P 260

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APPENDIX No. 22.

REPORT OF A CONFERENCE ON GRAVITY DETERMINATIONS, HELD AT WASHINGTON, D. C., IN MAY, 1882.

In pursuance of a correspondence between Major (now Lieut. Col.) J. Herschel, R. E., and the Superintendent of the United States Coast and Geodetic Survey, relative to the most advantageous mode of prosecuting pendulum observations and the scientific value of the same, the following-named gentlemen met at the Coast Survey Office, May 13, 1882, for an informal conference: The Superintendent of the Coast and Geodetic Survey; Major Herschel, R. E.; Prof. C. S. Peirce, Prof. S. Newcomb (on the part of astronomy), and Messrs. George Davidson and C. A. Schott (on the part of geodesy). Maj. J. W. Powell, Director of the United States Geological Survey (on the part of geology), was unable to attend.

The proceedings of the conference were as follows:

TWO LETTERS.

[Read to the conference to explain the immediate cause of the meeting.]

No. 1.

UNITED STATES COAST AND GEODETIC SURVEY OFFICE,
Washington, May 4, 1882.

Maj. J. HERSCHEL, R. E., *Brevoort House, New York:*

MY DEAR SIR: In pursuance of my letter of yesterday's date, I will now submit to you the proposition that, as Superintendent, &c., I invite at once, or at your earliest convenience, a conference on gravity observations, the participants in which would be, beside yourself and Peirce, Newcomb, on the part of astronomy; King and Powell, on the part of geology; and Davidson and Schott, on the part of geodesy. During such conference the greatest range of discussion would of course be in place, but its outcome, I conceive, must necessarily be formulated in a few propositions, some of which would be mainly intended to recite the scientific objects and usefulness of such work, and commend it to public patronage, others, to define the degree of accuracy to be attained in the observations, in order to entitle them to be ranked as contributions to science. As neither you nor ourselves are charged with any special powers in the premises, it appears to me that no other useful results can be reached by a conference than some such public declarations, the value of which rests upon the standing of the party making them. If this proposition meets your views, I shall be happy to make such arrangements for the earliest day you may find convenient. I regret that it will be necessary to tax you with coming to Washington, as all other parties are here, and being officially engaged it would be out of our power to meet you elsewhere.

It will be well if you will formulate in advance such expressions of opinion as appear to you desirable in the premises, in order that after comparing notes we may be able to submit propositions that will readily meet the assent of the conference.

Yours, very truly,

J. E. HILGARD,
Superintendent.

No. 2.

Prof. J. E. HILGARD: NEW YORK, *May* 5, 1882.

MY DEAR SIR: I am very glad to learn that you are well inclined towards the idea of a conference, and that now, in fact, it rests with me to indicate when I can be in Washington for the purpose.

As it is not a matter which presses until you have issued invitations, when of course it should not be delayed, and as it will be well to give a few days' notice in any case, I will not consider myself in any way required to hasten my departure from New York, but only to give you as early an indication as I can when I can undertake to be in Washington. At this moment I am not in a position to say precisely, but it will almost certainly be within a week from this date. I shall most likely be able to leave this city about Wednesday next.

With regard to the lines of discussion, it must depend to some extent on the degree of publicity which the proceedings would have. It is not to be denied or concealed that there is coming into existence a certain rivalry between what may be called the German and the English schools. I am anxious that the former shall not wrongly claim American adhesion on the one hand, and on the other that American opinion shall not be wrongfully interpreted as favoring the German system. With reference to this last, for instance, I have just received the following from M. C. Wolf, in the course of a reply to my Washington letter: "Je vous félicite vivement de vos travaux sur le pendule, et surtout d'avoir pris une autre voie que celle dans laquelle les Americains se sont lancés, á la suite des Allemands et des Suisses, J'ai eu ici de vives discussions avec M. Ch. Peirce au sujet de ses expériences avec le pendule reversible de Repsold, instrument qui me parait construit dans de déplorables conditions de stabilité."

Now there is just enough truth in this to make one regret the misapprehension as to the American position. But so long as your survey uses a reversible pendulum, without some very distinct statements as to the principles, such misapprehensions will continue, and the Germans will deny that the Americans stand by the differential method.

I hold it to be a very lamentable thing that men of zeal, eager to advance science, should continue to be misled by the old school of physics into launching upon the difficult and precarious enterprise of absolute determinations of gravity, generally in ignorance of the real difficulties of the research, and *always* indifferent to the utility of such determination. The German school is responsible for this.

This brings before us prominently the question of utility, a question which has always been shirked or disposed of by common-places, devoid of any real force. I know this through having urged (for nearly twenty years), very much in vain, the views which I hold at this day, and which I now see gaining ground so slowly. I sum it up in the broad statement that *we do actually know* the mean figure of the earth *as well as we can know it* so long as the irregularities which deform it remain unknown. It is not the force of gravity which we seek, but the irregularities of the surface.

Now this is one of the points on which, at a conference, I should wish to find unanimity, if it is true, or if not, then a better and more indisputable dogma to take its place.

With this as a foundation, the question becomes one of ways and means to study the irregularities with advantage. Here there is great room for difference of opinion. What can be done depends on the cost, in its most general sense, of doing it. Absolute measurements are indefinitely costly, and may be put aside. Differential measurements, also, are frightfully costly, if conducted as I have been conducting these; but I have had in view to prove incontestably that results of practical value can be obtained with a tenth, perhaps a twentieth, the labor that I devoted to them. All depends on the method.

Another point involved in the question of utility is, as you say, as to the degree of precision demanded. All stations of observation should be recognized from the first as belonging to one of two categories—either they are *points d'appui* or they are not. In the latter case the precision demanded is governed by the degree of irregularity which experience teaches as governing the quantity measured, the distances which separate the points being taken into account. A high

degree of precision is plainly needless (for points of the second order) if they are widely scattered, whereas if a number of such points are crowded in a small area, their precision ought to be higher because of the information to be gained by intercomparison. Points of the second order widely scattered have no present value other than as indicating tentatively the degree of disturbance. From this point of view there would seem to be an advantage in placing the stations always *in pairs* so as to indicate the variability as well as the variation.

