Should the Laws of Gravitation Be Reconsidered?

Part I—Abnormalities in the Motion of a Paraconical Pendulum on an Anisotropic Support

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Whenever a physical theory is revised or transformed, it is revealed that, nearly always, the observation of one or several facts which could not fit within the framework of the theory in its then current form is at the base of the changes. The facts always remain the keystone on which the stability of any theory is dependent, no matter how important it may be.

For a theoretician really worthy of the name, it may be said in passing that nothing could be more interesting than a fact which runs counter to a theory until then held to be sound; for him, the real work begins at that point.

Max Planck[1]

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THE MOTIONS of a pendulum, suspended on a ball and resting on an anisotropic support, have statistically significant amplitude and periodic components of periods approaching 24 and 25 hours.

The installation and the experimental technique are briefly described. The observed motions result from four conjugate effects: the Foucault effect, an effect of the suspension release, the aleatory influence of balls, and, finally, a periodic influence.

The observed periodic structure cannot be considered as due to the disturbances of an aleatory order. Neither can it be considered as produced by an indirect influence of known factors (temperature, pressure, magnetism, etc.). Finally, it cannot be identified with periodic lunisolar effects resulting from the actual theory of gravitation.

A remarkable disturbance has also been observed at the time of the total solar eclipse—June 30, 1954.

At this stage of the discussion, the observed effects must be considered as produced by the direct action of a new field.

Findings

From 1953 to 1957, I carried out various experimental research projects on the motion of a pendulum resting *on an anisotropic support* S″ through a steel ball, this anisotropic support being characterized by very small differences in its elasticity as measured in two rectangular planes.[2]

Even though various types of pendulum were used in succession, I shall limit myself to a brief description of the arrangements used and the results obtained during the series of continuous observations which were run for 30 days in June and July of 1955.

Description of the Pendulum

The paraconical pendulum used was an asymmetrical one, consisting of a vertical bronze disc weighing 7.5 kg., attached to a bronze rod hung from a bronze stirrup E resting on a steel ball 6.5 mm. in diameter, free to roll in any direction on a horizontal plane surface S.

The latter was itself on a hollowed-out circular support S′, made of aluminum, with an extension A, 4.5 cm. thick. This design (hollowed-out part) made it possible for the pendulum to rotate while in motion, over a total angle of 210 centesimal deg. This circular support S′ finally rested on three micrometric screws V. The pendulum rod and its stirrup weighed 4.5 kg. so that the total weight of the pendulum was 12 kg. and the length of the equivalent elementary pendulum approximately 83 cm.

The steel balls in use were high-precision SKF balls, with bearing surfaces of tungsten carbide and cobalt.

The experiments were conducted in a basement, and the center of gravity of the pendulum was moving at a level of approximately 1.50 m. below the surface of the natural ground. Support S″ was bolted to a beam, pressed against the ceiling by a set of beams.

The pendulum and hanging device are shown in Figs. 1 to 4.

[1] *Initiation to Physics*, French transl., page 40; Flammarion, Paris.

[2] My findings and the investigations to which they gave rise are dealt with in six notes of the Academy of Sciences: C.R.A.S., 245, 1697; C.R.A.S., 245, 1875; C.R.A.S., 245, 2001; C.R.A.S., 244, 2469; C.R.A.S., 245, 2467; C.R.A.S., 245, 2170. The reader may refer to these for useful data which cannot be quoted here for lack of space. The object of the present paper is to reveal only the general philosophy of the results achieved. It is but an abstract of a general report which will be published shortly.

In Part I we propose to review, in an extremely cursory manner
(a) the abnormalities observed in the motion of a paraconical pendulum (pendulum hanging from a steel ball) on an anisotropic support.
(b) the manner in which they should be interpreted, according to the present status of our information and discussions.
In Part II (to be published in the October issue), we shall point out the relationship that these abnormalities appear to have with the irregularities or abnormalities noted when investigating a number of phenomena in the realms of mechanics, optics, and electromagnetism.

Experimental Process

The pendulum was released from a resting position every 20 min., using an initial amplitude of about 0.11 radian, by the burning of a thread. Its motion was then observed for about 14 min. by aiming at a needle attached to its lower extremity.

Generally speaking, the point so observed generated a curve comparable to a flattened ellipse, the plane of the major axis of which was observed with an aiming system placed on a circle C centered on the axis of the pendulum as defined at rest, and equipped with a scale graduated in centesimal degrees and a vernier. This system made it possible to determine the azimuth of the plane of oscillation with a precision of about 0.1 centesimal deg.

After 14 min., the pendulum was stopped, and it was again released in the plane of the last observed azimuth. Thus the successive series of observations were *connected*, with releases every 20 min., day and night, so that each 24-hour period was made up of 72 series of connected azimuth observations.

In order to rule out any systematic effect, the steel ball which carried the pendulum was changed after *each experiment*, every 20 min., and surface S was changed at the beginning of each week of observations.

A curve showing the azimuths observed from June 7 to 12, 1955, is given in Fig. 5. Each point represents the release azimuth corresponding to each series of 14-min. observations, equal to the azimuth of the plane of oscillation established after 14 min. in the above experiment.

Anisotropy of the Support

Since support S″ was characterized by a very *small* difference in its elasticity values in two rectangular planes, the mean position of the plane of oscillation tended, under this influence, to locate itself parallel to the plane of greatest elasticity of the support indicated by vector $\overrightarrow{PQ}$ in Figs. 1 and 3, the azimuth of which was approximately 171 centesimal deg., measuring azimuths from the south in the direct sense. Here again, *the overall tendency was to generate ellipses* when the pendulum was released in a plane other than $\overrightarrow{PQ}$. These influences were determined more accurately by releasing experiments in various azimuths, in eliminating the influence of the epoch with a random choice of the release azimuths.

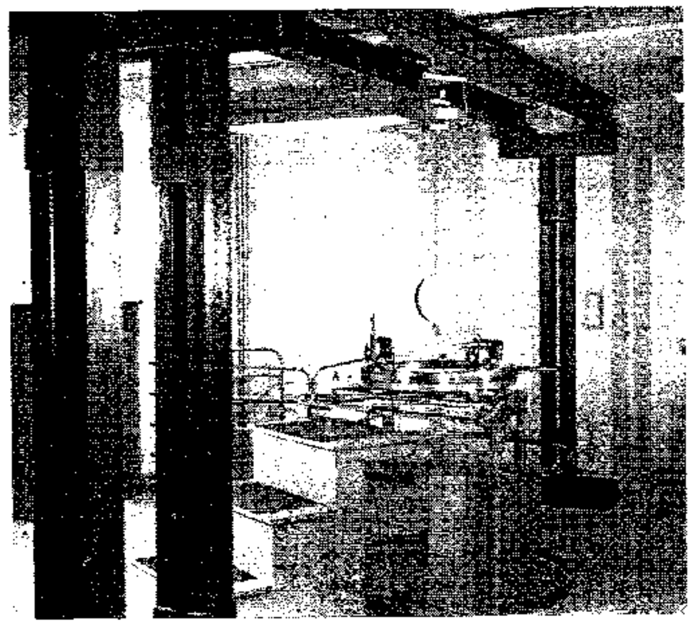

Fig. 1. General view.

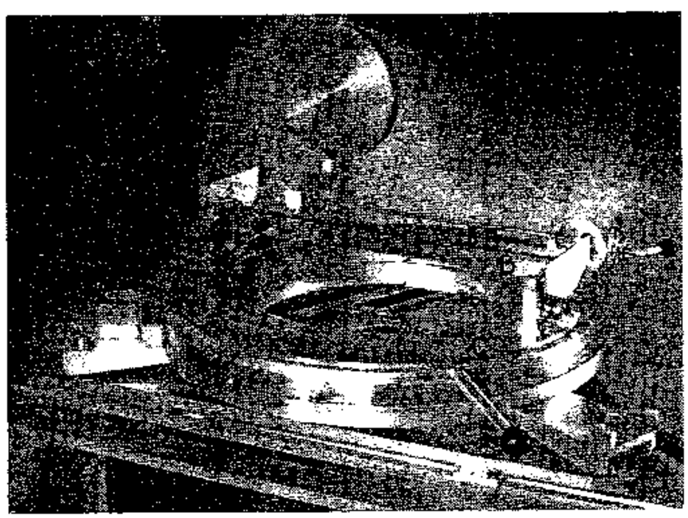

Fig. 2. Measuring circle.

