravity, but about half an inch longer. The results differ from each other more than I could have imagined; although each set is consistent in itself: for we have

	June 14.	Aug. 2.
	4·232	3·975
	4·179	3·947
Mean =	4·206	3·961
Weight of air =	1·536	1·431

I have examined all the steps of each experiment, and of the computations connected therewith; but cannot detect any source of error. In fact, it is one of those perplexing anomalies which occasionally occur in our researches after such minute quantities.

* I cannot trace the exact law of the variations in the three values in the column, indicating the weight of adhesive air dragged by each rod; but the nearest approximation is, that the numbers are nearly in the ratio of the square root of the cubes of their diameters.

of this paper; to which I added a third, 3 inches in diameter, weighing 29114 grains, and whose specific gravity I found to be 8·020. The ends of the brass and steel rods were screwed into the several spheres: but the copper rod was attached by means of an adapting screw. The results are given in the following Table: where it will be seen that in each of the three rods the value of n is diminished by appending either of the spheres thereto. The march of these values, however, does not appear to be very regular. Indeed, the conducting of the experiments when the spheres were attached to the *ends* of the rods, required great attention on account of the slowness of the vibrations, and the consequent frequency of the coincidences with the mean solar clock, with which they were compared; and they may consequently be subject to some little uncertainty *. In the case of the brass and steel rods the intervals of the coincidences did not exceed *eleven seconds*: but, on the other hand, I sometimes took a mean of several *thousand* of them, for the result.

Fourteenth set.—*Results with the spheres at the ends of the long rods.*

Diameter of the spheres.	Copper rod.				Brass rod.				Steel rod.			
	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.
inches.												
0·00†	42	2·932	4·904	......	43	4·083	1·484	......	44	7·530	0·479	......
1·46	45	2·458	4·564	0·342	48	2·356	1·417	0·463	51	2·344	0·834	0·607‡
2·06	46	2·234	5·076	1·273	49	1·982	1·973	1·157	52	1·793	1·259	1·063
3·03	47	1·873	6·425	3·251	50	1·933	4·868	4·066‡	53	1·759	3·670	3·480

* Should it be considered desirable to repeat these experiments with greater accuracy, arrangements might be made for that purpose, by altering the rate of the mean solar clock; which I was unwilling to disturb during the course of the present experiments.

† The values in the first line are the same as those given in the preceding Table; and are here inserted in order to show their relative values as compared with the results when the spheres are attached to the rods. This plan will be pursued in the subsequent experiments.

‡ These two experiments (with the pendulums No. 50 and 51) are very unsatisfactory; and are marked as such in my journal. It was consequently my intention to have repeated them: but the subject was overlooked till it was too late. I should propose their being rejected altogether.

Now although there is enough on the face of the above experiments, to confirm the leading principles we are in search of, yet for the reasons already mentioned I should not select them as the most proper for the deduction of any very minute results, when compared with others made under more favourable circumstances.

If we examine the values, denoting the weight of adhesive air dragged by the compound pendulums, formed of the spheres attached to the ends of the several rods, they will be found to exhibit some apparent anomalies; more especially in the case of No. 45 and 48, where the weight of adhesive air seems to be *less* when the spheres are applied, than with the plain rod. But, it must be borne in mind that the deduced weight of adhesive air for each pendulum is in each case supposed to be applied to the *centre of gyration* (which is a different point of the rod, in each pendulum), and therefore requires correction. The collateral column, showing the weight due to the sphere alone (agreeably to the formula in page 440) will exhibit more accordance in the results; and denotes more distinctly the quantity we are in search of.

With a view of obtaining greater accuracy on the points in question, I resolved to try the effect of placing the spheres at, or near to, the centre of oscillation of the rods: whereby the above-mentioned inconvenient change in the intervals of the coincidences would be avoided, and the results rendered more trust-worthy. For this purpose I divided the brass and steel rods into two unequal parts at, or near to, the centre of oscillation: so that by screwing the longest of the two parts into the upper portion of the spheres, and the shortest into the lower portion, I might accomplish this object. But, as the whole length of the pendulum (from end to end) would, in such case, be longer than the rods, by the diameter of the inserted sphere, I cut off one inch from each part, in order that the length of the pendulum, from the knife edge to its extreme end, might, when thus used with the different spheres, be more nearly the length of the rods prior to the alteration. The two parts therefore of the rods, thus reduced, were 36·4 and 18·0 inches respectively. The copper rod was the property of Mr. Troughton, and could not be thus divided. The following are the results with the spheres thus placed.

Fifteenth set.—*Results with the spheres at the centre of oscillation of the long rods.*

Diameter of the spheres.	Brass rod.				Steel rod.			
	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.
inches.								
0·00*	43	4·083	1·484	....	44	7·530	0·479	....
1·46	54	2·722	1·749	0·446	57	2·248	0·774	0·405
2·06	55	2·186	2·352	1·180	58	1·863	1·367	1·039
3·03	56	1·870	4·528	3·382	59	1·774	3·719	3·371

These experiments confirm the results of the preceding set, inasmuch as they show that, by fixing the spheres to this point of the rods also, the value of n is diminished: and there is moreover a greater regularity in the march of the values; as the intervals of the coincidences were much more adapted for correct observation. They consequently furnish us with the means of deducing with a greater probability of accuracy, the quantity of air adhering to, or dragged by, each of the spheres independent of the rod. These values are given in the preceding Table, and have been deduced agreeably to the formula to which I shall presently allude, on the assumption that the weight of air dragged by the brass and steel rods, is accurately shown in the 13th set of experiments. The following Table exhibits in a different form the values above alluded to.

Rods.	Diameter of the spheres.		
	1·46	2·06	3·03
Brass	0·446	1·180	3·382
Steel	0·405	1·039	3·371
Mean =	0·425	1·109	3·377

The quantity of air dragged by the two separate portions of a rod (whether it be actually divided, as in the present case, or a portion of its influence on the circumambient atmosphere be interrupted and destroyed, as in the case of the discs in the 18th and 19th sets of experiments,) as well as the distance of

* See the first note in page 443.

their centre of gyration from the axis of suspension, have been computed agreeably to the following formulæ, which have been obligingly furnished me by Professor AIRY*.

" Let r denote the weight of adhesive air dragged by one inch of the rod " (equal, in the present cases, to $\frac{1}{56\cdot 4}$ of the whole quantity dragged by these " rods as found in the 13th set of experiments); and let us suppose that any " one rod begins at x inches from the axis of motion, and ends at y inches " from the same axis: then will the effect of the air adhering to that rod be " represented by $\frac{r}{3}(y^3 - x^3)$. This is the same as if the whole quantity of " air, $r(y-x)$, had been attached at the distance $\sqrt{\frac{y^3 - x^3}{3(y-x)}}$; which, in " fact, is the distance of the centre of gyration of that rod from the axis of " motion. The effect of the air adhering to several such rods will be repre- " sented by $\frac{r}{3}\Sigma(y^3 - x^3)$. Therefore the ratio which such quantity will bear " to that carried by a rod of the length of the whole rod, if in one uninter- " rupted piece from end to end of the given pendulum, will be as $\frac{r}{3}\Sigma(y^3 - x^3)$ " to $\frac{r}{3}(Y^3 - X^3)$; where X and Y are the distances, from the knife edge, of " the extremities of the whole rod: whence, the weight of adhesive air, to be " used in the formula (10), will be

$$\alpha = r(Y - X) \times \frac{\Sigma(y^3 - x^3)}{Y^3 - X^3} \tag{11}$$

" And the distance of the centre of gyration, from the axis of motion, for a " system of rods, is

$$a = \sqrt{\frac{\Sigma(y^3 - x^3)}{3\Sigma(y - x)}} \tag{12}$$

" where, in each formula, $x = 0$ when the rod begins from the knife edge†."

* I am indebted to Professor AIRY not only for these and other formulæ noticed in this paper, but also for various hints and suggestions during the progress of the experiments; and in general for the lively interest which he has taken in this inquiry: without which encouragement I certainly should not have extended the subject to its present length.

† It is in this manner that I have computed the weight of adhesive air due not only to the spheres in this set of experiments, but also to the cylinders and discs in the 17th, 18th, and 19th sets of experiments.

The values above given are nearly (although not exactly) in proportion to the cubes of the diameters: but, it is possible that some other element, at present unknown, may affect the results; and indeed some portion of the air may adhere to, or be dragged by the *sides* of the sphere. As the exact measure of these three brass spheres was, however, a matter of importance in this inquiry, I examined them more minutely, and found them to be 1·465, 2·065, and 3·030 inches respectively. So that the weight of adhesive air for the last two spheres will be almost exactly as the cubes of their diameters; and, for the first two, not materially differing therefrom. In fact, if the weights of air were ·387, 1·084, and 3·422 grains respectively, the whole would agree precisely with this hypothesis. It is worthy of remark that, in the case of the spheres No. 1 to 7, suspended by a wire (see the Table in page 433), and No. 66 in the following set, if we consider the weight of air, dragged by the wire alone, as equal to 0·10 grain, and deduct this value successively from the mean weight of air dragged by the 1·46 and the 2·06 inch spheres respectively, as there given, and by the 3·03 inch brass sphere as given in the following set of experiments, we shall have 0·373, 1·040, and 3·444 grains for the weight of air dragged by the spheres alone. So that, on the whole, I consider the hypothesis adduced as not far from the truth; and that the general expression for the quantity of air dragged by a pendulum consisting of a sphere suspended by a rod or wire, will be as follows: viz.

$$\text{R} + \overset{\text{grains.}}{0{\cdot}123} \times d^3$$

where d denotes the diameter of the sphere in inches, and R the quantity of air dragged by the rod or wire. And if, in the case of a sphere suspended by a fine wire, of the length of the seconds pendulum, we suppose R to be (as already stated) equal to 0·10 grain, this formula will become

$$\cdot 002564\, l + \cdot 123\, d^3$$

where l denotes the length of the wire, in inches.

These values do not differ materially from those obtained by the same spheres attached to the *ends* of the *long* rods, as given in the 14th set of experiments: but I have already stated that those results were obtained under less favourable circumstances, and are not to be relied on with the same degree of confidence as the present set. They will be found however to accord more nearly with the following set of experiments where the spheres are attached to the *ends* of the *short* rods.

I next took away the lower rod from the spheres, and they were then attached to the upper rod only; whereby the pendulums became nearly of the same length as No. 3 and No. 6, mentioned in the preceding part of this paper: with the results of which it was my object to compare them. And as the 3 inch brass sphere had not yet been swung with the iron wire, I now made some experiments with this mode of suspension, for the express purpose of the comparison*. The following are the results:

Sixteenth set.—*Results with spheres at the end of the short rods.*

Diameter of the spheres.	Brass rod.				Steel rod.				Iron wire.			
	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.
inches.												
1·46	60	2·198	1·047	0·465	63	1·904	0·537	0·410	3	1·834	0·457	0·357
2·06	61	1·901	1·513	1·078	64	1·785	1·227	1·104	6	1·751	1·140	1·040
3·03	62	1·830	4·202	3·719	65	1·779	3·720	3·587	66	1·748	3·544	3·444

If the results with these brass and steel short rods be compared with those of the same spheres attached to the end of the long rods, stated in page 443, we shall find that as far as the value of n is concerned, it is, with one slight exception, greater in long pendulums than in short ones: but, the difference appears to depend chiefly on the relative magnitudes of the spheres and of the rods. With respect to the weight of adhesive air I regret that I could not conveniently swing these short rods without the spheres attached thereto; which would have enabled me to ascertain (agreeably to the formula in page 440), whether the weight of air adhering to, or *dragged* by, each sphere respectively is the same in this set of experiments, as in the preceding set. But, if we suppose that the weight of air, dragged by these short rods, is proportional to their lengths, and employ such quantities in the formula above mentioned,

* The iron wire used with this heavy sphere was ·023 inch in diameter; or about one third of the thickness of the steel rod; and nearly twice the thickness of the wire used in the experiments with the pendulums No. 1 to No. 20. It weighed 26 grains.

we shall find that the weight due to the spheres alone, when attached to the brass and steel rods, will be as stated in the preceding Table. The values annexed to the spheres, when suspended by the iron wire, are deduced from the assumption that the weight of the air dragged by the wire is equal to 0·10 grain, as already stated. These values, like most of those deduced from the 14th set of experiments, agree very well with those which result from the spheres when annexed at the centre of oscillation: and the whole show that the effect of the air on a pendulum consisting of a sphere suspended by a fine rod or wire, although principally due to the sphere, is partly owing to the wire also: but that this influence of the wire diminishes with its diameter; and, when extremely fine, probably becomes a small constant quantity, of nearly equal value in the most usual cases that occur*.

In order to place the subject of this inquiry in a clearer point of view with respect to other bodies, I caused three additional brass cylinders to be made; which, with the cylinder No. 10, described in the preceding part of this paper, were proposed to form the subject of a new set of experiments. The diameters of all these cylinders were ordered to be made exactly alike; viz. 2·06 inches: and their respective heights, or thicknesses, were 2·06 inches, 1·00 inch, 0·50 inch, and 0·18 inch. This latter thickness was chosen on account of its being precisely the diameter of the brass rod. The 1 inch cylinder weighed 6611 grains, and its specific gravity I found to be 7·805: the $\frac{1}{2}$ inch cylinder weighed 3352 grains, and its specific gravity I found to be 8·116: and the ·18 inch cylinder weighed 1266$\frac{1}{2}$ grains, and its specific gravity I found to be 8·145. The other cylinder has been already described. All these cylinders were tapped in the circumference, with two screw holes, opposite to each other, for the purpose of affixing thereto the two unequal portions of the rods above mentioned: whereby the cylinders became placed nearly in the centre of oscillation of the whole length of the rod. The cylinders, thus placed, were swung with their flat sides vertical, and opposed to the line of motion; similar to the

* This appears from the slight difference in the quantity of adhesive air dragged by the steel rod and iron wire, in this set of experiments; which is very small. And moreover, in the case of the ivory sphere (No. 4), which was suspended by a *very fine* silver wire, the result is precisely the mean of the other spheres, which were suspended by the *much coarser* iron wire.

pendulum No. 12, as described in the preceding part of this paper. The following are the results.

Seventeenth set.—*Results with the 2-inch cylinders placed at the centre of oscillation of the long rods.*

Thickness of the cylinders.	Brass rod.				Steel rod.			
	No.	n	Weight of adhesive air.	Weight due to cylinder alone.	No.	n	Weight of adhesive air.	Weight due to cylinder alone.
inches. 0·00*	43	4·083	1·484	......	44	7·530	0·479	......
0·18	67	5·547	2·852	1·284	71	7·694	1·806	1·350
0·50	68	3·941	2·942	1·523	72	4·136	1·900	1·490
1·00	69	2·892	2·972	1·681	73	2·745	2·046	1·661
2·06	70	2·141	3·111	1·902	74	1·988	2·309	1·946

Here we find a regular increase in the value of n, as the thickness of the cylinder diminishes; till it approaches nearly equal to the thickness of the rod itself, when the effect of the cylinder on the value of n is eliminated, and the result is the same as if no cylinder were attached to the rod. Passing this point, and the thickness of the cylinder becoming equal to, or less than, the diameter of the rod, the effect of the cylinder becomes positive; and the value of n is now greater than when the rods are swung without any thing attached thereto. Setting aside however the value of n, and confining our attention to the quantity of air adhering to, or dragged by, these pendulums, we find that it varies with the thickness of the cylinders. And, pursuing the same steps, as in the case of the spheres (see page 446), we obtain the values above given, for the weight of air due to the cylinders alone; and which are more conveniently arranged in the following form: viz.

* See the first note in page 443. It must be noted here that in the first horizontal line, no cylinder is supposed to be attached to the rod: and therefore these values are not directly comparable with the rest.

Rods.	Thickness of the 2-inch cylinders.			
	0·18	0·50	1·00	2·06
Brass	1·284	1·523	1·681	1·902
Steel	1·350	1·490	1·661	1·946
Mean =	1·317	1·506	1·671	1·924

The *differences* between these mean values would indicate the quantity of air dragged by the *sides* of a cylinder of this diameter, according to its thickness: but which does not appear to be very regular in its march; since the thin cylinders drag more in proportion than the thicker ones. Till this fact is more fully ascertained, we cannot deduce a correct general formula for determining the quantity of air dragged by cylinders of different diameters and thicknesses, swung in the manner above mentioned.

The next set of experiments were made with thin circular discs of brass, having about the same thickness as common thick post paper. Twenty pieces, screwed together in a vice, measured ·08 inch; consequently the thickness of each of the brass discs may be assumed equal to ·004 inch. One of these discs was intended to be 2·06 in diameter, in order to correspond with the cylinders above mentioned; but it is in fact somewhat larger, being 2·07; and weighs 28 grains: the second was 3·01 inches in diameter, and weighed 57·5 grains: and the third was 4 inches in diameter, and weighed 106·5 grains. Their specific gravity I found to be 8·450. The long brass rod above mentioned* was then tapped with a screw hole at 38 inches from the knife edge, and the three discs, in succession, were respectively fastened thereto; and swung with their flat sides opposed to the line of motion. The long steel rod could not be used on this occasion, not only because the discs could not be conveniently attached thereto, but also on account of its coming to rest so soon.

* This was the *second* brass rod, 56·9 inches long, mentioned in the second note in page 441.

Eighteenth set.—*Results with the thin brass discs placed near the centre of oscillation of the long brass rod**.

Diameter of the disc.	No.	n	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·00†	43	4·083	1·484	....
2·07	75	7·439	3·111	1·405
3·01	76	14·362	6·511	4·185
4·00	77	27·033	12·873	9·367

In these experiments the value of n, and also the weight of air *dragged* by the pendulum increase as the diameter of the disc increases. If we examine the values in the last column (in computing which, the quantity of air dragged by the rod has been assumed of a different value in each case, or proportionate to the length of the rod, *minus* the diameter of the disc), we shall find that the quantity of air dragged by these thin discs, is also nearly in the ratio of the cubes of their diameters: and the general expression for the amount of the same will be nearly

$$\text{R} + \overset{\text{grains.}}{0·149}\, d^3$$

R and d denoting the same quantities as before.

