

Maurice ALLAIS

L SPACE SOTROPY

*LA NECESSAIRE REVISION*

*DE CERTAINS POSTULATS DES THEORIES CONTEMPORAINES*

\*

*LES DONNEES DE L'EXPERIENCE*

CLÉSIENT.JUt.1AH  
*Éditions juridiques et économiques*



09-1

*This book is dedicated to :*

- *To all those who do not consider today's "well-established truths" to be intangible, and who do not have blind faith in their durability,*
- *And to all those who believe that any real progress in our knowledge can only be based on experience.*



*"The scientist should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biased by appearances ; have no favorite hypothesis ; be of no school ; in doctrine have no master. He should not be a res- pecter of persons but of things. Truth should be his primary object".*

Michael Faraday -

*"Experience shows that the ideas of 'competent' men are often completely at odds with **reality**, and the history of science is the history of the errors of 'competent' men".*

Vilfredo Pareto --

*"The physicist who has to give up one of his hypotheses should be full of joy, for he has found an unexpected opportunity for discovery. His hypothesis, I imagine, had not been adopted at face value: it took into account all known factors that seemed likely to intervene in the phenomenon. If the verification doesn't happen, it's because there's something unexpected, something extraordinary: it's because we've found finconntz and notzuenii".*

Henri Poincaré

*"The history of science shows that the progress of science has constantly been hindered by the tyrannical influence of certain conceptions that have come to be regarded as dogmas. For this reason, the principles that have come to be accepted without further discussion should be periodically subjected to a very close examination".*

Louis de Broglie ----

*This ostracism of innovators is by no means an exception; very few innovators escape it, and we can, without hesitation, formulate the general rule that any scientist who discovers a principle that differs from conventional conformism, finds it impossible to have his ideas accepted, no matter how rigorous the arguments that formally demonstrate their accuracy. ...*

*The court i q/uste of the nouateurs, the rnéconnaisaisance and fouòfi of their 'euvres, the iniquitous judgments which one pronounces against them, the persecutions even which one inflicts to them, are the **rule**, many saounts and philosophers pointed out and deplored them, maig it does not seem that one thought still to **react** against this state of things...*

*The conformists occupying the highest social positions continue, as in the past, to fight or stifle all discoveries that do not fit in with their **prejudices** and with the dogmas in force in the classic Z'raités ...*

*We, personally, are well placed to know this. What's the point of encouraging scientific research if the fruits of these investigations are destined to be buried and their authors condemned to oblivion or even persecution?*

**Auguste Lumière\*\*\*\*\***

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# SPACE ANISOTROPY

*La nécessité d'une révision  
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MIJEit'S INTERFEROMETRIC EXPERIMENTS  
ANALYSES AND INTERPRETATION

- A - huif Groph@ws /oWomeMoux Me 4fiWr  
žf - Zo d ferznžzzcUžon '7e fžnxfognojo7že ene fo uBesse de /o Zbrre
- C - Azta7yae crž 'žg'ue de čo th ozfie de Miller et de san inYerprdčafž@n  
de aes oBservazžona
- D - Dezzx thfi'ae.Ł'aniefz-opie de l'eepace et Ia uitease  
eosmiqze de la Z'erre veze la Constellation d'žiemule

## CHAPTER V

A POSSIBLEINTERDEPENDENCE  
BETWEENLBSAZD4UTSMOYENS DUPENDULEPARACONIQ  
UE ASUPPORTANISOTIitGPE  
ANDLESAEIMUTSMOYENSDEMILLER

- A - Overview

## CHAPTER VI

## HARMONIC ANALYSIS OF OBSERVATIONS

A - Peržodžcifes asčzozsomi 'ues

B - Z'ests dep z-ž@đžcizš

## CHAPTER VII

## REPRESENTATIVE EXPERIENCES

A - Zčpččifčon des expčrčences sur fe pendule par-aconi@ue Zš

- J'fi'p'fintžon dee exp'fizficiencies de uisfi'es optix/tzes

## CHAPTER VIII

## OPPOSITIONS DOGMATICS

A - ěfne frfis /orte opposition dc cerfniris *membeee dc l'Acadkmie dcc*  
**Sciences**

B - Articles de presse piitifiés cuz- *mec expėMencec - Ma Iettz-e du 14*  
*févrioe 1958*      Secrétaires perpėfuefs '2e f      'zdėznieches **Sciences**

C - Z-e red      *du 5 mci 1958 de l'Académie dec lsciencce de publier ma*  
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*février 1960 sur les anomalies optiques miscce en ěvidence lore*  
*des expėriences cruci'zfes de Juillet 1958*



A - *7nuitatlon à nm Conférence du 22 féuzfier 1958*

B - Zào "zY-on reconsióérer fee foçs de fa GrauzYaJzon  
7 (Mémoire de 1958)

C-ÆeruFAsoreflcozomdErpeñmsntMAæorcβWorbonGrAu  
(Mémoirededécembre1958)

D - Z'est cfepériodicifé:Générofisofion 'fs test cfc Sçheater on *ear de séMeø emporelleø autocorreléø &uztø l'hypotNee d'un proaeøø a '2e* perYurbofJo ø oféotoicøø 'fuzi *øyatème* tfabfe (Mdmoire de 1961)

Z2 - Z 4 2 q u e n c e , proáaóicifé eč /*trzsarct*, aoec dezcx Appendices:  
1. *fi-'z'éqzaezaneø empizfipmzeø and qfzez neø nmft éznxatùg'zaes - 2-*. Z Z'fzéorènze Z'. simufafzon cfu *Aosarzzpaz-des/onc-* fzonepresg'uejzérzódigues  
(Memoir of 1982)

f' - Ser fa *distribution* iormufe deø modern ò cms citstozits régu-  
fifirement espocés 'f'une sonime 'fe øinusoiñ2eø  
(Note by Maurice Allais presented4 by Robert Fortet, CRAS,  
May 30, 1983)

G - C! itsízférofione str fes lhéories pñysi9ues  
(Excerpts fr om my 1983 dissertation: *Les fondements de la  
théorie de l'utilitè et du risque*)

Zf - *hea expézfienc es* by Damon C. MÙler 1925-1926 and the Z7séoz-ie  
  
(Memoir published in "Ln ufatine et la Rouge", revue polyteeh-  
nienne, September 1996, pp. 29-37).

I - Zteø régulaMt:è's trimti,g'nific'M:rues in leø obsertiaM s in e ê-  
romètr new de Dafon C. Miller J92S-192ð  
(*Comptes Rendus de l'Académie des Scienes*, March 1997).

## APPENDICES I

A - E Correspondence *lines J958-J960*

Zf - ñfon memory of *J957 on the speed of light*

C - *Amiclen de Preøøe*

O - Sr'zridofe ô Poïyfecfiniqite (Louis Rougher, July 1959)

## APPENDICES II

Notes presented to the Académie des Sciences  
and not published in the Comptes Rendus

*Harmonic analysis of muffles      fa of the paraconic pendulum*

- 1958      *sur l'existence d'une Composante Périodique de Période Voisine de 24h CO' dans les Mouvements du Pendule Paraconique à Support Anisotrope dans les observations de Novembre-Décembre 1954 et Juin-Juillet 1955*  
IRSID, November 20, 1958, 11 p.
- 1959      *Paraconic Pendulum Movement - Searching for Hidden Periodicities by Considering Gram Frequencigrams Associated with the Generalization of the Slchuster Test*  
IRSID, November 2, 1959, 8 p.
- 1959      *Paraconic Pendulum Movement - Searching for Ticked Periodicities by Considering Corrélogrammes*  
IRSID, November 26, 1959, 9 p.

***Eclipse de soleil***

- 1959      *Mechanics - Movements of the Paraconic Pendulum el Total Ssoleil Eclipse of October 2, 1959*  
IRSID, November 10, 1959, 7 p.

***Caractéristiques du dispositif expérimental***

- 1958      *Experimental determination of the coefficient of friction of the ball swivel of a paraconic pendulum.*  
IRSID, November 20, 1958, 9 p.

*Zhéoz-ie dia Pendulum Pazaoonfiqzze*

- 1958 *Application of Bour's Theorem to the case of earth movements in the most general case*  
IRSID, March 19, 1958, 9 p.
- 1958 *Equations of Motion of the Paraconic Pendulum with Anisotropic Support at Small Oscillations - First and Second Approximation*  
IRSID, August 18, 1958, 7 p.
- 1958 *Variations of the Osculating Parameters of the Ellipse Described in the Motion of the Poroconic Pendulum with Anisotropic Support and Small Oscillations - First and Second Approximation*  
IRSID, August 18, 1958, 6 p.
- 1958 *First Approximation Corrections of the Paracoid Pendulum with Anisotropic Support and Small Oscillations*  
IRSID, August 18, 1958, 4 p.
- 1958 *Second Corrections Poraco-Pendulum Approximation niqtie ô lsupport Anisotrope et à Petites Oscillations*  
IRSID, August 18, 1958, 5 p.

*Paraconic pendulum with isotropic support - Reeherc ec Expêz-imen: talea*

- 1959 *Paraconic Pendulum. Creation of a support as isotropic as possible.*  
IRSID, November 5, 1959, 10 p.
- 1959 *Paraconic Isotropic Suspension Pendulum. Determination of the Variations over Time of the Characteristics of the Correlation of the Movement with the Azimuth.*  
IRSID, November 10, 1959, 7 p.

*C-éocféoie et Optique - Search ExpérimenfuJec*

- 1968 *Anomalies in Triangulation and Levelling Operations - Possible Explanation and Comparison with Experience*  
IRSID, May 21, 1958, 5 p.
- ICEÖ *Existence of Periodic Components in Variations of Readings Corresponding to Tracks Performed with a fixed bezel on a fixed sight in conjunction with the movements of the paraconic pendulum*  
IRSID, February 23, 1960, 6 p.

## INTRODUCTION

*"The theory must give as simple a description as possible of the world of physics..."*

*"It must call upon a new grandeur, accept a new hypothesis only when an inescapable necessity compels it to do so.*

*"When, therefore, the physicist discovers facts unknown to him, when his experiments have enabled him to formulate laws which the theory had not foreseen, he must first of all seek with the greatest care whether these laws can be presented, to the degree of approximation required, as consequences of the accepted ideas..."*

*"It is only when we are certain that the quantities hitherto treated by theory cannot serve as symbols for the qualities obtained, and that the established laws cannot be derived from the hypotheses received, that we are authorized to enrich the physics of a new quantity, to complicate it with a new hypothesis.*

*"These principles are the very essence of our physical theories".*

M.P. Duhem \*

*"It is the constant facts that alone have a physical reality".*

Max Born \*\*

*"There's always a nutsiøfes te ndance in letting  
opt ntions crystallize into beliefs. This tendency  
is manifested sptcioleme nt  
when an eminent author . . . begins to be  
recognized as an autoritt . "But "to err is human"  
and tf should always be allowed to criticize ou-  
vrages metffeurs. lsi instead of welcoming, as  
òtenue- nues, recherches, et  
critiques, odmtroteurs d'un grand auteur accept  
l'autori té de ses  
writings this is most detrimental to the causede the  
truth.*

*"In the su jets of philosophy and scence, outoritt  
has always been the great adversary of vérité.  
Despotic calm is usually the triumph of error .*

*"En scence et en pAtfosop/ite rten ne doit être  
temu pour sacré".*

Stanley Jevons \*\*\*

## A

## WHY THIS BOOK

**1.- Des données expérimentales nouvelles, contributions aux débats sur les fondements de la Mécanique et de l'Optique**

*Debates on the foundations of Mechanics and the Optics*

1- This book deals with one of the most debated areas of physics over the last three centuries, that of the foundations of mechanics and optics, especially with regard to the transmission of actions to the observer.

distance and the influence of the Earth's motion on terrestrial phenomena <sup>1</sup>

(-) M .P. Duhem, 1899, *des théories Electriques de -f. Clerl 2'faxwelf. Etude hisse - ri9ue ef critique*, Annales de la Sociètà Scientifique de Bruxelles, 24\*^ année, 1899- 1900, p. 2d5.

(- - ) Max Born,1920,Lo tAdorie de fn refotiuitd d'Einstein et ses boses physiques, Gauthier-Villars, 1923, p. 291.

(---) Stanley Jeyons, 1888, *Théorie de l'économie f-oiitique*, French translation of the **Third** Edition, Giard, 1909, pp. 369-370.

(1) For the pre-relativist period, see in particular four synthesis papers:

*W. Maacart, 1872-1874, Sur les modi'cotions qu'tprouut la lumière par cuite du mouvement de fe source lumineu se et du mouvement de l'obceruofeur*, Annales de l'Ecole Normale Supérieure, 1872, p. 157-214 ; et 1874, q. **361420** . Oliver Lodge, 1893, Atierntion *Problemc. A Discussion concernng' the mo- tion of fée Eorth, ond concerning the connection bel ween Ether ond Gross 2'ofter, ioitfi some nez Axperiments*. Philosophical Transactions of the Royal Society of London, 1894, Vol. 184, p. 727-8%.

Edmund Whittaker, *History of the theories o(Aether and Electricity*, Tome I, 'Flic Cloasicof 7'heories, 1951 YO\*F especially Chapter XIII, *Clæsical 7/seory* in i'te age o/ Wrentz, **p. 386-428**.

Augustin Sesmat, 1937, 2-'Optique des Corps en 3'fouvement, Hermann, Paris.

This field has been the subject of the most heated controversies, where passion has all too often won out over objectivity, especially since the progressive domination of relativistic theory in **literature**,

*For this reason, this book will confine itself to the analysis of experimental data, the only true source of our knowledge, and in particular to the analysis of new experimental data that opens up new perspectives.*

*New experimental data in four areas*

2- The new experimental data analyzed in this book cover *four fields that are considered a priori to be different yet closely related to each other*:

my experiments at the Institut de Recherche de la Sidérurgie (IRSID) on the paraconic pendulum on *an anisotropic support (Chapter I)*, and my experiments on the paraconic pendulum on *an isotropic support (Chapter II)*;

my experiments on the optical deviations of *sightings on test patterns, and the subsequent sighting experiments on test patterns and collimators* carried out by the Institut Géographique National (IGN) (*Chapter III*);

regularities characterizing *optical aiming experiments* d'Esclançon and *not seen by him (Chapter 'V)* ;

regularities characterizing *interferometric observations* by Dayton C. Miller *and not seen by him (Chapter IV)*.

(2) Edmund Whittaker, id. in Volume II, 2 "fie *Modem* "i "fiesories, 1953; see especially Chapter II, the *2telotituity of Theory of Poincaré end Z-orentz*, pp. 27-77, and Chapter *W*, *Croovitotian*, pp. 144-146; and *Chopitree IV, VI, and VII* below.

Whittaker planned to publish a third volume covering the period 1926-1950. **En** Due to its disappearance, this third volume was unfortunately never

All these experiments differ from all those that preceded them in their fields in one essential respect. *They were based on a large number of continuous observations, day and night, over long periods of time.*

*The danger of preconceived ideas*

3- There are at least two reasons why this work *refrains from any general interpretation*. Secondly, and more importantly, the very nature of the new phenomena highlighted in this book, if it is to be fully defined, would first require a great many complementary experiments, which are indispensable if we are to identify the precise laws of the new phenomena.

Given the current state of available information, such an overall theoretical construction *would be quite premature*. In fact, the new experimental data analysed in this book, *the existence of which is quite indisputable*, are manifestly *so complex*, and involve so many phenomena, that it is the *experimental approfondissement préalable of their structure and regularities* that conditions any overall theoretical construction<sup>3</sup>.

We absolutely must avoid the kind of *preconceived ideas that have been so detrimental to the development of theoretical physics over the last two centuries*.

(3) In his memorandum of January 3, 1980, "An interim report on the **Allais Experiment**" Robert Latham (Imperial College of Science and Technology, 70 p.) écrit (p. 5) :

*'Indeed in all his work there is a complete absence of any detailed attempt at explanation, coupled however with very great care and thoroughness in the conduct of the experiments'.*

It's 18, in fact, a principle I've always followed: never prematurely present a general theory to explain my results.

All that can be reasonably advanced at present is an *anisotropie de l'espace*.



*The new experimental data* presented in this book, when considered as a whole, appear to be *just as incompatible* with the theories of Mechanics and Optics of the pre-relativistic era as they are with the theory of Relativity<sup>4</sup>, whether restrained or general.

In the long conflict of doctrines, we must never forget that science is always in perpetual evolution. In *science*, there is no such *thing as a definitive truth*. The fundamental characteristic of scientific progress is a constant effort to understand the profound nature of a world that more often than not remains indecipherable.

May the new data on experience presented in this book inspire a new effort at reflection, *free from all* preconceived notions, *prejudices, biases and passions*.

Little by little, science is reaching new heights in its progressive discovery of the profound, hard-to-decipher nature of the world we live in.

Nature always responds to our questions, but all too often she seems to answer like the oracle at Delphi. Indeed, her answers don't always appear very clear to us, not so much because they are ambiguous and incomprehensible, but because we are too often imprisoned by preconceived ideas and established truths *that prevent us from understanding them*.

## 2.- *L'objet de cet ouvrage*

### *Four objectives*

1- This book has four basic objectives.