Observed Phenomena

During a continuous series of observations, however, the oscillating plane had not evinced any tendency to settle in the vicinity of direction PQ, as might have been expected—allowing for the Foucault effect—and the variation of the azimuth as a function of time was found to be an oscillation which appeared to be very irregular, at least at first glance, about the mean direction PQ′. The deviations observed over a given 24-hour period were very large; azimuth variations occasionally reached and exceeded 100 centesimal deg. The mean observed azimuth P′Q′, as a matter of fact, was 150 centesimal deg. on June-July, 1955, less than the azimuth of PQ by 22 centesimal deg.

It is noteworthy that the tangent to the start of the mean of the various curves that correspond to the 2160 series of 14-min. elementary observations making up the monthly series for June–July, 1955, is an accurate representation of the Foucault effect.

Factors Influencing the Motion

In the *present* condition of my information, it may be assumed that the observed azimuth movements are the result of four conjugated effects: the Foucault effect, a "return" effect due to the suspension, the random influence of the spherical ball, and, finally, a periodic effect.

This periodic effect, which constitutes the very striking aspect of the phenomenon reviewed, was revealed, in the time series made up of the azimuths observed over a given period, by a number of techniques of harmonic analysis, the results of which were in remarkable agreement: Buys-Ballot filter, adjustment to a given group of waves by the Darwin or the least square method, periodogram, and correlogram.

Fig. 6 shows the adjustment graph obtained by the application of the Buys-Ballot method to the June–July, 1955, series for the 25-hour wave.

Defining Σ as the typical deviation of the time series made up of the values of the azimuths observed and R as the radius of the wave which corresponds to the analysis made with a Buys-Ballot filter, we give (in Table 1) the results obtained for the series of observations of June–July, 1955:

Order of Magnitude of Effects Noted

The azimuth rates which correspond to the amplitudes of the two major periodic components revealed in the June–July, 1955, series—the periods of which are close, respectively, to 24 and 25 hours—are each of an order of magnitude equal to one-tenth of the Foucault effect. However, the disturbing influences noted are, on an average and as a whole, about twice the Foucault effect.

Table 1.

	$2R$			R/Σ	
Σ	24^H	25^H	R_{25H}/R_{24H}	24^H	25^H
19.99	11.66	14.01	1.20	0.29	0.35

The values are given in centesimal degrees.

Abnormality Noted During Total Solar Eclipse

Let us point out, finally, that an abnormal lunar and solar influence also became apparent in the form of a remarkable disturbance of the motions of the paraconical pendulum (which gave the very definite impression of a screen effect) during the total solar eclipse of June 30, 1954. The plane of oscillation of the paraconical pendulum shifted approximately 15 centesimal deg. during the eclipse (see Fig. 7).[3] The forces involved were of the same order of magnitude as those which correspond to the Foucault effect.

Here, reduced to essentials, are the facts noted to date.

Four Basic Questions

The interpretation of the experimental results leads to the following four basic questions, to be raised in the order given.

First Question

Do the monthly series of observations contain statistically significant periodic terms, with periods in the vicinity of 24 and 25 hours?

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[3] Fig. 7 shows an azimuth curve tracing for the period extending from June 28, 1954 (8 p.m.), to July 1, 1954 (4 p.m.), as well as a curve symmetrical to the left part of the curve, which represents the azimuth referred to, about the vertical straight line for June 30 (midnight). Just at the beginning of the eclipse, the azimuth of the plane of oscillation suddenly was raised 5 centesimal deg. above the trend which first characterized its motion. Twenty minutes before the maximum of the eclipse, which was recorded at 12:40, this deviation reached a maximum of 15 centesimal deg. and then decreased progressively—but more suddenly than it had increased. The deviation was no more than 1.20 centesimal deg. prior to the end of the eclipse.

It will be noted that, to such an extent as may be ascertained, the shift of the plane of oscillation resumed, after the eclipse, the appearance of a motion CD, analogous to AB, which had been noted prior to the said eclipse (Fig. 7b).

Fig. 7a reveals an approximate symmetry of the azimuth curve with respect to the vertical for June 30, at 12 midnight. This symmetry, which can be ascribed to the periodic structure of the motion, is noted for approximately 28 hours. If we assume, as is likely, that this symmetry corresponds to a physical reality independent of the disturbances created by the contact between the steel ball and the surface, it is notable that nothing in the branch of the azimuth curve which precedes the time corresponding to the center of symmetry is in any way comparable to the very strong deviation noted during the eclipse.

It must be further underscored that, during all continuous observation periods, no variation of the azimuth curve similar to branch BC, corresponding to the solar eclipse of June 30, 1954, was ever observed.

It should be noted that the maximum deviation due to the eclipse took place 20 min. prior to the maximum of the eclipse. Thus there is a measure of dissymmetry in the effect noted. A similar dissymmetry has been observed for terrestrial magnetism, but in the opposite direction, the maximum of the effect having been observed after the maximum of the eclipse. (Lion, C.R.A.S., 1851, T. 33, p. 202; Lion, C.R.A.S., 1852, T. 34, p. 207; Lion and Muller, C.R.A.S., 1874, T. 74, p. 199. For the terrestrial electric field: Nordmann, C.R.A.S., January, 1906, p. 40; Chevrier, C.R.A.S., 1933, T. 197, p. 1143; Rouch, C.R.A.S., 1954, T. 239, p. 465.)

Second Question

If so, can the periodic effects so noted be identified with those due to the current theory of gravitation (as derived from the double principle of inertia and universal gravitation, which is assumed to apply with respect to the whole set of Galilean frames of reference) as complemented (possibly) by corrections derived from the theory of relativity, and such as this current theory of gravitation is applied within the framework of the current theory of relative motions?

Third Question

If not, can the existence of periodic terms which are of significance in the series so obtained be ascribed to an indirect influence of a known periodic phenomenon, specifically, to one of the following phenomena:

(1) Deviation of the vertical (terrestrial tides).
(2) Variation in the intensity of gravity.
(3) Thermal effect:
  (a) General (temperature at Le Bourget).[4]
  (b) Local (laboratory temperature).

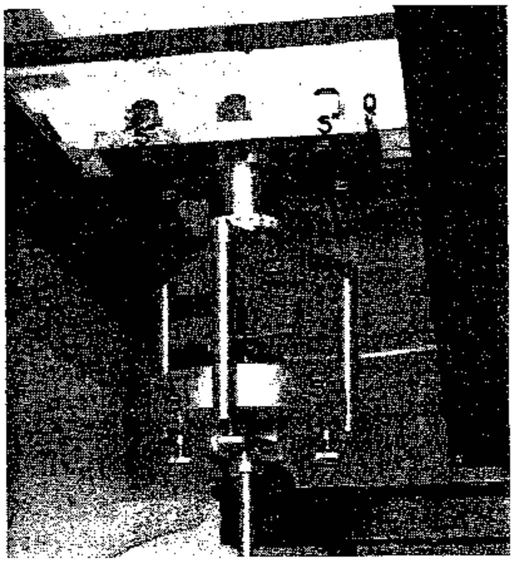

Fig. 3. Suspension.