I must now go farther. Scientific observation has two distinct aspects. Viewed in one way, it is seen as a means of livelihood, as an intellectual enjoyment, as an employment, as a pursuit worthy of recognition and encouragement, for every reason except that of its ultimate utility. Viewed in the other way, it is a source of expenditure and a drain upon the available power of the time and country, which can only be justified if it attains useful ends in a reasonably expeditious way. For myself, I doubt if I could conscientiously recommend the expenditure of public money on pendulum observations on the ground of their utility; although I could and would recommend it for pendulum *experiments*, having for their object to increase the facility of observation; for I imagine, as things now stand, the prospect of obtaining results in sufficient number and frequency to enable us to study the irregularities successfully is very remote. You will doubtless recognize in this the ground of my inability to offer my services, backed by hopes of support from the British Government, for the prosecution of differential work on this continent. But I have no business to press such considerations on other people, nor to bring them forward at a conference, except incidentally in discussing the proper distribution of stations and the degree of precision demanded. The same arguments and motives had a successful campaign in dictating my latitude work in India; and there is room for their application in the present case. They point out the urgent need for economy in every detail of installation and observation—in the choice of stations and the buildings to be occupied, in the distribution of time to be taken up by the observations, and by the calculations respectively, so as to get, in short, as many results of a sufficient degree of accuracy, and no more, as possible within the year. All this, and much more, seems to me to be involved in the broad question whether or not pendulum research can be satisfactorily carried on with a view to studying the earth's irregularities.

Another phase of this question should deal with the distinction between a study of the large and of the small irregularities. There is a vast difference between such work as that of Malaspina, of Freycinet, of Sabine, of Foster, of Lütke, and of all the other explorers; and that of Kater in England, and of Basevi in India. The work before you here has or may have the characters of both; for the vastness of your disposable area demands a large plan, while the numerous opportunities for prosecuting minuter internal exploration require more special consideration. The degree of precision to be aimed at must be governed partly by what we know of possible variation and partly by what the instruments are capable of. Here, as in other branches of research, we should bear in mind that there is almost always a point in the scale of precision where it becomes questionable whether it would not be wiser to change the whole system if higher precision is wanted. Below that point there is no difficulty. Above it the price to be paid becomes onerous.

You will readily perceive that I fully recognize, as one of the chief subjects upon which discussion should turn, this of requisite precision. At the same time I doubt if it can be discussed to much advantage by those who are not intimate with the figures actually to hand. I would therefore avoid the *vexata quæstio* of probable errors and keep to principles. It is by the latter alone that plans of operation can be governed reasonably. "Frequency to be preferred to accuracy," for example, is a principle easy to limit or extend as may be desired, and far more widely intelligible to the uninitiated than any specification in figures suited to certain categories of cases. Above all we should aim at being intelligible. Without that there will be no outside interest and no support.

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Yours truly,

J. HERSCHEL.

SIX REASONS FOR THE PROSECUTION OF PENDULUM EXPERIMENTS.

By C. S. Peirce.

1. The first scientific object of a geodetical survey is unquestionably the determination of the earth's figure. Now, it appears probable that pendulum experiments afford the best method of determining the amount of oblateness of the spheroid of the earth; for the calculated probable error in the determination of the quantity in question from the pendulum work already executed does not exceed that of the best determination from triangulation and latitude observations, and the former determination will shortly be considerably improved. Besides, the measurements of astronomical arcs upon the surface of the earth cover only limited districts, and the oblateness deduced from them is necessarily largely affected, so that we cannot really hold it probable that the error of this method is so small as it is calculated by least squares to be. On the other hand, the pendulum determinations are subject to no great errors of a kind which least squares cannot ascertain; they are widely scattered over the surface of the earth; they are very numerous; they are combined to obtain the ellipticity by a simple arithmetical process; and, all things considered, the calculated probable error of the oblateness deduced from them is worthy of unusual confidence. In this connection it is very significant, as pointed out by Colonel Clarke (*Geodesy*, p. vi), that while the value derived from pendulum work has for a long time remained nearly constant, that derived from measurements of arcs has altered as more data have been accumulated, and the change has continually been in the direction of accord with the other method. It is needless to say that the comparison of the expense of the two methods of obtaining this important quantity is immensely in favor of pendulum work.

2. Recent investigations also lead us to attach increased importance to experiments with the pendulum in their connection with metrology. The plan of preserving and transmitting to posterity an exact knowledge of the length of the yard after the metallic bar itself should have undergone such changes as the vicissitudes of time bring to all material objects, was at one time adopted by the British Government. It was afterwards abandoned because pendulum operations had fallen into desuetude, and because doubts had been thrown upon the accuracy of Kater's original measure of the length of the second's pendulum. Yet I do not hesitate to say that this plan should now be revived, for the following reasons:

First, because measurements of the length of the second's pendulum, although formerly subject to grave uncertainties, are now secure against all but very small errors. Indeed, we now know that the determinations by Kater and his contemporaries, after receiving certain necessary corrections, are by no means so inaccurate as they were formerly suspected to be. Secondly, metallic bars have now been proved, by the investigations of Professor Hilgard and others, to undergo unexpected spontaneous alterations of their length, so that some check upon these must be resorted to. To this end the late Henri Ste. Claire Deville and Mascart constructed for the International Geodetical Association a metre ruled upon a sort of bottle of platin-iridium, with the idea that the cubic contents of this bottle should be determined from time to time, so as to ascertain whether its dimensions had undergone any change. I am myself charged with, and have nearly completed, a very exact comparison of the length of a metre bar with that of a wave of light, for the same purpose. Neither of these two methods is infallible, however, for the platin-iridium bottle may change its three dimensions unequally, and the solar system may move into a region of space in which the luminiferous ether may have a slightly different density (or elasticity), so that the wave length of the ray of light used would be different. These two methods should therefore be supplemented by the comparatively simple and easy one of accurately comparing the length of the second's pendulum with the metre or yard bar. Thirdly, I do not think it can be gainsaid by any one who examines the facts that the measurements of the length of the second's pendulum by Borda and by Biot in Paris and by Bessel in Berlin do, as a matter of fact, afford us a better and more secure knowledge of the length of their standard bars than we can attain in any other way. So also I have more confidence in the value of the ratio of the yard to the metre obtained by the comparison of the measurements of the length of the second's pendulum at the Kew observatory by Heaviside in terms of the yard and by myself in terms of the metre than I have in all the