With a view of following up this inquiry relative to the discs, I caused the same brass rod to be tapped with 3 other screw holes: one at 5·1 inches from the knife edge, being the highest point to which I could fix anything; another at 30·0 inches from the knife edge, or near the centre of gravity of the rod; and the other at 57·3 inches from the knife edge, or near the lower end of the rod. The thin brass disc, 2·07 inches in diameter, was then successively fastened to the rod, at each of these distances, and swung in the same manner as in the preceding set, with the flat sides opposed to the line of motion. The following are the results; including that of No. 75 given in the preceding set.

* Owing to some mistake all these discs were placed 8 tenths of an inch *above* the centre of oscillation. This is allowed for in the computations for the weight due to the disc alone.

† See the first note in page 443.

Nineteenth set.—*Results with the 2-inch thin brass disc, placed at different distances from the knife edge on the long brass rod.*

Distance from knife edge.	No.	n	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·0*	43	4·083	1·484	....
5·1	78	4·155	1·523	....
30·0	79	6·115	2·457	1·330
38·0	75	7·439	3·111	1·405
57·3	80	12·124	5·368	1·426

The differences in the weight of adhesive air appear, at first sight, very anomalous: especially when we consider that the vibrating specific gravity of the mass, and the weight of the pendulum, are *exactly alike* in each case. But, it should be remembered that these weights of adhesive air are supposed, by the formula in page 431, to be applied to the *centre of gyration*: and, if we wish to determine the effect due to the disc alone, we must have recourse to the formula in page 440. It is in this manner that I have obtained the results given in the last column of the preceding Table, under the head of "Weight due to the disc alone." The mean of the last three values gives the weight of air due to the disc alone, equal to 1·387 grain. I have not included the case where the disc was only 5·1 inches from the knife edge; since no dependence can be placed on the result, on account of the magnitude of the multiplier. In fact, if the weight of adhesive air at that point of the rod, were only 1·466 instead of 1·523, the weight due to the disc alone would correspond with the mean of the rest.

As I was desirous of varying these experiments as much as possible, I next tried the effect of two of the thinnest of the cylinders above mentioned (having the same diameter as the disc used in the preceding set of experiments), whose thickness was respectively 0·18, and 0·50 inch: in order to see whether they would exhibit the same law. The cylinders were screwed to the *end* of the long brass rod; and swung, as in the preceding set, with their flat sides opposed to the line of motion. The following are the results:

* See the first note in page 443.

Twentieth set.—*Results with the 2-inch cylinders placed at the end of the long brass rod.*

Thickness of the cylinders.	No.	n	Weight of adhesive air.	Weight due to cylinder alone.
inch.				
0·00*	43	4·083	1·484	....
0·18	81	6·216	3·590	1·389
0·50	82	4·046	3·117	1·628

These results are somewhat greater than those deduced from experiments with the same cylinders in the 17th set; but here I should repeat the remark already alluded to in page 444. In fact, on referring to the observation book, I find that the intervals of the coincidences were only 14 seconds: and I fear that a sufficient number of them were not taken, to insure that degree of accuracy which is requisite in such minute inquiries. I should therefore give the preference to the preceding set of experiments.

The next and last class of experiments was necessarily very limited; as, from the construction of my vacuum apparatus, I could not conveniently extend them so far as I could wish. They were instituted for the purpose of determining the difference between the results of the brass cylinders, and the thin brass discs swung *edgeways*, and the results when swung in the manner already described; namely, with their *flat sides* opposed to the line of motion. The two cylinders, used in the preceding set, and the two discs respectively 2·07 and 3·01 inches in diameter, were chosen for this purpose. The two cylinders were screwed, as before, to the *end* of the long brass rod; in order to compare their results with the preceding set: but the two discs were screwed to the brass rod, at 38 inches from the knife edge, in order to compare their results with those of No. 75 in the eighteenth set. The several results are as follow:

* See the first note in page 443.

Twenty-first set.—*Results with the 2-inch cylinders, placed edgeways, at the end of the long brass rod.*

Thickness of the cylinder.	No.	n	Weight of adhesive air.	Weight due to cylinder alone.
inch.				
0·00*	43	4·083	1·484	....
0·18	83	2·771	1·219	0·149
0·50	84	2·208	1·239	0·353

Twenty-second set.—*Results with the thin brass discs, placed edgeways, near the centre of oscillation of the long brass rod.*

Diameter of the disc.	No.	n	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·00*	43	4·083	1·484	....
2·07	85	4·291	1·588	0·091
3·01	86	4·472	1·675	0·168

These experiments confirm the remark already made, that the *sides* of the moving body drag with them very little of the air which has so remarkable an effect on the pendulum. In this last set, the discs were placed (as in the 18th set), at 8 tenths of an inch above the centre of oscillation. In making the computations for the weight due to the disc alone, this has been allowed for: and I would also observe that the *whole* effect of the rod has been used in those computations; as it is evident that the position of the disc does not obstruct any part of its action on the air.

General results of the Additional Experiments.

I must here close the account of these *additional experiments*, which have been pursued up to the latest moment that could be conveniently spared by the printer; as I was desirous of communicating at once all the information I could procure on this interesting subject: and which consequently leaves me only time to offer a few brief remarks on the results obtained.

* See the first note in page 443.

It appears then that all these results accord with the theory that a quantity of air adheres to every pendulum when in motion: and, by thus forming a portion of the moving body, diminishes its specific gravity; or, rather adds to its inertia. This adhesive air is confined almost wholly to the two opposite portions of the pendulum, which lie in the line of its motion; (similar to what takes place with a body moving through still water), and very little of it adheres to, or is dragged by, the *sides* of the pendulum. The shape of this coating of air will consequently partake, in some measure, of the form of the pendulum; subject probably to some slight modifications, with the nature of which, however, we are at present unacquainted. The quantity of air dragged by a pendulum seems to depend on the extent and form of surface opposed to its action, and is not affected by the density of the body.

In the case of a *sphere,* 1 inch in diameter, suspended by a fine wire, the weight of air dragged by the sphere alone appears to be about 0·123 grain troy: and for spheres of any other diameter, in nearly the direct ratio of the cubes of their diameters. The weight of air dragged by the wire (of the length of the seconds pendulum), may amount to 0·10 grain, but probably does not exceed that quantity; and perhaps is nearly constant for all *fine* wires of that length: so that with small spheres (less than 1 inch in diameter), the weight of air dragged by the *wire,* is nearly the same as that dragged by the *sphere.*

With respect to *cylinders* suspended by rods, and swung with their flat sides opposed to the line of motion, the law of the variation is not so manifest; as we are at present ignorant of the precise effect caused by the *edge* of the cylinder. Neither have we obtained sufficient data to develope the effect of the air on cylinders, suspended by rods or wires, and swung with their flat sides in a horizontal position; similar to the pendulums No. 10 and 14. In these cases (see page 433), the 4 inch cylinder drags *much more* than double the quantity of air adhering to the 2 inch cylinder: although they have precisely the same diameter. And these are the only experiments, which I have made, connected with this branch of the subject.

With respect to very thin cylinders, or *discs,* swung with their flat sides opposed to the line of motion, the weight of air dragged by a disc, of 1 inch in diameter, appears to be about 0·149 grain; and for discs of any other diameter, nearly in the direct ratio of the cubes of their diameters. Whence it appears that a thin *disc* drags *more* air than a *sphere* of the same diameter.

As the quantity of air dragged by spheres is proportionate to the cubes of their diameters, I was induced to examine whether the quantity dragged by a sphere, and by a cylinder of the same diameter and height, would be proportionate to their solid contents; or, in the ratio of 1 to $1\frac{1}{2}$. But, from a comparison of pendulums No. 6 and 10 (see page 433) it appears that the cylinder drags more than that proportion, by about $\frac{1}{8}$th part of the whole.

If we compare the results of pendulums No. 10 and 13 (see page 433), the difference in the quantity of air dragged would appear to be that which is due to the difference in the effect produced by the wire and the rod. But we must bear in mind what has been stated in page 440, relative to bodies suspended at the *end* of a rod or wire; and reduce them, by the formula there given, to the same point: in which case, the weight of adhesive air, *due to the cylinder alone*, would be very nearly alike in both experiments.

From a review of the whole, it appears that even when a pendulum is formed of materials having the same specific gravity, yet, if it be not of an uniform *shape* throughout, each distinct portion must be made the subject of a separate computation, in order to determine the correct *vibrating* specific gravity of the body; since each part will be variously affected by the circumambient air. As an example, take the case of the pendulum No. 3, where the iron wire and the brass sphere have almost exactly the same specific gravity, viz. 7·66. If we suppose the sphere drags 0·40 grain of air, and the wire 0·10 grain, (or about $\frac{1}{4}$ of that dragged by the sphere), we shall have the specific gravity of the sphere, with its coating of air, reduced to about 4·43, and that of the wire with its coating of air, to about 0·14. Whence the *vibrating* specific gravity of the whole pendulum, deduced agreeably to the formula (2) in page 405, will be about 4·21; which would give the reduction to a vacuum equal to 13·380 seconds: differing very little from the true correction given in the Table in page 433. If the effect of the air on the wire had been neglected, this value would have been diminished about one second: which shows that in making experiments on pendulums of this kind in *water*, the *whole of the wire* should be immersed in the fluid, in order to deduce correct results.

In concluding these experiments I cannot flatter myself that no error has escaped me; especially when I consider the vast number of computations which have been employed in these investigations. The major part of them, however, have been revised, especially those which exhibited any remarkable anomaly: and I trust that no error of importance will be found to exist. During the progress of the experiments, the apparatus has been from time to time altered, in order to suit the circumstances of the case: and trifling differences of specific gravity, and of comparative lengths and weights arising therefrom, may consequently have passed unnoticed and unobserved. Indeed the subject has been altogether so new, that in commencing a set of experiments, I was not always aware of the precise points, to which it was most necessary to direct the attention; and which were not sufficiently apparent till after the result was obtained. Should it however be desirable to repeat any of these experiments, in a manner that may be considered likely to lead to more accurate results, I shall be happy to resume the enquiry.

The Chevalier Du Buat's *Experiments.*

During the course of these enquiries, it will be seen that I have, all along, considered M. Bessel as the first discoverer of that peculiar property of the *moving* pendulum, which it has been the object of this paper to elucidate: and undoubtedly, he is entitled to the merit of having *first* applied those principles, which he has investigated with so much accuracy and with such great ability, to the *modern pendulum*; and thus rendered it a more powerful and delicate instrument in the hands of the practical and theoretical philosopher. But, it has recently been found that this same property of the pendulum was known nearly fifty years ago, and distinctly treated by the Chevalier Du Buat in his Principes d'Hydraulique. In that work, the second edition of which appeared in 1786 *, the author has stated the results of a number of experiments on pendulums of various kinds, swung in air and in water; from which he was led to infer that a quantity of the fluid in which the pendulum oscillates, is *dragged* with it in its motion, and thus retards its vibrations. He remarks that "if a "denotes the length of a pendulum making any number of vibrations in vacuo, "l the length of a pendulum making the same number of vibrations in the "fluid, p the weight of the moving body in the fluid, P the weight of the fluid

* The *first* edition (1779) does not contain the experiments here alluded to.

"displaced by the body; then $p + \mathrm{P}$ will express its weight in vacuo, and "$\frac{p+\mathrm{P}}{p}$ will be the ratio of gravity in the two cases: whence we obtain

$$l = a \times \frac{p}{p+\mathrm{P}}$$

"This formula would give correctly the length of the pendulum, if the body "in moving did not *drag* with it a certain quantity of the same fluid, which "varies very little by the difference of velocity: so that the mass, when in "motion, consists not only of the mass of the body itself, but also of the fluid "*dragged with it* *." He then proceeds to show (page 229) that "if n be any "constant number such that $n\,\mathrm{P}$ expresses in all cases the weight of the fluid "displaced and *also that of the dragged fluid*, the mass, when in motion (or its "weight in vacuo) is no longer $p + \mathrm{P}$, but is represented by $p + n\,\mathrm{P}$; whilst "its weight in water is always expressed by p. The correct formula therefore "will be

$$l = a \times \frac{p}{p+n\,\mathrm{P}}$$

"whence we deduce

$$n = \frac{p}{\mathrm{P}}\left(\frac{a}{l} - 1\right)\text{"}$$

M. Du Buat then gives the result of 44 experiments made by swinging pendulums formed of spheres of lead, glass and wood, of different weights, and suspended by lines of different lengths: and the conclusion at which he arrives is, that the value of n (which, in his experiments, varies, with only 4 slight exceptions, from 1·67 to 1·45) may be assumed equal to 1·585 †. This certainly agrees with the fact much more nearly than might be expected from the rough manner in which those enquiries were conducted, as compared with more modern experiments. And, although it cannot be placed in competition with the more rigid investigations of M. Bessel, or the results detailed in this paper, yet it evinces the great talent and zeal of the author in being able to extract so near an approximation from such a mode of procedure. M. Du Buat then gives the result also of a vast variety of similar experiments on cylinders, prisms, cubes, &c.: and found in each of them a complete confirmation of his opinion relative to the *dragging* of the fluid in which the vibrations

* Edition 1816, vol. ii. page 226.

† Ibid. page 257.

were made. And although he remarks that the above mean value of n is given as generally suitable to all cases of spheres, yet he suspects that the quantity of *dragged fluid* is rather less with large spheres than with small ones, and also that it is rather less for short pendulums than for long ones *.

But, is it not a remarkable circumstance in the history of this subject, that these important and apparently conclusive experiments of M. Du Buat, which were made by the order and at the expense of the French Government, which were examined, at the request of the Minister of War, by the Royal Academy of Sciences at Paris, and by them favourably reported on, which were first published in the year 1786 (little more than 10 years prior to the experiments of M. Borda on the length of the pendulum †), and which excited so much interest that they led to the subject for the Prize Essay, proposed by the Academy in the following year; and which, not being answered, was repeated in the year 1791, with the offer of a *double* reward;—experiments which attracted at that time so much public attention that another edition of the work appeared in 1816, just about the time when the subject of the pendulum was revived in the different states of Europe; which has not only been translated into the German language ‡, and praised in the highest terms by some of their principal writers on that subject, but has been also largely quoted in many English works, and freely commented on in this country:—is it not singular that such experiments should have been so soon and so completely lost sight of, and forgotten, that not one of the many distinguished individuals actually engaged in those pursuits, and in the investigation of this subject, should have had the least idea or remembrance of the additional correction for the reduction to a vacuum so clearly pointed out by M. Du Buat: and that until the re-discovery of this principle by M. Bessel, as detailed in his valuable paper on the pendulum, no one should

* The first suspicion is verified by the present experiments; at least, in the light in which M. Du Buat viewed the subject. For though the quantity of dragged fluid is greater with large spheres than with small ones, yet the factor n, which he appears to have considered its index, is less. The second suspicion is also confirmed not only by some of the present experiments, but likewise by those of M. Bessel, alluded to in the note in page 434.

† I am unable to fix the precise date of M. Borda's experiments: for, although the month and the day, as well as the exact time to the nearest *second*, are minutely recorded, I have not been able to detect the *year* in which they were made.

‡ By J. F. Lempe, Leipsic 1796. See also the works of Langsdorf, and others.

have thought of verifying the suspicion of NEWTON that such an effect was probable *. M. PRONY, in his Nouvelle Architecture Hydraulique, and Dr. YOUNG, in his Lectures on Natural Philosophy (both of whom have taken an active part in the investigations relative to the pendulum) make frequent allusions to DU BUAT's work: yet neither of these distinguished mathematicians appears to have recollected the singular facts recorded by that author. And even in M. POISSON's late excellent memoir, inserted in the Connaissance des Tems for 1834, although in the Appendix thereto the author's attention has been called to M. DU BUAT's experiments by a notice from another quarter, yet it is evident that when that distinguished mathematician commenced his paper, he was not aware of the facts stated in M. DU BUAT's work: as he frequently, and very justly, alludes to M. BESSEL as the *first* person who had directed the attention of the public to the true correction. And it certainly is but a poor consolation to the practical philosopher, who thus devotes so much of his time to the elucidation of any particular branch of science, to find that his labours may be *so soon* forgotten, and probably lost sight of for ever.

Suspension over a Cylinder.

The principal portion of M. BESSEL's experiments on the pendulum were made by suspending the sphere, by means of a wire, over a steel cylinder not more than ·088 of an inch in diameter. Being desirous of pursuing the same plan with respect to some of the pendulums which are the subject of this paper, I suspended the lead and ivory spheres (No. 8 and 9) in this manner; the results of which have been already stated. I proceeded in a similar manner with some of the other pendulums; but in the course of the experiments I discovered some anomalies, for which I could not at first satisfactorily account; and at length found that they proceeded altogether from the mode of suspension. In the long cylindrical rod (No. 21) the discordancies were the most apparent: for not only would the intervals of consecutive coincidences differ from one another as much as 60, 70 and in one case as much as 90 seconds (*plus* and *minus*), but the arc also would be continually varying in magnitude in a similar manner, alternately diminishing and *increasing*. With a view to discover the cause of these singular anomalies, I erected an apparatus for more minutely

* Principia, lib. ii. prop. 27. cor. 2.

observing and watching the motion of the pendulum during its vibrations: and I found that when the sphere was suspended by a wire over a cylinder, the motion of the ball, although set off in a straight line, soon became elliptical; that the eccentricity of the ellipse was continually diminishing; and that the major axis was continually shifting its position with respect to the points of the compass: circumstances which were sufficient to account for all the appearances above described, and to destroy all confidence in experiments conducted in such a manner. And although I have retained the experiments with the pendulums No. 8 and 9, above alluded to, which were made in this way; yet it has been more to show the near accordance which may sometimes be accidentally attained by an incorrect method, and that we cannot examine too minutely into every step of so delicate an inquiry.

I wish it however to be fully understood that these remarks do not apply to M. Bessel's experiments, since there is this important distinction to be made between his mode of proceeding and mine: viz. that his wire, at the part where it passed over the cylinder, was purposely made *flat*, probably with a view of avoiding this very difficulty; whereas mine was *round*, as generally sold in the shops. I have not yet tried the *flat* wire, but have thought it right to point out the inaccuracies that may attend the use of the *round* wire, in order that others may not adopt it without the precaution of first ascertaining how far the results of any experiments may be affected by the anomalies above alluded to. In conclusion, I would add that, in the knife edge suspension, the vibrations of the ball were uniformly preserved in a straight line during the whole time it was in motion: and no anomalies were discoverable.