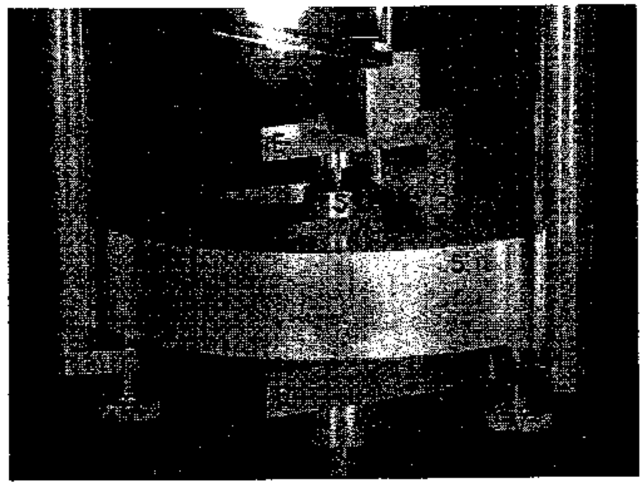

Fig. 4. Detail of suspension.

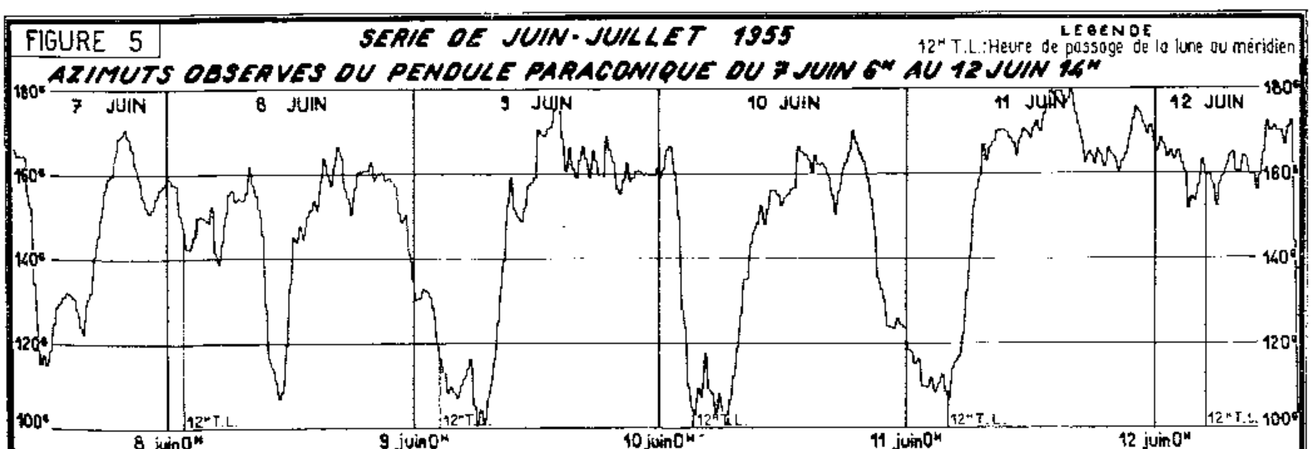

Fig. 5 (above). Series for June–July, 1955. Azimuths of the paraconical pendulum observed from June 7, 6:00 a.m., to June 12, 2:00 p.m.

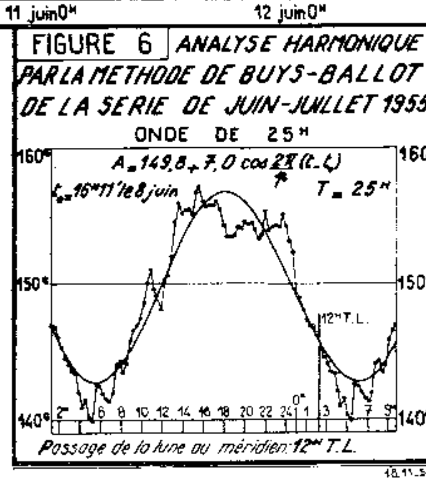

Fig. 6 (right). Harmonic analysis of the June–July, 1955, series, using the Buys-Ballot method.

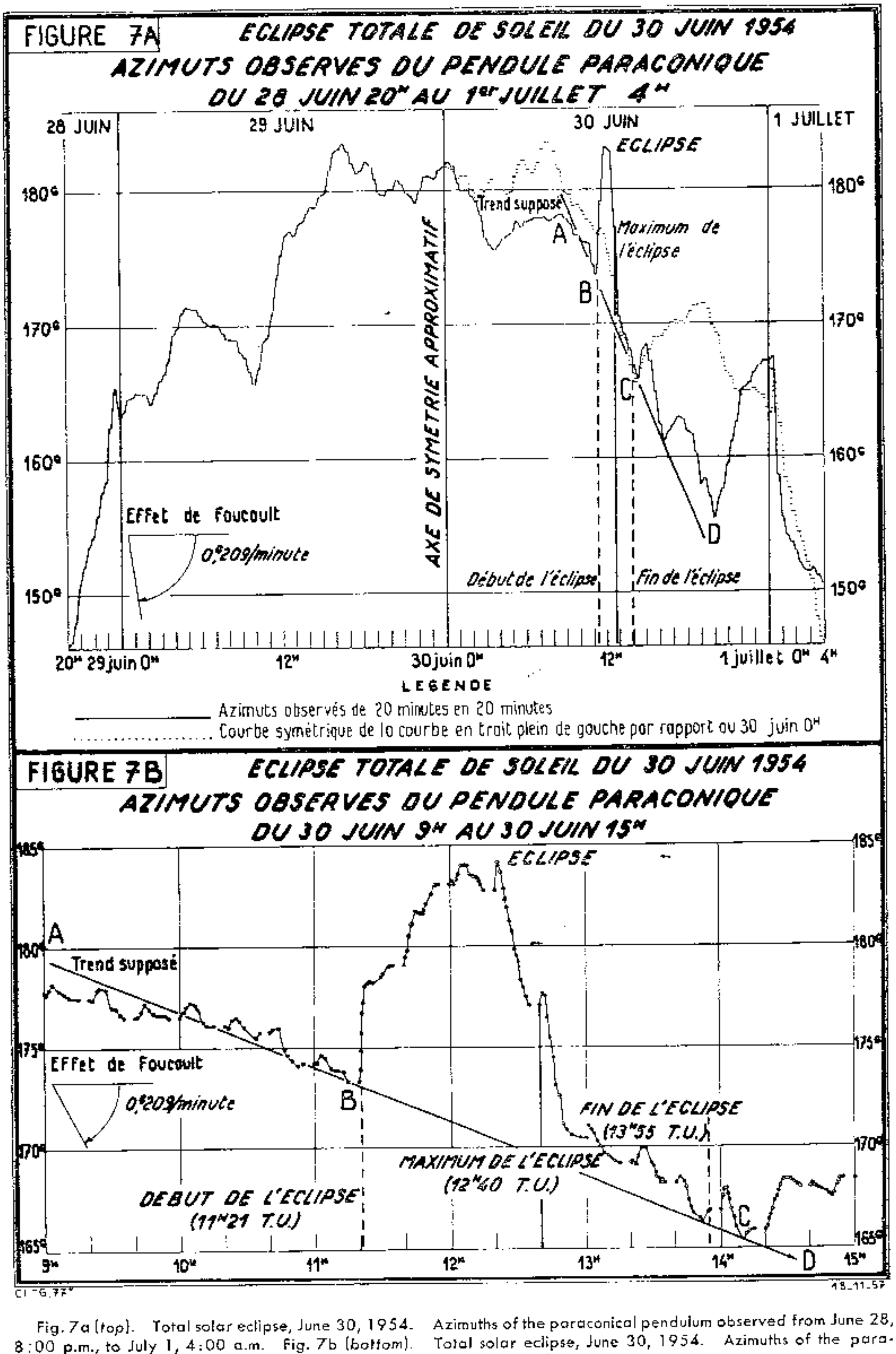

Fig. 7a (top). Total solar eclipse, June 30, 1954. Azimuths of the paraconical pendulum observed from June 28, 8:00 p.m., to July 1, 4:00 a.m. Fig. 7b (bottom). Total solar eclipse, June 30, 1954. Azimuths of the paraconical pendulum observed from June 30, 9:00 a.m., to June 30, 3:00 p.m.

(4) Barometric effect:

(a) General (pressure at Le Bourget).