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elaborate and laborious comparisons of bars which have been directed to the same end. I will even go so far as to say that a physicist in any remote station could ascertain the length of the metre accurately to a one hundred thousandth part more safely and easily by experiments with an invariable reversible pendulum than by the transportation of an ordinary metallic bar.

A new application of the pendulum to metrology is now being put into practice by me. Namely, I am to oscillate simultaneously a yard reversible pendulum and a metre reversible pendulum. I shall thus ascertain with great precision the ratio of their lengths without any of those multiform comparisons which would be necessary if this were done by the usual method. These two pendulums will be swung, the yard one in the office of the Survey, at a temperature above 60° F., which is the standard temperature of the yard, the other nearly at 0° C., which is the standard temperature of the metre; and thus we shall have two bars compared at widely different temperatures, which, according to ordinary processes, is a matter of great difficulty. The knife-edges of the pendulums will be interchanged and the experiments repeated. Finally, the yard pendulum will be compared with a yard bar and the metre pendulum with a metre bar, and last of all the yard pendulum with its yard bar will be sent to England, the metre pendulum with its metre bar to France, for comparison with the primary standards; and thus it is believed the ratio of yard to metre will be ascertained with the highest present attainable exactitude.

3. Geologists affirm that from the values of gravity at different points useful inferences can be drawn in regard to the geological constitution of the underlying strata. For instance, it has been found that when the gravity upon high lands and mountains is corrected for difference of centrifugal force and distance from the earth's centre, it is very little greater than at the sea-level. Consequently it cannot be that there is an amount of extra matter under these elevated stations equal to the amount of rock which projects above the sea-level; and the inference is that the elevations have been mainly produced by vertical and not by horizontal displacements of material. On the other hand, Mendenhall has found that gravity on Fujisan, the well-known volcanic cone of Japan, which is about 12,000 feet high, and which is said to have been upheaved in a single night, about 300 B. C., is as much greater than that in Tokio as if it had been wholly produced by horizontal transfer. This conclusion, if correct, must plainly have a decisive bearing upon certain theories of volcanic action. Again, it has long been known that gravity is in excess upon islands, and I have shown that this excess is fully equal to the attraction of the sea-water. This shows that the interior of the earth is not so liquid and incompressible that the weight of the sea has pressed away to the sides the underlying matter. But in certain seas gravity is even more in excess than can be due to the attraction of the ocean, as if they had been the receptacle of additional matter washed down from the land. It is evident that only the paucity of existing data prevents inferences like these being carried much further. On the two sides of the great fault in the Rocky Mountains gravity must be very different, and if we knew how great this difference was we should learn something more about the geology of this region; and many such examples might be cited.

4. Gravity is extensively employed as a unit in the measurement of forces. Thus, the pressure of the atmosphere is, in the barometer, balanced against the weight of a measured column of mercury; the mechanical equivalent of heat is measured in foot pounds, etc. All such measurements refer to a standard which is different in different localities, and it becomes more and more important to determine the amounts of these differences as the exactitude of measurement is improved.

5. It may be hoped that as our knowledge of the constitution of the earth's crust becomes, by the aid of the pendulum investigations, more perfected, we shall be able to establish methods by which we can securely infer from the vertical attractions of mountains, etc., what their horizontal attractions and the resulting deflections of the plumb-line must be.

6. Although in laying out the plan of a geodetical survey the relative utility of the knowledge of different quantities ought to be taken into account, and such account must be favorable to pendulum work, yet it is also true that nothing appertaining to such a survey ought to be neglected, and that too great stress ought not to be put upon the demands of the practically useful. The knowledge of the force of gravity is not a mere matter of utility alone, it is also one of the fundamental kinds of quantity which it is the business of a geodetical survey to measure. Astronomical latitudes and longitudes are determinations of the direction of gravity; pendulum experi-

ments determine its amount. The force of gravity is related in the same way to the latitude and longitude as the intensity of magnetic force is related to magnetical declination and inclination; and as a magnetical survey would be held to be imperfect in which measurements of intensity were omitted, to the same extent must a geodetical survey be held to be imperfect in which the determinations of gravity had been omitted; and such would be the universal judgment of the scientific world.

NOTES ON DETERMINATIONS OF GRAVITY.

By Assistant C. A. SCHOTT.

The conference was invited by the Superintendent of the Coast and Geodetic Survey for the purpose of eliciting an interchange of views respecting the utility and best means of prosecuting pendulum research in the interest of science in general, and with especial regard to the future work of the Coast and Geodetic Survey.

Major Herschel, R. E., having expressed his willingness to favor the meeting with his presence and give it the benefit of his great experience in pendulum work, the time of meeting must be considered extremely favorable.

The following rough notes are offered with a view of inviting discussion on some points considered of importance and interest.

Respecting the question of the utility to geodesy and geology of pendulum work as bearing on the figure and density of the earth, it is sufficiently answered by the resumption of this work in recent years in the leading government surveys conducted in Europe, Asia, and America; but in carrying on these operations different opinions continue to be held as to the best and most economical means both with regard to form of instrument and method of observation.

It may be added that the results already reached are in themselves sufficient to stimulate the further prosecution of the work, since they render it almost certain that still more valuable deductions may be reached.