- *The first* is to present an *overview* of my experiments with the paraconic pendulum on an *anisotropic support* from 1954 to 1960 (*Chapter I*), my experiments with the paraconic pendulum on an *isotropic support* from 1959 to 1960 (*Chapter II*), my experiments with optical sights on *test patterns* in 1958, and my experiments with optical sights on *test patterns and collimators* in 1959 (*Chapter III*); and the analyses I have carried out from 1954 to the present day.
- *The second objective* is to present an *analysis of two very significant and fundamental earlier experiments*, those of Esclangon in 1927-1928, and those of Dayton C. Miller in 1925-1926 (*Chapter IV*). Miller in 1925-1926 (*Chapter IV*).
- *The third objective* is to highlight the *common* features of these five series of experiments: observations that are *inexplicable* within the framework of currently accepted theories, *very marked structural connections*, and *very significant* temporal correlations with astronomical data, particularly with the position of the Earth in its orbit (*Chapters V, VI, and VII*).
- Finally, this book aims to encourage all those who are in a position to do so to carry out, or have carried out, a series of ensemble experiments, which are likely to provide *major information* on the very foundations of *contemporary physical theories* (*Chapter VIII*).

### *My 1958 memoir*

2 - In the magazine "*Perspectives X*" of the Ecole Polytechnique in 1958<sup>1</sup>, I published an overview of my work on the anisotropic paraconic pendulum from 1954 to 1957, under the title "*Doit-on Reconsidérer les Lois de la Gravitation ?*". The English version of this thesis was published in 1959 by the American journal "*Aero-SPACE Engineering*" of the institute of the Aeronautical Sciences under the title: "*Should the Laws of Gravitation be Reconsidered ?*" \*.

This dissertation was followed by a two-page \* *Note Complémentaire* in which I reported on *crucial experiments* carried out simultaneously with identical dispositions in July 1958, on the one hand in my laboratory at the Institut de Recherche de la Sidérurgie (IRSID) in Saint Germain, and on the other in a laboratory set up in April 1958 in an underground quarry at Bougival with 57 m. of cover and 6.5 km away.

### *The publication of my physics work*

3 - Naturally, I intend to publish all my *theoretical and experimental* work in physics as *soon as possible*... but the best is the enemy of the good, and I felt it necessary to present this *overview* of the five *highly significant*, and indeed *fundamental*, series of experiments on which this book is based.

(1) p. 90-104. This memoir is reproduced in *Appendix B* of the second volume of this ouvrage, "*Compléments expérimentaux et théoriques*" ( see Contents above, p. 31).

(2) *Aero-SPACE Engineering*, September 1959, n° 9, p. 46-52 ; October 1959, n° 10, p. 51-55 November 1959, n° 11, p. 55.

The translation was made in the United States on the recommendation of Werner von Braun, directeur de la *Net ionol Aemnn utics end Spnce Administration*.

(3) This *Supplementary Note*, which will be sent to the *Perspectives X* Editorial Office at a later date, is subject to the following conditions:  
/e8 exp4rience8 **cruciales** de 'Juillet 1958, a été4 simplement en car t4e sans pagination dans cette publication after proofreading.

The English version of this *Supplementary Note* was included in my *Aero-Spoece Engineering* dissertation (November 1959, p. 55).

(4) The first volume will be published sous le titre: "*Recherches expérimentales et théoriques sur les Théories Physiques 1953-1960*". It will include my main publications and dissertations (see the *Références* below at the end of this volume). It will also present the numerical values of all observations made between 1954 and 1960 during periods of one month or two weeks.

This volume will be followed by two others: "*Théorie du Pendule Paraconique*" and "*Annuaire des Compléments des Données de l'Observation*".

Like the two volumes of the present work, these volumes will be published by Editions Clément Juglar with the help of my friend Guy Berthault.

## B

**MES RECHERCHES EXPERIMENTALES ET THEORIQUES****1.- *L'origine de mes recherches expérimentales***

*My passion for exploring the "physical unknown"*

1- First of all, I'd like to clarify the origins of all my physics research over the last half-century.

I owe my passion for physics research and the exploration of *physics* to my physics course at the Ecole Polytechnique, and it has *never ceased to motivate me*.

It was she who sparked off my research into theoretical physics and my experiments with parabolic pendulum movements. I carried out these experiments from 1953 to 1960. Since 1960, and on several occasions, I have pursued theoretical research on the uni-

I've also worked on the interpretation of my experimental results, particularly in 1967, 1978, 1981, 1985, 1987, 1989, and from 1992 to 1996<sup>1</sup>.

(1) I carried out all this work in parallel with my work as an economist, for which I was awarded the *Nobel Prize in Economic Science* in 1988.

All in all, since 1950, I've certainly spent 90% of all my time on my theoretical and experimental research into physical theories.

In fact, originally, I wanted to devote myself entirely to physics. It was only as a result of the war that I was led to turn gradually towards economics (see the Third Edition of my *Traité d'économie Pure*, 1994, *Traité d'économie Pure*- Introduction, p. 19 and 26).

*A conviction*

2- I've always been convinced that the propagation of gravitation and electromagnetic actions takes place from *near to near*, and that it implies the existence of an intermediary medium, the "ether" of Fresnel and the physicists of the XIX\* century, without, however, being able to **consider**, as was generally accepted in the **XIX\* century**, that the "ether" of Fresnel and the physicists of the XIX\* century was the "ether" of Fresnel and the physicists of the XIX\* century.

**all** parts of this medium are perfectly immobile in relation to **each other**, **and** in particular in relation to the **fixed stars** <sup>2</sup>

This conviction led me to consider in the early fifties that a magnetic field corresponds to a local rotation of this intermediary medium. I deduced that it should be possible to establish a link between magnetism and gravitation by observing the action of a magnetic field on the movement of a pendulum consisting of a glass ball suspended by a wire about two meters long.

*Anomalies in the motion of the Foucault pendulum*

3- To detect such an action, I began by observing the motion of such a pendulum in the absence of any magnetic field other than the earth's field.

*To my great surprise*, I found that this movement was by no means reduced to the Foucault effect, but that it *exhibited very significant and time-varying anomalies* in relation to this effect <sup>3</sup> It was the study of these *completely unforeseen* anomalies that formed the essential object of my experiments from 1954 to 1960.

(2) See *Chopitre Yi* below.

(3) Certainly I was favored by luck. But making the most of that luck, as exciting as it was, turned out to be extremely *difficult at l'ouc point de vue*.

*The action of a magnetic field on pendulum motion*

4 From the *very limited number of* observations made at IRSID in 1953\* and again in 1954 and 1955, of the movement of a glass ball oscillating in a magnetic field of the order of a few hundred gauss, *I was unable to draw any definitive conclusions at the time*. Today, however, I consider that *the effects to be expected are too small to be conclusive. can be detected with the magnetic champs that can be made* <sup>5</sup>.

*Experimental study of paraco- nic pendulum anomalies*

5 - In view of the anomalies in the movement of the pen- dule, which had been *indisputably* demonstrated as early as February 1953, I devoted myself from 1954 onwards to the study of anomalies in the movement of a short pendulum suspended by a ball, which I called *the "paraconic pendulum"* <sup>6</sup>.

*So I wasn't driven to these experiments by theo- ritical ideas. They were merely a by-product of a completely different research project, which was not successful.*

(4) Thanks to the help of my friend Emmanuel André-Martin, my first experiments were carried out in February-June 1953 in a lo cal belonging to the Compagnie Clemençon (34, rue Milton, Paris) (see below § D. 1).

(5) So, in 1989, I gave up suggesting any new experiments on t h e action o f a magnetic field on a pendulum, as I had previously intended to do. In fact, in my experiments from 1953 to 1955, the magnetic field produced at the center of the sold-noid where the pendulum oscillated was only of the order of d00 gauss.

This question will be the subject of a future publication on my part.

(6) From October 16, 1953, thanks to the powerful support of Pierre Ricard (see G D.1 below), I was given a basement laboratory with two very large rooms (ten by ten meters) at IRSID in Saint-Germain-en-Laye, with two collaborators, Jacques Bourgeot and Annie Rolland.

IRSID's highly competent mechanics' workshop never ceased to help me. invaluable assistance in the precise construction of the various equipment I used from 195d to 1960.

My experiences at IRSID continued from February 1954 to June 1960.

For me, the results were *totally unexpected* in every respect, both in terms of their nature and their scope.

*It was experience, and experience alone, that prompted me to carry out systemic experiments on the paraconic pendulum. It was experience that constantly guided me, and it was experience that finally led me to the conviction that the observations I made did indeed correspond to a very real new phenomenon, totally inexplicable within the framework of currently accepted theories.*

Z- Mes rec7uerCes eqpézémezufofes sur flej>9nô'uZe poxoconJqane 7954-7960,  
e7 sin" Ms déuicuNona opfi#ues des uBées sur zmMes J9S8

*The existence of a 24 h 50 min Jtinoire periodic component in the motion of the ani-trope-supported paraconic pendulum of an amplitude totally inexplicable within the framework of currently accepted theories.*

1- To study anomalies in the movement of a short pen- dule, I mainly used a meter-long paraconic pendulum, consisting of a vertical bronze disk fixed to a bronze rod suspended from a stirrup resting on a steel ball.

In the absence of any magnetic field other than the terrestrial magnetic field, I have observed, on the basis of *continuous* observations followed for periods of the order of one month from 1954 to 1960, some *very remarkable* cir- constances, most notably the existence of a significant lunar diurnal periodicity of 24 h 50 min. of *considerably higher* amplitude than that calculated according to ac- tually accepted theories. The *observed* amplitude is about twenty or one hundred million times higher than the *calculated amplitude*, depending on whether we consider the anisotropic-suspension paraconic pendulum or the isotropic-suspension pa- raconic pendulum <sup>1</sup>. In fact, such a *lunar* diurnal peridic component is *completely inexplicable within the framework of currently accepted theories.*

In addition, the experiments carried out suggested *the existence of a direction of anisotropy i n space at each i nstont.*

(1) See **below** Chapter f, § A.5.3, p. 98 eC B.2.1, p. 118, and Chapter II, § Ê".2.2, p. 285.

(2) See Chapter f, § B.2.6, and B.2.7, pp. 123-125, and Chapter II, § F.2.2, pp.



*The two crucial experiments in July 1958 on the movement of the paraconic pendulum with anisotropic suspension*

2- *Identical* results regarding the existence of a lunar diurnal periodicity of significant amplitude were found, in the two crucial experiments of July 1958, in two laboratories about 6 km apart, one underground in Saint Germain, the other in an underground quarry in Bougival with 57 m of cover\*.

*Associated optical sighting experiments on test patterns from July 1958*

3- The existence of anomalies in precision levelling and triangulation operations <sup>4</sup>reconciled with the anomalies observed in the movement of the paraconic pendulum, prompted me to carry out, in *parallel with* my experiments on the anisotropic-supported paraconic pendulum at Saint-Germain and Bougival in July 1958, a series of optical North-South and South-North sightings on fixed sights at Saint-Germain. Due to technical difficulties, these optical sights could only be properly carried out in the second half of July 1958.

In fact, during this period I observed a *remarkable torrespondance* between the observations of the azimuths of the paraconic pendulum and the observations corresponding to the azimuths of the reci- procal optical sights of two azimuth glasses on two test patterns having the same supports as these glasses <sup>5</sup>.

In any case, the amplitudes of the optical deviations observed, *considered in themselves, are inexplicable within the framework of currently accepted theories.*

(3) See Chapter 7, Section C below.

(4) See my Note of May 21, 1958, Anomnlies des *opérations de trionguloioon el de nivellement. Explicol ion possible ef confrontation o avec l'expériente.* This note is reproduced in Appendix II of the second edition of this book (see above, p. 33).

(5) My initial intuition was thus *re morquobly confirmed.*

f6) Cf iopitre JJ, Section B, below.

*Anomalies observed during the two total solar eclipses of  
1954 and 1959*

4 - During the total solar eclipse of June 30 1954, a remarkable deviation of the oscillation plane of the paraconic pendulum was observed, a *deviation that is inexplicable within the framework of currently accepted theories*. A similar total deviation was again observed during the total solar eclipse of October 2, 1959.

*The existence of an anisotropic direction of variable space  
with time deduced from observations of the paraconic  
pendulum with isotropic support*

5- Finally, while in all my experiments from February 1954 to July 1958 the support of the paraconic pendulum was anisotropic, in October 1959 I realized *an isotropic support in order to be able, by a new method of analysis, to determine at each instant the anisotropic direction of space*.

In two series of monthly observations, in November-December 1959 and March-April 1960, this approach, which *differs* from the previous one, confirmed the existence of a periodic lunisolar structure *that is totally inexplicable* within the framework of current theories, and *demonstrated the existence of a time-varying direction of spatial anisotropy* (Chapter II).

*Two indisputable discoveries*

6 - At the end of the 19th century, many experimenters pointed out anomalies in the movement of the conical pendulum, but *nothing definite* had emerged from these experiments, and I believe that the *indisputable* evidence of the anomalies of the paraconic pendulum and their periodic structure constitutes a *genuine discovery* for which I can claim full *authorship*.

The same is true *o fortiori* of the optical deviations of sighting patterns and their periodic structure, which are completely independent of any trivial influence, since *no one had even suspected their existence*.

These are, in fact, two *incontestable* discoveries of *nourenuz phenomena* which, as things stand at present, seem to be explained only by the *anisotropy of space*.

(81 See for example Dejean de Fonroque, July 1879, *Du Pendule, Théorie de ses variations*, Chamerot, 32 p., and *Note du 14 avril 1979 à l'Académie des Sciences*, présentée par M. A. Cornu, *Sur diverses expériences faites avec un pendule oscillant avec de grandes amplitudes*. (see a detailed analysis of Dejean de Fonroque's memoirs in the *second volume* of this work, Chapter Cf, Section C, above, p. 28).

*because memoirs of Dejean de Fonroque are only qualitative if they cannot be the object of a quantitative analysis.*

(9) See below § C.2, p. 60-63.

*My Notes to the Academy of Sciences*

7 - From November 1957 to February 1959, my observations on the anisotropic suspension paraconic pendulum were the subject of eight Notes à l'Académie des Sciences presented by Albert Caquot, and two Notes sur leur signifi- cation statistique presented by Joseph Kampé de Fériet \*0.

*Visits to my laboratories*

8 - From September 1955 onwards, I became convinced that the movements of the paraconic pendulum on an anisotropic support did indeed constitute a new phenomenon, *totally inexplicable within the framework of currently accepted theories.*

(10) These notes are as follows:

- |                             |  |
|-----------------------------|--|
| Note of May 13, 1957        | - <i>Periodicity Test - Cenerology of the Periodicity Test S!thuster au cos de séries temporelles autorrelles</i>  |
| Note of November 13,        | 1957 - <i>Observation of the movements of the poroco pendulum nique.</i>   |
| Note from November 2S 1957  | - <i>Harmonic analysis of pendulum movements</i>   |
| Note dated December 4, 1957 | <b>paraconique.</b><br><i>Movement of the paraconic pendulum and eclipse of the sun on June 30, 1954.</i>  |
| Note of December 16,        | 1957 - <i>Poroconic pendulum theory and influence</i>  |
| Note of December 23,        | 1957 - <i>Application of the <b>Generalized S!cluster Test</b> à dec azimuth harmonic onalysis of the paraconic pendulum.</i>  |
| Note of November 3,         | 1958 - <i>Noncelles experiments on the poroconic pendulum onisotropic support.</i>   |
| Note to December 22, 1958   | - <i>Periodic s!rize of pendulum movements poroconic with onisotropic support à Botigioul et Soini-Germain in July 1958.</i>   |
| Note of January 19, 1959    | <i>Experimental determination of the influence of the inclination of the load-bearing surface on the motion of a paraconic pendulum with <b>onisotropic</b> support.</i> |
| Note dated February 9, 1959 | - <i>Experimental determination of the influence of support onisotropy on poroconique pendulum motion.</i>   |

The dates given are those of publication in the Comptes Rendus (and not the earlier dates of presentation). The dates of presentation were as follows: May 6, 1957, November 4, 1957, November 18, 1957, November 18, 1957, November 25, 1957, November 4, 1957, December 1957, October 20, 1958, November 10, 1958, December 1, 1958 and January 26, 1959.

In addition to their publication in the Comptes Rendus de l'Académie des Sciences, these various Notes were published à part by Gauthier-Villars in two **Fascicules** entitled: "*S!ruef ure périodig tie dec mou sementic du penduli poroconique à cuc - pension anisotrope el in/lueztce Iz/nisolaire. Résullals expérimentaux et anomalies*" (25 eC 17 p.1. The first includes the first six Notes, the second the last four.

I disseminated the main results of my research to various personalities, and organized visits to my laboratory at IRSID, puts visits to my laboratory in Bougival after the crucial experiments in July 1958<sup>11</sup>,

### *Mes Conférences*

9.- My work has been the subject of three Conferences organized by the *Cercle Alexandre Dufour* :

- the first, "*Faut-il reconsidérer les lois de la Gravitation? Slur une nouvelle expérience de Mécanique*", on Saturday February 22, 1958, in the Henri Poincaré amphitheatre at Ecole Polytechnique<sup>12</sup>.

- the second, "*Faut-il reconsidérer les lois de la Gravitation? Nouveaux résultats, bilan et perspectives*", on Saturday November 7, 1959, at the Société des Ingénieurs Civils de France<sup>13</sup>

- the third, "*Les périodicités constatées dans le mouvement du pendule paraconique sont-elles réelles ou non? Généralisation du test de Slchuster au cas de séries temporelles autocorrélées*", Saturday March 18

14

(11) A total of 127 people took part in these visits.

(12) 14 wall charts; 34 projections; stenotype of the Conference and the Discussion, 80 p.

An overview of this Conference was given in my 1968 Memorandum (§ A.2.2 above).

(13) 13 'tableaux muraiix ; 50 p[ro]jections ; stenotypie de la Con férence et de la Discussion, 69p.

(14) The text of this conference was published in Bulletins 120, 121 and 122 of the Cercle Alexandre Dufour, April, May and September 1967, pp. 80-97, 107-124, and 130-132.

In fact, I never had the time to publish my first two Conferences of February 22 1958 and November 7 1959, of which I have only the stenotypies and wall charts and projections appended.