Confined space of the Vacuum apparatus.

It has been suggested by some persons that the results of experiments, of the kind mentioned in this paper, may probably be affected by the confined space of the tube in which the oscillations of the pendulum are made. M. Poisson, in his valuable memoir above alluded to, has justly stated that, in all the analytical investigations, the oscillations are supposed to be made in a fluid which extends indefinitely in all directions: a circumstance, however, which cannot practically take place in experiments of this kind. But he imagines that when the pendulum is small, in comparison with the dimensions of the inclosed space, the results are not sensibly affected: and that they are least so, when the surface of the confining body is curved. In the Greenwich vacuum apparatus, where the tube is about 13 inches in diameter, Captain SABINE did not find any difference in the results of some experiments instituted for the express purpose of ascertaining the same; although the bob of his pendulum was 6 inches in diameter. In my own apparatus also, I have found the results of numerous experiments with the *bar* pendulums within the tube, agree very well with those in free air, before the vacuum apparatus was erected: and certainly no discordance has been observable, sufficient to warrant any material alteration in the results of the present experiments. In the Greenwich apparatus, the glass cylinder is formed of three separate pieces, which may be easily taken apart; and the pendulum may thus be, at any time, exposed to the free air: whereby the experiments may be alternately made in the confined cylinder, and in the free air. But my apparatus consists of one uniform brass tube, and is not adapted to such a change of experiments.

Anomalies of the knife edge suspension.

It has been shown by Captain SABINE, in his Account of Experiments, &c. page 195, that, in a pendulum with knife edges, a considerable difference may arise in the results, if they be used with *different planes*: but it does not appear to have occurred to any one, versed in these experiments, that a much greater difference than that which he has recorded may arise from using the *same* knife edge with the *same* plane. This fact has probably hitherto escaped detection from the peculiar manner in which pendulum experiments are usually conducted: for, on examining the detail of most of those experiments, it will be found that after the pendulum at any one station has been placed in its Y's, it has never been removed therefrom, but merely raised and lowered again as occasion may require, till it has been ultimately dismounted, and packed up for another station; whereby any anomaly that might otherwise have occurred, is thus avoided, and consequently escapes detection. Experiments, however, of this kind, ought to be varied in every possible way, in order to guard against any unsuspected source of error.

When Captain BASIL HALL returned from his voyage to the Pacific Ocean, where he had undertaken to swing the pendulum at various places, it was found that the number of vibrations which the pendulum made, on his arrival again in London, differed by 0·97 (or not quite a second of time) in a mean solar day, from the number of vibrations made by the same pendulum previous to his departure: and various causes were assigned for (what was called) so great and so singular a discordance *: for, I believe, at that time the results with the invariable pendulum were considered almost as infallible. It is true that we have a few instances of a contrary nature, where the pendulums, on their return home, have told precisely the same story as they did when they were sent off; and, in the case of the two pendulums that were taken out by Captain SABINE, *their coincidence during the whole of the voyage was very remarkable*, since the greatest variation from the mean did not exceed 0·32 at any one of the stations †; but, these I consider rather as singularly favourable circumstances in his particular case, than as tending to invalidate the results of other experiments. In the voyage of Captain FREYCINET, who took out three separate pendulums, we find a variation in the difference between them, amounting to several seconds in a day. Thus, at the Isle of Guam the difference between pendulums No. 1 and No. 2 was 1180·162 vibrations; whereas at the Isle of Rawâk the difference was only 1173·693; being a variation of 6·469 vibrations. At the Isle of France the difference between pendulums No. 2 and No. 3 was 1012·326 vibrations; whereas at the Isle of Rawâk, the difference was only 1008·557; being a variation of 3·769 vibrations. And at the Isle of France the difference between pendulums No. 1 and No. 3 was 164·948 vibrations; whereas at the Isle of Guam the difference amounted to 169·833; being a variation of 4·885 vibrations in a mean solar day ‡. Captain DUPERREY also, who took out two of these same pendulums (No. 1 and No. 3) in a subsequent voyage found the difference between them, at the Malouine Islands, to be 169·931 vibrations; whereas on his return to Paris the difference was only 168·235; being a variation of 1·696 vibration §. Now, in all these cases there ought to be little or no variation in the difference between any two of the pendulums: neither would there be if we could insure the making of the experiments precisely under the same circumstances; and no blame can

* Phil. Trans. for 1823, page 287.

† Account of Experiments, &c. page 189.

‡ Voyage autour du Monde, par M. FREYCINET, (Observations du Pendule,) page 22.

§ Connaissance des Tems for 1826, pages 294 and 300.

be attached to those zealous officers, surrounded as they must be with difficulties of every kind for carrying on such delicate experiments. In fact, amongst the multitude of experiments that I have myself made, I have seldom found, after I had *dismounted* a pendulum, and then replaced it (even on the same day, under all the favourable circumstances of equality of temperature &c., and with all the conveniencies of manipulation,) that I could make it tell the same story in the next series of experiments. Even the same pendulum, furnished with two different knife edges, rendered synchronous or nearly so, similar to those described in the above enumeration as convertible pendulums (No. 25—38), where the trifling difference in the results of each pair of knife edges, ought, after proper reductions, to be a constant quantity, will frequently differ by an amount much greater than can be attributed to the errors of observation.

The fact, I believe to be, that the pendulum furnished with a knife edge and agate planes, as at present constructed, is a very inadequate instrument for the delicate purposes for which it was originally intended: and a more rigid examination and adjustment of that part of the instrument are requisite, before we can depend on the experiments made with it, either for the determination of the length of the seconds pendulum, or even for the comparison of results obtained in different parts of the world. The knife edge is seldom or never perfectly straight; the planes are seldom or never perfectly true: at least, I have never found one so, amongst the number of those on which I have experimented. The consequence is that, as there is generally a little play in the Y's, the knife edge is not always let down on the same parts of the agate plane. This may be best detected by holding a lighted candle behind the knife edge when it is resting on the plane: by which method the smallest inequalities in the points of contact are readily discernible. But, the fact is rendered still more evident by reversing the pendulum in the Y's, when a sensible difference in the result generally takes place. Amongst the numerous pendulums in my possession, I have not met with more than one, that does not differ in the results by an appreciable quantity, when the pendulum is reversed in the Y's, or turned half round in azimuth. If the knife edge and planes were perfectly correct and true, there ought not to be any difference in the results, whichever side of the pendulum is placed fronting the observer: how then are we to account for a difference of upwards of two vibrations in a day which actually occurs in one of the pendulums above alluded to! The following Table, however, will set this matter in a clearer point of view, and show the real differences which I have found to take place in the results, merely by reversing the face of the pendulum. The numbers, in the first column, have reference to the enumeration of the pendulums in the preceding part of this paper. The brass bar, $\frac{3}{8}$ of an inch thick, was swung on two different agate planes; and the results by no means accord with each other.

Differences in the results, by merely turning the face of the pendulum.

No.	Pendulums.		Difference.
1—21	French knife edge		0·249
22	KATER's invariable, No. 11		0·914
25	Brass bar, $\frac{3}{8}$ inch thick	knife edge A	0·135
26		knife edge B	0·939
25	Same Brass bar on other planes	knife edge A	0·725
26		knife edge B	1·078
27	Copper bar $\frac{1}{2}$ inch thick	knife edge A	0·296
28		knife edge B	0·171
29	Iron bar $\frac{1}{2}$ inch thick	knife edge A	0·121
30		knife edge B	2·038
31	Brass bar, $\frac{3}{4}$ inch thick	knife edge A	0·707
32		knife edge B	0·044
33		knife edge C	0·473
34		knife edge D	0·614

As the experiments, here alluded to, were made for the express purpose of detecting any discordance arising from the position of the knife edges on the agate planes, they were at first followed up (as far as each pendulum is concerned,) in immediate succession; alternately turning the face of the pendulum at the end of each experiment. It is needless to swell this paper with a detail of the whole of the experiments that were made on these occasions; but as the 10th case above enumerated (the iron bar No. 30, knife edge B,) affords so remarkable a discordance, I trust I may be excused for putting on record the steps of the process; by means of which the results may be verified at pleasure. The magnitude of the discordance (like the case already mentioned in page 461), was the cause of its detection, which may therefore be considered as accidental: but the discovery of the anomaly led me to suspect that it might also take place in other pendulums; which, from repeated trials, as above stated, I found to be the case. And this furnishes us with another proof of the propriety of varying such experiments in all manner of ways, in order to guard against the effect of any unsuspected source of error.

Results by turning the face of the Iron pendulum (No. 30).

Exp.	Knife edge B.	Knife edge *b*.	Exp.
205	86220·190	86220·999	206
207	20·346	23·499	208
209	20·433	21·818	210
213	21·002	21·976	211
215	20·524	23·309	212
216	20·129	22·967	214
218	20·574	21·791	217
219	20·302	22·338	222
220	20·247	22·450	223
221	20·473	22·401	224
227	20·504	22·881	225
228	20·362	23·077	226
229	20·962	23·033	230
Mean =	86220·465	86222·503	

It may here be stated that the knife edges of all the convertible pendulums in my possession are marked on *both* sides of the pendulum: on one side with the capital letters A, B, and on the reverse side with the small letters *a*, *b*. Therefore the column, in the above Table, designated as the knife edge B, denotes the results obtained when the side of the pendulum, marked B, is next to the observer: and the other column denotes the results when the pendulum is turned half round in azimuth, and consequently the side marked *b* is next to the observer. The mean difference in the results will be found, as already stated, equal to 2·038 vibrations in a mean solar day. If we compare the several results we shall find the partial differences somewhat greater than what generally occur in a regular series of experiments: but these have arisen from designedly varying the position of the knife edge on the agate plane, with a view to the discovery of the cause of the principal discordance; and which I can attribute to no other source than inequalities in the knife edge, or agate plane, or both; but which are not immediately perceptible to the eye. From a review of the whole question, however, it is clear that different experiments, even with the same pendulum, are not strictly comparable with each other, unless we can either ensure the perfect accuracy of the knife edges and planes, or provide a method of making the vibrations, in all cases, from the same part of the knife edge and from the same part of the plane: or, in other words, that the knife edge and plane shall, in all cases, touch each other at the same points of contact. This, I conceive, would not be difficult; and it must be attended to in all future experiments. We must deal with the experiments, already made, in the best manner we can*.

Correction for the Arc of Vibration.

In a recent volume of the Transactions of this Society† Captain SABINE has stated that the usual formula for the reduction of the vibrations of a pendulum, to indefinitely small arcs, is erroneous; inasmuch as it does not agree with the result of his observations, which require that the hitherto assumed corrections should, in the case of the convertible pendulum tried by him, when the heaviest end is below the axis of suspension, be multiplied by 1·12; and when it is above the axis of suspension, be multiplied by 1·40. As this view of the subject was somewhat at variance with what I had imagined to be the case in my own experiments, I determined on making a few trials in order to ascertain more minutely the difference which arises from the use of large and small arcs: and for this purpose I took the brass bar convertible pendulum No. 25 above enumerated. Two series were made (in the vacuum apparatus, and at about one inch pressure of the atmosphere,) on the knife edge A, and two on the knife edge B: and each of these series was divided into three portions; in the first of which, the arc was taken from about 1°·00 to about 0°·60; in the second, from 0°·60 to about 0°·38; and in the last, from 0°·38 to about 0°·20 and 0°·10. The first series on the knife edge A showed that the usual correction ought to be increased about $\frac{1}{10}$th; which accords very nearly with

* Since this was written, I have caused my agate planes to be slightly *rounded*, so that a very fine thread of light can be seen under the knife edge, on each side of the small line where it touches the curve. By this method I have got rid of the discordancy in the pendulum No. 25—26, which is the only one I have yet tried in this way.

† Philosophical Transactions for 1831, page 461, &c.

Captain SABINE's determination: but the second series on the same knife edge indicated that it ought to be diminished by nearly the same quantity. I consider therefore these two series as neutralizing each other; and that the differences observed come within the errors of observation. With respect to the knife edge B, both series showed that the correction should be increased $\frac{1}{5}$th: which is only one half the amount indicated by Captain SABINE's experiments. Further inquiries therefore are requisite to clear up this point: not only as to the cause of the anomaly, whether it arises from a sliding of the knife edges on the agate planes (in which case, it may differ in different pendulums, and wholly vanish in M. BESSEL's mode of suspension); but also as to the accuracy of the assumed data on which the generally received formula is founded. When the arc is very large, the formula will not lead us to the true result: this has been already noticed by more than one author. But whether the difference arises from a defect in the formula, or from a sliding of the knife edges, or from the variable effect of the air on the pendulum, or from all three, remains still to be demonstrated. Should any experiments for determining this point be commenced, it would perhaps be better that the vacuum apparatus should not be used for the purpose: but that a heavy sphere, cylinder, or lens, suspended by a wire, be swung in free air, first on the knife edge, and afterwards over a steel cylinder; due care being taken, in the latter case, that the wire be *flat* at that portion of it which passes over the cylinder. A body of this kind will continue its vibrations for a sufficient length of time for such experiments: which was in fact the reason for adopting the vacuum apparatus for this purpose; but which may present difficulties of another kind; since it is difficult to prevent a leakage in the vacuum apparatus, which has a material effect on the arc of vibration; and moreover the proximity of the pendulum to the sides of the tube, when swinging in large arcs, may influence the results.

But, whatever be the cause of the discordancy, it is evident that in the present state of the subject we cannot strictly compare the results of experiments, where the arcs employed have been widely different. The initial arc ought in no case to exceed one degree: in my own experiments, I have generally commenced with an arc of about $0^{\circ}\cdot 9$ or $0^{\circ}\cdot 8$; but this I think is still too large, and were I again to undertake any delicate experiments on the pendulum, I should probably make the initial arc about half a degree only. In the experiments on the invariable pendulum made by the English, the initial arc has been about 1°·2 or 1°·3: but in those made by MM. FREYCINET and DUPERREY, the initial arc has sometimes amounted to upwards of 3½ degrees; and Mr. RUMKER, in his experiments on the length of the seconds pendulum, has, in one instance, commenced with an arc of 11 degrees*.

On Captain SABINE's *recent determination of the length of the seconds pendulum at Greenwich.*

In the volume of the Philosophical Transactions just quoted, Captain SABINE has also given, what he considers, the true length of the seconds pendulum at Greenwich; and which he makes equal to 39·13734 inches, as deduced from his own observations there. It is not my intention to make any remark on those observations; which, indeed, appear to have been made with all due regard to accuracy: but, I trust I may be allowed, whilst treating on a subject of this kind, to express my dissent from the mode in which he has deduced the result in question. In all cases of the convertible pendulum, either the perfect synchronism of the two knife edges, or (which will answer the same purpose), the difference in the results of the two knife edges, ought to be well established, by an *equal weight* of evidence for *each* knife edge. This is indispensable: and, unless it be accomplished, the problem cannot be considered as strictly solved. Each knife edge is independent of the other; and each ought to have equal weight in the determination of the result. It is true that the knife edge A (or that position of the pendulum where the great weight is below the axis of suspension), will, in case of any difference, always give a result nearer to the true value than the knife edge B: but, the proper correction to be applied to make them synchronous, can only be determined by first giving to B an equal weight in the experiments. Now, perfect synchronism I consider unattainable; or, at all events, not worth the trouble it would cost to pursue it: since the small difference which arises, in these cases, will always enable us to apply the proper correction, from the known principles of the pendulum; and which are a more sure guide on such occasions than any partial determination of the correction from actual experiment, where, in these minute inquiries, the errors of observation are sure to baffle us in our object.

* Memoirs of the Astronomical Society, vol. iii. page 289.

Captain SABINE however has preferred trusting to actual experiment for this minute correction: and, considering that the result shown by the knife edge A is the nearest to the truth, he has rested on the establishment of that result, without the requisite corroboration, by an *equal number* of trials, from the other knife edge; which are, in fact, equally essential to the establishment of the accuracy of the whole. Thus, he has swung the pendulum 188 hours on the knife edge A, and only 54 hours on the knife edge B. But, had this latter knife edge been employed during a longer period, it might probably have tended to correct the anomaly that occurs on the face of the observations. For, it appears that when the slider was moved about ·133 inch, it caused an *increase* of 0·10 vibration in a day, on the knife edge A; whilst it caused a *decrease* of 1·12 vibration on the knife edge B. But, this is contrary to the known principles of the pendulum, since the effect of a slider of this sort is to cause an alteration of the *same kind* in *each* knife edge, differing only in degree: the relative proportions of which may he ascertained by determining the distance of the centre of gravity from each knife edge *. In the state of the pendulum in question, when last used by Captain SABINE for the experiments here alluded to (the tail pieces being wholly removed), the distance of the centre of gravity from the knife edge A, I found by actual measurement, to be 26·23 inches; and from the knife edge B, 13·21 inches. We have therefore $\frac{26\cdot23}{13\cdot21} =$ 1·985 as the factor by which any alteration in the results of knife edge A must be multiplied, in order to show the corresponding alteration produced in the knife edge B: which will be *both* positive, or *both* negative. And if this is not shown by the experiment, we may reasonably suspect some error in the observations. Also, we have $\frac{13\cdot21}{26\cdot23 - 13\cdot21} = 1\cdot015$ as the factor by which the difference in the number of vibrations between A and B must be multiplied, to obtain the correction that should be applied to A, in order to ascertain the number of vibrations that the pendulum would make, if rendered perfectly synchronous: and which is the quantity to be used in determining the length

* The truth of this would have been shown, and the absolute amount easily determined, had Captain SABINE moved the slider through a larger space (one or two inches, for instance), so as to have produced a decided and powerful effect on the number of vibrations; sufficient to counterbalance the unavoidable errors of observation.

of the seconds pendulum. In all such cases, however, it is presumed that the two knife edges are adjusted *very nearly* to synchronism. If we apply these principles to Captain SABINE's results, we shall have the following values for the number of vibrations if the pendulum were rendered perfectly synchronous.