(b) Local (pressure in the laboratory).

(5) Magnetic effect:

(a) Normal effect (terrestrial magnetic field as recorded at Chambon la Foret).[4]

(b) Magnetic agitation (K numbers of Bartels).

(6) Microseismic agitation:

(a) Industrial microseisms.

(b) Wind effects.

(c) General microseismic agitation.

(7) Cosmic rays.

(8) Periodic character of human activity.

(9) Periodic modification of the structure of the device.

Fourth Question

If a negative answer to the third question is in order—namely, if we must conclude that the effects noted are due to the direct action of a new field—should the origin of this field be assumed to be solar, lunar, lunar and solar, or spatial?[5]

Answers to the Four Basic Questions

Answer to the First Question

Any discrete series of $2n + 1$ numbers may be represented by the sum of a constant and n sine waves.

The obtaining of a sine wave of a given period by any method of harmonic analysis can therefore be of real significance only if its radius is sufficiently large and if the periodic structure noted is found again in the various elementary periods into which the period of observation under review can be broken down.

(a) The generalization of the Schuster test[6] leads to the conclusion that, for the two monthly series of November-December, 1954, and June-July, 1955, the amplitudes of the waves (or groups of waves) which have periods close to 24 and 25 hours must each be considered to be very significant statistically (the significance level is at least 10^{-4}).

(b) The periodic structure of the monthly series must be considered to hold for the two periods of one fortnight, and even for the periods of one week, into which each one-month series can be broken down, for the following reason:

Simultaneous analysis, by the method of the least squares, for 13 waves of the tide series, gives 13 sine curves, the sum of which can be found with the help of Lord Kelvin's tide predictor. The calculated series so obtained, *which is the sum of the 13-sine curve*, can be analyzed, for 24 and 25 hours, by the same method of the Buys-Ballot filter.

Table 2.

Series	Values H-H′ June-July, 1955	
	24^H	25^H
1 + 2 + 3 + 4	0^H06^{mn}	0^H02^{mn}
1 + 2	-1^H09^m	0^H02^m
3 + 4	$+0^H43^{mn}$	$+0^H15^{mn}$
1	-1^H08^m	-0^H33^m
2	$+0^H59^{mn}$	$+2^H20^{mn}$
3	-1^H23^{mn}	-0^H56^{mn}
4	$+0^H59^{mn}$	$+0^H57^{mn}$

If the sine curves obtained *really* exist in the raw series, the Buys-Ballot method must give, for each elementary period, a sine curve having phases that are comparable for both the crude and the computed series. Table 2 shows the results obtained for the value H-H' representing the difference between those two phases in hours and minutes, for the series of June-July, 1955.

Notation 1 represents the first week; notation 1 + 2 represents the first 2 weeks, and 1 + 2 + 3 + 4 represents the whole of the month.

Allowing for the fact that each week can be considered an independent experiment, such agreement between the phases must be deemed to reveal the existence of true periodicity. This leads us to the conclusion that the monthly series of June-July, 1955, *actually* contains periodic elements with periods close to 24 and 25 hours.

Thus the answer to the first question must be "yes" in all certainty.[7]

Answer to the Second Question

The current theory of gravitation (being the result of the application, within the framework of the current theory of relative motions, of the principles of inertia and universal gravitation to any one of the Galilean spaces), complemented or not by the corrections suggested by the theory of relativity, leads to orders of magnitude for lunar and solar action (which are strictly not to be perceived experimentally) of some 100 millions of times less than the effects noted.[8]

These effects are so small that none of the nineteenth-century authors who worked on the theory of the pendulum, some of whom were excellent mathematicians, ever had a desire to compute them.

The extreme smallness of the effects computed can readily be accounted for if we allow for the fact that, in order to obtain the true gradient $\vec{f}$ of the moon and sun attraction at a point, on the surface of the ground, with respect to the earth, we must take the difference between the attractions at this point and at the center

[4] Observatory nearest my Saint-Germain laboratory.

[5] By this, I mean a field which could not be related to the sun or to the moon. This could be, for instance, a field resulting from a dissymmetry of the inertial sidereal space. A possible period would then be 24 sidereal hours.

[6] See footnote 2.

[7] Much store has been set in the defects of the equipment used—imperfect steel balls, imperfect horizontal positioning of the support, etc. I cannot stress enough that the only possible effect of imperfections in the equipment are effects of a systematic or random type and that, under no circumstances, could they entail, whatever they be, the existence of any real periodicity.

[8] On the order of 10^{-13} instead of the periodic effects noted of some 10^{-5} rad. per sec.

Table 3.

	2R			
	25^H	24^H	12^H30	12^H
June-July, 1955	14.01	11.66	3.71	2.69

of the earth, respectively. Gradient $\vec{f}$ is of the order of 10^{-8}.

Furthermore, the plane of oscillation of the pendulum can rotate, under the influence of the solar and lunar attraction, only because of the variations of the gradient about the point considered. Therefore, the difference $\Delta\vec{f}$ between the value of $\vec{f}$ at the mean position of the pendulum and its magnitude at a nearby point must be considered. It[9] is of some 10^{-13}.

Furthermore, nothing in the current theory of gravitation can be considered likely to account for the screen phenomenon observed during 1954.

Therefore, the answer to the second question must be *no, and this in all certainty*.

Answer to the Third Question

The very peculiar periodic structure of the series observed (amplitude of the 25-hour wave of the same order of magnitude as that of the 24-hour wave and very much larger than the amplitude of the 12- and 12.5-hour wave) leads to the elimination, as possible causes of the observed abnormalities, of all the phenomena noted above under the order numbers 1 to 9. (Table 3 shows the results obtained for the series of June-July, 1955, the periodicity of which is significant.)

Indeed, for all these phenomena, the total of the amplitudes of the waves having periods close to 25 hours is small as compared to the total of the amplitudes of the diurnal solar wave group, the semidiurnal solar wave group, or the semidiurnal lunar wave group.[10, 11]

The answer to the third question therefore must be, for elements 1 to 9 as limitatively listed above, that the effects observed cannot be assumed to arise indirectly out of the action of any of these elements. This statement can be made *categorically*.

As long as a phenomenon other than those listed above has not been proposed as a possible explanation, it will be necessary to assume that the phenomena observed are due to the direct action of a new field.

Thus the answer given to the third question is a qualified one.[12]

Answer to the Fourth Question

If, in the present condition of the discussion, we must answer the third question with a hypothesis of the direct action of a new field, there will arise the question of determining whether this field is derived from the action of the moon, from that of the sun, from their conjugated action, or, again, from a spatial influence.

Two remarks are in order:

(a) In reviewing monthly series, there is no way of specifying, when a wave with a period of 24 hours is revealed, whether one is dealing with a solar or sidereal 24-hour period.

(b) Similarly, when dealing with a period close to 24 hours, 50 min., nothing justifies the claim that one is dealing with a lunar—rather than a solar—effect. The mean synodic rotation of the sun about its own axis is 27.275 mean days, whereas the sidereal revolution of the moon is 27.321 days. As for the mean solar day, it is very close in value to the mean sidereal day.

In order to reach a definite decision, it would be necessary to use far longer periods of observation.

Thus it appears to me that it is impossible to conclude with definite certainty that the periods revealed, of an order of magnitude equal to 24 and 25 hours, are derived respectively from a solar and a lunar action. *In the current status of available information*, such an action only appears to me as *very likely*.[13]

The answer given to the fourth question, therefore, has to be of the qualified type.

(*This article will be concluded in the October issue. Part II will discuss experiments in connection with the abnormalities described in Part I.*)

[9] See the accurate expression of the effect in my paper to the Academy of Sciences (December 16, 1957).

[10] Thus, for instance, for the lunar and solar gravitational potential, the total of the amplitudes of the waves having periods of close to 24 hours is approximately 18 times greater than the total of the amplitudes of the waves with periods of close to 25 hours.