My 1958 memoir I§ A.2.2 above) presents an overview of my Conference of February 22, 1958.

### *Two Awards*

10 - My work on the paraconic pendulum with anisotropic support was marked by two prizes to which I was very sensitive: one French, the *Prix Galabert 19Ei9* from the Société Française d'Astronautique <sup>1</sup>, and the other American, in 1959, from the *Greuity Research Foundation!!!*

### *Stopping experiments*

11 - In the end, despite the *resounding success* of the two *cruciform* experiments in July 1958, the *very promising* results of my experiments on optical deviations in sighting sights in July 1958, and my experiments on the isotropic-supported paraconic pendulum in November-December 1959 and March-April 1960, I *had* to close my IRSID laboratory in June 1960 *for lack of funds*, and part company with two exceptional collaborators. Jacques Bourgeot and Annie Rolland <sup>17</sup>, <sup>1</sup>.

(15) The dissertation presented to the Société Française d'Astronautique was entitled '*Recherches théoriques et expérimentales relatives à la rotation des corps sur la fibre optique*' (December 1958, 21 p.).

Attached to this memoir are my memoir of May 13, 1958 "*Anomalies du mouvement de la pendule paraconique à support anisotrope*" (68 p.), my first eight Notes à l'Académie des Sciences (note 8 above), and my memoir of November 4, 1957 on the speed of light (see § S.S below).

(16) The dissertation presented to the U.S. Gravity Research Foundation in MRI was entitled: "*Neurotheoretical and Experimental Investigation of the Word on Craniology*" (January 1959, 9 p.).

(17) Whereas in 1868 Van der Willigen was able to write (*de Pendule de Foucault ou du mouvement de rotation*, Arch. Musée Teyler, I, 1868, p. 342)

*In the experiments on the Foucault pendulum, in terms of experiment, in 1959, the responsible French scientific authorities deliberately put an end to my work. Nothing happened there on the pendulum paraconic where the difficulties were essentially believed to be unresolvable.*

However, progress could not be made by refusing to examine the anomalies of the paraconic pendulum carefully and appropriately.

In fact, from a scientific point of view, the *unjustified* decision to put an end to my experiences was *totally incomprehensible*.

See below § D.S.1, p. 69-70, and Chapter f, Section G, p. 213-235.

(18) At my request, and in view of their very high qualities, my two colleagues - The engineers were immediately integrated into IRSID's technical departments.

s.- *Me" wc ernie" ihéo'-ivres, 9so- s9s*

### *Analysis of paraconic pendulum movements*

1 - Since 1953, I've never stopped working on the analysis and signification of paraconic pendulum movements and the optical deviations I've associated with them, particularly as regards their relationships with the search for a unitary theory of physics. In particular, I have elaborated a general theory of paraconic pendulum movements.

conics and wrote numerous memoirs on physical theories and the statistical significance of observations <sup>1</sup>.

### *Two major difficulties*

2- In fact, in my theoretical work on the paraconic pendulum I had to overcome two *major* difficulties.

Firstly, and as incredible as it may seem in view of the immense literature on the Foucault pendulum, *et zctzn author had not calculated the lunisolar influence on the motion of the Foucault pendulum*. In view of the *fundamental* importance of the *theorique* estimation of this influence, I was obliged to develop a complete theory. paraconic pendulum movements <sup>2</sup>.

(I) §A2.3.

(2) See my Mémoire d'ensemble "*Théorie dti Pendule Poroconique*", September 1956, 441 p.

In 1958, I prepared summary presentations of some of the essential principles followed in this Memoir in five notes intended for the Académie des Sciences but which could not be published (see above p. 33).

*Application dti Théorème de Bour ou c'is des mouvements terrestres dans le cos le plzss general*, March 14, 1958, 9 p.

*Equation du mouvement du pendule paraconique b support anisotrope b petites oscillations. Première et deuxième approximation*, August 18, 1958, 7 p.

- *Yoriotions des poromètres ostuloteurs de l'éclipse décrite dans ie mouvement du pendule poroconique à support anisotrope et d petites oscillations. Première et deuxième approximation*, 18 août 1958, 6 p.

- *Corrections of first approximation dti pendulum poroconique d'support anisotrope et d'petites oscillations*, August 18, 1958, 4 p.

- *Second approximation corrections of the isotropic supported paraconic pendulum el à petites oscillations*, August 18, 1958, 5 p.

I asked Henri Villat to present the first Note, but was refused.

Secondly, I had to admit that, in the vast literature on periodicity research in physics and economics, there was *no* periodicity test applicable to the general case of self-correlating time series. It wasn't until April 1957 that I was able to overcome this difficulty by developing a test generalizing the *Schuster Test*, which was only suitable for time series made up of independent terms!

*Investigations into the foundations of a unitary theory of physics*

3- From 1950 to 1960 I continued my research into the foundations of a unitary theory and wrote various Notes\*.

(3) My generalization of the Schuster test in *cos de series temporelles o ufoconrtltes* was the subject of my two Notes à l'Académie des Sciences of May 13 and November 13, 1957, and of my overall dissertation "*Test de périodicité. Généralisation du test de Schuster ou tes de ctries temporelles outorréltés dans l'hypothèse d'un processus de perturbation d'un système stable*", présenté en 1961 à l'Institut International de Statistique. This text is reproduced in Appendix D of the second volume of this book.

See Chapter I below, § B.1.3, note (6).

(4) Several of these Notes have been distributed in limited editions, including :

- *On a possible interpretation of the terrestrial magnetic thomp*, October 24, 1957, 7 p.

- *is on a solution of the part derivative equation such that*

$$\frac{1}{\sqrt{|g|}} \partial_i (\sqrt{|g|} g^{ij} \partial_j \varphi) - \frac{1}{c^2} \frac{\partial^2 \varphi}{\partial t^2} - \frac{2k_0}{c} \frac{\partial \varphi}{\partial t} - k^2 Q + 4 \times K \varphi = 0$$

*el on an interpretation of the constancy of the speed of light*, November 4, 1957, 12 p.

- *Sur une interprétation possible des anomalies de la gravité et ses applications*, November 5 1957, 54 p.

- *Interpretation of gravity anomalies as a screen effect of group actions*, March 1960, 29 p.

On my dissertation on the speed of light of November 4, 1957, see below *Chapter I*, § G.5.2, p. 226; *Chapter VI*, § C.1, p. 511-514; and *Chapter VII*, § C.4, note d, p. 599.



## WHAT'S IN QUESTION

**1.- *Connexions des observations du pendule paraconique et des déviations optiques des visées sur mires avec les expériences optiques d'Ernest Esclangon de 1927-1928 et les expériences interférométriques de Dayton C. Miller de 1925-1926***

*Esclangon and Miller experiments*

1- The paraconic pendulum anomalies and the sighting anomalies I've highlighted have *striking connections* with the anomalies encountered in the study of many other phenomena.

In what follows, I examine two series of anomalies in particular: those corresponding to Esclangon's optical experiments **of 1927-1928**, and those corresponding to Miller's interferometric experiments **of 1925-1926**. Both of **these anomalies**, *whose existence is very real*, turn out on analysis *to be of exceptional importance*.

*A general correlation with the Earth's position in its orbit*

2- What characterizes the observations of anisotropic and isotropic paraconic pendulum motion, the optical observations I've associated with them, Esclangon's optical observations, and Miller's interferometric observations, *is their correlation with the Earth's position on its orbit*, in contradiction with a fundamental postulate of the *Theory of Relativity, Restricted or Générale* \*.

(1) *Chapter IV* below.

(2) *Chapters V, VI and VII* below.  
*Chapter Y* is entirely devoted to the quantitative analysis of this correlation.

tion.

*No interpretation*

3- Dana mes *Notes à l'Académie des Sciences* de 1957-1959, et tout particulièrement dans mon mémoire d'ensemble de 1958, "*Doit-on reconsidérer les lois de la gravitation?*" I systematically abstained from any interpretation of the anomalies observed in the movement of the paraconic pendulum, for two reasons: firstly, because in my eyes, *what was essential were the facts observed; and secondly, because I wanted to steer clear of any pointless polemics on dogmas regarded as definitively untenable* in contemporary theories, and considered *intangible* by certain members of the Académie des Sciences <sup>3</sup>.

*Consistent anomalies*

4- In fact, the analysis of the observations of the paraconic pendulum with anisotropic support and isotropic support, the analysis of the sighting observations on sights that I associated with them in 1958, and that of the sighting observations on sights and collimators that followed them in 1959, the analysis of Esclanon's optical observations, and the analysis of Miller's interferometric observations, *all demonstrate the existence of a very remarkable coherence underlying all these observations*, and they *all* lead to the same conclusion:

*- The Earth's position on its trajectory can be determined by purely terrestrial experiments.*

They *all* lead equally to three guiding conceptions: the existence of a

*anisotropy of space,*

*the determination of this anisotropy of space by astronomical influences,*

*the existence of an intermediary medium, the material support for the transmission of these influences.*

f3) To avoid any difficulties with certain members of the Académie des Sciences who were very attached to Einstein's Relativity Theory, Albert Caquot never ceased to urge me to refrain from any interpretation (see in particular note 3 of § A 1.3 above, and § D.3 below; see also Section G of *Chapter 1* below).

It seems preferable to me today to free myself *completely* from any constraint whatsoever.

In-depth analysis of these five sets of observations does **not**, of course, allow us to **assert the intrinsic validity** of these three guiding conceptions, but it does allow us to assert that Motif *behaves as if* these three guiding conceptions *effectively* corresponded to a real-

In any case, *the validity, coherence and properties of the observations analyzed in the first three chapters of this book are totally independent of any hypothesis\* or theory whatsoever.*

*Only one certain conclusion*

5- Any theoretical interpretation of all five sets of observations *would be premature at the present time.* Numerous complementary experiments are obviously *necessary* before sufficient regularities emerge to lead to precise laws and a general theory.

*Only one certainty is currently emerging. Non-new phenomena have emerged that currently accepted theories are unable to explain.*

I have therefore limited myself in this work *to only the data from observation, systematically avoiding any theoretical synthesis* <sup>7</sup>

(4) See Chapter YC below.

(5) *En fait l'anisotropie de l'espace et son déterminisme sont des influences astronomiques et non des hypothèses. Elles sont des données observationnelles.*

(6) See A.1.1 above.

(D) The only exception I have made to this principle is to show that the observations of the paraconic pendulum can be explained by the existence of an anisotropy of space of inertia (see below, Chapter 5, § F.3, p. 206-212, and Chapter 6, Section I, p. 320-325).

*La Théorie de la Relativité*

6 - In reality, the reason why there has been so **much discussion and** so much passion about the *Theory of Restricted and General Relativity* is quite simple: a *fundamental error of judgment concerning the pre-*ndu négati ca* rattere of Michelson's experiment, and the failure to take into account Miller's observations of 1925-1926*. The result has been a kind of persistent misguidance in contemporary physics and the intolerant dogmatism that has accompanied it.

Nothing illustrates this kind of misguidance better than this judgment by Fénelon:

*"Most men's errors are not so much that they reason wrongly from true principles, but rather that they reason rightly from false principles or inaccurate judgments".*

*A golden rule*

7 - As for me, all my research and all my work *have been guided by one absolute conviction: that to be valid, all theories, whatever they may be, must be confirmed, both in their hypotheses and in their consequences, by the data available.*

This conviction is expressed by the maxim that has inspired me throughout my life in every field: *"Submission to the data of experience is the golden rule that dominates every scientific discipline"!*

f8) See below the C/inpifre YES, § A.4

(9) *Lettre de Fénelon, dite de Port-Royal*, for the education of the Duc de Chevreuse.

(10) Maurice Allais, 1989, en *Philosophie de ma Vie, Autoportraits*, Montchrestien, 1989, p. 70.

## 2.- OezzX decuerYes/ondamenfafes eç ouïzren "fzques

### *Two new phenomena*

1 - As I have already indicated 1, in my experiments from 1954 to 1960, I found *highly significant anomalies* in the movement of the paraconic pendulum with anisotropic and isotropic supports, and in the optical sights on test patterns. *I have not demonstrated their existence, independently of any perverse effects.* These anomalies are *totally inexplicable* within the framework of currently accepted theories.

In fact, the optical anomalies I highlighted in July 1958 show a striking connection with the anomalies of the anisotropic paraconic pendulum. *By the same token, a relationship has been established between two phenomena a priori quite foreign to each other, Optics and Nutromechanics*

*This is undoubtedly the discovery of two new phenomena, both unprecedented in literature, and whose implications have a major impact on the very foundations of contemporary theories.*

*These are two fundamental and authentic discoveries, made in 1958, and totally unknown to official science for thirty-eight years now!*

(1) § B.2.6 above, p. 50.

(2) Cfiopitre III, § B.3, p. 338, and B.6, p. 345, below.

(3) Perhaps some readers will accuse me of a lack of modesty here, but the fact that I can claim authorship of these two discoveries doesn't change their notoriety, and I don't really see why I should underestimate them, when their *co-pilot* importance is so obvious.

*For the past thirty years, official science has succeeded in maintaining a hotel-like silence on these two discoveries. It is in the interests of science itself to put an end to this silence.*

Ge really isn't unethical to claim authorship of discoveries. Didn't the great Ampère himself say

*One cannot deny the importance of these experiments, nor refuse to agree that No décou nerf e de l'oct ion de lo Terre cur les fils conducteurs m'opportient ouser compidtemment que celle de l'oct ion mm nelle de deux corps".*

(André-Marie Ampère, 1826, *Théorie mothématique des phénomènes dlectrody* - only deduced from experience, p. 103).

(4) See next page.

*The two crucial pendulum experiments of July 1958  
paraconic ô anisotropic support*

2 - In fact, *the two crucial experiments*, carried out *continuously and under identical conditions* for one month in July 1958, one in a basement at the Institut de Recherche de la Sidérurgie (IRSID) in Saint-Germain, the other in an underground quarry in Bougival, six kilometers away and with a fifty-seven meter cover, *radically swept aside all the objections presented earlier*, giving *the same results as to the existence of a lunar diurnal periodicity of 24 h. 50 mn. diurnal periodicity in the motion of the anisotropically supported paraconic pendulum, of an amplitude inexplicable within the framework of currently accepted theories.*

It is remarkable that during the same month of July 1958 in Saint-Germain, optical deviations of the North-South and South-North sights on two azimuth glasses revealed *the same periodic components of 24 h. 50 min. components*, and that these *are exactly in phase with those of the paraconic pendulum* in Saint-Germain and Bougival 5.

How can we fail to recall what André-Marie Ampère wrote in 1826 6

*"The periods when phenomena previously considered to be due to absolutely different causes have been reduced to a single principle have almost always been accompanied by the discovery of a large number of new facts, because a new way of conceiving causes suggests a multitude of experiments to be attempted and explanations to be verified.*

(4) For many, the fact that two fundamental discoveries were made "in one amateur" is *completely incomprehensible and, to put it mildly, impossible.*

For my detractors, it's beyond comprehension that essential phenomena could have escaped the sagacity of knowledgeable experimenters for decades. For them, these so-called discoveries can only be based on errors and illusions. There is therefore no reason to take them into consideration.

See § D.3 below.

(5) The validity and scope of these two discoveries are now *considerably reinforced* by analyses *based on the nonueoux colcuts présent4s* in this ouwage.

(6) André-Marie Ampère, 1826, 7/*Mathematical theory of electro-dy- phenomena namigues uni9ueezienî deduced from experience*, p. 118.

*Relatively large-scale effects*

3.- If one considers contemporary research, the considerable efforts deployed to highlight extremely small effects, and the *extremely complex and costly* equipment used, and if one compares them with my experiments on the paraconic pendulum and associated optical effects, one cannot fail to be struck by the very great simplicity of the experimental processes I used to study them, and the considerable relative quantitative importance of the effects observed, particularly for the paraconic pendulum, one cannot fail to be struck by the *very great simplicity* of the experimental processes I have used to study them, and *the considerable relative quantitative importance of the effects observed*, particularly for the paraconic pendulum, whose observed amplitude of the 24 h 50 min periodic component is *twenty to one hundred million times greater* than the amplitude calculated by the currently accepted theory of gravitation 7, Ü

*New perspectives*

4 - Like all new phenomena which, at a given moment, prove *inexplicable within the framework of accepted theories, and which controversially call them into question*, the anomalies of the paraconic pendulum and the optical anomalies I have highlighted open up *new perspectives* from a great many points of view, particularly with regard to the existence of *a time-varying anisotropy of space* 9

(7) Depending on whether we consider the paraconic pendulum with anisotropic support or the paraconic pendulum with isotropic support.  
See below, *Chapter I*, § B.2.1, p. 118, and *Chapter II*, § F.2. 2, p. 285.

(8) If we consider, for example, the advance of *d2 sexagesimal seconds per century* of Mercury's perihelion, whose explanation to within 5" is considered a great success of relativity theory (see *Chapter VII* below, § C.6.2), and if we consider the lunar influence of 24 h. 50 min. of *the order of 10<sup>5</sup> radians per second of time* on the paraconic pendulum (note 5 above), this corresponds to 6.51 billion sexagesimal seconds per century. The orders of magnitude *differ* greatly.

- $$D^* \text{ rad/sec} = 10' (.60 . 60) (100 . 365.25 . 24 . 60 . 60) \text{ seconds per century}$$

$$= 6.51 \cdot 10^9 \text{ seconds per century}$$

(9) Some may wonder why I waited so long to publish this book. The reasons are simple. In the soixante years, the hostility that was manifested was so *powerful*, the rumors about the invalidity of my experiments *so numerous and unstated*, some of them coming from personalities of great reputation and influence, that nothing could have enabled me to fight them. They *persist* to this day.