The pendulum work executed for some years past under the direction of the late Superintendent of the Coast and Geodetic Survey had for its immediate object the study, theoretical and practical, of the best methods available, and to gather the results at various important pendulum stations in Europe, to bring them into strict comparability, and to form a connected system which may be used for combination with similar operations commenced in the United States.

Mr. C. S. Peirce, Assistant, Coast and Geodetic Survey, having brought this work to a close in Europe,* its future prosecution at home now claims renewed attention, both with respect to the economy and efficiency of the plans which it may be desirable to adopt.

The value of the pendulum results depending largely upon their direct comparability and the geographical extent, it would in the first place appear most desirable, in order to form a second and independent connection of the pendulum work executed on the other side of the Atlantic, to swing the American pendulums at the two stations, Washington and Hoboken, just occupied by Major Herschel with the old pendulums belonging to the Royal Society, and to add thereto at least one more American station in order to secure three stations of satisfactory accord between these instruments.

It is, perhaps, the general opinion that differential measures are at present more desirable than absolute measures, since undoubtedly greater accuracy can be reached in the former and a greater number may be secured with the same expenditure; indeed, the determination of the length of a second's pendulum is, in geodesy, of less importance than a knowledge of ratios of times of oscillation of an invariable pendulum swung at stations on a line selected for investigation.

The determination of the length of a second's pendulum is quite a special operation, to be undertaken only at a base station.

While the mean figure of the earth may be considered as tolerably well known from the fact of the close approach of the value of the compression as deducted from purely geodetic operations and

*Mr. Peirce remarked that that work was not yet quite completed.

from pendulum work, yet this may be taken only as an encouragement for the joint prosecution of both operations.*

On the other hand, our knowledge of the magnitude of the mean figure of the earth is, in the opinion of some, not quite as satisfactory, and in support of this it may be stated that the recent abandonment, in the Coast and Geodetic Survey, of the Besselian spheroid of revolution for that of Clarke, involving in our latitude an increase of the radii vectores between one-third and one-half of a statute mile, was no inconsiderable change; and though we cannot look forward to any future change of such a magnitude, the difference was sufficiently large to make itself felt in our oblique arc lying along the Atlantic coast between Maine and Georgia.

The combination of the Peruvian arc, the only one in America as yet worked in with the meridional arcs measured in the eastern hemisphere, with the two arcs measured by the Coast and Geodetic Survey, viz, the Nantucket arc and the Pamplico-Chesapeake arc, showed a satisfactory accord (that is, within limits that may be explained by local deflections). This seems to prove that the curvature of North America does not sensibly differ from the curvature in the same latitudes of the eastern hemisphere; yet the conclusion is weakened by the fact that the Peruvian arc is extremely short, and, what is worse, is supported by but two astronomical latitudes, and that in a region where local deflection probably exists of an excessive magnitude. It is true the computed corrections to the two latitudes are small, and this might lead to too great a confidence in the assigned value of the magnitude of the earth's axis. A remeasure and extension of this arc to be supplied with a considerable number of astronomical latitudes would seem to be a great desideratum, especially when we consider the important position of the arc, giving it, so to say, undue leverage in comparison with the position of other arcs. It is not at all unlikely that the results of its remeasure and extension may have an important effect on our knowledge of the probable uncertainty in the assigned value for the resulting mean figure of the earth.†

This mean figure might be defined as that of a geometrical solid whose surface most nearly approaches the equipotential surface of the mean sea level, intersecting it so that the aggregate of the volumes above and below it may be equal and a minimum. It would be the object of geodesy to trace out on this geometrical surface the boundaries of these areas, and to determine their elevations above or depression below it; in fact, work out the actual irregularities with reference to this ideal mean figure.

For pendulum research the region of the Mississippi Valley would seem to be very favorable, both in regard to its geological structure, as presenting broad features, and with respect to gradual changes in elevation of surface between New Orleans and our northern boundary, near the forty-ninth parallel, the land rising but little above 1,000 feet. Here a study of the law of change of gravity with the latitude seems inviting.

Supplementary to the above line, the thirty-ninth parallel might be chosen for the study of the law of change of gravity with altitude, starting from the sea level and passing over the inconsiderable elevations of about 2,500 feet on the Appalachian range and the descent to the Mississippi Valley, we have the gradual rise of the great plains up to 8,000 feet, and next the lower Rocky Mountain plateau, with a final return to the sea level. While on the first named line about 6 or 8 stations might suffice, on the second from 12 to 15 ought to be contemplated.

Respecting the kind of pendulum most suitable for differential measures of gravity, there may be little difference in practice between the use of two invariable pendulums, the one to check the

*Major HERSCHEL. I do not regard the agreement of geodetic and gravity figures an argument for the latter. I can never regard the geodetic figure, derived from the comparisons of the curvatures of certain *land* portions only, as a true indication of a figure which is two-thirds sea. There is every reason to regard the land curvatures as too great.

†Major HERSCHEL. I should hardly advocate a remeasurement of the Peruvian arc as a step towards a better determination of the earth's figure. It has the fatal disadvantage of position in a valley between vast mountainous tracts.

Mr. PEIRCE. Major Herschel's objection to the important scheme of remeasuring the Peruvian arc would apply, *à fortiori*, against allowing that arc to enter into the determination of the figure. In my humble judgment an American figure of the earth, wholly from geodetic measurements on these continents, is so greatly wanted that it is the duty of this Survey to undertake it. Although the Peruvian arc is at present bad, I should think that if sufficiently extended and provided with an adequate number of latitude determinations, the objections to it would nearly disappear.

other, and an unchangeable pendulum of a plain rod (of lenticular cross-section) having two fixed knife-edges symmetrically disposed; the means for correcting for difference of temperature and for difference of pressure from respective mean quantities to be determined at a base station. Observations to be made in 4 positions (upper knife-edge, lower knife-edge, face front, and face back). The accord of the 4 results will furnish a criterion for the unaltered condition of the pendulum.