Slider.	A.	B.	(A—B)	If synchronous.
1·500	86069·00	86070·26	—1·26	86067·72
1·566	69·04	69·61	—0·57	68·46
1·633	69·10	69·14	—0·04	69·06

There is a difference, in these three values, of 6 and 7 tenths of a vibration; and if one is to be preferred to the other, it should be that which is the result of the greatest number of experiments, which appears to be the second value here given. But, they all want the requisite corroboration of the knife edge B.

Method of observing and of reducing the Observations.

Before I conclude this paper, it may be proper to say a few words on the method employed in making the experiments above alluded to, and of the data used in the reduction of the observations, in order that the circumstances, under which each experiment has been made, may fully appear, and that each step of the computations may be verified at pleasure.

The clock used for observing the coincidences is an excellent one made by MOLYNEUX, having a mercurial pendulum, with a long tail piece, furnished with two circular segments of gilt paper, which reflect a very brilliant light: the distance between these segments is variable at pleasure, in order to suit the size of the different pendulums under experiment. The rate of the clock is ascertained by a daily comparison with another clock (made by HARDY) regulated to sidereal time; the rate of which is determined by means of a 30-inch transit instrument. Both these clocks go very well; and with respect to the experiments detailed in this paper, which are merely *comparative*, do not afford the source of any appreciable error. The clock used with the experiments, and which I shall, for the sake of distinction, call the Pendulum-clock, has been in all the cases, regulated to mean solar time; except when used with the long cylindrical rod (No. 21) and with the long brass tube (No. 35—38); where it was necessary to alter the length of the clock pendulum in order to obtain convenient intervals for the coincidences. The daily rate of the pendulum clock has always been kept very low, for very obvious reasons: it has, in no case, exceeded $0^s,80$ in a day *.

Let t denote the total interval of time, expressed in seconds, employed in any given series, as shown by the pendulum clock, making $(86400 + r)$ vibrations in a mean solar day; r being the daily rate of the clock, which will be *minus* when losing: and let n denote the number of coincidences (always including the first) that have taken place during that interval. Then will $\frac{t}{n-1}$ be the time of the *mean interval* of the coincidences, expressed in seconds of the clock, which I shall denote by m: and the number of vibrations (N) made by the pendulum of experiment, in a mean solar day, will be

$$N = \frac{m \pm 2}{m} (86400 + r) = \frac{m \pm 2}{m} 86400 + r \left(1 \pm \frac{2}{m}\right)$$

where the upper sign is to be taken when the pendulum of experiment goes faster than the clock; and the lower sign when it goes slower. All the pendulums enumerated in this paper, from No. 1 to No. 20 inclusive, go faster than the clock, and consequently the upper sign must be used in the computations. All the bar pendulums from No. 25 to No. 34, and the pendulums No. 40 and 41, go slower than the clock; and therefore the lower sign must be adopted in those cases. In all my reductions, however, I have made N equal to $\frac{m \pm 2}{m} 86400$ only; and have afterwards applied $r \left(1 \pm \frac{2}{m}\right)$ as a separate correction for the rate of the clock. For the long cylindrical rod (No. 21) a special computation was made: and in the case of the cylindrical tube (No. 35—38) the pendulum clock was adjusted so as to make 90000 vibrations in a day: and the correction for the variation from that rate, applied afterwards.

In noting the coincidences I adopt the plan suggested by Professors Airy and Whewell, and always observe the first and last disappearance and the first and last reappearance of the luminous disc: the mean of the four is the

* A sudden change may sometimes be noticed between some of the series of observations: but this has occurred when the pendulum of experiment has been changed, and when it was necessary to stop the clock, in order to alter the luminous disc. In some cases where the variation in the daily rate has been an appreciable quantity, I have proportioned it, in the different experiments during the day, according to the intervals.

true time of the coincidence*. This is obviously the most correct mode of proceeding: more so than by observing only one disappearance, and one reappearance; and much more so than by observing the disappearance only or the reappearance only. It has also this convenience, that it obviates the necessity of attending to the minute adjustment of the diaphragm; and the eye is not in such case obliged to wander from one side of the pendulum to the other, doubting on which side the disappearance or reappearance will take place. I consider this part of the experiment as perfect: and that no appreciable error can occur when this mode of observing the coincidences is adopted†. In the detail of the experiments, the two moments of disappearance are written one over the other, with a line between, similar to a fractional quantity; and the same, with the reappearances: the mean of the four is annexed in the subsequent collateral column. Much has been said about the inutility of observing more than one of these phænomena; at which I must confess, I have been somewhat surprised‡. It is perhaps possible that, if the *same* person always made the observations, always under the *same* circumstances, always with the *same* magnitude of the disc, always with the *same* extent of the arc (and that not very small,) and always with precisely the *same* quantity of light, no great difference might be found in the results. But, as these are cases never likely to occur in practice, and from the nature of the subject must be perpetually varying, it is better to adopt a general and sure guide for determining the moment of coincidence: and had I not pursued this plan, I should in many instances have been led into considerable error.

The arc of vibration has always been observed by means of a diagonal scale affixed to the clock case; and the divisions can be easily read off to the hundredth part of a degree. The scale is 7 inches distant from the pendulum,

* The experiments with the long cylindrical rod (No. 21) form an exception: as, in this case, only *one* side of the rod could be seen in the vacuum tube.

† I have also adopted another suggestion of Professors AIRY and WHEWELL, by removing the diaphragm from the inside of the telescope, and placing it between the pendulum of experiment and the clock pendulum. It is, in fact, attached to the clock case; and is not only capable of being moved in every direction, for the purpose of adjustment, but also of being enlarged or contracted, to suit the different pendulums employed.

‡ See Philosophical Transactions for 1826, page 4 &c.: and the same volume Part II. page 2 &c., containing Lieut. FOSTER's experiments on the pendulum. See also (*contra*) Captain SABINE's Account of Experiments, pages 217—233.

and a proper correction has, in each case, been applied to the arc for the proportion which this distance bears to the distance of the telescope from the pendulum. The values in the Table are the readings thus corrected.

All the pendulums have been reduced to a common standard of temperature, which I have assumed equal to 62°. As I had no means of determining the expansion of the different metals, I have adopted such as I have considered most worthy of confidence. Any error arising from this source can be but trifling; as no considerable change of temperature has ever occurred during any two consecutive experiments. In the suspension by the iron and silver wire, I have taken into account the small piece of brass rod (about $1\frac{1}{2}$ inch,) attached to the knife edge, and also the radius of the sphere. The following are the assumed rates of expansion for 1° of Fahrenheit's thermometer: viz.

Iron wire, &c.	= ·000006666
Iron bar	= ·000006850
Copper bar	= ·000009444
Brass bar	= ·000010000
Silver wire, &c.	= ·000010600

The rate of expansion being denoted by e, the formula for the correction of the number of vibrations, on account of the temperature, will be

$$N \times \tfrac{1}{2} e (t^\circ - 62^\circ)$$

where t° denotes the mean height of the thermometer, during the interval of the coincidences. The mercurial pendulum (No. 39,) and the wooden rod pendulums (No. 40 and 41,) being compensation pendulums, do not require any correction for temperature.

For determining the temperature I have always used two excellent standard thermometers, made under Mr. Troughton's immediate inspection. These are placed inside the vacuum apparatus*; one of them on a level with the axis of suspension, and the other on a level with the centre of oscillation of the inclosed pendulum: the lower one can be read through the glass window of

* In a few of the experiments, before I had contrived a method of suspending the lower thermometer in the inside of the tube, it was placed in a similar position (as to the centre of oscillation) on the outside. The inner thermometer however has, in all such cases, been used in the reductions; adding ·05 to the mean height: this being half the quantity by which the outer thermometer exceeded the other.

the tube. When the air is exhausted from the tube, I have, in computing the corrections for temperature, added 0·75 to the mean of the thermometers, to compensate for the effect produced on the thermometers by the removal of the pressure of the atmosphere; as indicated by Captain SABINE in the Philosophical Transactions for 1829, page 214: this being the amount by which these thermometers were also affected by such removal. In the detail of the experiments, inserted in the *Appendix* to this paper, the readings of the thermometer are given, without this correction. In recording the barometer, the correction for capillarity is always included: but when the vacuum tube is exhausted, a syphon gauge is employed to indicate the pressure of the atmosphere, and no correction is required.

The subjoined *Appendix* consists of two Tables, in the first of which is given a detail of all the particulars (copied from the Observation-books,) requisite for deducing the corrections: and in the second of which is given the amount of those corrections under their respective heads. Table I. shows the time of the first and the last coincidence; the magnitude of the arc of vibration and the height of the barometer at those times respectively; the highest and lowest readings of the two thermometers, and the daily rate of the clock during the interval of each experiment: the number and date of which are always annexed. Table II. contains 1°. the corresponding number of each experiment in the preceding Table, for the sake of a convenient reference: 2°. the total interval of the experiment: 3°. the number of coincidences (minus unity) that have occurred: 4°. the mean interval expressed in seconds of the pendulum clock: 5°. the amount of the corrections for the arc, the thermometers, and the daily rate of the clock: and lastly the number of vibrations, N' or N'', (according as the experiments were made in air or in vacuo,) in a mean solar day, exclusive of the correction for the pressure of the atmosphere, which is the quantity sought in the present inquiries. In the latter part of this Table, however, viz. from experiment 205 to 230 both inclusive, the correction for the barometer is added, and the last column then contains the true number of vibrations in a mean solar day, including the correction for the pressure of the atmosphere: for, these experiments are of a totally different kind, and are inserted to show the effect produced merely by reversing the face of the pendulum, as alluded to in page 467.

APPENDIX.

TABLE I.—*Detail of the Experiments.*

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
No. 1. Platina Sphere.	1	Feb. 21	20 51 $\frac{53}{56}$	$\frac{59}{62}$	57,5	0·77	38·4	38·4	30·256	—0,16
			1 5 $\frac{5}{8}$	$\frac{11}{16}$	10,0	·29	39·0	39·1	30·228	
	2	Feb. 21	2 6 $\frac{53}{54}$	$\frac{59}{60}$	56,5	·77	38·6	39·1	0·860	—0,18
			11 50 $\frac{1}{2}$	$\frac{9}{12}$	6,0	·35	39·5	39·5	1·080	
	3	Feb. 21	11 51 $\frac{41}{42}$	$\frac{47}{48}$	44,5	·83	39·7	39·5	1·080	—0,20
			22 16 $\frac{7}{8}$	$\frac{20}{21}$	14,0	·34	38·8	38·5	1·250	
	4	Feb. 22	23 1 $\frac{35}{36}$	$\frac{43}{44}$	39,5	·82	39·8	39·7	30·374	—0,25
			4 30 $\frac{32}{35}$	$\frac{56}{58}$	45,2	·22	39·2	39·0	30·364	
	5	Feb. 23	20 9 $\frac{29}{32}$	$\frac{37}{40}$	34,5	·81	38·6	38·5	30·396	—0,30
			1 13 $\frac{17}{18}$	$\frac{33}{34}$	25,5	·24	38·5	38·4	30·344	
	6	Feb. 23	2 44 $\frac{23}{24}$	$\frac{29}{32}$	27,0	·82	38·6	38·6	1·020	—0,35
			11 45 $\frac{37}{40}$	$\frac{45}{48}$	42,5	·39	37·9	37·5	1·200	
	7	Feb. 23	11 46 $\frac{57}{60}$	$\frac{63}{66}$	61,5	·82	38·0	37·6	1·210	—0,40
			21 37 $\frac{49}{50}$	$\frac{59}{60}$	54,5	·34	37·0	37·0	1·360	
	8	Feb. 24	22 22 $\frac{25}{28}$	$\frac{33}{36}$	30,5	·81	38·0	38·0	30·154	—0,45
			2 1 $\frac{49}{50}$	$\frac{59}{60}$	54,5	·36	37·6	37·5	30·074	
No. 3. Small Brass Sphere.	9	Feb. 24	2 37 $\frac{17}{18}$	$\frac{23}{24}$	20,5	·82	38·9	38·5	30·064	—0,50
			5 11 $\frac{47}{52}$	$\frac{53}{62}$	53,5	·14	38·2	37·9	30·014	
	10	Feb. 24	5 53 $\frac{55}{58}$	$\frac{61}{64}$	59,5	·82	37·5	37·3	1·080	—0,50
			12 41 $\frac{55}{68}$	$\frac{63}{76}$	65,5	·14	37·0	36·7	1·250	
	11	Feb. 24	12 44 $\frac{15}{16}$	$\frac{19}{20}$	17,5	·96	37·4	37·2	1·250	—0,50
			19 24 $\frac{17}{34}$	$\frac{27}{42}$	30,0	·13	36·1	35·8	1·340	
	12	Feb. 25	20 12 $\frac{21}{22}$	$\frac{27}{28}$	24,5	·96	37·4	37·5	30·040	—0,45
			22 14 $\frac{15}{22}$	$\frac{23}{30}$	22,5	·23	37·0	36·7	30·090	
	13	Feb. 25	22 57 $\frac{9}{12}$	$\frac{15}{18}$	13,5	·92	37·4	37·2	30·100	—0,43
			1 15 $\frac{29}{42}$	$\frac{37}{48}$	39,0	·19	37·0	37·0	30·118	
	14	Feb. 25	2 47 $\frac{33}{34}$	$\frac{39}{40}$	36,5	·94	36·7	36·9	0·930	—0,41
			11 57 $\frac{3}{34}$	$\frac{15}{46}$	24,5	·10	37·1	37·2	1·090	
	15	Feb. 25	11 59 $\frac{43}{46}$	$\frac{45}{50}$	46,0	·92	37·2	37·1	1·090	—0,40
			21 32 $\frac{39}{62}$	$\frac{63}{88}$	63,0	·08	36·9	36·6	1·240	
	16	Feb. 26	22 15 $\frac{35}{36}$	$\frac{39}{42}$	38,0	·96	38·0	38·1	30·210	—0,40
			0 25 $\frac{53}{60}$	$\frac{61}{66}$	60,0	·19	37·9	37·9	30·184	

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s					s
No. 2. Small Lead Sphere.	17	March 5	0 51 39/42 3 42 1/4	49/50 31/34	45,0 17,5	0°·78 ·20	46°·0 45·9	46°·1 45·8	29·898 29·964	—0,06
	18	March 5	4 48 27/28 12 0 43/52	39/40 71/80	33,5 61,5	·76 ·22	45·1 44·5	45·1 44·8	0·870 1·010	—0,06
	19	March 5	12 5 19/20 20 39 25/32	29/30 67/74	24,5 49,5	·77 ·16	44·5 42·5	44·9 42·9	1·010 1·130	—0,03
	20	March 6	21 21 5/6 1 7 21/28	15/18 71/78	11,0 49,5	·75 ·17	43·5 43·2	43·5 43·0	29·764 29·534	0,00
	21	March 6	1 11 59/60 4 11 15/22	69/70 55/62	64,5 38,5	·77 ·19	43·3 43·5	43·0 43·5	29·534 29·384	0,00
	22	March 6	5 4 17/18 12 7 17/22	33/34 45/52	25,5 34,0	·72 ·20	42·8 43·7	43·1 43·5	0·930 1·060	0,00
	23	March 6	12 9 5/8 20 25 17/26	11/14 53/60	9,5 39,0	·78 ·19	43·7 42·6	43·5 42·2	1·060 1·190	0,00
	24	March 7	21 3 13/16 0 2 31/38	21/24 63/70	18,5 50,5	·80 ·20	43·8 43·5	43·7 43·2	29·422 29·418	0,00
No. 4. Small Ivory Sphere.	25	May 10	0 31 27/28 1 15 29/40	29/30 31/42	28,5 35,5	·77 ·12	53·5 53·5	53·3 53·3	30·420 30·428	—0,43
	26	May 10	2 17 53/56 4 1 31/38	55/58 33/40	55,5 35,5	·77 ·19	52·6 52·9	52·5 52·8	1·040 1·140	—0,43
	27	May 11	20 16 52/53 22 0 26/39	58/59 56/67	55,5 47,0	·78 ·16	50·2 50·5	49·8 50·5	2·070 2·150	—0,46
	28	May 11	22 58 32/33 23 49 32/46	38/39 66/77	35,5 55,2	·78 ·12	51·5 51·7	51·7 51·7	30·310 30·284	—0,46
	29	May 12	18 52 39/40 19 43 41/48	47/48 89/96	43,5 68,5	·78 ·12	51·5 51·5	51·1 51·3	30·014 30·004	—0,48
	30	May 12	20 26 55/56 22 42 51/56	61/62 91/94	58,5 73,0	·79 ·12	50·6 51·0	50·1 50·8	1·000 1·130	—0,48
	31	May 12	22 46 33/34 1 2 39/44	41/42 77/84	37,5 61,0	·79 ·12	51·0 51·4	50·8 51·4	1·130 1·280	—0,48
	32	May 12	1 57 37/38 2 48 51/56	45/46 93/98	41,5 74,5	·78 ·11	52·4 52·5	52·5 52·5	29·910 29·894	—0,48
No. 6. Large Brass Sphere.	33	Feb. 13	23 5 39/40 2 0 19/30	49/50 47/56	44,5 38,0	·88 ·34	42·0 41·7	42·0 41·4	30·118 30·100	—0,52
	34	Feb. 13	3 17 51/52 12 1 17/30	61/62 35/46	56,5 32,0	·96 ·36	41·0 40·7	41·4 40·5	0·910 1·050	—0,56
	35	Feb. 13	12 5 45/46 20 48 35/46	51/52 59/72	48,5 53,0	·87 ·27	40·8 40·1	40·5 39·9	1·050 1·150	—0,60
	36	Feb. 14	21 38 33/34 2 19 33/54	43/43 59/80	38,5 56,0	·91 ·16	41·3 40·7	41·2 40·5	30·088 30·060	—0,63
	37	Feb. 14	2 32 14/15 6 44 33/50	22/23 53/68	18,5 51,0	·96 ·19	41·0 40·6	40·8 40·5	30·058 30·064	—0,63
	38	Feb. 14	11 12 42/43 20 32 33/42	49/50 49/58	46,0 45,5	·96 ·32	40·1 37·9	39·6 37·3	0·960 1·090	—0,63
	39	Feb. 15	*20 32 33/42 23 47 49/66	49/58 63/80	45,5 64,5	·32 ·19	37·9 37·5	37·3 37·0	1·090 1·140	—0,63
	40	Feb. 15	0 37 53/54 3 32 9/18	60/61 25/32	57,0 21,0	·96 ·34	38·5 38·2	38·4 38·0	30·050 30·028	—0,63

* The preceding series continued.