[11] Aside from the general argument of the specific periodic structure of the results obtained, which, of itself, is enough to rule out causes 1 to 9, a certain number of additional arguments may be presented, some of which are of considerable value and lead to the same conclusion but, for lack of space, are not presented here.

[12] I believe I should point out that nothing else can be the case. I can give my answer only with regard to the phenomena which I considered or which were suggested to me as being such as to account for the effect noted. It is quite possible that an explanatory phenomenon be propounded very soon which would definitely prove decisive when investigated. However, in the present condition of the discussion and allowing for all the factors already reviewed, the existence of such a phenomenon seems at least to be unlikely.

[13] The only known phenomenon related to the rotation of the sun is that of the spots. It really seems quite unlikely that the variations in the radiation due to the spots can have effects of an order of magnitude similar to that of the suppression of radiation during the night. As a matter of fact, there is no connection at all between the observed azimuths and the Wolf numbers, which are characteristic of lunar activity (for which, unfortunately, we have only one value per diem).

However, it cannot be claimed that there are no other effects related to solar rotation. This is enough to rule out a fully certain and unequivocal conclusion.

Should the Laws of Gravitation Be Reconsidered?

Part II—Experiments in Connection With the Abnormalities Noted in the Motion of the Paraconical Pendulum With an Anisotropic Support

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Part I (published in the September issue) reviewed and interpreted the abnormalities observed in the motion of a paraconical pendulum on an anisotropic support.

Part II describes experiments in connection with these abnormalities—relating them to phenomena noted in mechanics, optics, and electromagnetism—and concludes that they can be accounted for "only by considering the existence of a new field."

THE IRREGULARITIES observed in the motion of a paraconical pendulum on an anisotropic support, whose order of magnitude is of approximately a few millionths of gravity, do not in fact disagree with any of the experimental results arrived at either in the astronomical domains or on the earth's surface.

The periodic irregular components of the pendulum motion appear to be in connection with the irregularities encountered during the study of multiple mechanical, optical, and electromagnetic phenomena—particularly in Michelson, Morley, and Miller's tests—and all these irregularities can probably be attributed to the same single source.

It would be advisable to continue the experimental study of these phenomena by continuous measurements over a period of at least 1 month. Such a study is of great interest for the development of a unitary theory on gravitation, electromagnetism, and quanta.

Theory

Part I outlined the very remarkable abnormalities evinced by the motion of a paraconical pendulum with an anisotropic support. These abnormalities appear to reveal some shortcomings in the currently accepted laws of gravitation.

When theory has been verified by countless facts, it is impossible to modify it slightly. In the first place, an attempt must be made at relating the new facts observed to the already known phenomena. If this appears to be impossible, which indeed is the case as I indicated, one is justified in wondering whether the abnormalities so noted can be assumed to be isolated or, again, whether they can be related to other abnormalities already noted elsewhere.

(A) Abnormalities Noted in the Classical Foucault Experiment

The bibliography of experimental research on the Foucault pendulum is extensive, but any search through it cannot fail to reveal two facts:

(1) There is a great scarcity of really significant papers.

(2) The numerical data on the results obtained are very few.

The only important experiments on the conical pendulum with numerical data on the findings are, to our knowledge, those of Bravais (1851), Willigen (1866), Kamerling Onnes (1879), Longden (1919), and Dasannacharya (1937–1939).

The experiments conducted by Bravais bore on the motion of the circular pendulum; those of Willigen, on the conical pendulum; those of Kamerling Onnes, on the asymmetrical pendulum; those of Longden, on various types of supports; while those of Dasannacharya bear on the influence of the support. The very best study by far, both from the experimental and theoretical standpoints, is that of Kamerling Onnes.[14]

Taken as a whole, these investigations strike us by the relative scarcity of observational data. To my knowledge, *the motion of the Foucault pendulum never was observed continuously, day and night, over a period of time of about a month.* Foucault himself never published the results of his findings other than in a general form,[15] and it is truly surprising to read, in the very hand of so eminent an experimenter: "Even though the amplitude of the oscillations decreases rather rapidly, they are still large enough, some five or six hours later, to reveal a deviation which is, by then, of 60 to 70 degrees,"[16] or again: "Watch in hand, it can be seen that, in Paris, the deviation is one degree in five minutes."[16]

Even though a number of spectacular experiments have been carried out (1852, the Pantheon, Paris; 1852, Cologne Cathedral; 1902, the Pantheon, Paris; 1904, Hall of Justice, Brussels; about 1930, St. Isaac Cathedral, Leningrad; 1951, Hall of Justice, Brussels; 1955, United Nations building, New York[17]), nowhere could I find the numerical series corresponding to the observations made. This is a detail which, to say the least, is surprising.

The only series I was able to find were fragmentary, but they *all include substantial abnormalities*, which are generally ascribed to defects in the support. They do give a Foucault effect, *but only on an average.*[18]

Finally, and to such an extent as might be possible on the strength of the information currently available, nobody ever achieved a perfect $-\omega \sin \lambda$ rotation other than on averages derived from numerous series of observations.

All the numerical series of observations now available—and, incidentally, there is a very small number of them—reveal, on the contrary, some variations in the rate of rotation as a function of time.

Having brought out these facts, I do not believe it would be amiss to clarify the following:

(1) The mean curve of our elementary experiments bearing on a connected series has *exactly* the Foucault slope as its tangent at the origin, when the oscillation is in a plane.

Thus our experiments are not by any means in contradiction with the general result of Foucault's experiment as has been claimed all too often.

(2) The support used for the pendulum is *anisotropic*, and it tends, on the whole, to bring back the plane of oscillation to a given direction.

The effect of this anisotropy is, on the one hand, to compensate the Foucault effect on an average during a 14-min. experiment and, on the other hand, to cause the development of ellipses, whereas, in Foucault's classical experiment, these are nearly planes.

Now there is every reason to believe that the phenomena noted, if they are not due to elliptical oscillations, are at the very least amplified by them, so that it is entirely possible for the disturbance to have a zero effect when the trajectory is plane, and a substantial effect as soon as the trajectory is elliptical.

(3) The pendulum used is a *short* one, the length of which is about 1 meter against several meters, indeed several tens of meters, as in the experiments conducted by Foucault and those who followed him. It is a known fact that it is very difficult to achieve the Foucault effect with short pendulums. Abnormalities are nearly always noted.

(4) The pendulum used can *rotate about itself*, whereas, in the Foucault pendulum, it is bound to the wire which carries it.

(5) The motion of the pendulum used was observed *without any single interruption*, day and night, for periods running to about a month. This never was the case with the Foucault pendulum.

From all these indications, it is quite clear *that nothing in the result of my experiments runs contrary to those obtained earlier.* All the earlier results, on the contrary, consistently show that there are abnormalities with respect to which we were quite remiss up to date so far as the investigation of them is concerned.

(B) Abnormalities Noted in the Realm of Mechanics

The abnormalities noted in the motion of the paraconic[†] pendulum strike me as being closely related with the difficulties or abnormalities encountered when one has to account for a number of dynamic phenomena, which, until now, still have to be explained:

(1) Abnormalities in the tide theory.[19]

(2) Motions of the top of the Eiffel Tower.[20]

(3) Size of the deviations to the south noted on falling bodies.[21]

(4) Variations in the amplitude of the deviations to the east noted on falling bodies.[21]

(5) Abnormalities noted in the action of terrestrial rotation on the flow of liquids (Tumlirz's experiments).[22]

(6) Abnormalities noted in the motion of the horizontal gyroscope of Föppl.[23]

(7) Abnormalities noted in the experiments carried out with the isotomeograph.[24]

(8) Abnormalities noted in experiments carried out with a suspended pulley.[25]

(9) Various abnormalities noted in the geophysical measurements, ascribed until now to experimental errors.