A reversible pendulum of outer symmetrical form may also be made to answer the purpose, provided it be swung only with heavy end down (face front and back) and no change whatever is made in the supporting knife-edge or in any other part of the instrument. Two such pendulums would seem desirable in order to detect any change due to accident. With such a pendulum the correction for difference of pressure can be applied with greater certainty than in one of the other forms.

Respecting the stand of the Repsold apparatus, experience has shown it to be unfit for the work, and stiffer support should be provided.

If pendulums could be swung through 24 hours the result could be made independent of variations in the clock rate due to the daily variation of temperature and pressure. The same standard time stars should be observed each night. For shorter durations of swing, say for 6 hours only, this advantage might in a measure be secured either by making four fresh starts and thus continue the work during twenty-four hours, or if that be too laborious, to observe on the first day, say from 6 to 12 a. m. and p. m., and on the following day from 0 to 6 p. m. and a. m., and unite the results into one, or in general, for any station by a *symmetrical distribution* of the swings over the twenty-four hours.

Time furnished telegraphically by an observatory whose clock is protected from changes of temperature and pressure will be preferable to any local determination at a field station.

Should the duration of swing be too limited for this scheme, night work may be recommended, with a set of transit observations just before and another immediately after the close of a swing, the same two sets of stars to be used each night and for several stations as long as practicable.

Three days successful work at any one station may suffice, and about two weeks might be estimated for the time required for occupation during the best season. The observatory to be prepared by an advance party.

The method of coincidences furnishes all needful accuracy, but if, in the absence of a clock or otherwise, a chronometer be used (as more portable and less liable to injury), coincidences of the chronometer beat with the transit of the pendulum over a vertical line might be tried.

The question whether or not it is advisable to swing in a vacuum chamber (say at a density just below any that might naturally be expected at a place which it is proposed to visit) would seem to depend largely upon the time a pendulum can be made to swing advantageously. If its sectional dimensions are such as to displace much air and require it to do much work against friction, the duration of swing may be so short as to demand the use of an exhausted receiver. What the experience is with the new reversible pendulums of the pattern of the one sent last summer to one of the polar research stations of the Signal Corps the writer is not informed.

The above notes are respectfully submitted.

MAY 13, 1882. CHAS. A. SCHOTT.

COMMUNICATIONS.

GENERAL REMARKS UPON GRAVITY DETERMINATIONS.

By Major (now Lieut. Col.) JOHN HERSCHEL, R. E.

The following propositions are *from my point of view*, but seem likely to be assented to in the main by other members of the conference.

1. *Figure of the earth.*—By this we imply the actual (or conceivable) continuous water surface as exemplified by the mean sea level; which surface may be everywhere nearly, though nowhere fully, represented by some assumed simple geometrical figure, such as an elliptic spheroid, to be known *ad hoc* as the *mean* figure.

2. *Object of pendulum research.*—If we regard the mean figure as known, then the object of pendulum research is, in the first place, to trace out the degree of separation everywhere subsisting between the actual and the mean figures; or, if it should appear that by a change of the mean figure there would result a less degree of separation, then to ascertain, first, what should be the amount of this alteration, and then to trace out the residual separation. Bearing in mind the large body of past work, which has undoubtedly sufficed to indicate very closely what the mean figure is, it should now be recognized as more particularly the object of pendulum research to enlarge our knowledge of the *irregularities of figure* rather than to aim at improving the *mean* figure; which after all can never be anything more than one *of reference*, by which to describe the actual figure.

3. *Extension of research among the irregularities.*—This is *prima facie* desirable, especially when geodetic surveys are in progress, or are certain to be instituted as civilization advances. But gravimetrical exploration in regions which can never be reached by surveying operations is of scarcely less importance.

4. As regards *distribution of stations* of observation, there seems to be nearly equal advantage in laying them out in a *linear series* at sufficiently close intervals, or superficially scattered over a limited selected area, with a view to tracing out the *sectional* or *solid* forms of the existing *irregularities.*

5. *The absolute force of gravity.*—If this also be admitted as an ultimate object of pendulum research, it must be remembered that it can only be determined for the whole earth when the exact relation of the place of observation to the whole surface is correctly known. It follows that a precise knowledge of the absolute force of gravity for the earth as a whole is not at present attainable. There are, nevertheless, reasons for now determining, with all the precision at present possible, *the length of the second's pendulum* at different places on the earth's surface.

6. *Reasons for prosecuting absolute determinations.*—Regarding the local force of gravity as a constant, the length of a pendulum is a function of its rate of oscillation; or, in other words, its rate is a measure of its length. From this it follows that lengths, otherwise incommensurable, can be compared through their corresponding times of oscillation, because we have means (in the pendulum itself, for instance) of comparing together, with any desired degree of precision, these times. Thus, for example, the metre and the yard can be compared by this means (as I understand) with greater precision than by the complicated system of linear comparisons requisite to measure their difference in terms of each.

7. *Constancy of gravity tested against constancy of length.*—This is another reason for determining with the utmost precision the length of the second's pendulum in terms of this or that standard. For if, in the far distant future, there should appear a concurrence of testimony indicating change, it might be brought home to either of the bars, or even to gravity itself, according to the evidence. The absence of the requisite evidence in the past would be a grave reproach hereafter.

8. *The invariable pendulum.*—The impossibility of ascertaining the exact relation of any station to the whole surface, short of a general knowledge of the latter, calls necessarily for such explorations as are set forth in Article 2. It is generally acknowledged that the differential pendulum—of which the "invariable" may be regarded as the type—is best adapted for such work. The pattern known as Kater's has hitherto been without a rival; but any pattern will answer the purpose in which the principle of invariability—*i. e.*, fixity of knife-edge and absence of all movable parts—is embodied.