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s					s
No. 7. Large Ivory Sphere.	41	Feb. 16	23 55 $\frac{38}{41}$ 1 1 $\frac{42}{45}$	$\frac{46}{47}$ $\frac{62}{67}$	43,0 54,0	0°·89 ·19	35°·8 35·8	35°·7 35·8	29·804 29·780	—0,60
	42	Feb. 17	20 17 $\frac{39}{40}$ 22 50 $\frac{27}{34}$	$\frac{52}{53}$ $\frac{37}{42}$	46,0 35,0	·96 ·22	36·0 36·4	36·0 36·4	0·890 0·940	—0,64
	43	Feb. 17	22 53 $\frac{38}{41}$ 1 26 $\frac{31}{36}$	$\frac{44}{47}$ $\frac{39}{44}$	42,5 37,5	·96 ·21	36·4 36·9	36·4 37·0	0·940 1·000	—0,66
	44	Feb. 17	2 3 $\frac{18}{21}$ 3 9 $\frac{31}{42}$	$\frac{22}{25}$ $\frac{45}{56}$	21,5 43,5	·96 ·15	38·1 38·1	38·4 38·3	29·842 29·864	—0,68
	45	Feb. 18	20 34 $\frac{34}{37}$ 21 40 $\frac{57}{72}$	$\frac{42}{45}$ $\frac{75}{78}$	39,5 73,0	·94 ·16	39·4 39·5	39·2 39·5	30·202 30·220	—0,72
	46	Feb. 18	22 35 $\frac{36}{39}$ 1 16 $\frac{51}{56}$	$\frac{42}{45}$ $\frac{59}{64}$	40,5 57,5	·98 ·20	38·7 39·6	38·6 39·8	0·920 1·000	—0,73
	47	Feb. 20	22 30 $\frac{20}{21}$ 1 11 $\frac{47}{54}$	$\frac{24}{27}$ $\frac{59}{64}$	23,0 56,0	·98 ·19	37·9 37·9	37·5 37·6	1·540 1·570	—0,80
	48	Feb. 20	1 58 $\frac{30}{33}$ 3 4 $\frac{53}{62}$	$\frac{36}{39}$ $\frac{65}{74}$	34,5 63,5	·96 ·17	38·8 38·8	38·7 38·7	30·326 30·324	—0,80
No. 5. Large Lead Sphere.	49	March 8	0 4 $\frac{27}{28}$ 5 25 $\frac{7}{14}$	$\frac{41}{42}$ $\frac{41}{48}$	34,5 27,5	·86 ·21	42·9 42·8	43·0 42·5	29·606 29·714	—0,23
	50	March 8	6 4 $\frac{59}{60}$ 12 31 $\frac{17}{18}$	$\frac{69}{72}$ $\frac{33}{36}$	65,0 26,0	·82 ·47	42·0 41·5	42·0 41·0	1·000 1·170	—0,23
	51	March 8	12 33 $\frac{27}{28}$ 20 3 $\frac{23}{24}$	$\frac{39}{40}$ $\frac{45}{46}$	33,5 34,5	·84 ·41	41·7 39·7	41·4 39·0	1·170 1·270	—0,22
	52	March 9	20 42 $\frac{6}{9}$ 1 6 $\frac{50}{51}$	$\frac{19}{22}$ $\frac{69}{70}$	14,0 60,0	·80 ·29	40·6 40·2	40·4 39·8	30·054 30·124	—0,22
	53	March 9	12 5 $\frac{21}{22}$ 19 48 $\frac{41}{42}$	$\frac{33}{36}$ $\frac{35}{36}$	28,0 38,5	·86 ·10	41·5 39·0	41·1 39·0	30·340 30·398	—0,22
	54	March 10	20 28 $\frac{1}{2}$ 4 51 $\frac{1}{2}$	$\frac{12}{13}$ $\frac{21}{22}$	7,0 11,5	·87 ·39	38·9 40·0	38·5 40·0	0·950 1·150	—0,22
	55	March 10	4 52 $\frac{19}{20}$ 12 43 $\frac{45}{46}$	$\frac{31}{32}$ $\frac{63}{66}$	25,5 55,0	·78 ·40	40·0 40·2	40·0 39·9	1·150 1·270	—0,23
	56	March 11	21 15 $\frac{51}{54}$ 2 46 $\frac{49}{50}$	$\frac{65}{68}$ $\frac{77}{88}$	59,5 63,5	·83 ·20	40·0 40·0	39·7 39·8	30·208 30·126	—0,24
No. 9. Large Ivory Sphere.	57	April 9	19 40 $\frac{8}{11}$ 20 52 $\frac{52}{73}$	$\frac{24}{29}$ $\frac{120}{141}$	18,0 96,5	·87 ·11	49·5 49·5	49·0 49·4	30·254 30·260	+0,20
	58	April 9	21 59 $\frac{2}{5}$ 0 37 $\frac{0}{11}$	$\frac{16}{19}$ $\frac{38}{51}$	10,5 25,0	·91 ·21	48·8 49·5	48·5 49·5	1·080 1·180	+0,20
	59	April 9	0 41 $\frac{38}{41}$ 3 37 $\frac{25}{26}$	$\frac{50}{55}$ $\frac{72}{73}$	46,0 49,0	·91 ·14	49·5 50·7	49·5 50·6	1·180 1·280	+0,20
	60	April 9	4 10 $\frac{54}{57}$ 5 23 $\frac{47}{50}$	$\frac{68}{73}$ $\frac{115}{120}$	63,0 83,0	·89 ·11	52·0 52·0	52·2 52·0	30·230 30·232	+0,20
No. 8. Large Lead Sphere.	61	April 10	20 3 $\frac{54}{55}$ 23 17 $\frac{29}{40}$	$\frac{66}{67}$ $\frac{51}{64}$	60,5 46,0	·87 ·20	48·8 49·0	48·3 48·5	30·268 30·244	+0,23
	62	April 10	0 10 $\frac{52}{53}$ 5 26 $\frac{45}{46}$	$\frac{62}{63}$ $\frac{61}{64}$	57,5 54,0	·90 ·38	48·3 50·0	48·3 50·0	0·930 1·170	+0,23
	63	April 10	5 28 $\frac{7}{8}$ 12 18 $\frac{1}{4}$	$\frac{16}{17}$ $\frac{27}{30}$	12,0 15,5	·90 ·36	50·0 49·5	50·0 49·0	1·170 1·430	+0,23
	64	April 11	20 3 $\frac{12}{13}$ 23 16 $\frac{1}{4}$	$\frac{22}{23}$ $\frac{23}{28}$	17,5 14,0	·90 ·19	48·1 48·5	47·5 48·3	30·214 30·184	+0,20

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s					s
No. 10. 2-inch Brass Cylinder.	65	March 21	21 37 $\frac{6}{7}$ 1 44 $\frac{35}{36}$	$\frac{17}{18}$ $\frac{64}{65}$	12,0 50,0	0°97 ·23	47°1 47·7	47°1 47·7	30·060 30·114	+0,60
	66	March 21	2 47 $\frac{33}{34}$ 11 44 $\frac{27}{28}$	$\frac{44}{45}$ $\frac{47}{48}$	39,0 37,5	·90 ·38	47·2 48·5	47·2 48·3	0·790 0·960	+0,60
	67	March 21	11 47 $\frac{17}{18}$ 20 2 $\frac{49}{50}$	$\frac{27}{28}$ $\frac{69}{70}$	22,5 59,5	·93 ·38	48·7 48·0	48·5 47·6	0·960 1·060	+0,57
	68	March 22	20 49 $\frac{27}{28}$ 0 14 $\frac{24}{25}$	$\frac{40}{41}$ $\frac{46}{47}$	34,0 35,5	·96 ·29	48·8 49·0	48·6 49·0	30·178 30·164	+0,57
No. 11. 2-inch Brass Cylinder.	69	April 16	21 5 $\frac{7}{10}$ 23 51 $\frac{19}{40}$	$\frac{25}{30}$ $\frac{69}{92}$	18,0 55,0	·83 ·24	55·0 54·5	54·5 54·3	30·056 30·050	+0,50
	70	April 16	0 54 $\frac{31}{34}$ 4 46 $\frac{19}{28}$	$\frac{49}{52}$ $\frac{47}{56}$	41,5 37,5	·86 ·43	54·0 55·2	54·0 55·3	1·700 1·880	+0,50
	71	April 16	4 49 $\frac{5}{6}$ 8 53 $\frac{25}{36}$	$\frac{21}{24}$ $\frac{63}{74}$	14,0 49,5	·85 ·42	55·2 55·5	55·3 55·3	1·880 2·060	+0,50
	72	April 16	9 33 $\frac{27}{30}$ 11 54 $\frac{39}{58}$	$\frac{47}{52}$ $\frac{80}{104}$	39,0 70,2	·83 ·27	56·5 56·0	56·6 55·5	30·086 30·080	+0,50
No. 12. 2-inch Brass Cylinder.	73	April 17	20 52 $\frac{41}{44}$ 22 52 $\frac{1}{16}$	$\frac{75}{80}$ $\frac{81}{94}$	60,0 48,0	·89 ·32	53·5 53·2	53·1 53·0	30·074 30·056	+0,52
	74	April 17	0 0 $\frac{3}{6}$ 4 37 $\frac{13}{22}$	$\frac{31}{36}$ $\frac{65}{74}$	19,0 43,5	·91 ·46	52·3 53·1	52·0 53·2	1·190 1·370	+0,52
	75	April 17	4 39 $\frac{13}{16}$ 9 4 $\frac{25}{34}$	$\frac{43}{46}$ $\frac{75}{84}$	29,5 54,5	·90 ·46	53·1 53·5	53·2 53·3	1·370 1·540	+0,52
	76	April 17	9 45 $\frac{15}{16}$ 11 31 $\frac{35}{50}$	$\frac{43}{46}$ $\frac{103}{116}$	30,0 76,0	·90 ·38	54·4 53·9	54·5 53·5	29·972 29·946	+0,52
No. 13. 2-inch Brass Cylinder.	77	April 18	19 45 $\frac{37}{38}$ 23 50 $\frac{11}{32}$	$\frac{55}{56}$ $\frac{61}{82}$	46,5 46,5	·86 ·11	52·4 53·0	52·0 53·0	29·810 29·726	+0,60
	78	April 18	1 15 $\frac{29}{30}$ 11 30 $\frac{9}{24}$	$\frac{43}{44}$ $\frac{47}{60}$	36,5 35,0	·96 ·20	52·8 54·5	53·0 54·2	1·060 1·480	+0,60
	79	April 18	11 32 $\frac{31}{32}$ 19 44 $\frac{41}{56}$	$\frac{47}{48}$ $\frac{79}{90}$	39,5 66,5	·96 ·25	54·5 53·8	54·3 53·5	1·480 1·800	+0,60
	80	April 19	20 35 $\frac{25}{26}$ 23 0 $\frac{11}{20}$	$\frac{43}{44}$ $\frac{30}{50}$	34,5 30,0	·96 ·33	54·5 54·5	54·5 54·3	29·664 29·702	+0,60
No. 18. Hollow Brass Cylinder.	81	March 14	2 42 $\frac{34}{35}$ 3 59 $\frac{30}{31}$	$\frac{44}{45}$ $\frac{60}{61}$	39,5 45,5	·96 ·29	45·0 45·0	45·5 45·0	29·394 29·372	+0,49
	82	March 14	6 56 $\frac{42}{43}$ 11 3 $\frac{58}{59}$	$\frac{53}{54}$ $\frac{78}{79}$	48,0 68,5	·98 ·34	44·3 44·5	44·1 44·4	1·030 1·160	+0,49
	83	March 15	20 39 $\frac{32}{33}$ 0 37 $\frac{50}{55}$	$\frac{46}{47}$ $\frac{68}{73}$	39,5 61,5	·87 ·29	43·0 43·0	42·6 42·9	1·250 1·290	+0,45
	84	March 15	1 27 $\frac{8}{9}$ 3 22 $\frac{18}{33}$	$\frac{20}{21}$ $\frac{70}{85}$	14,5 51,5	·96 ·20	44·1 44·0	44·1 44·0	29·574 29·628	+0,45
No. 15. Hollow Brass Cylinder.	85	March 16	20 36 $\frac{6}{7}$ 22 8 $\frac{44}{57}$	$\frac{18}{19}$ $\frac{74}{83}$	12,5 64,5	·97 ·18	42·0 41·8	41·5 41·5	29·840 29·836	+0,58
	86	March 16	22 48 $\frac{6}{7}$ 1 56 $\frac{40}{43}$	$\frac{14}{15}$ $\frac{56}{59}$	10,5 49,5	·87 ·36	40·8 41·4	40·5 41·3	0·950 1·090	+0,58
	87	March 16	1 58 $\frac{22}{23}$ 5 7 $\frac{10}{17}$	$\frac{32}{33}$ $\frac{28}{33}$	27,5 22,0	·96 ·40	41·4 41·8	41·3 41·8	1·090 1 150	+0,58
	88	March 16	7 42 $\frac{46}{49}$ 9 7 $\frac{48}{51}$	$\frac{56}{57}$ $\frac{74}{79}$	52,0 63,0	·94 ·17	42·8 43·0	42·5 43·0	29·672 29·610	+0,58

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
No. 16. Hollow Brass Cylinder.	89	March 17	20 16 56/57 22 15 14/39	72/73 44/77	64,5 43,5	0·90 ·15	44·6 45·4	44·5 45·4	29·448 29·464	+0,66
	90	March 17	0 58 9/10 5 4 36/51	20/21 72/83	15,0 60,5	·98 ·28	45·6 46·5	46·0 46·4	1·990 2·060	+0,66
	91	March 17	5 7 11/12 8 54 50/63	25/26 74/87	18,5 68,5	1·01 0·31	46·5 46·5	46·4 46·0	2·060 2·090	+0,66
	92	March 17	9 51 25/28 11 49 56/75	39/40 86/109	33,0 81,5	·97 ·16	47·3 47·0	47·0 46·4	29·564 29·564	+0,66
No. 17. Hollow Brass Cylinder.	93	March 19	20 28 23/29 22 8 50/67	37/38 72/89	33,0 69,5	·97 ·15	45·0 45·0	44·5 44·8	29·882 29·872	+0,43
	94	March 19	22 54 7/8 1 57 2/5	16/17 12/17	12,0 9,0	·98 ·41	44·2 45·1	44·0 45·3	1·200 1·290	+0,43
	95	March 20	0 33 9/10 4 2 38/45	18/19 52/59	14,0 48,5	·98 ·37	46·1 46·6	46·0 46·6	1·570 1·640	+0,54
	96	March 20	4 37 0/1 6 10 8/21	8/9 56/67	4,5 38,0	·98 ·27	47·8 47·8	48·0 47·8	29·806 29·864	+0,54
No. 14. Solid Lead Cylinder.	97	April 4	12 16 0/1 20 1 19/36	16/17 67/84	8,5 51,5	·78 ·11	55·8 54·4	55·5 54·0	30·562 30·520	+0,50
	98	April 5	20 54 56/57 4 33 32/33	72/73 50/53	64,5 42,0	·77 ·47	53·6 56·5	53·6 56·8	0·950 1·190	+0,60
	99	April 5	* 4 33 32/33 10 54 43/50	50/53 69/74	42,0 59,0	·47 ·33	56·5 57·2	56·8 57·0	1·190 1·360	+0,60
	100	April 5	11 52 25/26 19 54 15/28	41/44 63/84	34,0 47,5	·77 ·10	58·0 55·5	58·0 55·5	30·464 30·446	+0,60
No. 19. Hollow Brass Cylinder.	101	April 13	19 36 7/10 20 48 35/48	17/20 61/74	13,5 54,5	·96 ·26	48·1 48·2	47·6 48·0	29·819 29·829	+0,20
	102	April 13	22 1 49/50 1 7 5/12	59/60 23/30	54,5 17,5	·98 ·36	47·6 48·9	47·5 48·9	1·130 1·340	+0,20
	103	April 14	19 20 19/20 22 32 37/44	27/28 55/62	23,5 49,5	·97 ·37	48·9 48·9	48·4 48·7	1·070 1·260	+0,30
	104	April 14	23 30 17/18 0 59 15/30	29/30 45/60	23,5 37,5	·98 ·22	50·2 50·5	50·3 50·4	30·176 30·178	+0,30
No. 20. Lead Lens.	105	March 22	3 32 30/31 7 38 36/41	46/47 74/79	38,5 57,5	·77 ·25	51·1 51·0	51·5 50·6	30·132 30·114	+0,52
	106	March 22	11 3 33/34 20 7 20/25	45/46 48/53	39,5 36,5	·88 ·26	50·4 49·9	50·2 49·5	1·030 1·160	+0,52
	107	March 23	1 18 3/4 11 59 36/41	17/18 76/79	10,5 58,0	·78 ·19	50·2 50·0	50·2 49·4	1·230 1·380	+0,44
	108	March 24	20 22 3/4 0 27 6/11	16/17 34/39	10,0 22,5	·77 ·24	47·8 47·2	47·0 46·8	29·870 29·878	+0,44
No. 21. Long Cylindrical Rod.	109	April 26	20 26 54 22 18 30	55 50	54,5 40,0	·87 ·06	51·6 51·4	51·5 51·0	29·800 29·794	The clock pendulum altered, so that it made 86045·291 seconds in a mean solar day.
	110	April 26	23 10 14 2 33 34	15 39	14,5 36,5	·90 ·21	50·4 50·5	50·0 50·4	0·960 1·140	
	111	April 26	2 35 6 5 36 58	7 65	6,5 61,5	·89 ·21	50·5 50·6	50·4 50·5	1·140 1·280	
	112	April 26	6 15 4 8 17 6	5 33	4,5 19,5	·89 ·06	51·6 51·3	51·9 51·0	29·844 29·864	

* The preceding series continued.

Table I.—Continued.

No. 25—26. Brass bar, $\frac{3}{8}$ inch thick.