(10) The apparently unaccountable results obtained by Louis Pasteur (General, French Medical Corps) in his experiments on the oscillation of the pendulum (1954).[26]

(11) Remarkable characteristics of the solar system, for which there has been, until now, no satisfactory explanation.[27]

To these abnormalities—which are related to motion—we should add the static types:

(1) The abnormalities of gravity. There is an excess of gravity over the ocean and a deficiency above the continents. The theory of isostasis provided only a pseudoexplanation of this, in my view.[28]

(2) The abnormalities in the experiments on Newtonian attraction. There is, on the one hand, some absorption of gravity (experiments of Majorana[29]), but also—and mainly—a variation of the Newtonian force according to the medium where it is exercised (Cremieu's experiments[30]).

Accuracy of the Verification of the Laws of Gravitation

It is not without interest, at this point, to investigate the accuracy with which the laws of gravitation are verified, both in the realm of astronomy and on the surface of the earth. This may come as a great surprise, but all the treatises of mechanics and astronomy remain notoriously silent on this fundamental question. This is a very significant gap in our knowledge and an obvious deficiency from the standpoint of scientific discipline. Any law is devoid of significance if we do not know with what degree of accuracy it has been verified.

(a) Accuracy of the Astronomical Verification of the Postulates of Mechanics

The fundamental laws of mechanics at the surface of the earth are due to an extrapolation of the results obtained in astronomy; it is not without interest, therefore, to ascertain the accuracy with which these laws actually are verified.

Unfortunately, this discussion is not given anywhere, for it is stated as a matter of principle that Newton's laws are accurately verified. Without going into a detailed discussion which would go beyond this article, it is relatively easy to determine what this degree of accuracy is.

A consideration of the remainders left by the adjustments in keeping with the least square method, as used to draw up the tables currently employed in astronomy, shows that the order of magnitude of the deviations noted for angular displacements between observation findings and theory is of some 1 sexagesimal second of arc, giving a relative error of some

$$1/90 \times 60 \times 60 \quad \approx \quad 3 \times 10^{-6}$$

Such is the order of magnitude of the accuracy with which Newton's laws have been verified astronomically. These laws, as all experimental ones, are verified with only some approximation. This conclusion runs counter to the ideas which are commonly accepted—without a true discussion, to be sure; but it seems to me that it must be accepted.

(b) *Accuracy of the Verifications of the Postulates of Mechanics at the Surface of the Earth*

The mechanical experiments at the surface of the earth which have been carried out with the greatest precision are those bearing on the pendulum which gives one second. These experiments, in effect, assume the well-known formula

$$T = 2\pi \sqrt{I/Mgl}$$

which is deduced from the postulates of mechanics. The quotient I/Ml is computed from the length measurements; T is measured and g deduced. The experiments of M. Volet at the Pavillon de Breteuil at Sèvres—which enable us to measure g directly by the photograph of the fall of invar metal rulers—enabled us to confirm the values deduced from the observations of the pendulum to 10^{-6}. Such is the order of magnitude of the accuracy with which the principles of mechanics appear to be verified at the surface of the earth.

Order of Magnitude of the Abnormalities Noted in the Motion of the Paraconical Pendulum

It is of interest to relate these figures to the order of magnitude of the abnormalities noted. This order of magnitude is that of the Foucault effect, which, in the case of the pendulum used, is itself some 3×10^{-6} of the gravity. *The effects noted, therefore, are of an order of magnitude smaller than or equal to the order of magnitude with which we may consider that the principles of mechanics are verified at the surface of the earth or in the field of astronomy.*

As a matter of fact, it should be pointed out that the abnormalities noted have a periodic structure and that, on an average, they are cancelled. If new forces must be considered, therefore, they apply only within the framework of the solar, the sidereal, or the lunar day. In the field of astronomy, where planetary motion is dealt with, it is therefore necessary to match them with forces, the integral of which would add up to zero over the path of these planets. Thus, their order of magnitude is indeed comparable to the order of magnitude with which it may be thought that Newton's laws are verified during a revolution.[31]

From this it will be seen that the abnormalities that have been revealed do not in any fashion run contrary to the earlier experimental data, either on the surface of the earth or even in the field of astronomy.

(C) Abnormalities Noted in Some Optical and Electromagnetic Phenomena

The abnormalities revealed in the motion of the paraconical pendulum with an anisotropic support strike me as having an obvious relationship with the abnormalities revealed by Michelson, Morley, and Miller in their experiments designed to show the absolute motion of the earth with respect to the ether,[32] by Esclangon on the dissymmetry of space,[33] by Fizeau in his experiments on the polarization of light,[34] and by J. Hely and P. Malsallez in their electromagnetic experiments on the anisotropy of space.[35]

It would strike me as difficult not to be impressed by the similarity in the appearances of the curves derived by Miller and others, and I can hardly refrain from concluding that all these phenomena are due to one and the same cause.[36, 37]

It appears to me that the phenomena I have revealed are such as to suggest a thorough and rewarding reappraisal, on the experimental and theoretical planes, of the findings made as the outcome of the various experiments mentioned above. The lunar and solar components revealed would indeed have remained undetectable if we had not carried out continuous observations, and the temptation would have been great to ascribe the differences observed to simple accidental disturbances, as was the case, for instance, in the interpretation given to the results of Miller by his critics.[38]

I must insist once more on the remarkable abnormalities mentioned by J. Vignal.[39] Leveling operations revealed systematic errors having to do with the direction in which the work progressed. These systematic errors behave as accidental errors as soon as series of stretches over a few tens of kilometers in length are considered. They doubtless bear a close relationship to the results obtained by Miller.

I believe the same applies to the systematic lateral refraction errors observed in triangulation operations.

From all these data it would appear, in my opinion, to be of the greatest interest to carry out the various mechanical and optical experiments mentioned above with the utmost care, with all the accuracy which the equipment now available can provide, and with *continuous* observations over a period of at least a month. In such a case, it would seem likely if not actually certain, that we should note, in the phenomena so observed, some periodic influences which are entirely similar to those I believe I have revealed in the case of the paraconical pendulum.[40, 41] These simple observations, which unfortunately cannot be developed in detail here, appear to throw the fullest light on the scientific interest of my findings.

(D) Significance of the Abnormalities Discovered

From our examination of the abnormalities mentioned and the discussion of the accuracy with which the principles of mechanics have been verified, it is plain that these principles do not have, by any means, the absolute value which an all too prevalent body of opinion appears to ascribe to them. These principles have acquired a sort of metaphysical quality which places them above the realm of discussion. In reality, mechanics is not at all a perfect science, a pure science in which we have nothing else to find. It is, and remains, an experimental science which can and should be improved.

The time has come, indeed, when one should go over all these phenomena once again. This re-examination manifestly seems to be of considerable interest for the development of a unified theory which could embrace, in one synthesis, the theories of gravitation, electromagnetism, and quanta. It is not at all the same thing to state

$$\Delta G + 4\pi\mu d = 0 \qquad (1)$$

or to state

$$\Delta G + 4\pi\mu d(1 + \epsilon) = 0 \qquad (2)$$

where

$$|\epsilon| \leqslant 5 \times 10^{-6}$$

in which G is the Newtonian potential, d the density, and μ the constant of universal gravitation.

Whoever has worked on these difficult questions knows that Eq. (1) cannot be substituted for Eq. (2) without a considerable measure of danger for, in the integration of partial derivative equations, the corrective terms are very important. Thus, at a time when the development of a consistent unified theory of physics might appear to offer more difficulty than ever before, an examination in depth, on the experimental and theoretical planes, of all the abnormalities mentioned above strikes me as being of the greatest interest; for it appears to be of such a type as to call for the revision of some postulates, the rigorous validity of which was accepted without true experimental support.

Facts alone must guide us, rather than mummified principles, even though they may be most useful for a first approximation. We learn only through experiment, and any thought which permanently withdraws into a set of abstract principles thus sentences itself automatically to a form of sclerosis.

Conclusions

The whole set of data given in Parts I and II seems to me to lend itself to the following summary:

(1) The motion of the paraconical pendulum using an anisotropic support is made up of periodic components having a significant statistical amplitude, of the order of the Foucault effect, with periods in the vicinity of 24 and 25 hours.