9. *The reversible pendulum* is recognized as having many excellent qualities; and is capable of being used *temporarily* as an invariable pendulum. But its proper field is the absolute measurement for which it was designed; for if its knife-edges are interchangeable it is liable at any time to have its invariable character destroyed, either intentionally or accidentally.

10. With regard to the degree of precision to be aimed at, nothing very definite can be laid down, since it depends largely on the circumstances. A gross error in a solitary arctic station, for instance, might be of little consequence, while an error of even a small fraction of a second in the difference between two central points would entail far-reaching consequences. When the object is tentative exploration only, accuracy may well be sacrificed to expedition and frequency. And in general it should be remembered that the local disturbance varies with the change of site. What the rate of this change may be can only be guessed until data are obtained. A group of contiguous determinations of a low order of accuracy would always be more valuable than a single one of the

very highest order. A solitary station can contribute only to the general problem of mean figure and will of necessity be vitiated by the amount of the local disturbance, as to which there is no evidence. If the range of such disturbance *on the whole of that parallel* were known, it would not be unreasonable to take one-fourth part of that as the range of probable error permissible in the determination itself. Every consideration which takes into account the existence of local disturbance points to the preference to be given to frequency of distribution rather than accuracy of result. Moreover, it is difficult, if not impossible, to estimate the probable error in any case whatever. The history of pendulum observation abounds with inexplicable contradictions and anomalies indicative of unknown causes of error; and hardly a single observer has ventured to estimate the probable error of his result. Practically, the question of precision is cut by a variety of circumstantial exigencies; and it would seem best to leave it at the discretion of the observer, or director of the work.

11. *Other modes of research.*—The foregoing indicates so plainly the need of tentative exploration of a low order of accuracy that it is very much to be desired that some simpler means should be found of obtaining at least a rude measure of the local deflection. Various statical modes have been proposed, but none has yet shown a satisfactory test. That a "stathmometer"—a term designed to leave untouched the present use of "gravimeter"—will some day be invented is highly probable. It might be, perhaps, the sooner if the very great need for it were more widely known, and if, at the same time, it were understood that its object would be served even though it should fail to rival the pendulum in accuracy.

J. HERSCHEL.

Mr. PEIRCE. The conception which Major Herschel has presented for the purpose of gravity determinations requires thorough study. Considered from a purely mathematical point of view, it is certain that if we know the distribution of gravity over the whole earth, or even over a large region, we can deduce corrections of the earth's radius vector. Within 20° of the station whose radius vector was to be corrected an accurate knowledge of the residuals of gravity would be necessary, while beyond that point a rougher determination would suffice. But whether this conception of the nature of pendulum work could be usefully adopted at the present time, or until two or three times the existing number of stations have been occupied, is a practical question in regard to which there is something to be said on both sides. The views of Major Herschel, though founded on known propositions of mathematics, are so novel and so far-reaching in their consequences that we cannot commit ourselves to an immediate decision in regard to them. But they offer much food for reflection and study, and I am quite sure that apart from the important service that Major Herschel has done us in connecting the American (and through that the continental European) system of stations directly with the great *réseau* of the English work by means of the Kater invariable pendulums, American geodetical science is under great obligation to him for the suggestions contained in the paper he has presented to the conference.

OPINIONS CONCERNING THE CONDUCT OF GRAVITATION WORK.

By C. S. PEIRCE.

I. There are six reasons for determining gravity, which I have already set forth.

II. In determining the compression of the earth's spheroid from the variation of gravity, it is best, for the present, to reject all experiments not made with Kater's invariable pendulums. But the completion of Major Herschel's history of pendulum determinations is greatly to be desired.

Major Herschel thought the limitation to Kater's invariable pendulum too narrow; and pointed out that it would exclude the work of Freycinet and of Duperrey as well as a great part of that of Foster.

III. The ordinary correction for continental attraction is vastly too great. It should be omitted.

Major Herschel remarked: "Admitting this as a conclusion drawn from the facts, it must not be forgotten that this is nothing but an *à posteriori* dogma. I do not see how it can be lawfully acted upon, unless the assumption that it has a true *à priori* cause is kept continually in view *as such.*" Mr. Peirce replies as follows: "In my opinion, the correction for continental attraction is not only refuted by observation but it has no *à priori* support from premises which we have any reason to suppose true. If we could make our pendulum experiments underground at the level surface of which the sea-level is a part, there would be no correction to be made for continental attraction. Since they cannot be made there, the observed gravity had to be reduced to what it would be at that level. The coefficient of this reduction depends entirely on the distances of the successive level surfaces without reference to the situation of the material masses, except so far as this situation affects those distances. To calculate the reduction exactly upon this principle would be impossible; but we approximate to it within the limits of other neglected terms if we use Young's rule* without the term depending on continental attraction. Stokes reaches this same result; but having reached it, he remarks that if this theoretically correct procedure were used the figure of the earth would be less regular than in using the old rule. He offers no proof of this, however; and the facts which have been ascertained since his memoir was written prove that the contrary is true. Young's rule supposes that if all the rock rising above the sea-level were annihilated, the present level surface would remain a level surface, which is certainly not true. When Major Herschel admits, as he seems to do, that a certain conclusion is proved by the facts but at the same time maintains it cannot be 'lawfully' acted upon, he seems to be using the language of a game with conventional rules. I would propose to act upon any proposition that seems to be true." Mr. Schott agreed with Mr. Peirce.

NOTE BY MAJOR (NOW LIEUTENANT-COLONEL) HERSCHEL.—I should like the issue between Mr. Peirce and myself, on the general question of the reduction on account of continental or mountain attraction, to be somewhat differently stated than it appears here. In the first place, what I have said about an "*à posteriori* dogma" had reference, if I remember rightly, not so much to the rejection of the continental reduction *in toto* as to its modification by an arbitrary constant, about which Mr. Peirce is now silent. However, my words are general enough, no doubt, to cover this rejection in any form, but all I maintain is that the assumption on which it rests shall be plainly presented and never disregarded. Mr. Peirce contends that the reduction for continental attraction has no claim to any such apologetic treatment, urging that, as it has no rational foundation, it should go; the displacement of matter, which appears as land elevation, being in all probability a merely vertical displacement, while for the continental attraction to have any jurisdiction, it would be necessary to show the existence of at least a very considerable *lateral* displacement as the cause, or part cause, of elevation. Now it is just here that I would step in and urge the claim of the latter, of which there is ample proof in the enormous thickness and extent of stratified deposits, all of which must be due to erosion and removal horizontally. Something also might be said for glacial transfer, and for lava streams.—J. H.