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s					s
113	A	August 7	18 28 $\frac{49}{50}$ 2 33 $\frac{9}{10}$	$\frac{53}{54}$ $\frac{17}{20}$	51,5 14,0	0°·90 ·31	69°·6 70·9	69°·0 70·0	1·020 1·150	+0,23
114	A	August 7	3 28 $\frac{32}{33}$ 7 2 $\frac{9}{14}$	$\frac{37}{38}$ $\frac{51}{56}$	35,0 32,5	·98 ·14	71·4 71·3	71·0 71·0	29·698 29·730	+0,23
115	B	August 8	19 27 $\frac{29}{31}$ 22 19 $\frac{57}{60}$	$\frac{32}{34}$ $\frac{61}{66}$	31,5 61,0	·99 ·23	68·5 68·9	68·4 68·6	29·852 29·876	+0,23
116	B	August 8	23 5 $\frac{15}{16}$ 3 55 $\frac{9}{11}$	$\frac{17}{20}$ $\frac{12}{16}$	17,0 12,0	1·00 0·51	69·3 71·5	68·0 70·0	0·950 1·130	+0,20
117	B	August 8	4 15 $\frac{49}{50}$ 12 7 $\frac{47}{50}$	$\frac{53}{54}$ $\frac{52}{55}$	51,5 51,0	·97 ·35	71·8 71·4	70·3 70·6	* 0·650 0·830	+0,20
118	B	August 9	20 38 $\frac{25}{26}$ 23 44 $\frac{41}{42}$	$\frac{31}{32}$ $\frac{43}{45}$	28,5 42,7	·98 ·19	70·2 71·0	70·0 70·5	29·976 29·980	+0,15
119	A	August 21	23 52 $\frac{24}{27}$ 19 33 $\frac{53}{130}$	$\frac{32}{35}$ $\frac{173}{240}$	29,5 149,0	·98 ·03	65·0 63·5	64·0 63·2	1·290 1·510	—0,18
120	A	August 22	20 29 $\frac{8}{13}$ 0 49 $\frac{35}{58}$	$\frac{16}{19}$ $\frac{63}{92}$	14,0 62,0	·97 ·08	63·8 64·8	64·1 64·5	30·314 30·308	—0,18
121	A	August 22	0 51 $\frac{34}{37}$ 4 57 $\frac{1}{28}$	$\frac{40}{45}$ $\frac{41}{66}$	39,0 34,0	·97 ·09	64·8 66·2	64·4 65·6	30·308 30·274	—0,18
							† Upper.	† Lower.		
122	*a*	Dec. 5	2 6 $\frac{42}{43}$ 3 6 $\frac{33}{34}$	$\frac{61}{62}$ $\frac{77}{86}$	52,0 55,0	·79 ·42	49·0 48·9	48·8 48·5	29·764 29·748	+0,43
123	*a*	Dec. 5	6 50 $\frac{16}{19}$ 8 49 $\frac{55}{70}$	$\frac{30}{31}$ $\frac{109}{120}$	24,0 88,5	1·01 0·31	48·7 48·8	48·5 48·5	29·680 29·650	+0,45
124	*a*	Dec. 5	10 22 $\frac{51}{54}$ 20 46 $\frac{1}{54}$	$\frac{67}{70}$ $\frac{73}{120}$	60,5 62,0	·98 ·18	48·6 47·4	48·2 47·4	1·060 1·600	+0,50
125	*a*	Dec. 6	21 20 $\frac{4}{7}$ 23 34 $\frac{53}{76}$	$\frac{16}{19}$ $\frac{83}{106}$	11,5 79,5	·99 ·25	48·6 48·4	48·6 48·2	29·464 29·478	+0,52
126	*b*	Dec. 7	23 50 $\frac{52}{53}$ 1 5 $\frac{25}{26}$	$\frac{62}{65}$ $\frac{36}{37}$	58,0 31,0	·99 ·45	50·0 50·1	49·8 50·1	28·934 28·924	+0,63
127	*b*	Dec. 7	1 14 $\frac{2}{7}$ 4 12 $\frac{47}{62}$	$\frac{10}{15}$ $\frac{69}{86}$	8,5 66,0	·99 ·19	50·1 50·5	50·1 50·5	28·924 29·074	+0,70
128	*b*	Dec. 7	7 33 $\frac{36}{41}$ 21 37 $\frac{1}{22}$	$\frac{48}{51}$ $\frac{35}{58}$	44,0 29,0	1·01 0·10	50·0 49·9	49·9 49·7	0·890 1·560	+0,70
129	*b*	Dec. 8	22 30 $\frac{42}{47}$ 1 29 $\frac{9}{20}$	$\frac{52}{59}$ $\frac{31}{48}$	50,0 27,0	1·00 0·18	50·9 51·0	50·7 50·9	29·198 29·202	+0,70
130	*b*	Dec. 8	1 55 $\frac{8}{13}$ 5 8 $\frac{29}{48}$	$\frac{16}{21}$ $\frac{35}{52}$	14,5 41,0	1·01 0·16	51·1 51·5	51·0 51·3	29·206 29·260	+0,70
131	B	Dec. 9	21 2 $\frac{29}{32}$ 23 0 $\frac{43}{57}$	$\frac{35}{38}$ $\frac{70}{82}$	33,5 63,0	1·01 0·29	53·3 53·1	52·9 52·9	29·222 29·304	+0,70
132	B	Dec. 9	23 8 $\frac{21}{24}$ 1 21 $\frac{15}{50}$	$\frac{27}{30}$ $\frac{33}{66}$	25,5 41,0	1·01 0·27	53·1 53·3	52·9 53·0	29·304 29·244	+0,70
133	B	Dec. 9	2 12 $\frac{31}{34}$ ‡ 20 56 $\frac{}{53}$	$\frac{41}{46}$ $\frac{}{168}$	38,0 60,5	1·00 0·03	52·5 51·4	52·6 51·1	0·910 1·760	+0,70
134	B	Dec. 10	21 31 $\frac{0}{1}$ 0 58 $\frac{23}{80}$	$\frac{11}{12}$ $\frac{43}{100}$	6,0 61,5	1·00 0·14	53·0 52·5	52·9 52·5	29·454 29·534	+0,71

* Pumped out a little more air. † Both inside. ‡ Observed only on one side of the pendulum.

Table I.—Continued.

No. 25—26. Brass bar, $\frac{3}{8}$ inch thick (continued).

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s	°	°	°		s
135	B	Dec. 10	1 14 $\frac{45}{46}$ 4 42 $\frac{7}{68}$	$\frac{49}{50}$ $\frac{23}{86}$	47,5 46,0	1·01 0·13	52·6 52·7	52·5 52·6	29·546 29·626	+0,72
136	A	Dec. 12	21 20 $\frac{58}{59}$ 0 33 $\frac{5}{52}$	$\frac{63}{64}$ $\frac{37}{80}$	61,0 43,5	·99 ·15	53·0 52·8	53·0 52·6	29·240 29·086	+0,70
137	A	Dec. 12	0 38 $\frac{18}{21}$ 2 21 $\frac{45}{50}$	$\frac{24}{29}$ $\frac{61}{70}$	23,0 56,5	1·00 0·36	52·8 52·9	52·6 52·9	29·086 29·064	+0,70
138	A	Dec. 12	2 24 $\frac{28}{31}$ 5 7 $\frac{17}{23}$	$\frac{36}{41}$ $\frac{36}{42}$	34,0 29,5	·99 ·19	52·9 53·1	52·9 53·0	29·164 29·164	+0,66
139	A	Dec. 12	9 6 $\frac{16}{17}$ 22 29 $\frac{47}{80}$	$\frac{22}{23}$ $\frac{63}{96}$	19,5 71,5	·99 ·12	52·7 51·6	52·6 51·3	0·770 1·470	+0,64
140	A	Dec. 13	23 44 $\frac{6}{9}$ 2 12 $\frac{1}{14}$	$\frac{12}{17}$ $\frac{33}{46}$	11,0 23,5	·98 ·20	52·6 52·6	52·5 52·5	29·492 29·512	+0,62
141	A	Dec. 13	2 16 $\frac{54}{57}$ 4 44 $\frac{43}{76}$	$\frac{62}{67}$ $\frac{77}{112}$	60,0 77,0	·98 ·20	52·6 52·6	52·5 52·5	29·502 29·534	+0,58

No. 31—34. Brass bar, $\frac{3}{4}$ inch thick.

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Outside.	Inside.		
			h m s	s	s	°	°	°		s
142	A	Nov. 15	1 59 $\frac{15}{16}$ 4 53 $\frac{2}{5}$	$\frac{27}{28}$ $\frac{26}{27}$	21,5 15,0	0·94 ·27	44·5 43·7	44·0 43·6	29·394 29·374	+0,15
143	A	Nov. 15	11 49 $\frac{7}{8}$ 2 6 $\frac{17}{24}$	$\frac{17}{20}$ $\frac{40}{43}$	13,0 31,0	·91 ·14	42·1 40·4	41·5 39·5	1·330 2·080	+0,15
144	A	Nov. 16	4 46 $\frac{8}{11}$ 7 57 $\frac{1}{12}$	$\frac{18}{19}$ $\frac{35}{52}$	14,0 25,0	1·00 0·23	40·5 40·8	40·3 40·5	29·419 29·460	+0,15
145	B	Nov. 16	8 19 $\frac{43}{44}$ 10 2 $\frac{34}{35}$	$\frac{53}{54}$ $\frac{58}{61}$	48,5 47,0	1·03 0·44	42·5 40·8	41·3 40·6	29·460 29·494	+0,15
146	B	Nov. 16	11 27 $\frac{32}{35}$ 20 57 $\frac{31}{42}$	$\frac{42}{45}$ $\frac{55}{64}$	38,5 48,0	1·02 0·33	40·9 39·1	40·0 38·0	1·140 1·620	+0,15
147	B	Nov. 17	21 23 $\frac{29}{30}$ 0 5 $\frac{57}{71}$	$\frac{41}{42}$ $\frac{72}{82}$	35,5 70,5	·98 ·31	39·6 39·0	39·8 39·0	29·594 29·574	+0,10
148	D	Nov. 17	11 34 $\frac{28}{31}$ 0 38 $\frac{32}{39}$	$\frac{40}{43}$ $\frac{50}{53}$	35,5 43,5	·83 ·17	38·0 37·3	37·4 36·3	1·390 2·030	+0,10
149	D	Nov. 18	1 46 $\frac{8}{11}$ 4 29 $\frac{36}{41}$	$\frac{14}{17}$ $\frac{72}{79}$	12,5 57,0	1·01 0·27	38·0 37·5	37·5 37·3	29·740 29·782	0,00
150	D	Nov. 19	21 25 $\frac{50}{51}$ 3 8 $\frac{31}{52}$	$\frac{56}{57}$ $\frac{73}{90}$	53,5 61,5	·99 ·09	37·4 39·2	36·8 38·2	29·446 29·500	+0,20
151	C	Nov. 19	3 44 $\frac{31}{32}$ 6 24 $\frac{31}{50}$	$\frac{47}{48}$ $\frac{71}{88}$	39,5 60,0	1·01 0·33	39·9 39·5	38·8 39·0	29·520 29·582	+0,20
152	C	Nov. 19	12 16 $\frac{57}{60}$ 2 7 $\frac{53}{84}$	$\frac{73}{88}$ $\frac{81}{112}$	67,0 82,5	1·02 0·15	39·5 38·4	38·8 37·4	1·920 2·740	+0,20
153	C	Nov. 21	20 47 $\frac{53}{56}$ 23 59 $\frac{5}{18}$	$\frac{69}{70}$ $\frac{25}{38}$	62,0 21,5	1·00 0·27	41·9 43·5	40·9 42·4	29·672 29·732	+0,30

Table I.—Continued.

No. 31—34. Brass bar, $\frac{3}{4}$ inch thick (continued).

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers, both inside. Upper.	Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
154	A	Dec. 15	21 19 $\frac{8}{11}$ 0 43 $\frac{3}{8}$	$\frac{16}{21}$ $\frac{23}{26}$	14,0 15,0	0·82 0·19	48·3 47·8	47·8 47·5	29·800 29·790	+0,40
155	A	Dec. 15	1 51 $\frac{0}{3}$ 20 44 $\frac{45}{46}$	$\frac{10}{13}$ $\frac{71}{72}$	6,5 58,5	1·01 0·08	47·3 45·8	47·0 45·6	1·100 2·020	+0,40
156	A	Dec. 16	22 9 $\frac{18}{23}$ 1 17 $\frac{49}{52}$	$\frac{24}{29}$ $\frac{65}{68}$	23,5 58,5	1·04 0·05	46·7 46·4	46·6 46·2	29·832 29·736	+0,40
157	B	Dec. 17	1 45 $\frac{44}{47}$ 4 40 $\frac{41}{42}$	$\frac{52}{53}$ $\frac{57}{58}$	49,0 49,5	1·06 0·29	46·6 46·5	46·5 46·2	29·796 29·784	+0,45
158	B	Dec. 17	4 42 $\frac{52}{55}$ 7 52 $\frac{21}{28}$	$\frac{56}{59}$ $\frac{29}{36}$	55,5 28,5	1·04 0·27	46·6 46·5	46·4 46·2	29·784 29·734	+0,45
159	B	Dec. 18	12 6 $\frac{18}{21}$ 2 7 $\frac{55}{84}$	$\frac{24}{27}$ $\frac{73}{102}$	22,5 78,5	0·99 0·16	47·0 46·4	46·8 46·4	1·300 2·100	+0,45
160	B	Dec. 18	3 1 $\frac{6}{7}$ 5 55 $\frac{23}{27}$	$\frac{16}{17}$ $\frac{40}{48}$	11,5 34,5	0·98 0 29	47·8 47·5	47·8 47·2	29·420 29·444	+0,45
161	D	Dec. 19	20 33 $\frac{46}{53}$ 0 57 $\frac{21}{26}$	$\frac{58}{65}$ $\frac{31}{36}$	55,5 28,5	0·99 0·13	46·0 45·7	45·9 45·5	29·564 29·624	+0,40
162	D	Dec. 19	2 26 $\frac{16}{21}$ 21 3 $\frac{12}{15}$	$\frac{22}{27}$ $\frac{107}{110}$	21,5 61,0	0·88 0·09	45·1 43·8	45·0 43·6	0·960 2·080	+0,40
163	D	Dec. 20	22 34 $\frac{4}{7}$ 1 29 $\frac{57}{60}$	$\frac{10}{13}$ $\frac{75}{78}$	8,5 67,5	1·01 0·27	44·5 44·4	44·3 44·1	29·776 29·738	+0,40
164	C	Dec. 20	1 57 $\frac{57}{58}$ 4 51 $\frac{51}{64}$	$\frac{73}{74}$ $\frac{73}{86}$	65,5 68,5	1·00 0·29	45·0 44·5	44·5 44·4	29·728 29·686	+0,40
165	C	Dec. 20	6 50 $\frac{10}{11}$ 20 30 $\frac{55}{78}$	$\frac{24}{25}$ $\frac{139}{160}$	17,5 108,0	0·99 0·17	44·1 43·7	43·9 43·6	1·050 1·790	+0,40
166	C	Dec. 21	21 19 $\frac{13}{14}$ 0 29 $\frac{3}{26}$	$\frac{27}{30}$ $\frac{27}{48}$	21,0 26,0	0·86 0·23	45·1 44·9	45·1 44·7	29·670 29·720	+0,40

No. 35—38. Brass tube.

No.	Plane.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Inside.	Barometer.	Rate.
			No. 3. Knife edge. No. 8. Diameter.							
			h m s	s	s	°	°			s
167	*c*	March 15	9 0 $\frac{8}{11}$ 11 13 $\frac{6}{9}$	$\frac{20}{23}$ $\frac{22}{27}$	15,5 16,0	0·98 ·51	46·5 46 1		1·140 1·350	0,00
168	*c*	March 15	11 29 $\frac{38}{41}$ 12 28 $\frac{26}{31}$	$\frac{48}{51}$ $\frac{48}{53}$	44,5 39,5	·99 ·54	46·5 46·6		29·680 29·660	0,00
169	A	March 16	6 58 $\frac{42}{45}$ 8 48 $\frac{16}{23}$	$\frac{52}{55}$ $\frac{50}{55}$	48,5 36,0	·94 ·13	48 6 49·0		29·660 29·700	0,00
170	A	March 16	9 16 $\frac{2}{5}$ 10 51 $\frac{8}{9}$	$\frac{12}{15}$ $\frac{20}{23}$	8,5 15,0	·96 ·64	49·8 50·0		0·950 1·120	0,00
171	C	March 17	7 13 $\frac{8}{9}$ 9 25 $\frac{52}{57}$	$\frac{20}{21}$ $\frac{82}{87}$	14,5 69,5	·98 ·10	52·9 52·6		29·880 29·860	0,00
172	C	March 17	9 49 $\frac{8}{9}$ 13 23 $\frac{34}{35}$	$\frac{16}{17}$ $\frac{52}{53}$	12,5 43,5	·99 ·42	53·0 53·8		0 850 0·990	0,00
173	*a*	March 18	8 18 $\frac{22}{23}$ 11 35 $\frac{22}{23}$	$\frac{32}{33}$ $\frac{38}{39}$	27,5 30,5	·98 ·43	50·8 50 6		0·880 1·000	0,00
174	*a*	March 18	11 56 $\frac{50}{51}$ 13 20 $\frac{5}{6}$	$\frac{60}{61}$ $\frac{25}{26}$	55,5 15,5	·98 ·26	51·0 51·0		30·200 30·200	0,00

Table I.—Continued.

No. 35—38. Brass tube (continued).

No. 1. Knife edge. No. 3. Diameter. Adjustment altered.