What's more, in the years following the end of my experiments, I was

completely absorbed by my economic work on monetary analysis, the theory of the capitalist optimum, surplus theory, the theory of random choice, and the theory of probability, all of which gave rise to numerous publications on my part (see Allais, 1989, *Autoportraits*, p. 121-144).



As Max Planck <sup>10</sup> pointed out:

*"Whenever there is a revision or transformation of a physical theory, we find that the starting point is almost always the observation of one or more facts which could not fit into the framework of the theory in its present form. facts are in fact always the keystone on which the stability of any theory depends, however important it may be.*

*For the theoretician truly worthy of the name, there's nothing more interesting than a fact that contradicts a theory hitherto held to be true, and that's when the real work begins.*

And as Henri Poincaré wrote <sup>11</sup>:

*"I imagine that his hypothesis had not been adopted lightly: it took into account all the known factors that seemed to play a part in the phenomenon. If verification doesn't take place, then there's something unexpected, something extraordinary: we're going to find the unknown and the new".*

(10) Max Planck, 1941, *Animations à la Physique*, Flammarion, p. 40.

(11) Henri Poincaré, 1906, *La Science et l'Hypothèse*, Flammarion, 1927, p. 178.

## D

## SOIMIENSFFOPPOSIMONS

• I owe a debt of gratitude to Emmanuel André-Martin, Pierre Ricard, Albert Caquot and René Dugas <sup>18</sup>, who have since passed away, and who placed their trust in me. and thanks to their support, from 1954 to 1960 I was able to continue my experiments on the paraconic pendulum and the optical sighting experiments on test sights that I associated with them.

It was *Em man nel Andrè -Mortin* who provided me with my first laboratory in January 1953 <sup>2</sup>.

I owe it to *Pierre Ricard* that in October 1953 I was able to set up a laboratory at the *Institut de Recherche de Loisiderurgie* in Saint-Germain with two colleagues, Jacques Bourgeot and Annie Rolland. It's thanks to them, and especially to Jacques Bourgeot, w h o s e efficiency, professionalism and sense of- sion, intelligence and dedication were exemplary, that my experiments could be carried out with full success <sup>4</sup>

(1) Emmanuel André-Martin (July 16, 1900 - June 23, 1978), Pierre Ricard (April 3, 1899 - 4 avril 1956), Albert Caquot (1 4-\* juillet 1881 - 27 novembre 1976), René Dugas (11 août 1897 - 15 June 1957).

(2) On the premises of Compagnie Clem ençon, 34 rue Milton, Paris, whose president was André-Martin **with** the very active and efficient assistance of Mr. Cou pry, Engineer with Compagnie Clemençon. See § B.1.4 above.

(3) At the time, Pierre Ricard was Chairman of Industries Métallurgiques et Minières. In May 1953 I telephoned Pierre Ricard to ask if he could help me to continue the experiments I had carried out at the Compagnie Clemençon with sufficient resources. His reply was <sup>4</sup> astonishing

*I have read our 1943 economics book. I consider it to be absolutely fundamental and comparable to Lagrange's "Métonique Analytique" fo r economics. 'Je fais nous donc con mnce. What do you nine-if?*

*"ñfof/ieureusement je pars demoin oux Etats -Unis, mois adressez nous de m'í part ou Directeur de l'nsfif ut de Recherche de lo lSidArurgie. He will do whatever is necessary".*

Ten days later, the decision was finally taken to assign me to IRSID, a large two-storey laboratory with two employees.

In all my life, I've never seen such decision-making capacity based on confidence in men.

(4) When I hired Jacques Bourgeot (who had already worked for several months at the Institut Géographique National) in October 1953, he was only 23 years old, but his exceptional abilities were confirmed in the years that followed.

My laboratory at IRSID operated from October 16, 1953 to June 30, 1960.

I am indebted to *Albert Caquot* for the presentation, *despite numerous and persistent oppositions*, of eight *Notes* to l'Académie des Sciences sur le pendule paraconique<sup>5</sup>. These *Notes* dealt respectively with the experimental set-up, the observation process and the harmonic analysis of the paraconic pendulum's movements, the effects of the total solar eclipse of June 30, **1954**, the theory of the paraconic pendulum and the lunisolar influence, the crucial new anisotropic-supported paraconic pendulum experiments in Bougival and Saint-Germain in July 1958, the periodic structure of the azimuths observed during these experiments, the experimental determination of the effects of the inclination of the bearing surface and the anisotropy of the support. My last communication was<sup>6</sup> that of February 9, **1959**<sup>6</sup>

I am indebted to *Zené Dugas*<sup>7</sup> for his constant and effective support of numerous personalities, and in particular of many members of the Académie des Sciences.

The untimely deaths in 1956 and 1957 of Pierre Ricard and René Dugas deprived me of two *vital* sources of support.

I am indebted to Joseph Kampé de Fdriet for having presented my *two Notes on harmonic analysis* to the Académie des Sciences, the first on the generalization of Schuster's test to the case of self-correlated time series, and the second on the application of this test to harmonic analysis.  
*rique des azimuts du pendule paraconique*<sup>8</sup>

(5J CRAS, November 13 and 25, 1957, December 4 and 16, 1957, November 3 and December 22)

1958, January 19 and February 9, 1959 (see above § B.2.7, p. 51).

(6) From that date onwards, a powerful cabal was launched against both my work in physics and my liberal positions in economics.

See below, Cf. *in* p. 225-230, and *Chapter X*, § B.2, p. 685-686. See also Louis Rougier, Juillet 1959, *Sciences de Polytechnique* (This text is reproduced in the Second Volume of this work in *Appendix I.D*, p. 31 above).

(7) René Dugas was a director at the SNCF, but he had one violin d'Ingres, the analysis of the foundations of Mechanics, and he published two fundamental works: *l'histoire de la Mécanique* (1950), and *La Mécanique au XVIIIème siècle* (1954), both with a preface by Louis de Broglie.

René Dumas made a prediction to me at the time: "Cf *errivero* un jour où notre pendulum *paraconique sera* *observatoires du monde*". However his prediction has so far failed to materialize.

(8) CRAS, May 13 and December 23, 1957 (see above § B.2.7, p. 51).

• It was thanks to the constant support of several members of the Académie des Sciences, including Albert Caquot, Donation Cot, Georges Darrieus, Joseph Kampé de Fériet, André Léauté, Albert Pérard, Maurice Roy, Pierre Tardi and René Thiry, and of Generals Paul Bergeron and Jean Guérin, successive presidents of the Comité d'Action Scientifique de la Défense Nationale, and to the impact of my Conference of February 22, 1958, that funds were granted to me by the *Comité d'Action Scientifique de la Défense Nationale* and by the *Centre National de la Recherche Scientifique*, and that it was thus possible to carry out *the two crucial experiments*, pursued continuously and under identical conditions, for one month in July 1958, one in a basement at *the Institut de Recherche de la Slidèrurgie* in Saint-Germain, the other in an underground quarry in Bougival, six kilometers apart and fifty-seven meters above ground\*.

*The full success of these experiments* was marked by *the 1959 Prix Galabert from the Société Française d'Astronautique*, and a 1959 award from the *Gravity Research Foundation!*

These experiments in July 1958 were accompanied at IRSID by sighting experiments on test patterns, which gave *quite decisive* results [1].

(9) See § C.2.2 above.

(10) See above t B.2.10.

(11) CAnpitre *IM*, Section B, p. 334-344 below.

In 1992 and 1993, thanks to my friend Guy Berthault, we were able to carry out *continuous* sighting tests on test patterns. An analysis is given in the second volume of this book, CAnpitre *III*, Section B (see p. 29 **above**).

It's thanks to Guy Berthault that we'll be able to publish the present volume and most of my physics work.

P. - Z-'inférét raised by rries *expêrierinez*

*From 1953 to 1956, I refrained from publishing anything, because I wanted to be absolutely sure of the reality of the new phenomenon I had highlighted: lunisolar effects whose amplitudes were totally different from those of the lunar effect. inexplicable within the framework of currently accepted theories.*

In 1956, I considered that I was in a position to publish most of my results. They were the subject of five communications to the Académie des Sciences by Albert Caquot and Joseph Kampé de Fériet <sup>1</sup>.

On February 22, 1958, I presented a summary of my results at the of my Conference, "*Faut-il reconsidérer les lois de la gravitation à la lumière d'une nouvelle expérience de mécanique*", chaired by Albert Caquot, in the Henri Poincaré amphitheatre at Ecole Polytechnique <sup>2</sup>

Between 1956 and 1958, one hundred and twenty-seven leading figures from the world of science, including more than fifty specialists in mechanics and geophysics, visited my laboratory in Saint Germain, then the one in Bougival. *None of them was able to come up with a valid explanation for the effects observed within the framework of accepted theories.*

In 1958 the Polytechnic journal *Perspectives X* published my comprehensive dissertation "*Doit-on Reconsidérer les Lois de la Gravitation ?*". The English translation was published in 1959 by the American journal *Aero-SPACE Engineering*, under the title "*Should the Laws of Gravitation be Reconsidered ?*" <sup>\*</sup>.

(1) See above, § B.2.7, p. 51, and below, § D.3.1, note 1, p.69.

(2) § B.2.9 above, p. 52

The amphitheatre was packed. Over 600 personalities attended my conference. It was widely reported in the press. See in particular: Pierre de Latil, *La pendule et les lois de la mécanique*, Le Figaro Littéraire, January 18 1968; René Sudre, *L'énigme de la gravitation*, Revue des Deux Mondes, February 1<sup>e</sup> 1958; Henri François, *Faut-il reconsidérer les lois de la gravitation?*, Le Monde, February 22, 1958; Pierre Devaux, *fonction scientifique à Polytechnique*, Le Figaro, February 25, 1958.

The main analyses are reproduced in Appendix C in the last volume of this book.

(3) Including fourteen members of the Académie des Sciences: Albert Caquot, Pierre Chévenard, Donatien Cot, Jean Coulomb, André Danion, Georges Darmon, Joseph Kampé de Fériet, André Léauté, Albert Pérard, Joseph Pérès, René Perrin, Maurice Roy, Pierre Tardi, René Thiry.

(4) See above § A.2.2, p.42.

*All in all, my experiments have aroused considerable interest on all sides, in France and abroad* 5

In fact, anyone who **has seriously analyzed my work** have never ceased to be struck by the consistency of my analyses and results  
 and by their scientific value 7,

(5) I received a huge amount of mail from all over the world.

For example, just recently, on August 1 -\* 1996, I received a letter from a physicist in Chinese S.W. Zhong of the Department of Physics, Huazhong University of Science and Technology, informing me of research and publications on the anomalies observed in mechanics, optics and atomic physics during the **solar eclipses of 1987, 1992 and 1995, undertaken** following *my observations of the anomalies of the paraconic pendulum during the eclipse of June 30, 195d*, and underlining my role as a pioneer in the field of research into gravitational anomalies.

(6) See in particular the judgment of the English physicist Robert Latham (note 3 of the § II.3 above, p. 39). See also the assessment made in May 1959 by General Paul Bergeron, Chairman of the Comité d'Action Scientifique de la Défense Nationale, in his letter to Werner von Braun, Cf. note f, § G.6, note 2, below, p. 231.

(7) Following the publication in September 1996 of my article "**Les expériences de Doyton C. Miller et la Théorie de la relativité**" in the Revue Polytechnicienne, *Lo Jaune et la Bouge*, one of my 1958 correspondents, Paul Ernest de Montaigne, a former student of the Ecole Polytechnique, wrote to me in the following terms

"-Foi suivi avec un très grand intérêt vos conférences sur le pendule parabolique.

"J'ai été très heureux de vous voir à la cérémonie de la Prim Nobel d'Economie. Je vous félicite pour la Prim Nobel de Physique. Vous êtes un grand scientifique, et comment! J'espère que vous ne serez pas oublié à l'avenir".

When I was awarded the 1988 Nobel Prize in Economics, Robert Latham, of Imperial College of Science and Technology, wrote to me

"Please accept my most hearty congratulations

"It is a pity, considering being what it is, if only you could get a similar recognition for the pendulum work. I know I am in a minority but my personal view is that it is just as important, and will be fully acknowledged as such in due course".

*A very strong and growing opposition*

1 - In fact, ever since the results of my experiments were published in 1956, I've constantly come up against "*established beliefs*" and dogma. *esloblishments*" of all kinds who dominate <sup>1</sup>.

Many objections kept coming my way, most of which were unfounded, or even totally unfounded, and some of which *were* based on *unspoken rumors* spread in the corridors, *all* too often by prominent figures, and to which it was quite impossible for me to respond. I asked, in vain, to be heard by a Commission of the Académie des Sciences. Nothing happened.

*A leaden silence masked the full success of the 1958 cruciole experiments and their significance* \*.

(1) My first two Notes to the Académie des Sciences, "*3foueuements pdrio- digues du pendule poroconique*" et "*Analyse harmonique des mouvements du pendule pnoconique*", presented by Albert Caquot in 1956 were initially et4 refused by the two Secrétaire perpétuels, R. Courier and Louis de Broglie (see their letter of November 20, 1956 to Albert Caquot reproduced in Anneze *Y.A of the Second Volume* of this work. See p. 31 above). These two Notes were not published until a year later, on November 13 and 25, 1957.

*Fortunately, opposition to the publication of all my Notes continued to mount at the Académie des Sciences.*

(2) In a recent book, "*L'univers de lo relatiuitt gntrale*", Editions Vues nouvelles, May 1996, Ecole Polytechnique alumnus Marcel Macaire writes (pp. 11 and 66-67):

*tL'onol yse de Maurice Allois) ou mit dm susciter un intérêt passionné turieusement, il n'y avait que très peu d'éthos et quelques critiques gntrolement hostiles. Yet his orgumentot ion étoif solide...*

*'Lo trifique qui a Ht forte - en 1958 - des résultats de Meurtre Allois est dpouruee d'objettuité*

Finally, for lack of funds, and *despite the resounding success of the crucial experiments in July 1958*, I had to close my laboratories at IRSID and Bougival and stop all my experimental research in June 1960.

*An urgent protest*

2- In fact, this resistance to new ideas, all the more violent for being more ignorant and incompetent, derives from an underlying assumption: *any theory, model, experiment or study that deviates from or contradicts established truths can only be wrong.*

(note 2 continued)

*What strikes you about Maurice Allais's work is the profusion of results that invalidate Newton's faith. Many experiments show anomalies. If there were only one, one could doubt it; but their number and repetition should have induced the scientific community to analyze the results and take them into account. However, their publication only aroused indifference. As I said, the arguments in support of the theses of Meurtre Allois are contradictory. The years - for example - confirm the periodicity of the curves representing the recession of the perihelion of Mercury, and in the general press, editors would be pleased with the novelty of the results obtained, equalling the arguments of their opponents. However, nothing of the sort happened, and in 1958, disinterest was shown in experiments that put into question theories that had been held for three centuries".*

(3) See below, *Chapter f*, § G.2, G.5 and G.6, pp. 2 15-216 and pp. 225-235; and *Chapter X*, § B.2, B.3, and B.4, p. 685-689.

See also *Chapter VIII, Des oppositions dogmatiques*, and *Appendices I.h and I. D* (see Contents above, p. 30 - 31).



This resistance, too often blind and but4e, to new ideas is certainly one of the greatest obstacles to the progress of science in all fields. Discoveries *in all eras have met with leneflque opposition from the mandarins of science* \*.

But whatever opposition I have encountered, and whatever obstacles of all kinds they have placed in my way, *they have never Jomnts rē used to prevent me and they never will prevent me* from defendingR: cR: who I pensSR: to be the verilē<sup>s</sup>.

(4) The most formidable and treacherous tactic against new ideas is the conspiracy of silence, against which no defense is possible.

If, in the end, and despite all the obstacles placed in its way, an idea triumphs, the person who first defended it is often depOutised of it, and William 'lames w a s' right to write (Allais, 1966, L'ñconomique en tent que *iscience*l

*Tome doctrine trouerse trois étots: on l'attaque d'abord, en la décloront absurde; puis on admet qu'elle est craie, évidente, mois insigni- toute. Then it's accepted that it's chalky, self-evident, but not insigni- all. It's retonnéd in(in so mérite ble importance and its ad uersoires reuendiquent mort l'honneur d e l'oooir découverte".*

(5) Nonetheless, I felt that *the* rejection by the Académie des Sciences of the publication of my *Hole of 23 février 1960* on the *purely experimental* results of my optical observations of July 1958 (see below *Chapter III*, § B.4), and the development of a veritable cabal against me, were very *harsh and unfair*.

which finally forced me to close my laboratory at IRSIDand, for lack of funds, <sup>7 juin 1960</sup> to cease all experimental research (see below).

**Chapitre I,** Section G, pp. 213-235, and *Chapter X*, § B.2 and B.3, pp. 685-689, and *Second Partie* of this work *Chapter VIII*, see above p. 30).

*Que officielle mandarins de lo science officielle pu participer à ceSte cobale n'en chonge unfortunately nothing lo n'itue. It only 'cit the oggroner. A c'ibole remains a c'ibole.*

This cabal was not limited to the Académie des Sciences. Louis Armand, president of the Ecole Polytechnique's Conseil de perfectionnement in 1958-1959, used rumors about the invalidity of my work on the paraconic pendulum to help defeat my bid for the Ecole Polytechnique's economics chair in 1959.

See *Troisième Introduction à mon Unité d'économie Pure*, § 3d, p. 124-126, and Louis Rougier, July 1959, "*Iscondole à Pol ytechnique*". Louis Router's memoir is reproduced in Appendix *I.D* in the *Second Volume of* this work (see p. 31 above).