JULY 4, 1883.

IV. The residuals of the different stations are materially diminished by subtracting the entire downward attraction of the ocean, liberally estimated.

Mr. Peirce admitted that this would involve a falsification of the earth's figure, so as to give a sort of mean figure.

Major HERSCHEL. "The addition of the sea attraction has a legitimate *raison d'être*, as it is reasonable to affirm that the *sea* matter is *added* matter."

Mr. PEIRCE. It seems to me if the attraction of the sea is to be allowed for because it is added or horizontally displaced matter, then the attraction of the continents should not be allowed for, because it is not added, that is, is only vertically displaced matter."

V. The occupation of additional arctic stations, if done well, would probably improve the value of the compression. New equatorial stations are also desirable, but new stations in middle latitudes can hardly affect the value of the compression.

Major HERSCHEL. "The actual distribution is shown in a diagram given in my Appendix to Vol. V of the India Survey. This diagram shows how very restricted is the area actually occupied by differential stations. The southern hemisphere is very poorly represented."

* This so-called rule is identical with *Bouguer's* formula for the same.

VI. In middle latitudes, the main thing at present is to study the relation of gravity to geographical and geological conditions.

Major Herschel concurred.

VII. Gravity determinations should be made at intervals on lines of geodetic levels, and the levels be corrected accordingly.

Mr. Schott concurred.

VIII. Economical questions should, as far as possible, be solved by the application of mathematics.

IX. The invariable reversible pendulum reunites the advantages of the two instruments possessing the one and the other of these characters, and is to be recommended under the limitation implied in No. II above.

Major HERSCHEL. I am obstinately opposed to any attempted combination of the invariable and reversible principles in one instrument. They are incompatible; and the combination is impossible without so modifying the invariable principle that it is practically abandoned altogether. It is very undesirable that any new element of doubt should be imported into the already much abused term "invariable." It was first used by Godin, as well as by Bouguer, and by la Condamine. They all meant a rigid pendulum with fixed knife-edge. Kater borrowed the word from the French, but as he at the same time introduced a "convertible" pendulum, with two fixed knife-edges, which made a great noise abroad, the two got mixed up, and the German text-books (copying from Muncke*) flagrantly confused the two. Still, the German use is strict in denoting a rigid pendulum with fixed knife-edges. But Mr. Peirce now intends to upset this last stronghold of the "invariable" pendulum by making it variable at the will of the observer. The invariable pendulum proper *can* undergo no change without violating its name. Closely connected with the term "invariable," as designating a particular form of construction, is the term "invariability" as denoting a principle involved in its design. I cannot possibly demur to the construction of any form of pendulum which may be thought desirable; but I do urgently protest against the designation of it in a way to create needless confusion. The principle of the invariable pendulum supposes it to continue unchanged as long as human carelessness will permit, or longer if possible. But by making its knife-edges interchangeable, with a view to giving it a greater range of utility, this first characteristic is voluntarily destroyed; and in becoming reversible *in the full sense of the word* it ceases to be invariable. Why, then, adopt a self-contradictory compound name which serves no purpose but to ruin the word as well?

At the same time I must say that Mr. Peirce seems to *read the word* differently.

Mr. PEIRCE. By an invariable reversible pendulum, I mean one in which the knives remain in place from one station to another. Major Herschel's objections seem to be directed against the use of the word invariable as applied to such an instrument; but it is not so much the word as the thing that I advocate. The Geodetical Association has unanimously recommended the reversible pendulum, and I should certainly think that their opinion ought to be respected, even if I did not share it. On the other hand, there is much to be said in favor of differential instruments. I am not aware that Major Herschel has brought forward any objection to reuniting the differential or invariable and the reversible principles in one instrument except this, that if the knives can be changed they might be changed by carelessness. But it appears to me that the whole weight of this argument, such as it is, is against the invariable pendulum. For there is no fabrication of human hands that cannot be changed by carelessness. Can a Kater invariable pendulum be safely exposed to careless treatment? The difference between the ordinary invariable pendulum and the invariable reversible pendulum in this respect is that if the former suffers injury the work is hopelessly vitiated, while if the latter is injured, it is only necessary to fall back on the reversible principle. The following are the advantages which I think I see in the use of the invariable reversible pendulum:

1st. It satisfies the requirements of those who advocate the reversible pendulum, who constitute the greater weight of living authority.

* *Gehler's Physikal. Wörterbuch.* Art. *Pendel.* VII, pp. 304-407. Leipzig, 1833. By far the ablest treatise, historical and otherwise, of its day, and perhaps so still.

2d. It ought to satisfy those persons who advocate the invariable pendulum.

3d. It determines gravity in two nearly independent ways, without more experiments than are necessary for a single determination. When these results agree they may be assumed to be correct.

4th. If the instrument be considered as a differential one, the difference in the reduced time of oscillation with heavy end down and with heavy end up must remain unchanged so long as the instrument is invariable and can hardly escape change otherwise. And from this change the necessary correction can be calculated and applied. If on the other hand the instrument be considered as an absolute one, the same difference is the best test of the accuracy of the work.

Mr. SCHOTT. For the strict intercomparability of results at two or more stations, I think it to be almost essential to satisfactory work that an absolutely invariable pendulum be employed. This condition would, however, not exclude the use of a pendulum having interchangeable knife-edges, provided that between any two stations no such interchange took place, while the interchange might be effected after the particular comparative measures were secured.