No.	Plane.	1831.	Disappearance. (h m s)	Reappearance. (s)	Coincidence. (s)	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate. (s)
175	A	May 17	11 30 $\frac{26}{29}$ 14 58 $\frac{18}{21}$	$\frac{34}{35}$ $\frac{34}{37}$	31,0 27,5	0°97 ·32	61°4 62·5	60°5 61·7	1·500 1·820	—0,30
176	A	May 17	15 11 $\frac{40}{43}$ 21 0 $\frac{6}{17}$	$\frac{48}{49}$ $\frac{74}{77}$	45,0 43,5	·97 ·13	62·9 61·5	62·0 61·0	1·580 2·100	—0,30
177	A	May 18	6 41 $\frac{18}{19}$ 8 47 $\frac{28}{29}$	$\frac{24}{25}$ $\frac{52}{53}$	21,5 40,5	·95 ·11	60·1 61·0	60·0 60·8	30·040 30·010	—0,30
178	A	May 18	8 52 $\frac{48}{49}$ 11 24 $\frac{4}{5}$	$\frac{54}{55}$ $\frac{33}{34}$	51,5 19,0	·91 ·06	61·0 62·0	60·8 61·9	30·010 29·990	—0,30
179	C	May 18	13 47 $\frac{46}{47}$ 23 9 $\frac{9}{10}$	$\frac{50}{51}$ $\frac{133}{134}$	48,5 71,5	·98 ·03	63·0 62·8	62·4 62·4	1·790 3·010	—0,30
180	C	May 19	6 43 $\frac{54}{55}$ 9 15 $\frac{9}{26}$	$\frac{59}{60}$ $\frac{37}{48}$	57,0 30,0	·94 ·08	61·3 61·8	61·3 61·8	29·810 29·780	—0,30
181	*a*	May 21	2 23 $\frac{46}{47}$ 10 46 $\frac{25}{36}$	$\frac{52}{53}$ $\frac{41}{56}$	49,5 39,5	1·02 0·08	64·4 62·8	64·1 62·4	1·220 1·820	0,00
182	*a*	May 22	10 50 $\frac{57}{58}$ 16 12 $\frac{55}{62}$	$\frac{62}{63}$ $\frac{65}{72}$	60,0 63,5	1·01 0·19	62·8 63·1	62·4 62·7	1·820 1·980	0,00
183	*a*	May 22	19 37 $\frac{53}{54}$ 22 26 $\frac{13}{28}$	$\frac{57}{58}$ $\frac{73}{90}$	55,5 51,0	0·99 0·08	64·0 64·5	64·0 64·5	29·982 29·980	—0,17
184	*a*	May 23	11 46 $\frac{39}{40}$ 14 35 $\frac{47}{62}$	$\frac{43}{44}$ $\frac{65}{82}$	41,5 64,0	1·00 0·06	62·1 62·5	62·3 62·5	29·912 29·894	—0,17
185	*c*	May 23	4 19 $\frac{38}{39}$ 12 55 $\frac{10}{15}$	$\frac{44}{55}$ $\frac{36}{43}$	41,5 26,0	1·02 0·06	66·0 64·8	65·6 64·4	1·530 1·830	+0,18
186	*c*	May 24	12 59 $\frac{39}{40}$ 19 5 $\frac{2}{7}$	$\frac{44}{45}$ $\frac{12}{17}$	42,0 9,5	1·00 0·13	64·8 66·1	64·4 65·4	1·830 2·070	+0,18
187	*c*	May 24	22 41 $\frac{2}{3}$ 1 2 $\frac{26}{31}$	$\frac{6}{7}$ $\frac{66}{71}$	4,5 48,5	0·99 0·06	67·9 67·5	68·1 67·6	29·810 29·832	+0,18
188	*c*	May 24	1 5 $\frac{32}{35}$ 3 18 $\frac{44}{53}$	$\frac{40}{41}$ $\frac{70}{79}$	37,0 61,5	1·01 0·08	67·5 67·0	67·6 67·1	29·832 29·864	+0,18

Pendulum.	No.	1832.	Disappearance. (h m s)	Reappearance. (s)	Coincidence. (s)	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate. (s)
No. 39. Mercurial, on spring.	189	March 28	23 21 $\frac{42}{45}$ 2 36 $\frac{11}{36}$	$\frac{90}{93}$ $\frac{63}{88}$	67,5 49,5	0°77 ·21	48°7 48·6	48°5 48·3	30·124 30·074	+0,39
	190	March 28	3 37 $\frac{6}{7}$ 11 27 $\frac{27}{44}$	$\frac{48}{49}$ $\frac{75}{106}$	27,5 63,0	·77 ·18	48·4 47·2	48·4 46·8	0·830 0·980	+0,39
	191	March 28	11 31 $\frac{33}{50}$ 19 20 $\frac{27}{48}$	$\frac{51}{76}$ $\frac{105}{124}$	52,5 76,0	·77 ·17	47·4 44·7	47·0 44·0	0·980 1·100	+0,39
	192	March 29	20 2 $\frac{46}{49}$ 23 32 $\frac{5}{30}$	$\frac{94}{97}$ $\frac{51}{82}$	71,5 42,0	·77 ·21	45·6 45·0	45·3 44·5	30·018 29·954	+0,32
	193	March 29	1 11 $\frac{12}{15}$ 5 28. $\frac{21}{34}$	$\frac{56}{59}$ $\frac{113}{130}$	35,5 74,5	·77 ·16	45·5 46·3	45·3 46·2	29·966 29·958	+0,32
	194	March 29	6 9 $\frac{24}{27}$ 11 58 $\frac{5}{38}$	$\frac{74}{77}$ $\frac{57}{92}$	50,5 48,0	·77 ·20	45·6 45·5	45·5 45·1	2·620 2·780	+0,32
	195	March 29	12 4 $\frac{27}{48}$ 20 8 $\frac{21}{42}$	$\frac{57}{78}$ $\frac{127}{152}$	52,5 85,5	·72 ·14	45·7 43·9	46·0 43·3	2·780 2·930	+0,32
	196	March 30	20 52 $\frac{26}{27}$ 0 52 $\frac{25}{60}$	$\frac{74}{75}$ $\frac{105}{138}$	50,5 82,0	·71 ·16	45·0 46·0	44·7 45·9	30·014 29·968	+0,26

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
No. 41. Lead bob, flat wood rod.	197	May 14	20 18 $\frac{21}{22}$ 23 54 $\frac{8}{19}$	$\frac{25}{26}$ $\frac{34}{45}$	23,5 26,5	0·86 ·23	50·4 50·7	50·0 50·7	29·828 29·812	—0,49
	198	May 14	2 6 $\frac{35}{36}$ 12 35 $\frac{26}{41}$	$\frac{39}{40}$ $\frac{58}{71}$	37,5 49,0	·86 ·19	50·6 51·5	50·8 51·5	1·160 1·830	—0,49
	199	May 14	12 37 $\frac{55}{56}$ 20 41 $\frac{54}{65}$	$\frac{57}{60}$ $\frac{80}{91}$	57,0 72,5	·85 ·25	51·6 50·5	51·5 50·1	1·830 2·390	—0,49
	200	May 15	21 12 $\frac{25}{26}$ 0 56 $\frac{50}{61}$	$\frac{27}{28}$ $\frac{80}{91}$	26,5 70,5	·87 ·21	51·5 51·2	51·5 51·2	29·814 29·806	—0,49
No. 40. Lead bob, round wood rod.	201	May 16	19 43 $\frac{15}{16}$ 23 30 $\frac{30}{45}$	$\frac{18}{19}$ $\frac{60}{73}$	17,0 52,0	·81 ·19	49·8 50·6	49·5 50·6	29·890 29·864	—0,49
	202	May 16	1 3 $\frac{45}{46}$ 12 14 $\frac{26}{45}$	$\frac{47}{48}$ $\frac{66}{83}$	46,5 55,0	·90 ·17	50·6 53·0	50·7 52·7	1·060 1·760	—0,49
	203	May 16	12 16 $\frac{31}{32}$ 19 30 $\frac{22}{35}$	$\frac{35}{36}$ $\frac{54}{67}$	33,5 44,5	·77 ·22	53.0 51·6	52·7 51·0	1·760 2·240	—0,49
	204	May 17	20 26 $\frac{11}{12}$ 0 13 $\frac{2}{21}$	$\frac{15}{16}$ $\frac{40}{57}$	13,5 30,0	·78 ·18	52·3 52·0	52·1 51·7	29·882 29·864	—0,49

No. 30. Iron bar. (See page 467.)

Knife edge.	No.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
B	205	Nov. 6	1 37 $\frac{21}{24}$ 2 7 $\frac{25}{32}$	$\frac{29}{30}$ $\frac{37}{44}$	26,0 34,5	0·96 0·77	46·0 46·6	45·7 46 0	29·482 29·444	+0,40
b	206	Nov. 9	20 5 $\frac{10}{16}$ 21 51 $\frac{2}{27}$	$\frac{25}{31}$ $\frac{16}{39}$	20,5 21,0	1·00 0·38	46·5 46·5	46·4 46·4	30·164 30·172	+0,30
B	207	Nov. 9	21 57 $\frac{37}{40}$ 22 57 $\frac{49}{58}$	$\frac{43}{44}$ $\frac{57}{64}$	41,0 57,0	1·02 0·60	46·8 46·5	46·4 46·3	30·172 30·210	+0,30
b	208	Nov. 9	23 6 $\frac{30}{37}$ 0 23 $\frac{4}{9}$	$\frac{34}{41}$ $\frac{14}{19}$	35,5 11,5	1·06 0·54	46·8 46·6	46·4 46·4	30·214 30·216	+0,30
B	209	Nov. 9	0 43 $\frac{8}{11}$ 2 13 $\frac{27}{46}$	$\frac{13}{16}$ $\frac{37}{58}$	12,0 42,0	1·02 0·46	47·5 47·0	46·4 46·7	30·224 30·238	+0,30
b	210	Nov. 9	2 23 $\frac{0}{3}$ 3 38 $\frac{49}{50}$	$\frac{2}{7}$ $\frac{63}{64}$	3,0 56,5	1·06 0·53	47·2 47·3	46·6 46·8	30·238 30·270	+0,30
b	211	Nov. 10	22 46 $\frac{43}{52}$ 23 1 $\frac{55}{64}$	$\frac{57}{64}$ $\frac{67}{76}$	54,0 65,5	1·02 0·91	45·0 45·0	44·4 44·4	30·434 30·434	+0,26
b	212	Nov. 10	23 8 $\frac{20}{35}$ 23 54 $\frac{16}{33}$	$\frac{24}{41}$ $\frac{24}{43}$	30,0 29,0	1·05 0·67	45·0 44·7	44·4 44·3	30·434 30·414	+0,26
B	213	Nov. 10	0 7 $\frac{44}{45}$ 1 8 $\frac{13}{22}$	$\frac{50}{51}$ $\frac{27}{34}$	47,5 24,0	1·02 0·59	45·3 44·6	44·5 44·4	30·409 30·394	+0,26
b	214	Nov. 10	1 15 $\frac{6}{25}$ 2 16 $\frac{12}{39}$	$\frac{12}{31}$ $\frac{28}{55}$	18,5 33,5	1·04 0·58	44·6 44·6	44·4 44·4	30·394 30·370	+0,26
B	215	Nov. 10	2 23 $\frac{18}{19}$ 3 23 $\frac{35}{42}$	$\frac{27}{28}$ $\frac{59}{70}$	23,0 51,5	1·02 0·58	44·8 44·6	44·4 44·4	30·370 30·354	+0,26
B	216	Nov. 13	21 4 $\frac{33}{34}$ 22 34 $\frac{19}{30}$	$\frac{39}{40}$ $\frac{31}{42}$	36,5 30,5	1·02 0·46	51·5 51·6	51·3 51·3	30 014 30·174	+0,14
b	217	Nov. 13	22 41 $\frac{44}{63}$ 0 42 $\frac{32}{69}$	$\frac{48}{67}$ $\frac{50}{85}$	55,5 59,0	1·03 0·32	51·6 51·3	51·3 51·0	30·174 30·006	+0,14

Table I.—Continued.

No. 30 Iron bar (continued).

Knife edge.	No.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
B	218	Nov. 13	0 48 $\frac{44}{45}$ 1 48 $\frac{39}{48}$	$\frac{46}{47}$ $\frac{49}{56}$	45,5 48,0	1·03 0·61	51·3 51·1	51·0 51·0	30·006 29·994	+0,14
B	219	Nov. 21	1 11 $\frac{10}{13}$ 2 11 $\frac{37}{40}$	$\frac{14}{17}$ $\frac{45}{48}$	13,5 42,5	1·00 0·58	44·8 45·0	43·7 44·0	29·758 29·770	+0,47
B	220	Nov. 21	2 15 $\frac{54}{57}$ 3 31 $\frac{19}{24}$	$\frac{60}{63}$ $\frac{43}{48}$	58,5 33,5	1·00 0·48	45·1 45·4	44·1 44·5	29·770 29·806	+0,47
B	221	Nov. 22	20 25 $\frac{6}{7}$ 21 10 $\frac{11}{12}$	$\frac{24}{25}$ $\frac{26}{27}$	15,5 19,0	·96 ·67	49·3 49·8	48·5 49·0	29·864 29·878	+0,54
b	222	Nov. 22	21 17 $\frac{6}{19}$ 22 32 $\frac{54}{77}$	$\frac{10}{25}$ $\frac{62}{83}$	15,0 68,0	·98 ·48	50·1 50·1	49·0 49·4	29·878 29·894	+0,54
b	223	Nov. 22	22 37 $\frac{46}{63}$ 0 8 $\frac{44}{71}$	$\frac{50}{67}$ $\frac{52}{79}$	56,5 61,5	·99 ·45	50·1 50·4	49·4 49·7	29·894 29·892	+0,54
b	224	Nov. 22	0 11 $\frac{36}{49}$ 1 27 $\frac{16}{39}$	$\frac{38}{53}$ $\frac{28}{49}$	44,0 33,0	1·02 0·48	50·4 50·7	49·7 50·0	29·892 29·904	+0,54
b	225	Nov. 23	20 1 $\frac{10}{25}$ 21 16 $\frac{50}{73}$	$\frac{24}{37}$ $\frac{58}{81}$	24,0 65,5	1·02 0·50	53·2 53·3	52·6 52·8	29·990 29·990	+0,54
b	226	Nov. 23	21 21 $\frac{20}{35}$ 22 37 $\frac{0}{23}$	$\frac{24}{39}$ $\frac{8}{31}$	29,5 15,5	·98 ·50	53·3 53·5	52·8 53·0	29·990 29·996	+0,54
B	227	Nov. 23	22 41 $\frac{31}{36}$ 0 11 $\frac{1}{24}$	$\frac{33}{40}$ $\frac{9}{30}$	35,0 16,0	1·02 0·46	53·5 53·9	53·0 53·3	29·996 29·984	+0,54
B	228	Nov. 23	0 13 $\frac{45}{52}$ 1 28 $\frac{17}{32}$	$\frac{49}{56}$ $\frac{25}{44}$	50,5 29,5	·99 ·50	53·9 54·0	53·3 53·5	29·984 29·984	+0,54
		1832.					Upper.*	Lower.*		
B	229	Feb. 3	4 30 $\frac{13}{18}$ 6 17 $\frac{1}{16}$	$\frac{25}{30}$ $\frac{18}{33}$	21,5 17,0	·80 ·31	43·0 42·9	43·1 42·6	29·754 29·776	−0,01
b	230	Feb. 4	20 37 $\frac{21}{24}$ 22 40 $\frac{17}{54}$	$\frac{37}{40}$ $\frac{53}{88}$	30,5 53,0	·79 ·27	43·6 44·4	43·6 44·3	29·848 29·894	−0,01

* Both inside.

TABLE II.—*Results of the preceding Table.*

Pendulum.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for			N′ and N″. See page 407.
					Arc.	Therm.	Rate.	
		h m s		s				
	1	4 13 12,5	30	506 417	+·429	—6·914	—·160	86734·576
	2	9 43 9,5	70	499·850	·493	6·558	·180	86739·469
	3	10 24 29,5	75	499·593	·530	6·572	·200	86739·640
No. 1. Platina	4	5 29 5,7	39	506·301	·392	6·706	·250	86734·735
Sphere.	5	5 3 51,0	36	506·417	·407	6 980	·300	86734·348
	6	9 1 15,5	65	499 623	·578	6·861	·350	86739·228
	7	9 50 53,0	71	499·338	·523	7·083	·400	86739 099
	8	3 39 24,0	26	506·308	·536	7·196	·450	86734·184
	9	2 34 33,0	19	488 053	+·304	—6·988	—·500	86746·876
	10	6 48 6,0	52	470·885	·304	7·166	·500	86759·607
	11	6 40 12,5	51	470·823	·372	7 315	·500	86759·564
No. 3. Brass	12	2 1 58,0	15	487·866	·502	7 381	·450	86746·867
Sphere.	13	2 18 25,5	17	488·559	·424	7·380	·430	86746·307
	14	9 9 48,0	70	471.257	·323	7·221	·410	86759·371
	15	9 33 17,0	73	471·192	·282	7·217	·400	86759·395
	16	2 10 22,0	16	488·875	·449	7·137	·400	86746·377
	17	2 50 32,5	18	568 444	+·342	—4·766	—·060	86699·504
	18	7 12 28,0	47	552·085	·349	4·865	·060	86708·419
	19	8 34 25,0	56	551.161	·295	5.213	·030	86708·572
No. 2. Lead	20	3 46 38,5	24	566 604	·293	5·554	·000	86699·714
Sphere.	21	2 59 34,0	19	567·053	·324	5·548	·000	86699·510
	22	7 3 8,5	46	551·924	·302	5·340	·000	86708·048
	23	8 16 29,5	54	551·657	·331	5·419	·000	86708·150
	24	2 59 32,0	19	566·947	·355	5·480	·000	86699 666
	25	0 44 7,0	6	441·166	+·254	—3·947	—·430	86787·567
	26	1 43 40,0	16	388·750	·324	3 924	·430	86840·470
	27	1 43 51,5	16	389·469	·302	5·049	·460	86838·473
No. 4. Ivory	28	0 51 19,7	7	439·964	·259	4·751	·460	86787·807
Sphere.	29	0 51 25,0	7	440·714	·259	4·888	·480	86786·983
	30	2 16 14,5	21	389 262	·264	4·878	·480	86838·826
	31	2 16 23,5	21	389·690	·264	4·636	·480	86838·568
	32	0 51 33,0	7	441·857	·250	4·360	·480	86786·489
	33	2 54 53,5	18	582·972	+·571	—6·008	—·520	86690 455
	34	8 43 35,5	56	560·991	·669	6·044	·560	86702·092
	35	8 43 4,5	56	560·437	·482	6·216	·600	86701·997
No. 6. Brass	36	4 41 17,5	29	581·982	·380	6·246	·630	86690·420
Sphere.	37	4 12 32,5	26	582·788	·449	6·320	·630	86690 005
	38	9 19 59,5	60	559·992	·615	6·691	·630	86701·869
	39	3 15 19,0	21	558·047	·109	7·063	·630	86702·064
	40	2 54 24,0	18	581·333	·640	7·048	·630	86690·210
	41	1 6 11,0	7	567·286	+·404	—7·790	—·600	86696·622
	42	2 32 49,0	19	482·579	·489	7·440	·640	86750·485
	43	2 32 55,0	19	482·895	·476	7·300	·660	86750·359
No. 7. Ivory	44	1 6 22,0	7	568·857	·399	7·063	·680	86696·423
Sphere.	45	1 6 33,5	7	570·500	·399	6·712	·720	86695·860
	46	2 41 17,0	20	483·850	·478	6·558	·730	86750·326
	47	2 41 33,0	20	484·650	·464	6·988	·800	86749·223
	48	1 6 29,0	7	569·857	·424	6·914	·800	86695·944

TABLE II.—Continued.

