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 [illegible] 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 [illegible] 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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