(2) These periodic components cannot be identified with those due to the gravitational effect of the moon and sun, such as they may be computed from the double principle of inertia and of universal attraction, for those are approximately one hundred million times smaller.

(3) The very peculiar periodic structure of the phenomena observed, being due to the relative importance of the wave having a period of about 25 hours, rules out any explanation based on one of the already known periodic phenomena which had been taken into consideration as possible explanations for the periodic nature of the phenomena.

In the present status of the discussion, the abnormalities observed can be accounted for only by considering the existence of a new field—namely, by envisioning the existence of complementary terms which until now had remained unnoticed.

(4) The effects observed, the order of magnitude of which is about a few millionths of gravity, are not actually incompatible with any of the earlier experimental findings for the accuracy with which these results have been obtained does not exceed a few millionths.

(5) The abnormalities noted are not isolated. Many abnormalities have been observed by other workers in a number of geophysical phenomena, and it seems likely, if not certain, that they are derived from one and the same cause.

(A Complementary Note on Professor Allais' work will be published in the November issue.)

Notes

[14] Bravais, Journ. de Math. pures et appl., XIX, 1854, pp. 1–50; Willigen, Arch. Musée Teyler, I, 1866, pp. 341–363; Kamerling Onnes, *Nieuwe Bewijzen voor de Aswenteling der aarde*, Dissertation submitted to the University of Groningen, July 10, 1879 (this remarkable work was analyzed in detail by J. Stein in the second appendix to the work of Hagen, *La rotation de la terre, ses preuves mécaniques anciennes et nouvelles*, Tipographia Poliglotta Vaticana, Rome, 1911); Longden, Phys. Rev. XIII, 1919, pp. 241–258; Dasannacharya and Hejmadi, Phil. Mag. XXIII, 1937, pp. 65–88; Dasannacharya and Balram Singh Gantom, Phil. Mag. XXV, 1938, pp. 601–622.

[15] Foucault, L., C.R.A.S., 1851, pp. 135–137; *Set of Scientific Works*, Gauthier Villars, Paris, 1878.

[16] *Set of Scientific Works of Léon Foucault*, Gauthier Villars, Paris, 1878, p. 386.

[17] Haringx and Suchtelen, Philips Technical Rev., 19, 1957–1958, pp. 248–254.

[18] The reader also will find a number of interesting references in Hagen, *op. cit.*, Second Part; and in Wolf, C., *Bibliographie du Pendule (1629–1885)—A Set of Memoranda Published by the French Society of Physics, Vol. IV, Papers on the Pendulum*, Gauthier Villars, Paris, 1889, pp. B1–B216.

[19] For instance, the western coasts of the continents, all other things being equal, are subject to far higher tides than the eastern coasts.

Similarly, the full tide does not coincide with the passage of the moon at the meridian, but rather follows it by about 3 hours. At the syzygies and quadratures, the maximum or minimum tide is encountered only a day and a half after that of the relevant syzygy or quadrature. This happens anywhere on the earth. It is to be noted that these two phase shifts are *very different* although the solar force is no more than a fraction of the lunar force. It should also be underscored that the delay is the same at the quadratures, where the solar action, instead of being added to that of the moon as it is at the syzygies, is deducted from it.

Let us further point out that no satisfactory theory of marine currents has been given as yet, although some of their characteristics are very remarkable, such, for instance, as the preponderance of an easterly trend.

It is not without interest to note that mechanical effects of the tides are relatively large when compared to the tide-inducing forces due to the moon and sun, which cause them and which represent, at most, the following fractions

$$2\,(M_l/M_t)(r_t^3/d_l^3) = 11.2 \times 10^{-8}$$
$$2\,(M_s/M_t)(r_t^3/d_s^3) = 5.18 \times 10^{-8}$$

of the gravity.

This abnormality has been accounted for by mentioning the possibility of oceanic areas of resonance, but this assumption, quite plainly, is entirely gratuitous.

[20] Hagen, *op. cit.*, p. 24. See also Inst. Géo. Nat., *Report on Control Measurements of the Eiffel Tower Stability (1893–1951)*, May 1, 1952.

[21] Hagen, *op. cit.*, pp. 22–40.

[22] *Ibid.*, pp. 110, 111.

[23] *Ibid.*, pp. 94–96.

[24] *Ibid.*, pp. 142–147. See also the second appendix to this work, pp. 36, 37, and 46.

[25] *Ibid.*, pp. 166, 167.

[26] The objection has been raised that the experimental equipment used by Gen. Louis Pasteur could not be viewed with any degree of confidence. I must stress that his pendulums start only in the east–west direction and in the presence of some close obstacles arranged in a certain fashion. This rules out a great many hypotheses. (In the same sense, see the results obtained by Victor Panisetti, Cosmos, 1856, p. 503.)

[27] These numerous characteristics cannot be due to chance. The reader is referred to Gaussen, C.R.A.S., 90, 1880, p. 518; Gaussen, C.R.A.S., 90, 1880, p. 593; Belot, C.R.A.S., 143, 1906, p. 1126; Belot, C.R.A.S., April, 1907, p. 885; Delauncy, *Lois des distances des satellites du soleil*, Gauthier Villars, Paris, 1909; Butavand, *Les lois empiriques du système solaire*, Gauthier Villars, Paris, 1913; Ollive, F., C.R.A.S., 157, 1913, p. 1501; Blagg, M. A., Roy. Astr. Soc., 73, 1913, p. 414; Demozay, *Relations remarquables entre les éléments du système solaire*, Gauthier Villars, Paris, 1919; Delauney, *Problèmes Astronomiques*, Gauthier Villars, Paris, 1920; Vilar, *Notes sur les distances des planètes au soleil*, Jouve, Paris, 1923; Bourgeois and Cox, C.R.A.S., 198, 1934, p. 53.

[28] The reason mentioned in this case is about equivalent to the "sleep-inducing virtue" of opium as mentioned in Molière's play!

[29] Majorana, C.R.A.S., 173, 1921, p. 478; Journ. de Phys. et Rad., I, 1930, pp. 314–324; Phil. Mag. XXXIX, 1920, pp. 488–504; Schlomka, Zeit. für Geophys., 1927, p. 397.

[30] Cremieu, C.R.A.S., December, 1906, p. 887; Rev. Gén. Sc. Pur. et Appl., 18, 1907, pp. 7–13. According to Cremieu, everything takes place as though gravitation measured in water were greater than that computed by means of the theory of attraction from a distance, the difference being about 1/10. Therefore, it is a *considerable* difference.

[31] In other words, if, to the Newtonian effects, we added actions 10^{-6} times smaller and which would have a zero value on an average during the revolution of the planet, these would probably stay unrevealed.

[32] One should read, in particular, the remarkable paper by Miller, *The Ether Drift Experiment and the Determination of the Absolute Motion of the Earth*, Rev. of Mod. Phys., 1933, p. 203, the findings of which—derived from 200,000 observations—are remarkably consistent; the proceedings of the Mount Wilson Conference of 1927, The Astrophysical Journal, 1928, p. 341. One should also consult the references given by Miller at the end of his paper. It is startling that the findings published in this paper should have been ignored for 25 years. The outright pigeonholing of Miller's paper strikes me as one of the scandals of contemporary physics.

[33] *Sur l'existence d'une dissymétrie optique de l'espace*, Journ. des Obs. XI, pp. 49–63. Here again, the internal consistency of the results obtained goes beyond any and all doubt. By the same author, *La dissymétrie de l'espace sidéral et le phénomène des marées*, C.R.A.S., 183, 1926, pp. 116–118; *Sur la dissymétrie mecanique et optique de l'espace en rapport avec le mouvement absolu de la terre*, C.R.A.S., 1926, pp. 921–923.