Pendulum.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for			N′ and N″. See page 407.
					Arc.	Therm.	Rate.	
		h m s		s				
No. 5. Lead Sphere.	49	5 20 53,0	29	663·897	+·406	—5·702	—·230	86654·756
	50	6 26 21,0	36	643·916	·663	5·830	·230	86662·961
	51	7 30 1,0	42	642·881	·614	6·178	·220	86663·005
	52	4 24 46,0	24	661·916	·451	6·460	·220	86654·831
	53	7 43 10,5	42	661·679	·279	6·490	·220	86654·723
	54	8 23 4,5	47	642·224	·618	6·504	·220	86662·959
	55	7 51 29,5	44	642·943	·550	6·305	·230	86662·779
	56	5 31 4,0	30	662·133	·375	6·572	·240	86654·538
No. 9. Ivory Sphere on Cylinder.	57	1 13 18,5	7	628·357	+·296	—5·816	+·200	86669·683
	58	2 38 14,5	18	527·472	·440	5·591	·200	86722·649
	59	2 56 3,0	20	528·150	·354	5·132	·200	86722·602
	60	1 13 20,0	7	628·571	·306	4·567	·200	86671·208
No. 8. Lead Sphere on Cylinder.	61	3 13 45,5	24	484·396	+·401	—3·965	+·230	86753·400
	62	5 15 56,5	40	473·912	·632	3·594	·230	86761·893
	63	6 50 3,5	52	473·144	·608	3·454	·230	86762·601
	64	3 12 56,5	24	482·354	·408	4·054	·200	86754·797
No. 10. Brass Cylinder, with iron wire.	65	4 7 38,0	23	646·000	+·507	—4·336	+·600	86664·263
	66	8 56 58,5	52	619·587	·688	3·995	·600	86676·189
	67	8 15 37,0	48	619·521	·660	3·876	·570	86676·280
	68	3 25 1,5	19	647·447	·574	3·905	·570	86664·134
No. 11. Brass Cylinder, with Brass Rod.	69	2 46 37,0	13	769·000	+·419	—3·189	+·500	86622·450
	70	3 51 56,0	19	732·421	·655	2·858	·500	86634·217
	71	4 4 35,5	20	733·775	·634	2·556	·500	86634·058
	72	2 21 31,2	11	771·927	·451	2·522	·500	86622·289
No. 12. Brass Cylinder, with Brass Rod.	73	1 59 48,0	9	798·666	+·553	—3·793	+·520	86613·640
	74	4 37 24,5	22	756·568	·740	3·707	·520	86625·933
	75	4 25 25,0	21	758·333	·728	3·439	·520	86625·669
	76	1 46 46,0	8	800·750	·632	3·384	·520	86613·568
No. 13. Brass Cylinder, with Brass Rod.	77	4 5 0,0	17	864·706	+·291	—4·051	+·600	86596·677
	78	10 14 58,5	45	819·967	·462	3·289	·600	86608·533
	79	8 12 27,0	36	820·750	·524	3·116	·600	86608·548
	80	2 24 55,5	10	869·550	·624	3·254	·600	86596·694
No. 18. Hollow Brass Cylinder, both ends closed.	81	1 17 6,0	8	578·250	+·574	—5·012	+·490	86694·885
	82	4 7 20,5	29	511·741	·655	5·028	·490	86733·788
	83	3 58 22,0	28	510·786	·503	5·459	·450	86733·797
	84	1 55 37,0	12	578·083	·462	5·331	·450	86694·500
No. 15. Hollow Brass Cylinder, both ends open.	85	1 32 52,0	12	464·333	+·442	—6·029	+·580	86767·140
	86	3 8 39,0	27	419·222	·584	6·014	·580	86807·342
	87	3 8 54,5	27	419·796	·714	5·845	·580	86807·078
	88	1 25 11,0	11	464·636	·410	5·696	·580	86767·199
No. 16. Hollow Brass Cylinder, top open, bottom closed.	89	1 58 39,0	11	647·182	+·692	—5·058	+·660	86663·298
	90	4 6 45,5	26	569·442	·578	4·493	·660	86700·200
	91	3 47 50,0	24	569·583	·641	4·425	·660	86700·256
	92	1 58 48,5	11	648·045	·416	4·479	·660	86663·246
No. 17. Hollow Brass Cylinder, top closed, bottom open.	93	1 40 36,5	14	431·178	+·402	—5·102	+·430	86796·493
	94	3 2 57,0	28	392·036	·746	4·930	·430	86837·026
	95	3 29 34,5	32	392·953	·694	4·434	·540	86836·540
	96	1 33 33,5	13	431·808	·565	4·202	·540	86797 081

Table II.—Continued.

Pendulum.	Knife edge.	No.	Total Interval.	No. of Coin-cid.	Mean Interval.	Corrections for			N' and N''. See page 407.
						Arc.	Therm.	Rate.	
			h m s		s				
No. 14. Solid Lead Cylinder.		97	7 45 43,0	34	821·853	+·250	−2·079	+·500	86608·911
		98	7 38 37,5	35	786·214	·610	1·820	·600	86619·150
		99	6 21 17,0	29	788·862	·263	1·301	·600	86618·602
		100	8 2 13,5	35	826·671	·234	1·559	·600	86608·295
No. 19. Hollow Brass Cylinder hermetically sealed.		101	1 12 41,0	9	484·555	+·536	−4·167	+·200	86753·184
		102	3 5 23,0	26	427·808	·681	3·870	·200	86800·931
		103	3 12 26,0	27	427·629	·685	3·721	·300	86801·364
		104	1 29 14,0	11	486·727	·501	3·460	·300	86752·366
No. 20. Lead Lens.		105	4 6 19,0	22	671·773	+·383	−3·252	+·520	86654·881
		106	9 3 57,0	50	652·740	·476	3·341	·520	86662·386
		107	10 41 47,5	59	652·670	·331	3·588	·440	86661·942
		108	4 5 12,5	22	668·750	·383	4·396	·440	86654·820
No. 21. Copper Cylindrical Rod.		109	1 51 45,5	32	209·547	+·230	−4·327	The clock making 86045ˢ·291 in a day.	85219·963
		110	3 23 22,0	57	214·070	·432	4·449		85237·383
		111	3 1 55,0	51	214·019	·425	4·376		85237·209
		112	2 2 15,0	35	209·571	·239	4·294		85220·085
No. 25—26. Brass bar, ⅜ inch thick.	A	113	8 4 22,5	32	908·203	+·548	+3·577	+·230	86214·089
	A	114	3 33 57,5	15	855·833	·396	3·910	·230	86202·627
	B	115	2 52 29,5	12	862·458	·522	2·822	·200	86203·186
	B	116	4 49 55,0	19	915·526	·903	3·362	·200	86215·720
	B	117	7 51 59,5	31	913·532	·659	3·997	·200	86215·700
	B	118	3 6 14,2	13	859·578	·462	3·577	·150	86203·150
	A	119	19 42 59,5	77	921·812	·221	1·164	−·180	86213·748
	A	120	4 20 48,0.	18	869·333	·305	1·012	·180	86202·363
	A	121	4 5 55,0	17	867·941	·321	+1·314	·180	86202·362
	a	122	1 0 3,0	4	900·750	·580	−5·689	+·430	86203·480
	a	123	2 0 4,5	8	900 562	·641	5·765	·450	86203·446
	a	124	10 24 1,5	39	960 038	·449	5·754	·500	86215·202
	a	125	2 15 8,0	9	900·889	·548	5·832	·520	86203·425
	b	126	1 14 33,0	5	894·600	·807	5·172	·630	86203·106
	b	127	2 58 57,5	12	894·792	·469	5·043	·700	86203 008
	b	128	14 3 45,0	53	955·189	·357	4·904	·700	86215·147
	b	129	2 58 37,0	12	893·167	·464	4·797	·700	86202·898
	b	130	3 13 26,5	13	892·808	·442	4·646	·700	86202·949
	B	131	1 58 29,5	8	888·687	·613	3·858	·700	86203 010
	B	132	2 13 15,5	9	888·389	·586	3·849	·700	86202·927
	B	133	18 44 22,5	71	950·176	·234	4·030	·700	86215·043
	B	134	3 27 55,5	14	891·107	·408	3·987	·710	86203·215
	B	135	3 27 58,5	14	891·321	·400	4·051	·720	86203·200
	A	136	3 12 42,5	13	889·423	·416	3·944	·700	86202·889
	A	137	1 43 33,5	7	887·642	·700	3·965	·700	86202·762
	A	138	2 42 55,5	11	888·682	·469	3·892	·660	86202·791
	A	139	13 23 52,0	51	945·725	·374	3·965	·640	86214·332
	A	140	2 28 12,5	10	889·250	·477	4·074	·620	86202·702
	A	141	2 28 17,0	10	889·700	·477	4·074	·580	86202·760
No. 31—34.	A	142	2 53 53,5	11	948·500	+·533	−7·823	+·150	86210·677
	A	143	14 17 18,0	50	1028·760	·354	8·922	·150	86223·613
	A	144	3 11 11,0	12	955·925	·522	9·288	·150	36210·624
	B	145	1 42 58,5	7	882·642	·838	9·051	·150	86196·160
	B	146	9 30 9,5	36	950·263	·678	9·568	·150	86209·415
	B	147	2 42 35,0	11	886·818	·616	9·718	·100	86196·144
	D	148	13 4 8,0	49	960·163	·342	10·495	·100	86209·977

Table II.—Continued.

	Knife edge.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for Arc.	Corrections for Therm.	Corrections for Rate.	N′ and N″. See page 407.
			h m s		s				
Brass bar, ¾ inch thick.	D	149	2 43 44,5	11	893·136	+·587	−10·580	+·000	86196·531
	D	150	5 43 8,0	23	895·130	·331	10·539	·200	86196·947
	C	151	2 40 20,5	10	962·050	·669	9·934	·200	86211·318
	C	152	13 51 15,5	48	1039·073	·434	9·955	·200	86224·377
	C	153	3 11 19,5	12	956·625	·581	8·749	·300	86211·497
	A	154	3 24 1,0	13	941·615	·358	6·099	·400	86211·144
	A	155	18 53 52,0	67	1015·403	·324	6·392	·400	86224·153
	A	156	3 8 35,0	12	942·917	·291	6·694	·400	86210·736
	B	157	2 55 0,5	12	875·416	·660	6·703	·450	86197·015
	B	158	3 9 33,0	13	874·846	·612	6·711	·450	86196·830
	B	159	14 1 56,0	54	935·482	·429	6·293	·450	86209·868
	B	160	2 54 23,0	12	871·917	·590	6·220	·450	86196·636
	D	161	4 23 33,0	18	878·500	·388	6·973	·400	86197·116
	D	162	18 37 39,5	71	944·500	·275	7·297	·400	86210·423
	D	163	2 55 59,0	12	879·916	·586	7·620	·400	86196·983
	C	164	2 54 3,0	11	949·364	·607	7·499	·400	86211·491
	C	165	13 41 30,5	48	1026·885	·443	7·499	·400	86225·068
	C	166	3 10 5,0	12	950·416	·428	7·348	·400	86211·665
	Plane.								
No. 35—38. Brass tube.	*c*	167	2 13 0,5	14	570·036	+·918	−7·065	−·000	90080·030
	c	168	0 58 55,0	7	505·000	·974	6·948	·000	90039·355
	A	169	1 49 47,5	13	506·731	·375	5·940	·000	90040·986
	A	170	1 35 6,5	10	570·650	1·082	5·445	·000	90082·155
	C	171	2 12 55,0	16	498·437	·357	4·163	·000	90036·808
	C	172	3 34 31,0	23	559 609	·803	3·870	·000	90077·201
	a	173	3 17 3,0	21	563·000	·807	5·085	·000	90077·935
	a	174	1 23 20,0	10	500·000	·576	4·950	·000	90037·374
	A	175	3 27 56,5	22	567·114	·648	−0·045	·313	90084·833
	A	176	5 48 58,5	37	565·905	·393	+0·135	·313	90084·076
	A	177	2 6 19,0	15	505 267	·353	−0·630	·322	90044·918
	A	178	2 31 27,5	18	504·861	·257	−0·293	·322	90044·872
	C	179	9 22 23,0	60	562·383	·236	+0·540	·313	90082·324
	C	180	2 31 33,0	18	505·167	·305	−0·203	·313	90045·236
	a	181	8 22 50,0	53	569·245	·346	+0·922	·000	90087·004
	a	182	5 22 3,5	34	568·338	·505	0·607	·000	90086·341
	a	183	2 48 55,5	20	506·775	·330	1·012	·177	90047·747
	a	184	2 49 22,5	20	508·125	·302	0·180	−·177	90047·835
	c	185	8 35 44,5	55	562·627	·312	1·710	+·187	90084·209
	c	186	6 5 27,5	39	562·244	·412	1·665	·187	90084·045
	c	187	2 21 44,0	17	500·235	·297	2·632	·187	90045·034
	c	188	2 13 24,5	16	500·281	·341	2·407	·187	90044·886
No. 39. Mercurial Pendulum.		189	3 14 42,0	12	973·500	+·344	No correction required.	+·390	86578·238
		190	7 50 35,5	31	910·822	·315		·390	86590·424
		191	7 49 23,5	31	908·500	·304		·390	86590·898
		192	3 29 30,5	13	966·961	·344		·320	86579·368
		193	4 17 39,0	16	966·187	·295		·320	86579·462
		194	5 48 57,5	23	910·326	·335		·320	86590·477
		195	8 4 33,0	32	908·531	·245		·320	86590·762
		196	4 0 31,5	15	962·100	·260		·260	86580·127
No. 41. Lead Cylinder with flat Rod.		197	3 36 3,0	25	518·520	+·428	No correction required.	−·490	86066·681
		198	10 29 11,5	70	539·307	·383		·490	86079·423
		199	8 5 15,5	54	539·176	·443		·490	86079·473
		200	3 44 44,0	26	518·615	·412		·490	86066·726
No. 40. Lead Cylinder with round Rod.		201	3 47 35,0	25	546·200	+·351	No correction required.	−·490	86083·481
		202	11 11 8,5	71	567·162	·384		·490	86095·220
		203	7 14 11,0	46	566·326	·355		·490	86094·740
		204	3 47 16,5	25	545·460	·322		·490	86083·032

Table II.—Continued.

No. 30. Iron bar.

Knife edge.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for				True number of vibrations in a mean solar day.
					Arc.	Therm.	Barom.	Rate.	
		h m s		s					
B	205	0 30 8,5	2	904·250	+1·224	−4·637	+14·301	+·400	86220·190
b	206	1 46 0,5	7	908·643	·726	4·478	14·625	·300	86220·999
B	207	1 0 16,0	4	904·000	1·052	4·493	14·638	·300	86220·346
b	208	1 16 36,0	5	919·200	1·016	4·478	14·651	·300	86223·499
B	209	1 30 30,0	6	905·000	·856	4·435	14·651	·300	86220·433
b	210	1 15 53,5	5	910·700	·997	4·392	14·657	·300	86221·818
b	211	0 15 11,5	1	911·500	1·527	5·054	14·821	·260	86221·976
b	212	0 45 59,0	3	919·667	1·194	5·069	14·818	·260	86223·309
B	213	1 0 36,5	4	909·125	1·052	5·040	14·803	·260	86221·002
b	214	1 1 15,0	4	918·750	1·048	5·054	14·795	·260	86222·967
B	215	1 0 28,5	4	907·125	1·024	5·054	14·786	·260	86220·524
B	216	1 29 54,0	6	899·000	·856	3·085	14·432	·140	86220·129
b	217	2 1 3,5	8	907·937	·670	3·128	14·430	·140	86221·791
B	218	1 0 2,5	4	900·625	1·077	3·172	14·396	·140	86220·574
B	219	1 0 29,0	4	907·250	1·000	5·213	14·511	·470	86220·302
B	220	1 15 35,0	5	907·000	·860	5·073	14·508	·470	86220·247
B	221	0 45 3,5	3	901·167	1·081	3·802	14·405	·540	86220·473
b	222	1 15 53,0	5	910·600	·837	3·672	14·398	·540	86222·338
b	223	1 31 5,0	6	910·833	·807	3·571	14·391	·540	86222·450
b	224	1 15 49,0	5	909·800	·881	3·485	14·397	·540	86222·401
b	225	1 15 41,5	5	908·300	·912	2·664	14·338	·540	86222·881
b	226	1 15 46,0	5	909·200	·868	2·607	14·338	·540	86223·077
B	227	1 29 41,0	6	896·333	·854	2·534	14·323	·540	86220·504
B	228	1 14 39,0	5	895·800	·872	2·462	14·313	·540	86220·362
B	229	1 46 55,5	7	916·500	·473	5·501	14·544	−·010	86220·962
b	230	2 3 22,5	8	925·312	·423	5·193	14·561	·010	86223·033

Note to page 417.—It was omitted to be stated, with reference to the pendulum No. 35—38, that the steel collars, which are attached to the tube, and on which the pendulum swings, are divided, on their outer circumference, into 16 equal parts; thus making 8 several diameters (numbered from 1 to 8) on which the vibrations of the pendulum may be varied. I have also got 3 separate pairs of agate knife edges, differing from each other in sharpness, for the purpose of ascertaining whether the results are affected by such an alteration of this part of the apparatus. But, at present, I have not made any experiments with this view.