I am fully aware of the risks I am taking by persisting in engage in a field that, by all official standards, is not mine. But **would** that **be** a major reason to **keep quiet** <sup>6 7</sup>

*In [ait, this book is an urgent protest against the entrenched prejudices and blind fanaticism of all those who oppose the progress of science with all their might. As Rabelais once wrote: "Ignorance is the mother of all evils".*

(6) In my *Conférence* of February 28, 1958, at the Ecole Polytechnique and before the crucial events of July 1958 I was already saying

*"Il est hors de doute d'il y a pour moi de très grands risques, étant un économiste, à faire des recherches de physique, et d'encourir le risque de me tromper. You can forgive a professional to be wrong, but you can't pardon someone who is not of his profession, to be wrong".*

As for whether I'm wrong, or whether it's my opponents who are wrong, only the strong can decide.

## E

## TO THE READER

7.- Zo récoçtôn of *this work**the two-volume presentation*

1.- On reflection, I felt it would be preferable to present this work, "*L'anisotropie de l'espace. la nécessaire révision de certains postulats des théories contemporaines*", in two volumes entitled respectively, "*Les données de l'expérience*" and "*Compléments expérimentaux et théoriques*", and to immediately publish the first volume <sup>1</sup>,

## The first volume

2- In what follows, I examine <sup>2</sup> :

My experiments with the paraconic pendulum on an anisotropic support (*Chapter I*),

My experiments with the paraconic pendulum on a n isotropic support (*Chapter II*),

My experiments on optical deviations in sighting sights in 1958, and their extension in 1959 (*Chapter III*),

Two sets of *highly significant* earlier experiments, those of Esclangon and Miller (*Chapter IV*),

The semi-annual and annual periodic structure of the analyzed observations and their interdependencies (*Chapter V*),

The anisotropy of space (*Chapter VI*),

Interpretation and scope of analysts' observations (*Chapter VII*),

A plan of simultaneous experiments to be carried out (*Chapter VIII*),

Incessant opposition to new ideas throughout history (*Chapter LX*),

- Finally, the new perspectives that are opening up today (*Chapter X*).

(1) The second volume won't be published for a few months yet.

(2) The present work owes a *great debt of gratitude* to my wife Jacqueline, whose suggestions and constructive criticism have always been extremely helpful.

At the end of *Part I*, you'll find all the references to the developments in this book\*, as well as an index of contents.

*The second volume*

3- In the *second volume* of this work, I will examine some of the *most essential* developments of the questions dealt with in the various chapters of *Part One*, which, for lack of space, could not be **analyzed** there.

In addition, I am enclosing in *Appendices* to this volume various *Memoranda* directly related to the developments of the ten *Chapters* of this *First Part*, as well as texts relating to the oppositions made to me and which I had to face (*Appendices I*) and the Notes prepared for the Academy of Sciences and which could not be published in the *Comptes Rendus (Appendices II)* <sup>4</sup>.

*Unavoidable difficulties*

4- Some passages in this book are *highly technical*. As far as possible, I have avoided any mathematical formalism by rejecting in principle all developments involving mathematical developments in the *Second Volume* of this book.

But certain questions, such as the presentation of the principles of the theoretical calculation of the lunisolar influence on the motion of the paraconic pendulum according to the current theory of gravitation, are *so important* that I felt it necessary to include them in my presentation.

The text also contains a large number of notes and cross-references, which may present a few difficulties for the reader; but above all, I wanted my developments to be rigorous and unambiguous.

(3) I particularly recommend that readers always refer to our *leader originou*, et non *pos aun: commentires de seconde main*.

(4) See *Sommire* above, p. 28-33.

(5) See in particular *Cfopiire f*, § B.2, and F.3.2, pp. 118-129, and 206-212; and *Chapter II*, Section I, p. 320-325.

**Finally**, the following text contains a few repetitions. These were *unavoidable in this case*, since the presentation in each chapter is in itself a whole, linked to the presentations in the other chapters, and all the questions studied are linked to each other by numerous and relatively complex re- lations.

*Quant Platines analyses*

5- A large number of calculations carried out *since January 1995* have complemented my 1954-1960 analyses. All have *fully confirmed* the high degree of coherence underlying all the observations made between 1954 and 1960, and have clarified their significance and port4e. In fact, these observa- tions are *completely inexplicable* within the framework of currently accepted theories.

In any case, *the quantitative analyses* presented in the first five Chapters of this *Part One* are *totally independent of any hypothesis or theory whatsoever*.

*Editing is all about rigor and precision.*

6- As it stands, and despite the many successive versions it underwent during the period from July 1995 to February 1997, this work is certainly *highly imperfect*. The drafting process was fraught with difficulties, due to the complexity of the issues involved.

and the need to limit the number of presentations ,6 were only They partially overcome.

A single principle has guided me "*Sacrifice everything to rigor and clarity*."

(6) I also had to track down all the materials I needed to write the story. tion of this book. After more than thirty-five years, it hasn't always been4 easy.

(' ) I'd like to take this opportunity to extend my warmest thanks to Anne-Marie and Alain Villemur, exceptional collaborators. Anne-Marie Villemur presented the successive versions of this book with remarkable efficiency. Alain Villemur ezécut4 with great intelligence the numerous calculations and graphs corresponding to the quantitative analyses.

• There's no doubt that the analyses in this book, which run *totally counter to today's "established truths"*, will provoke violent reactions. It's hard to believe that the economist I am could, through his experiences and Bee analyses, defeat the physical theories taught everywhere as definitive truths.

Against the fanatics, I remain convinced, as I was in 1959, that there is nothing to be done. Blind and deaf, stubborn in their certainties, they will deny everything en bloc <sup>1</sup>. But today, as in 1958 <sup>2</sup>, there are honest men, ready to examine the facts, even if they may be wrong. appear, at first glance, to be opposed to their own convictions.

I've been told that my claim to two fundamental discoveries is bound to exasperate some readers. Admittedly, such a warning is not without value; it is indisputable. But, once again, it is not by underestimating the significance of the new phenomena I have highlighted that it would be possible to really alert the scientific world, or at least that part of it which is not blinded by prejudice, b i a s and blind faith in established truths.

There can be no doubt that the *undisputed* existence of the paraconic pendulum and optical anomalies I have highlighted, the implications of Esclangon's optical observations, and those of Miller's interferometric observations, are likely to lead to a profound revision of the very foundations of current theories.

(1) More likely, they will try to remain silent. In *Chapter X*, I'll give a particularly significant recent example of this, relating to the quantitative analysis of Miller's observations present4e in *Chopifre IV* below.

f2) In 1960, at least nine members of the Académie des Sciences (Caquot, Cot, Darrieus, Kampè de Fériet, L4autd, Pèrard, Roy, Tardi, Thiry) were convinced of the need to continue my experiments, the results of which they considered absolutely essential, and they made every effort to ensure that I was granted the necessary resources.

(3) Implications largely *unnoticed* by these two authors.

This is a certainty, but as in the 1950s, it will come up against opposition as blind as it is relentless from all those who base their thinking solely on established truths.

- At first sight, some of my judgements in this Introduction and the following Chapters may appear somewhat excessive. But what is really excessive, and indeed inadmissible, is the kind of indifference with which the observations of the paraconic pendulum movement, the optical observations I have associated with them, and Miller's interferometric observations have been ignored and buried\*. As Bouasse once said:

*"One would not be obliged to use such harsh terms if one were speaking to pure minds; but naked and de-charmed truths touch little and leave in the brain only light traces that are easily erased... the great advantage of the hard way is to force people to think".*

May I paraphrase here what Alexis de Tocqueville once wrote in an entirely different context:

*"I hope I have written the present book without prejudice, but I do not pretend to have written it without passion.*

*"Whenever I have encountered manifest errors in accepted theories or recognized facts, I have taken care to shed light on them, so that when we see the obstacles opposed to the progress of science, we can better understand their nature.*

*"To achieve this goal, I have not feared, I confess, to hurt anyone, neither individuals, nor opinions ... however respectable they may be. I have often done so with regret, but always without remorse. May those to whom I might have thus offended forgive me, in consideration of the unselfish and honest aim I pursue".*

*The motto /ondnmenfnf of my approach is to express what I believe to be vérité.*

Saint-Cloud, February 15, 1997

(4) See below *Chapter f*, Section G, pp. 213-235; *Chapter X*, § B.2, B.3, and B.4, pp. 685-689; and in the *Second volume* of this work, *Chopitre VIII*, and Appendices IA è ID (see Sonimnire above, pp. 30-31).

***Message aux lecteurs***

*I'd be very grateful if readers of this publication could send me their comments.*

*Thank you very much in advance.*

Maurice Allais  
15, rue des Gâte-  
Ceps 92210 - Saint-  
Cloud



**MI s & e x æ w C E S s U R U & P E N D u c P A R A C O N I Q U E  
A S U P P O R T A N I S O T R O P E**

19M - 1SKI

*The important facts are the crucial ones, i.e. those that can confirm or refute a theory. After that, if the results are not as expected, the learned orators don't feel a sense of embarrassment, which they are eager to shake off with the help of nudges; on the contrary, they feel their curiosity unquenched; they know that their efforts, their momentary discomfort, are going to pay off a hundredfold, because the truth is there, nearby, still hidden and adorned, as it were, with the allure of mystery, but on the verge of unravelling.*

Henri Poincaré\*

My experiences with the isotropic pendulum support<sup>1</sup> were pursued from 1954 to 1960. They gave rise to ten Notes to the Académie des Sciences in 1957, 1958 and 1959 and to an overall presentation in 1958, "*Do We Need to Reconsider the Laws of Gravitation?*"

In addition, I have written a large number of papers, the references of which are given at the end of this book, and which will be published in a forthcoming volume\*.

My work was the subject of three Lectures organized by the *Cercle Alexandre Dufour*: "*Faut-il reconsidérer les lois de la Gravitation à la lumière d'une nouvelle expérience de Mécanique*", February 22, 1958; "*Faut-il reconsidérer les lois de la Gravitation? Nouveaux résultats, bilan et perspectives*", November 7, 1959; and "*Les périodicités constatées dans le mouvement du pendule paraconique sont-elles réelles ou non à la généralisation du test de Ischuster au cas de séries temporelles autocorrélées*", March 18, 1967.

In view of the very conception of the present work, the presentation of the following discussion of anisotropic paraconic pendulum anomalies will necessarily be confined to the essentials<sup>6</sup>.

\* Henri Poincaré, 1913, *Dernières Pensées*, Flammarion, p.336.

(1) My 1959-1960 experiments on the isotropic paraconic pendulum are examined in *Chapter II* below.

(2) See above, *Introduction*, § B.2.7, p. 51.

(3) See above, *Introduction*, § A.2.2, p. 42.

(4) See above, *Introduction*, § A.2.3, p. 42.

(5) See above, *Introduction*, § B.2.9., p. 52.

(6) All useful supplements are presented in the *Second Volume* of this book (see introduction § E.1.3 above, p. 74).

**CARACIESESGENNRAUKDESEXPEWENCES**

BURU PENDuunPARACO:wqiæasuePORT

PE

**WIDELFURSRESULTAI***Pendufe utifisé*

1- Although I have successively used various types of pendulum, I will limit myself here to a very brief description of the device most commonly used 1,

The photographs opposite show the entire device, pendulum and supply 2.

The pendulum was a *dissymmetrical* pendulum consisting of a vertical bronze disk weighing 7.5 kg, 21.8 cm in diameter, fixed to a bronze rod suspended from a bronze stirrup E resting on a steel ball 6.5 mm in diameter, capable of rolling in any direction on a flat horizontal surface S.

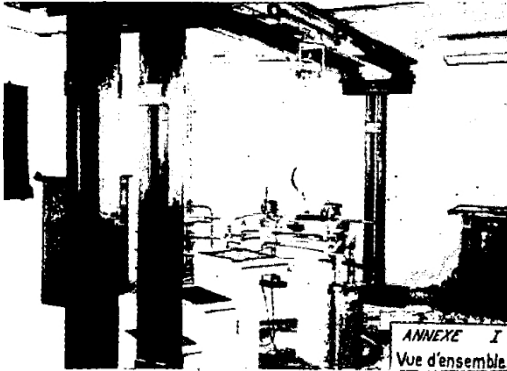
This surface in turn rested on a 4.5 cm thick, circular, hollowed-out S' aluminum support with an A appendage. The recess allowed the pendulum to rotate through a total angle of 210 degrees. This support S' was supported by three screws mi-  
V attached to a support S'' bolted to a beam, itself clamped to the ceiling by a system of joists ,<sup>3</sup>

(1) This pendulum was notably used in the monthly observation series of November-December 195d, June-July 1955, July 1958, November-December 1959, and March-April 1960.

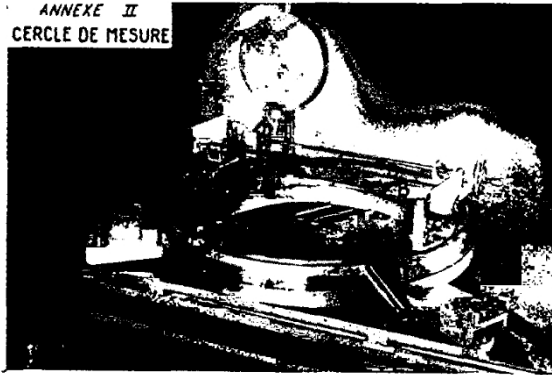
(2) These four photos **grapnies sc** h' have reproductions of nnzes I to IV of my Note from November 13, 1957 **Académie** des Sciences "Obseroement des mouoementz du pen-  
è dule poroconique

In addition, I've reproduced (p. 83) a photo of my laboratory head making an observation (taken in 1958 by Georges Lacoste).

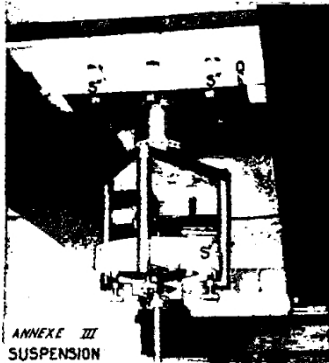
f3) The direction of these beams is indicated o n *Appendix i* by the vector PQ . This vector is perpendicular to the beam supporting the pendulum.



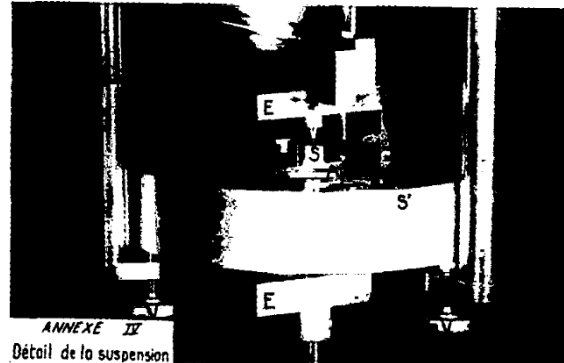
ANNEXE I  
Vue d'ensemble



ANNEXE II  
CERCLE DE MESURE



ANNEXE III  
SUSPENSION



ANNEXE IV  
Détail de la suspension

Source: Noie du 13 novembre 1957è l'Académie des Sciences "Obserrotrion des mouv emenlz du pendulep or n coni9ue".

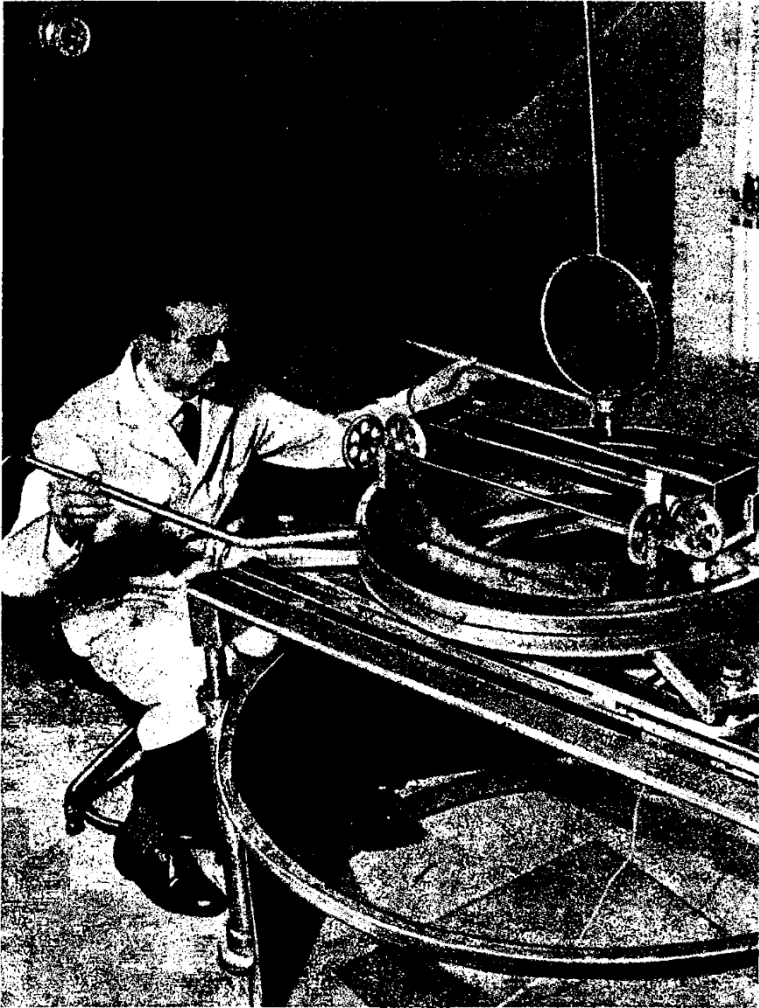


Photo of my lab manager Jacques  
Bourgeot

Georges Lacoste, *progress in our physical knowledge of the fields around the drowning*  
*à de passionnelles recherches*, Sciences et Avenir, n° 135, May 1958, p. 272.

With the pendulum rod and stirrup weighing 4.5 kg, the total weight of the pendulum was 12 kg, and the length of the equivalent single pendulum was around 83 cm\*.