NOTE BY MAJOR (NOW LIEUTENANT-COLONEL) HERSCHEL.—The view of this subject here presented by Mr. Schott, in this last paragraph, is so sensibly correct that only a strong conviction that it does not meet the whole case, and is directly opposed to the principle of inviolability which I wish to see recognized, would tempt me to add to this discussion. We are agreed, and universal practice shows it to have been widely recognized, that invariability must be maintained during at least the whole course of a series of differential determinations. [In the East Indian series, for instance, it was maintained during eight or nine years, and at more than thirty installations.] No one pretends to set any limit, either to the time or to the number of stations, which is to restrict a series of differential measures. But it is said, "when the series is completed there is no longer any need to guard or preserve the pendulum from change; its work is done." But it is just at this point, I contend, that we ought, on the contrary, to be growing more and more solicitous for the protection of the pendulum. The more stations it has visited, the more intimate is our knowledge of its time of vibration, or vibration-number, or whatever be the function we may adopt by which the results of observation are to be expressed. Even if, at the time, only one of the stations visited was a "known" station, we ought yet to contemplate and anticipate the time when, by the superposition of later series, the fundamental vibration-number (*i. e.*, its equatorial vibration No.) shall rest on more than one, perhaps on many known stations. Even if such considerations as these fail to convince, some weight will surely be conceded to the argument that, as one continuous series is better than two or more, covering the same stations, and as by merely guarding the pendulum stringently during the temporary pause between two sets of operations, otherwise called series, these will in fact constitute one only, it is right to take the proper precautions to bring this about. I confess I am surprised, not that this principle has not been acted upon, in times past, but that it should at this day need more than the most cursory enunciation, and that we are even now debating whether we shall not continue to throw away one-half of the net results of each set of observations with invariable pendulums. We do no less, when we break off a series and, by interchange of knife-edges, interrupt the continuity of a series.

JULY 4, 1883. J. H.

X. Four classes of errors affect the observed period of oscillation, as follows:

1st. Those which are nearly constant throughout the work at any one station. Such arise, for example, from the flexure of the support, from an error in the adopted coefficient of expansion at a tropical or arctic station, and from other causes.

2d. Those which remain nearly constant for a considerable time, say an hour or a day, but which vary from day to day. Such arise, for example, from the knife resting differently on the supports on different days, from erroneous determinations of temperature, or from similar causes.

3d. Those which are continually varying throughout the observations.

4th. Those which arise from errors in the comparison of the pendulum with the time-piece.

The first class of errors demand the most solicitous scrutiny. The other three classes may be distinguished by the study of the residuals of the observations. The third class is the most important of the last three.

XI. Further insight into the nature of the errors is obtained by comparing the residuals with large and with small arcs, and by comparing the residuals of the reversible pendulum in its two positions.

Small arcs and heavy pendulums are to be recommended.

Major HERSCHEL. In recommending "small arcs," Mr. Peirce leaves us to guess what magnitude he contemplates. In setting out upon my recent experience, I intended to swing in arcs as small as I could anyway *see* them, certainly below 30″. But I found that both Sir G. Airy and Professor Stokes were strongly opposed to such a course, and I abandoned the intention in favor of arcs falling from say 70′ to 7′. The objections urged were all theoretical. I should still advocate the practical testing of the doubt in any series of observations of an experimental character.

Mr. PEIRCE. I find the errors of observation are not increased by continuing the oscillation down to arcs of 1′.

XII. The method of coincidences as perfected by Major Herschel is to be recommended, especially in connection with a clock whose pendulum swings from knives.

XIII. The experiments should be continued for 24 hours, beginning and ending with star observations, when this is convenient. But this should not be absolutely required.

XIV. The swinging in *vacuo* is to be recommended.

Major Herschel dissented.

XV. The flexure of the support may be advantageously avoided by swinging two pendulums simultaneously on the same support with opposite phases. When this is not done the flexures should be measured, and in doing this the measures must be made at the middle of the knife-support or else the position of the instantaneous axis of motion must be determined.

XVI. The separation of the atmospheric effect into two parts is requisite for an exact temperature correction.

XVII. The influence of atmospheric moisture ought to be studied.

XVIII. The use of rollers in place of knives is to be condemned.

XIX. The probable accidental error of a determination of gravity must not exceed 5 millionths ($\frac{1}{200000}$), and the total which may reasonably be feared must not exceed 10 millionths ($\frac{1}{100000}$).

Professor Newcomb and others agreed to this, but Major Herschel and Mr. Schott objected to any numerical criterion of this sort.

XX. A good gravimeter is an important desideratum.

CONCLUSIONS PROPOSED BY PROFESSOR NEWCOMB, AMENDED AND ADOPTED BY THE CONFERENCE.

1. The main object of pendulum research is the determination of the figure of the earth. From a sufficient number of observations suitably distributed over the surface of the earth the actual figure may be determined.

2. A complete geodetic survey should include determinations of the intensity of gravity. These determinations should be made at as many critical points of local deflection and physical structure within the area of the survey as possible; and these should be combined with others distributed over the whole globe.

3. A minute gravimetric survey of some limited region is at present of such interest as to justify its execution.

4. Extended linear gravimetric exploration is desirable, to be ultimately followed by similar work distributed over large areas.

5. Each series of such determinations should be made with the same apparatus, so that the differential results should not be affected by constant errors peculiar to the apparatus.

6. While it is inadvisable at present to strictly fix a numerical limit of the permissible probable error of pendulum work, yet such determinations ought commonly to be accurate to the $\frac{1}{200000}$ part.

7. Since different pendulums may be used in different regions, all should be compared at some central station.

8. Determinations of absolute gravity will probably prove useful in comparing the yard and the metre, and they should at any rate be made in order to test the constancy of gravity against the constancy of length of a metallic bar.

9. In the present state of our experience, unchanged pendulums are decidedly to be preferred for ordinary explorations.

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