It has been possible to relate satisfactorily the results obtained by Miller and Esclangon (see Carvallo, E., *Vitesse de la terre mesurée par des expériences purement terrestres*, C.R.A.S., 1934, p. 247; *Vitesse de la terre et Relativité*, Rev. Scientif., 1934, pp. 405–410; *Les lois absolues de la lumière et la loi de relativité*, Rev. gén. Electricité, XXIX, pp. 493–546). The results obtained by Esclangon at Strasbourg have not been found again in Paris (C.R.A.S., 1935, p. 1165), *but the experimental setup was not the same.*

[34] Ann. de Chim. et Phys., 1860, p. 129.

[35] Mesures, 1937, No. 11, pp. 13–17, and No. 12, pp. 19–21.

[36] There is no doubt, in our view, that the phenomena which I have revealed are such as to support Miller's papers indirectly in such a way as to confirm their validity. The very bases of the theory of relativity thus could again come under scrutiny.

[37] That some of the experimenters (A. Piccard and E. Stahel, Georg Joos for the Miller effect, B. Strasser, D. B. Brace for the Fizeau effect) should not have found the same results is perfectly accounted for, in our opinion, by the fact that, at some time, the total lunar and solar effect, as actually noted in the case of the paraconical pendulum, is very small (Piccard and Stahel, Journ. Phys. IX, 1928, pp. 49–60; C.R.A.S., 185, 1927; Joos, Ann. der Phys., 1930, pp. 385–407; Strasser, Ann. der Phys., 24, 1907, pp. 137–144; Brace, Phil. Mag., 1904, pp. 317–329; Phil. Mag., 1905, pp. 591–599).

As a matter of fact, no parallel can be drawn between the Piccard and Stahel experiments carried out from a baloon and the 200,000 observations by Miller carried out in a very well organized laboratory, with every possible precaution (see Brylinski, etc., C.R.A.S., 185, 1927, p. 1198). Finally, the experiments carried out by Esclangon reveal that some small modifications in the equipment, which are of no apparent significance, might cause a cessation of the effects observed (see footnote 33 above).

[38] See, in particular, R. S. Shankland, S. W. McCuskey, F. C. Leone, and G. Kuersi, Rev. of Mod. Phys., 27, 1955, pp. 167–178. and the appended references. The authors ascribe the effects obtained by Miller to the conjugate effect of random disturbances and temperature disturbances. However, this criticism does not account for the extraordinary consistency of Miller's results with the motion of the earth about the sun (see Figs. 23 and 28 of his paper, pp. 232, 237). Similarly, it does not account for the remarkable adjustments with phases which agree with sidereal time, as shown on p. 235 of his work. It also leaves out the agreement between Miller's and Esclangon's results (see the papers by Carvallo already mentioned in footnote 33 above).

[39] *Evaluation of the Accuracy of the Leveling Method*, Bull. Géod., 53, 1936, and *Precision Leveling*, Publ. de l'Inst. Géog. Nat., Paris, 1948.

[40] The fact that the abnormalities noted in most of the phenomena mentioned are distributed according to the normal law cannot by any means be interpreted by ascribing the effect to an accidental cause. I was thus able to note that the sum of the eleven sine curves obtained in the analysis of my numerical series by the method of the least squares is distributed according to *the normal law*. Yet we are dealing indeed with eleven sine curves, thus with a periodic magnitude. The random aspect of a numerical series does not by any means thus rule out the possibility that it may represent a periodic phenomenon.

[41] A remarkable fact is that the relative order of magnitude of the different departures noted (whether we are dealing with Miller effects, systematic errors related to the leveling or triangulation operations, abnormalities in the fall of bodies, or again abnormalities in the motions of the paraconical pendulum) always remains the same—namely, 5×10^{-6}. Everything happens as though all these phenomena had one and the same cause.

Should the Laws of Gravitation Be Reconsidered?

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Complementary Note

New information concerning the results indicated in Parts I and II of "Should the Laws of Gravitation Be Reconsidered?" published in the September and October issues.

THE EXPERIMENTS which I have made simultaneously (during June and July, 1958) on two identical installations in my laboratory at Saint-Germain and in a new laboratory at Bougival, in an underground gallery 57 meters deep, have shown that the previously observed anomalies are still present and that they possess, in both cases, periodic structures which are interrelated in a remarkable manner.[1]

For illustration purposes, I give in Fig. 1 a graph representing the results of harmonic analysis obtained by the Buys-Ballot filter method over a period of 24 hours, 50 min., starting from day and night observations made simultaneously in both laboratories during a month (from July 2, 0 hour Universal Time to July 31, 23 hours, 40 min., Universal Time) under the conditions *identical* to those of my experiments during the period of June-July, 1955.

This graph permits a comparison of results obtained in both the Saint-Germain and Bougival laboratories. The difference in the amplitude of both waves is very small, and they possess a remarkable concordance of phase.

These parallel experiments made at the same time and under the same conditions allow the introduction into my earlier argument of elements of great value for the elimination, with a high probability if not absolute certainty, of almost the totality of proposed explanations for the observed periodic effects. I will simply indicate that

(1) Alone, the practical identity of the periodic effects of 24 hours, 50 min., observed at Saint-Germain and Bougival permits the elimination of any explanation by a casual cause.

(2) In the same way, the practically invariable temperature conditions realized in the Bougival laboratory permit the elimination of any thermal effect.

(3) The parallelism of the periodic effects observed at Bougival and Saint-Germain leads to the elimination of any effect based on the influence induced by the building or on that of any superficial cause.[2]

(4) The relatively large variability with time observed in the amplitude of the periodic effects permits the elimination of any explanation based on the actually admitted laws of gravitation.

Thus, the results of my experiments (July, 1958) confirm, in a striking manner, my earlier argument leading to the conclusion that there exist in the motion of a paraconical pendulum on an anisotropic support anomalies of the periodic character which, at this point of the discussion, cannot be tied with any known phenomenon.

Notes

[1] The reader could usefully refer himself to the four following notes which I presented at the Academy of Sciences at the end of 1958:

New Experiments on the Paraconical Pendulum on an Anisotropic Support, C.R.A.S., 247, 1958, p. 1428.

Periodical Structure of the Motion of the Paraconical Pendulum at Saint-Germain and Bougival, July, 1958, C.R.A.S., 247, 1958, p. 2284.

Experimental Determination of the Effect Produced by the Anisotropy of the Support on the Motion of the Paraconical Pendulum, C.R.A.S., 248, 1959, p. 764.

Experimental Determination of the Effect of Inclination of the Support on the Motion of the Paraconical Pendulum, C.R.A.S., 248, 1959, p. 359.

[2] In particular, the explanation presented by Mr. Goguel (*Remarks on the So-Called Paraconical Pendulum*, C.R.A.S., Vol. 246, No. 16, April 21, 1958, p. 2340) attempting to explain the observed phenomena by the combined effect of the wind and building cannot be accepted.

ADDENDUM I—

Simultaneous Experiments Made During the Month of July, 1958, at Bougival and Saint-Germain

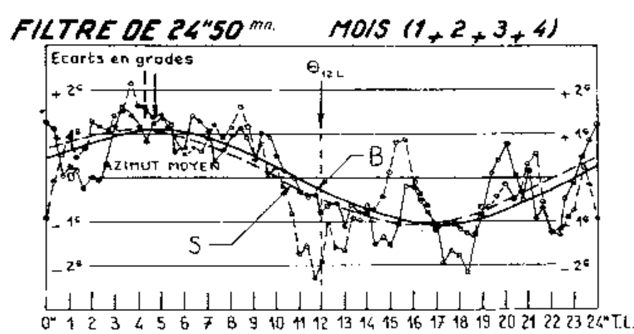

Fig. 1. Buys-Ballot's filter of 24 hours, 50 min. (deviation in centesimal degrees). Month (1+2+3+4)—test sequences. Key: ____________ represents tests at Bougival, ------------- represents tests at Saint-Germain; thin line represents mean cycles, heavy line represents adjustment sinusoids.

	2R (in centesimal deg.)	$\Theta-\Theta_{12\,L}$ (local time)
Bougival	2.17	−7 hours, 23 min.
Saint-Germain	2.10	−7 hours, 55 min.