The steel balls were high-precision **S.K.F.** balls, and the bearing surfaces were **tungsten** carbide and cobalt. Because it was suspended by a ball, I called it a *paraconic* pendulum.

### *Experimental process*

2- The experiments took place in the basement where my laboratory was located, and the pendulum's center of gravity moved about 1.50 m below the natural ground surface.

The pendulum was released every 20 minutes with an initial amplitude of about 0.11 radian from a rest position by burning a wire. The pendulum's movement was observed for around 14 minutes, aiming at the tip of a needle located at its lower end and displaced 105 cm<sup>6</sup>.

In general, the point described a curve similar to a flattened ellipse, whose major axis was observed with a sighting system placed on a circle C, centered on the axis of the pendulum at rest and bearing a grade division and a vernier. This system made it possible to determine the azimuth of the recti oscillation plane with *an accuracy of the order of a tenth of a grade*.

(4) The corresponding pendulum period  $T = 2\pi \sqrt{\frac{I}{mgl}}$  was 1.828 seconds.

The moment of inertia B of the pendulum with respect to an axis passing through the center of the ball and perpendicular to the disk was 83.11.10 and the moment of inertia A with respect to the axis passing through the center of the ball and parallel to the disk was 82.89.10 in.

unit4s CGS. The coefficient of dissymmetry  $b = \frac{2(B-A)}{(B+A)}$  was thus  $0.269 \cdot 10^{-2}$ . The moment of inertia C of the pendulum about its vertical axis was  $270 \cdot 10^{-4}$ . Its coefficient of gyrostotification  $= \frac{2C}{(A+B)}$  was  $y = 0.325 \cdot 10^{-2}$

(5) The amplitude chosen corresponded to the maximum value to avoid any slippage of the ball on surface S.

In a 14-minute experiment, the amplitude increased from 11 cm to 11.5 cm.

(6) This distance  $l' = 105$  cm is naturally different from the length  $l = 83$  cm of the equivalent pendulum.

In addition, a system of two movable parallel bars B, which could be moved in relation to the reading circle, made it possible to measure the two axes of the ellipse in cm and to determine the azimuth of the disk plane, i.e. of the pendulum's central inertial trihedron.

After 14 minutes, the pendulum was stopped. Six minutes later, it was released again *in the plane of the last azimuth observed*. The series of azimuth observations were thus *chained together*, with successive releases every twenty minutes, day and night. Each 24-hour period thus comprised 72 series of chained observations <sup>7</sup>.

To avoid any systematic influence, the ball supporting the pendulum was changed every 20 min in *each experiment*, and the surface S was changed at the start of each week of observation.

#### *Anisotropy of the substrate*

3- The "S" support was characterized by a very noticeable difference in its elasticity in two rectangular planes. Due to this anisotropy of the support, the plane of oscillation was located in a plane of direction Z perpendicular to the beam, the azimuth of which was approximately 171 grades, counting azimuths from North in the direct direction.

The result was a *tendency to form ellipses* when the pendulum was released in a different plane.

(7)\*\* A monthly 30-day observation series corresponded to 2160 20-minute - experiments.

(8) This Z direction is parallel to vector PQ (note 3 above).

(9) This influence of the support has been precisely determined by flooring experiments in different azimuths, eliminating the influence of time by a random choice of starting azimuths (see § E.3 below).

*Continuous embedded observations*

4- During a series of continuous observations, day and night, the observers took turns averaging every 3 hours <sup>10</sup>

To my knowledge, *this is the only example in the literature of observations of continuous moni re for durations of the order of months!*

(10) The average number of observers was seven. In the July 1958 simulated experiments at Bougival and Saint-Germain (Section C below), the total number of observers was fourteen.

These observers were IRSID technicians working overtime. Their conscientiousness was remarkable.

(11) The Eselangon experiments (Cfopitre iY, § B.2, below) continued for around a year, but averaged only around 15 observations a month.

Miller's experiments continued continuously, but only for 6 or 8 days at four different times of the year (see Chapter IV, § C.3, below).

f12) Some experiments were carried out in 1954 using a long pendulum suspended by a gr ee wire from a circular aperture about one metre in diameter between the two superimposed rooms in the laboratory (note 6 in § B.1.5 of the above Introduction).



## 2.- *Observations enchainées - Illustration dans le cas de la série mensuelle de Juin-Juillet 1955*

In the course of a *continuous series of observations*, the tendency of the plane of oscillation was not to settle in the vicinity of the Z direction of the support's anisotropy, and the variation of its azimuth as a function of time showed itself to be a highly irregular oscillation, at least at first sight.

So, during the continuous observation session of June 7 at 12 noon. U.T. at July 7, 12 h. U.T. of 1955, the variations observed were considerable\*. Over the same 24-hour period, azimuth variations were

sometimes reached and exceeded 100 grades. The average azimuth was 150 grades, 21 grades below the Z anisotropy azimuth of 171 grades, azimuths being counted *in the direct direction from North*.

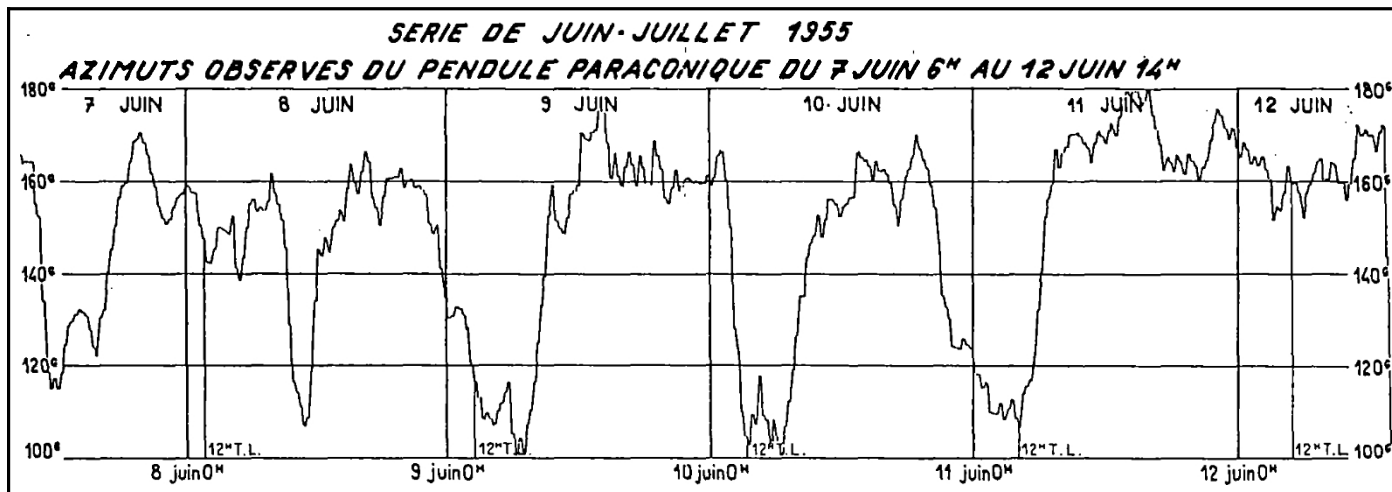
*Graph J* shows the chained azimuth observations of the June 7 12 h. to June 12 14 h. The times of the Moon's transits at midday are indicated by 12 o'clock TL<sup>2</sup>. *Graph II* represents the azimuth variations for the entire period from June 7, 12 p.m. to July 7, 12 p.m. . Similar azimuth variations were observed in the other series of monthly observations\*.

(1) \* In all experiments, the time considered is *Universal Time* (U.T.).

(2) The indication 12 h. TL means 12 hours in lunar time.

(3) All graphics drawn from 1954 to 1960 are reproduced by photography. *without any change*.

(4) For illustration, see GrepAiqe XXff corresponding to the monthly series of chained observations from Bougival in July 1958 (§ C.2.4 below).

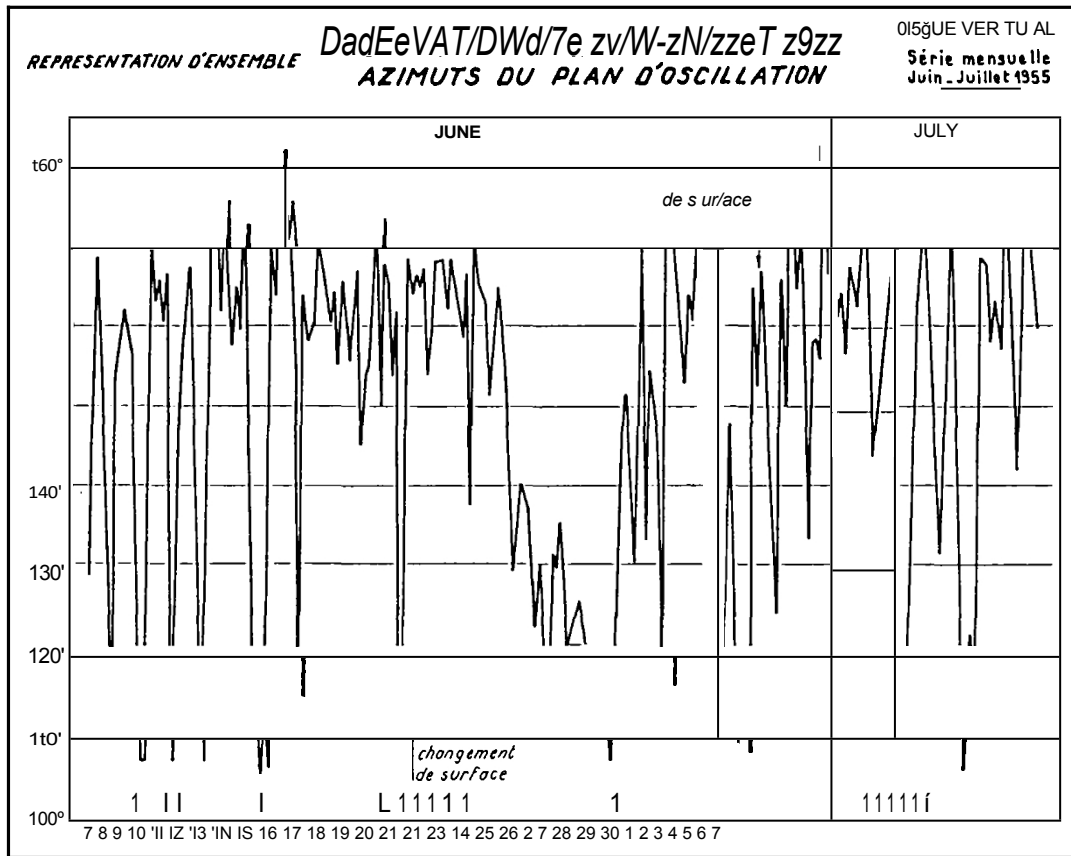


**Légende :** Angles are counted in grades from North in the direct direction.

The 100-grade azimuth corresponds 1 to the direction perpendicular to the meridian. The 200-grade azimuth corresponds to the meridian.

12h T.L.: time when the Moon crosses the meridian.

*Sources :* Note b l'Académie des Sciences du 18 novembre 1957, "Analyse l'inrmonique des mouvements du pendule paraconique" i eL *Grophique III A* de ma Conférence du 22 février 1958.



I.A.2

*Legend:* Angles are counted in degrees in a direct direction from North. Azimuth 100 grades corresponds to the direction perpendicular to the meridian. Azimuth 200 grades corresponds to the meridian.

*Source:* Graph II B 3 of my Conference of February 22, 1958.

### ***réalisées 1954-1960 3.- Les expériences***

From 1954 to 1960, a large number of experiments were carried out either for B periods of one month, or for shorter periods, with different types of pendulum. Below I confine myself to one-month and two-week series.

#### *General characteristics of the seven observation series monthly incùef nées*

1- From 1954 to 1960, *seven series of continuous monthly observations* of the asymmetrical paraconic pendulum with anisotropic support were carried out.

The T'oöfeou *I* opposite shows for each monthly series the duration in days, the mean azimuth  $\bar{M}$ , the minimum azimuth  $M_{\min}$ , maximum azimuth

$M_{\max}$ , their mean  $(M_{\min} + M_{\max}) / 2$ , the ratio  $(M_{\max} - M_{\min}) / 2$ , the total azimuth variation  $D = M_{\max} - M_{\min}$ , the amplitude  $2R_{24}$  of the 24 h wave, the amplitude  $2R_{25}$  of the 25 h wave, the ratio  $2R_{25} / 2R_{24}$ , and the ratios

$D / 2R_{24}$  and  $D / 2R_{25}$ . These values are presented in both *grades and degrees*.  
*sandstone 2t 3*

The total azimuth variation always remains below 166 grades, *due* to the *support's* anisotropy. In fact, the average azimuth of 164 grades is relatively close to the support's anisotropy direction of 171 grades.

The pendulums used during the series of chained monthly observations were always identical to those used for the June-July series.

(1) On the 24 h. and 25 h. diurnal waves, see § A5 below, p. 96-101.

(2) The graphs in *Chapter Y*, Section B are presented in degrees to make it easier to and their comparison with representative graphs from Miller's experiments.

(3) In view of the *limited* computing resources available at the time, the largest part of the calculations were carried out using a period of 25 h. instead of 24 h. thus avoiding the need to interpolate observations from 10 mn to 10 mn when applying the Buys-Ballot filter (on the Buys-Ballot filter, see § 5, note 1, p. 96).

1955\* except for the June-July 1954 series, where the pendulum consisted of one vertical and two horizontal bronze disks <sup>5</sup>,

As can be seen from *Table I*, the amplitudes of the 24 h. and 25 h. periodic components are *relatively* much larger for the November-December **1954 and** June-July **1955** series than for the November-December 1954 and June-July 1955 series.

for all other series. It was only *recently*, in 1995, that I was able to give a plausible explanation <sup>6</sup>,

#### *Observation series chained over two weeks*

2- Two further series of two-week observations with the bronze disc were carried out from March 18 to April 2, **1955 and** from June 14 to June 30, 1958 in Saint-Germain and Bougival.

In addition, from September 21 to October 6, 1955, a continuous series of chained observations was made with a symmetrical pendulum (A = B) **consisting of a 12.2 kg** lead sphere. Azimuth **variations** were quite comparable to those corresponding to the asymmetrical pendulum constituted by the bronze disk <sup>7</sup>.

#### *Presentation of observations*

3- The azimuths observed at the end of each 14-minute experiment were presented in large tables, with each column corresponding to a given day.

(4) § A1 above.

(5) Its total weight was 19.8 kgs. After the series of experiments, **Ides july** ri-july 1954 lightened the pendulum to reduce the disruptive influence of aous **billes** (see below) § E.4).

(6) See *Chapter Y* below, § B.2 (7) §

A. 1 above.

(8) These Tables will be published in the Second Volume of this work, Cùopi - tre i, Section A (see p. 28f. above).

DISSYMMETRICAL PARACONIC PENDULUM WITH ANISOTROPIC  
SUPPORT CHAINED MONTHLY EXPERIMENTS 1954 - 1960

Azimuths and periodic components of 24 and 25 hours in grades and degrees

$X = \text{Azimuth of support onisotropy} - 171.16 \text{ grodes} - 154.04 \text{ degrees}$

PENDUL PARA  
QUE A SUPP  
TROO <

Periods	Duration in days (1)	Average date (2)	$\bar{\phi}$ (3)	$\phi_m$ (4)	$\phi_M$ (5)	$\frac{\phi_m + \phi_M}{2}$ (6)	$\frac{\phi_m + \phi_M}{2}$ (7)	$D = \phi_M - \phi_m$ (g)	$\frac{D}{2}$ (9)	$\frac{D}{2}$ (10)	'12s' R2g (11)	$\frac{4}{D}$ (12)	is D (13)
1 1954 June 9 - July 9	@	174,5	164 (148)	102 (92)	268 (241)	185 (166)	1,13	166 (149)	2,0 (1,8)	3,2 (2,9)	1,58	0,012	0,019
2 1954 Nov. 16 - Dec. 22	36	337,5	161 (145)	5g (84)	253 (228)	173 (156)	1,08	160 (144)	10,3 (9,3)	12,9 (11,6)	1,25	0,064	0,080
3 1955 June 7 - July 7	@	537,8	150 (135)	g9 (89)	180 (162)	140 (126)	0,93	81 (73)	11,7 (10,5)	14,0 (12,6)	1,20	0,129	0,155
4 1958 B July 2 - August 1	&	1658,5	161 (II)	145 (130)	177 (159)	161 (145)	1,00	32 (&)	1,4 (1,3)	2,2 (2,0)	1,60	0,044	0,068
5 1958 July 2 - August 1	If	1658,5	164 (148)	141 (127)	187 (168)	164 (148)	1,00	46 (41)	0,8 (0,7)	2,1 (1,9)	2,71	0,017	0,045
6 1959 Nov. 20 - Dec. 15	CE	2161,75	171 (154)	142 (128)	200 (180)	171 (154)	1,00	58 (52)	2,5 (2,3)	1,3 (1,2)	0,54	0,043	0,023
7 1960 March 16 - April 16	31	2282	174 (157)	150 (135)	206 (18S)	178 (160)	1,02	56 (50)	1,8 (1,6)	1,5 (1,4)	0,84	0,032	0,027
Averages			164 (148)	125 (112)	210 (189)	167 (150)	1,02	86 (77)	4,4 (4,0)	5,3 (4,8)	1,39	0,049	0,060

Norm 1 - All experiments, with the exception of Experiment 4 at Bougival, took place at IRSI D in Saint-Germain  
 2 - All measurements are given in *grodes*. Angles are counted from North *in the direct direction*. Measurements in *degrees* are indicated in brackets.  
 3 - The average date of each monthly series is counted in *days from*  $\wedge$  *Jonaier 1954*.  
 4. -  $\phi_p$  and  $\phi_M$  denote the minimum and maximum values of the oscillation plane azimuth.  $\phi$  represents the mean values of the azimuths  $\phi$ .

#### 4.- Effet de Foucault

In fact, it is *particularly significant* that in chained series, the tangent at the start of the mean curve of the different azimuth curves corresponding to the series of elementary observations of 14 minutes *corresponds exactly to the Foucault<sup>1</sup> effect.*

*Graph III* shows the azimuth displacements of the plane of oscillation and the inertial trihedron for the lead sphere (symmetrical pendulum) during the series of observations from December 7 to 13, **1955, and** for the bronze disk (asymmetrical pendulum) during the series of observations from December 7 to 13, 1955. series of January 4, 1956<sup>2</sup>.

*In both cases,* the average azimuth of the oscillation plane corresponds *exactly* to Foucault's movement. This is only different when the minor axis of the ellipse has a significant value.

*Graphs IV* show the average azimuths of the oscillation plane and the central inertial trihedron for the lead sphere from September 21 to October 5, 1955, from 0 to 12 h., from 12 h. to 24 h., and from 0 to 24 h. Here again, on average, we initially observe the Foucault effect. It disappears with the appearance of ellipses<sup>3</sup>.

In general, the azimuthal movement of the central inertia trihedron is different from that of the plane of oscillation. In the case of the asymmetrical pendulum, we *demonstrate, and experience confirms,* that the plane of the disk tends to merge with the pendulum's plane of oscillation\*.

(1) At Saint-Germain, the angular rotation of the Foucault effect -  $m \sin L$  is - 0.55.10 radian/sec., which in 14 minutes corresponds to an angular displacement of

$$- 0.55 \cdot 1060 \cdot 14 = - 2.94 \text{ grades in 14 minutes}$$

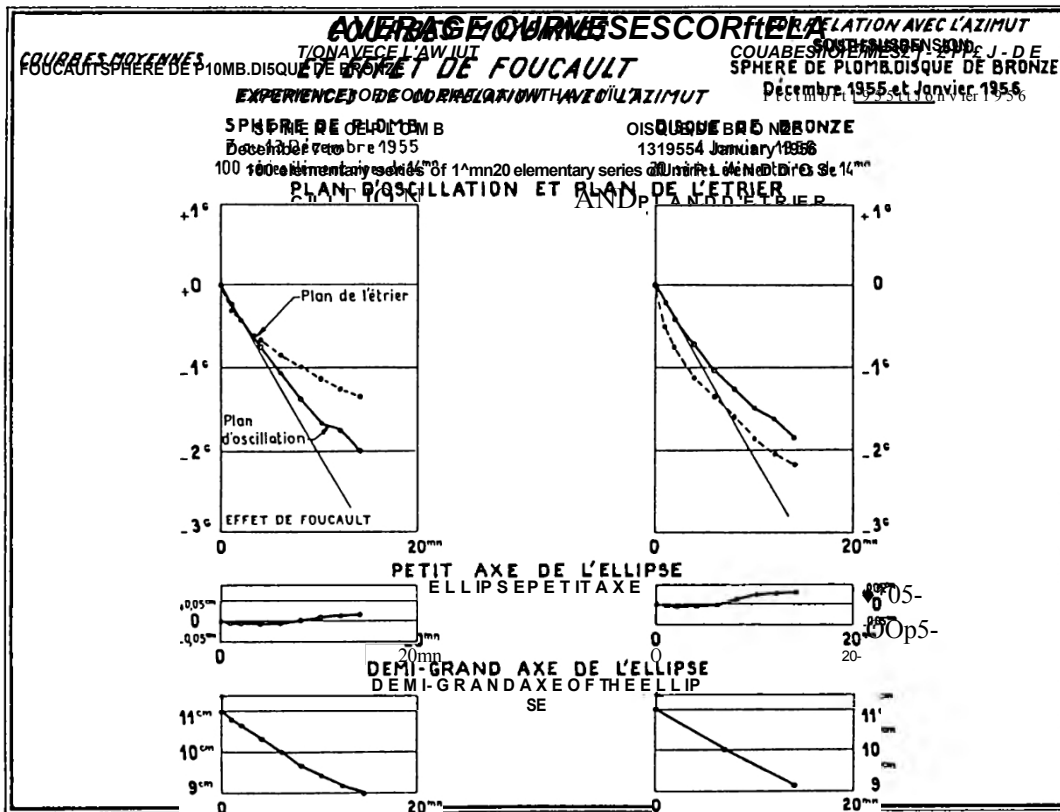
(2) Choosing the lead sphere corresponding to a symmetrical pendulum ( $B = A$ ) has the advantage of better demonstrating the movement of the inertial trihedron than in the case of the asymmetrical pendulum ( $B \neq A$ ) made up of a disk.

(3) *Trophies III and IV* show that over the course of a 14-minute experiment, the amplitude decreases from 11 cm to around 9 cm, i.e., for  $l' = 105$  cm, from around 0.105 to around 0.086 radians, with an average value of around 0.10 radians.

(4) A detailed analysis of the motion of the support and trope asymmetrical parabolic pendulum is presented in the Second Volume of this book, *Chapter I, Section*

B (see above, p. 28).





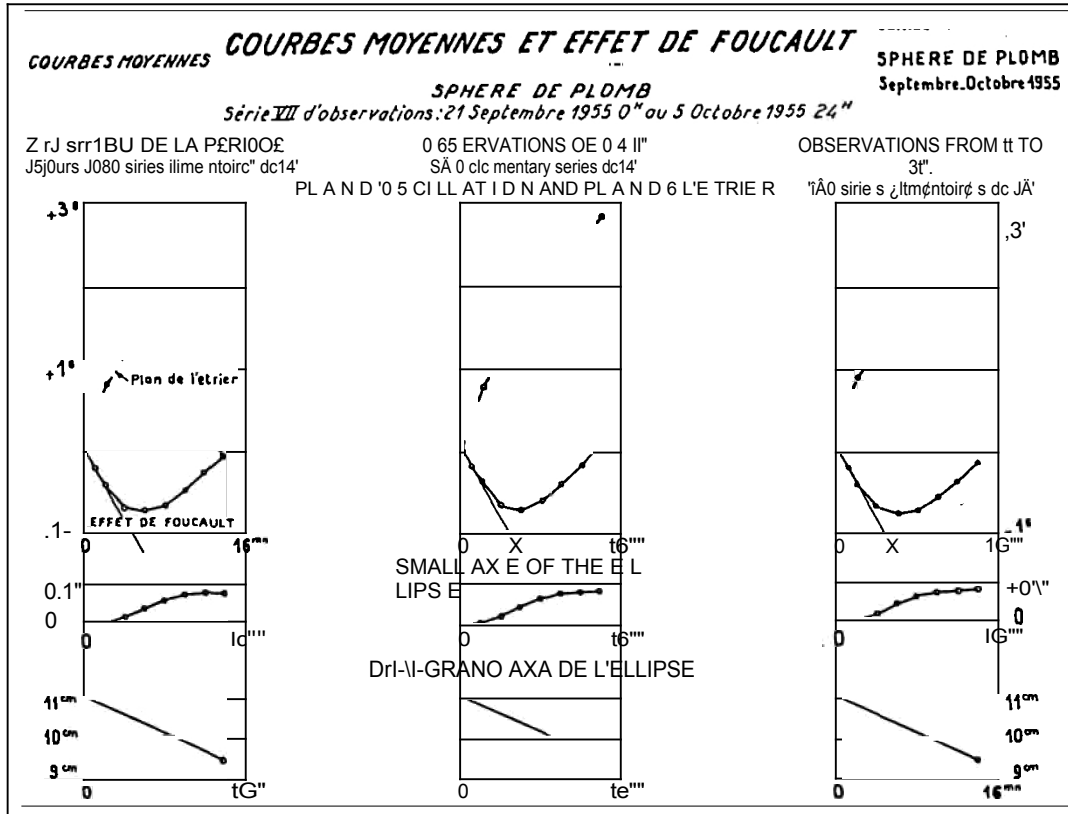
Légende : ●—● azimut du plan d'oscillation

---● azimut du trièdre principal d'inertie

Islande : Graphique III A of the Conference du 22 février 1958.

azimuth of the main inertia trihedron

*Source: Graph III A 2 from my Conference of February 22, 1958.*



Legend: — azimuth of the oscillation plane - - - azimuth of the main inertia trihedron

Source: Graph III n° 1 of my Conférence of February 22, 1958.

S.- Zo afructurepériodi9ue fiere 'leo obeeruofioria encfiotnées de penzfuJe

*he harmonic analysis of enchanted observations*

1- The sequence of azimuths of the oscillation plane of the anisotropically supported paraconic pendulum, observed over a series of chained observations, forms a time series that can be analyzed by various means: graphical representation, harmonic analysis (Buys-Ballot filter, Darwin or least-squares wave group fitting, pdriodogram and correlogram), re-presentation by autordgressive diagrams.

**The results obtained can be assessed according to three criteria:**

- the probability of obtaining an amplitude by chance greater than a given value for a given harmonic component <sup>2</sup>

(1) Because of its convenience, and given the very limited computational resources at our disposal, we made extensive use of the Buys-Ballot method at the time.

The principle behind this method is essentially as follows

Consider a series of  $N = pq$  values  $x_j$  where  $q$  is the period under consideration. The observations are arranged in a table of  $p$  rows, each containing  $q$  successive values, and the averages are calculated for each column.

$x_{q+1}$	$x_{q+2}$	$x_{2q}$
$pp-1$	$q+1pp-1)q+2$	$x_{pq}$
Averages :	$x_j$	$x_q$

The trend may be eliminated by considering a  $(q + 1)^*$ -column whose mean is  $x_{qqj}$ . The trend is defined by the ratio  $Cx_{qq} - x_q / q$ .

(2) See § B.1.3. below, and *Part II* of this work, Chapter VI.

the concordance of the phases for the original series and for the sum of its periodic components for each of the two series of 15 days into which a series of 30 can be decomposed **days** <sup>3</sup> ;

The quality of the fits is characterized by a low dispersion of the points around the fitting sinusoid.

All these analyses applied to various series of observations have shown that the series of observations obtained present a remarkable periodic structure, including the existence of a periodic component of 24 h. 50 min. \*

*the series of observations of June-July 1955*

2- By way of illustration, I shall limit myself here to the results obtained from an *overall harmonic analysis* of 13 tidal theory waves applied to the monthly series of **2163** chained **observations** from June-July 1955 (*Poblet II*).

By way of comparison, I also indicate the results corresponding to the series of atmospheric pressures observed at Le Bourget during the same period (*Zoblet*).

The  $K_i$  ( $T = 23.93$  h.) and  $M_i$  ( $T = 24.84$  h. = 24 h. 50 mn.) **components** of the azimuth series are particularly *significant*.

It is worth noting that for all 13 waves, the total number of percentages relating to atmospheric pressure is around four times lower.

3) See § B.1.2. below.

$$\frac{1}{29.5305} = \frac{1}{29.5305} - \frac{1}{29.5305}$$

$$24.8412 \text{ h} = 24 \text{ h.} 50 \text{ mn } 28'' \quad (24.8412 / 24 = 1.03505)$$

0.0295305 days represents the Moon's synodic period.

(5) The analysis of this series and atmospheric pressure analyses were carried out by the *Service Hydrographique National des Ports* and by the *Institut Hydrographique de Hambourg*.

The 13 waves considered are those generally used by Hydrographic Institutes.

lea azimuths, although atmoaphöric pressure is not a purely random quantity and includes well-known p4riodica luniaolara compoantea 6,

For illustrative purposes, *Graph V* shows the ajusŁement obtained directly by applying the Buys-Ballot method to the June-July adrie.

1955 of the paraconic pendulum for the 25 h wave. 7 T h e amplitude of this p4riodic component is 14 grades.

### *Orders of magnitude*

3 - T h e minor elliptical wave Mb corresponding to a period of 24 h. 50 mn. (24.84 h.) of amplitude equal to 10.46 grades (*Table II*) corresponds to an angular displacement velocity of  $0.37 \cdot 10^{-9}$  rad./sec., i.e. approximately one-fifteenth of the Foucault effect equal to  $0.55 \cdot 10^{-9}$  rad./sec.

Again, we see that the *total* amplitudes of the 13 components above for the June-July 1955 azimuth series are of the order of half the Foucault effect\*. *The forces involved are therefore of the order of magnitude of the eddy current force corresponding to the acctltration of*

(6) For coefficients corresponding to tidal theory, see below § M Z'oôieou X7, p. 187.

(7) Once again, given the extremely *limited* resources available at the time (hand calculations with an electric calculating machine) (see below § A3), most of the calculations were carried out by substituting, as a first approximation, a period of 2S h. for a period of 24 h. 50 min.

In fact, 1e calcul shows that if we analyze a wave with a period  $T = 24$  h. 50 min. with a period  $T_2 = 25$  h, the amplitude of the eat wave is reduced by 6 m and the phase shift is 2.25 h. (See Chapter Vf of the Deusidme uofume of this work, p. 30 above).

f8) For a period of 2d.84 hours and an amplitude of 10.46 grades, we have a average variation

$$9' = 10, \quad \text{—————} \quad d6 = 0.367 \cdot 10 \text{ rad/sec.} \quad 0.55 \cdot 10 / 0.367 \cdot 10^{-5} 9 \cdot 15.0$$

If instead of taking  $2R = 10.46$  grades fTobleor *II*), we take  $2R = 5.3$  grades (average corresponding to Z'o&/eou 1) we have

$$\phi' = \frac{10.46}{10.46} \cdot 0.367 \cdot 10 = 0.186 \cdot 10 \quad \text{rad/sec.}$$

which corresponds to approximately one thirtieth of the eddy current effect ( $0.186 \cdot 10 / 0.55 \cdot 10$

$$= 1 / 29.6). \text{ t9) } \quad 67.04 / 10.46 = 6.41 \quad .41 / 15.0 =$$

0.427

(10) The set of results corresponding to this § A5 was presented in my *Note* of November 25, 1957 & l'Acad'mie des Sciences, "Ano/yse /zormonique des mouuemenfs du pen - dufc paraconique'.

PARACONIC PENDULUM AZIMUTHS AND ATMOSPHERIC PRESSURE

*Annual series June-July 1955*

*Ajuotemento à 13 pèrîodez de lo théorie dee morêeo  
iservice Hydrographique de Poris el Institut Hydrographique de Hambourg*

I.A.5

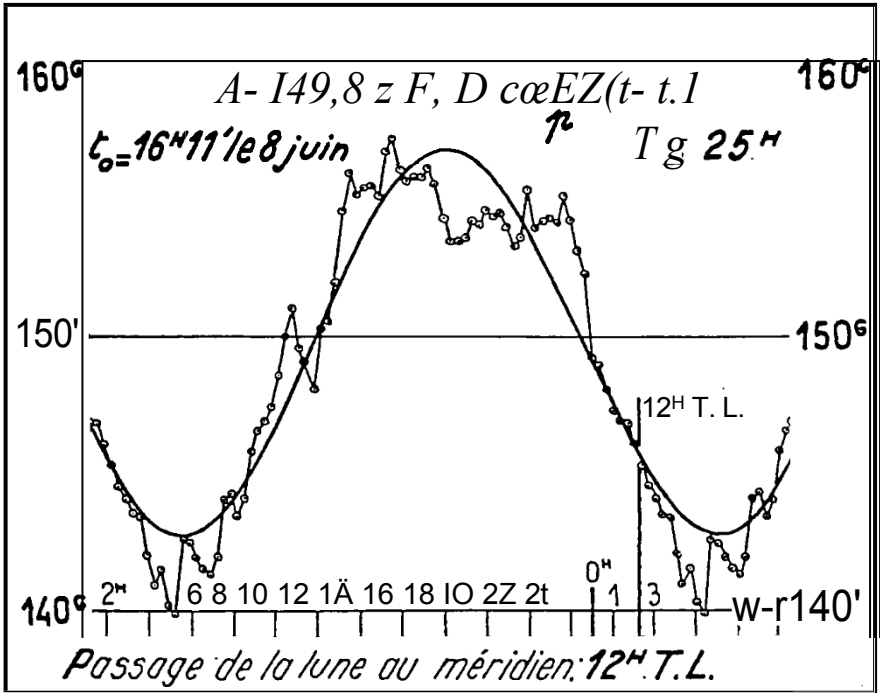
		2R wave diameter													
SeMBg	kBéiB tI0EI total observed	Ict 23* 93	Mi 24* 84	Oi 25h 82	9i 26* 87	M2 12 42	12*	8* 28	@ 8*	g2 12h 87	12* 19	N2 12* 66	√/Lt 6* 21	** 6* 10	TOTAL
Azimuth s values in grades	83,10	13,00	10,46	4,78	7,78	1,40	3,94	2,54	4,88	3,70	5,30	5,30	1,64	2,32	67,04
in 1/10 of millibar	281,00	11,20	4,24	1,20	3,00	4,40	8,80	1,46	1,96	5,20	1,40	3,40	3,80	3,60	53,66
					amete	of the	de en N	of the	riation	total	observe	2R/D			<b>TOTAL B</b>
<b>Azimuts</b>		15,64	12,59	5,75	9,36	1,69	4,74	3,06	5,87	4,45	6,38	6,38	1,97	2,79	80,67
Pressure		3,99	1,50	0,43	1,07	1,57	3,13	0,52	0,70	1,85	0,50	1,21	1,35	1,28	19,10

Tourres: Noie du 25 novembre 1957 a l'Atadémie des Sciences, *Analyze harmonique des mouvements du pendule poroconique*, and *Tableau III A* de ma Conférence du 22 février 1958.

## AZIMUTH OF THE PARACONIC PENDULUM

*Monthly serie of jciin-JtiiIfeP 1955*

JJistemeiit by fr Buys-Belloc method at a  
fine orb of 25h.



Source : Ma Noie du 25 novembre 1957 à l'Académie des Sciences, *An'ilyee harmoniq ue iconic pendulum movements*, and *Crrrophiq ue III A* of my Conference of February 22, 1958.



*An almost periodic structure*

4- In general, the observation aeriials corresponding to the series of oscillation azimuths of the paraconic pendulum have all the characteristics of *almost periodic functions!*

They feature numerous asymmetries or double symmetries with respect to certain dates, numerous similarities by translation, and local periodicities<sup>\*1</sup>.

*Ajustements par les moindres carrés*

5- In the foregoing and the following, we make continuous use of least-squares adjustment based on the general theory of simpler and multiple linear correlations<sup>1°</sup>.

(10) Une fonction presque périodique est une somme de composantes sinusoïdales de périodes incommensurables (Voir l'Appendice E de la *Deuxième Partie* de cet ouvrage, Allais 1983, "Préquence, Probabilités, et Itacord. Appendice JJ", p. 31 above).

(11) I gave numerous illustrations of this in my lecture of February 22, 1958, for symmetries and translations.

On this property of almost periodic functions, see Allais, 1983, id. *Appendice II, § P.9, Régularités locales des fonctions presque périodiques.*

(12) I think it's worth recalling here very briefly the principle of these calculations in the case of a simple correlation.

Consider two functions  $z(x)$  and  $y(x)$  where  $z(x)$  is assumed to depend linearly on  $y(x)$  in the first approximation, and for which we have  $n$  pairs of observations  $(z_i, y_i)$ , functions of  $x_i$ . The estimation of the correlation between  $z$  and  $y$  requires to determine the function

$$(1) z - y = a + b$$

where  $a$  and  $b$  are constants, so we have

$$(2) \sum_{i=1}^n (z_i - y_i)^2 = \min$$

and such that for the  $n$  pairs of observations available, the sum  $\sum_{i=1}^n (z_i - y_i)^2$  of the squares of the differences is a minimum.

The correlation coefficient  $R$ , a measure of the dependence considered, is such that we have

$$(2) 1 - R^2 = \frac{\sigma_z^2}{\sigma_y^2}$$

where  $\sigma_z$  and  $\sigma_y$  respectively represent the standard deviations of  $z(x)$  and  $y(x)$ .

In principle, for all correlations in this volume, I indicate the values of  $R$ ,  $\sigma_z$  et  $\sigma_y$ .

On the calculation of correlation coefficients, see Harald Cramer's remarkable method *Method of Stochastic Processes*, Princeton University Press, 1946.

## B

**THREE FUNDAMENTAL QUESTIONS  
ON PARACONIC RRNDULE WITH ANISOTROPIC SUPPORT**

Interpretation of the experimental results for the anisotropically supported paracone pendulum leads to three fundamental questions:

*First question:* Do the series of linked observations contain *statistically significant* periodic terms with periods of 24 h and 24 h 50 min?

*Second question:* If so, *can* the periodic effects thus observed be *identified, or not*, with the periodic effects resulting from the *current* theory of gravitation (as it results from the dual principle of inertia and universal gravitation assumed to be valid with respect to any Galilean reference frame), supplemented, or not, by the corrections of the theory of relativity<sup>4</sup>, and as applied within the framework of the *current* theory of relative motion?

*Third question:* If they can't be, *can* the existence of significant periodic terms in the series of chained observations obtained *be attributed, or not, to an indirect influence of a known periodic phenomenon?*

A la première question fondamentale l'expérience permet de donner une réponse *totale* affirmative pour trois raisons tout à fait essentielles : the observation of triply chained series, the comparative structure of the series observed and reconstituted from their periodic components determined by harmonic analysis, and the application of the *Generalization of the Ischuster Test to the case of auto-retarded striae*.

### *Triplicately enchanting observations*

1- If the variations observed were purely fortuitous, we would have to admit that they were essentially due to the random influence of the marbles. If this were the case, three triply concatenated series -  $3n + 3$  observations with  $3n + 1$  observations,  $3n + 4$  observations with  $3n + 5$  observations with  $3n + 2$  observations - would behave *independently* of each other, since the logs are changed in each experiment.

In fact, three series of triple-chained 14-minute experiments carried out from May 5 to 10, 1957, for 87 hours, showed that the oscillation plane movements in the three series were *similar* (Graph VI).

These experiments enabled us to estimate the standard deviation of the random influence of the beads for each elementary 14-minute experiment at approximately  $G = 2.5$  grades. Taking into account the recall influence of the support \*, this means that the confidence interval at 95% of the deviation that may exist between two series of independent observations is 1:12.5 grades. The influence of the beads is therefore very important, but cannot explain the similarity of the azimuth variations observed (Graph M) \*.

It's remarkable that all three, starting from the same azimuth of 220 grades, rapidly converged on the same azimuth of around 160 grades.

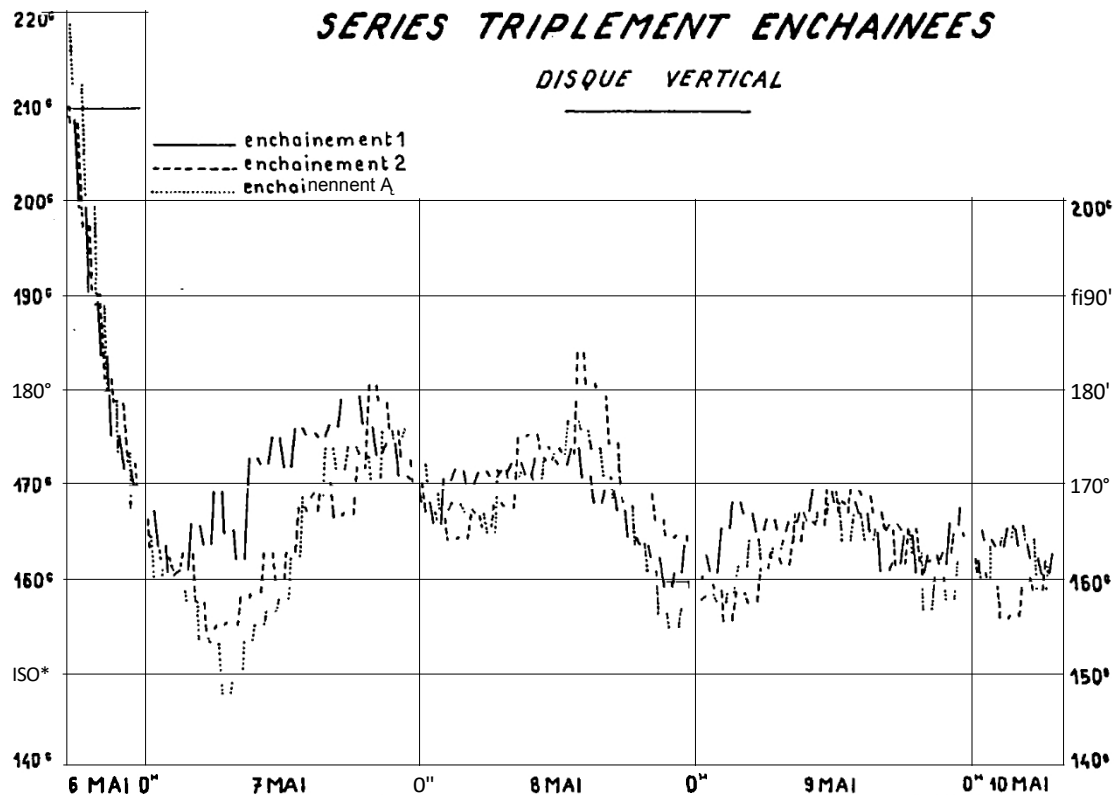
(1) "A.1.3 above, p. 85.

(2) Ma Note du 25 novembre 1957 à l'Académie des Sciences, Anofyte *harmonique* du mouvement de la pendule porronique.

INFLUENCE DES BILLES

SERIES TRIPLEMENT ENCHAINÉES

6 au 10 Mai 1957  
87 Heures



*Héries obsenite- et -éries reconstitutes*

2- Both the elementary analysis using the Buys-Ballot filter for different periods and the ensemble harmonic analysis (using the least-squares method) of 13 tidal waves *simultaneously* show the existence of significant components.

periods of 24 h. and 24 h. 50 min. (waves  $< \lambda$  and Mb of tidal theory) \*.

Since the analysis is carried out over a month, we may well ask whether these waveforms really exist at *any instant in time*. In fact, any discrete series of  $2n + 1$  numbers can be represented by the sum of a constant and  $n$  sinusoids. Obtaining a sinusoid of a given period by any method of harmonic analysis *can therefore only have real significance* if not only its *relative* amplitude is sufficiently large, but also if the periodic structure observed *for* the series as a *whole* is *actually* found in the various elementary periods into which the observation period under consideration can be broken down.

In fact, it is easy to verify that the periodic structure of the two monthly series of linked observations from November-December 1954 and June-July 1955 can be considered to be maintained for the two fortnightly periods and even for the four one-week periods into which each of these two series of

- For an observed monthly series, *simultaneous* least-squares estimation of the amplitudes of the 13 waves usually considered in tidal theory yields 13 sine waves, which can be summed using Lord Kelvin's *Tide Predictor*\*. The calculated series thus obtained can be analyzed for the 24 h. and 25 h. periods using the same Buys-Ballot filter method as for the observed aerie.

(3) § A.5 above.

(4) 7oô/eou See § A.5.3 above, p. 99.

If the ainusoids obtained *really* exist in the adrie observed, the Buys-Ballot method should give sinusoidsa of phases H and H' com- parable for each of the sea components (fortnights or weeks) for the aerie observed and the series calculated.

• *Table III* shows the H - H' differences of these phases in hours and minutes, for the two monthly series of November-December 1954 and June-July 1955, for the month, the two fortnights and the week. every four weeks. *H - H' phosis differences remain relatively small* <sup>5</sup>

Given that the experimental process means that each week can be considered an independent experiment, *such remarkable phase toneordances must be regarded as proof of the existence of real periodicity of 24 h and 24 h 50 min. periods.*

By way of illustration, *Charts MI, VIII, and 2X* show the results of this analysis for the 24 h. and 25 h. periods *for the monthly June-July 1915 series*<sup>5!</sup>

f5) Except for the 24 h. period and the first week of November-December 1954, and for the 25 h. period and the second week of June-July 1955, for which H - H' = - 4 h. 02 mn. and + 2 h. 20 mn, respectively, the H - H' differences remain relatively small.

The  $\bar{H} - \bar{H}'$  indicates the algebraic averages of H - H' and the averages of their absolute values for the month, fortnight and week.

It's worth pointing out that the observed series probably contains other periodic components than the 13 waves under consideration, and that there are also random perturbations such as the beads. Both can displace the vertices ofa ainusoida. The two relatively large deviations of H - H' are not surprising. What is really astonishing is that lea deviations remain relatively small in all other caa.

f6) On the Y $\bar{H}$  Gropfi $\bar{H}$ 9ue and the 24 h. wave, we have H - H' = 4 h. 54 mn. - 4 h. 48 mn. 6 min. and on the Yffi graph and the 25 h. wave we have H - H' = 18 h. 11 mn. - 18 h. 12 mn. = - 1 mn.

To simplify calculations, we considered the 25 hours instead of 1 wave of 24 n 50 mn (see A.3.1, note 3, p. 90).

*The practical identity of the results of the two analysis methods can be seen not only for the month, but also for the two fortnights 7,*

This analysis shows that the 24 h. and 24 h. 50 mn. highlighted by the overall harmonic analysis *actually exist in each of the elementary periods into which the month can be broken down.*

- It can also be seen that if, for the two fortnights, the 25 h. filter gives two sinusoids from the raw series whose phase differs by 3 h. 23 min., this difference is due to the influence of the 24 h. wave, not completely eliminated in an analysis covering only a fortnight, since *the same difference* exists in the reconstituted series that we know to be *a sum of sinusot'des* (Graph yfj 8,

- All in all, the Buys-Ballot filter analysis for the 24 h. and 25 h. periods gives comparable results for the *observed* series of November-December 1954 and June-July 1955 and the *reconstructed* series of these two series based on the results of the ensemble harmonic analysis of 13 tidal theory waves, whatever the elementary period considered within the framework of each month. *The conclusion is that these periodic components do exist.*

(7) Such an identity would not generally occur if the observed series did not actually contain the waves considered in each elementary period, as the least-squares operation that provided the amplitudes of the 13 waves **considered** was **only** obtained in an operation covering the whole month.

(8) We have fGroph<sup>9</sup>ue D

$$19 \text{ h } 59 \text{ mn} - 16 \text{ h } 36 \text{ mn} = 3 \text{ h } 23 \text{ mn}$$

**et** (Graphiques IX)

$$19 \text{ h } 59 \text{ mn} - 16 \text{ h } 36 \text{ mn} = 3 \text{ h } 23 \text{ mn}$$

$$19 \text{ h } 41 \text{ mn} - 16 \text{ h } 16 \text{ mn} = 3 \text{ h } 15 \text{ mn}$$

(9) The preceding analysis, the 'Peblenu ffj, and the C-rephi<sup>9</sup>ues YES, Yifj, fX, et X have <sup>öt4</sup>preant4a dana ma **Nett** unpublished, but very widely circulated, of November 20, 1958: "*Slur l'exietenct d'une composante ptriody ue de période de 24 h. 50 mn. in the mou- uementz of the onisotropic zuppon paraconic pendulum*".

PARACONIC PENDULUM WITH ANISOTROPIC SUPPORT  
MONTHLY VALUES OBSERVED AND CALCULATED FROM  
JANUARY TO DECEMBER 1954 AND FROM JUNE TO  
AUGUST 1955

Comparison of CZ and £ 'sentences

oos-nespom xazat ù f'azzof ðe's aéries

obæruées et calculées jzar fa mé7fzode cie Ziüy-Ziaffot

for feapdrñ "fea life Sd Aeurea and 25 fzeurea

	H - H' values			
	Nov-Dec 1954		June-July 1955	
	Ah	25h	2§h	25h
I + 2 + 3 + 4	0* 30-^	- 0h 2smn	0* 06""	- 0E 01
1 + 2 3 + 4	- i* 02mn - 0h 56=n	- 0h 20mn 0h 06mn	- 1* 09"" 0^ 4smn	oh 20 0h 15-'
i 2 3 4	oh 16 -4h 02 -1h 41 1h43	1h31- - lh 27mn - lh 2mn 0 36	}h 08mn oh 59 - i*23^ 0h 59mn	-oh 33 2h 20 -0^ 56=n o*57-
Averages	$\overline{H - H'}$		$\overline{ H - H' }$	
month	3 =		15mn	
fortnight	- 15		37mn	
week	- 15--'		83mn	

*Caption:*

The notations 1, 2, 3, 4 represent the first, second, third and fourth weeks; the notations 1 + 2 and 3 + 4 the first and second fortnights; and the notation 1 + 2 + 3 + 4 the whole moia.

For example, for the first fortnight of June-July 1955

and for the 25 h fGrnpui9ues fH period: 16 h 36 mn - 16 h 16 mn = 20 mn.

$$\overline{H - H'} = \text{algebraic mean of } H - H'$$

$$\overline{|H - H'|} = \text{average of absolute } H - H' \text{ values}$$

Source :

Ma Nale ù au 20 novembre 1958, Jur l'existezrce d'une composante périodique voisine de 24 h. 50 mn. in the movements of the anisotropically supported poroconigüe pendulum.



## ANALYSE HARMONIQUE

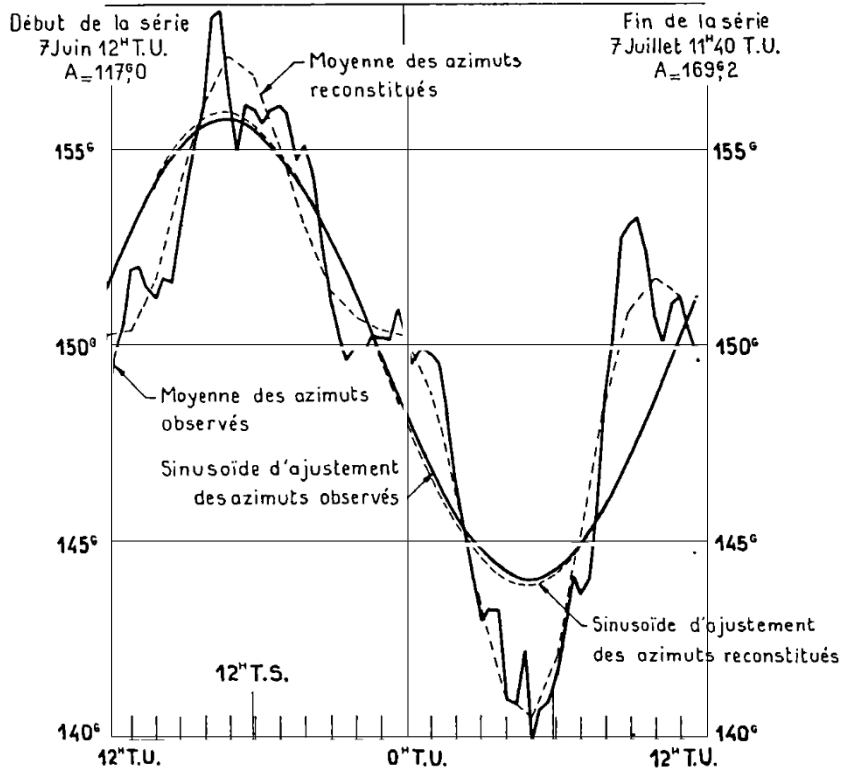
xwxzrze wxemowzeae

pu zx szp/r œgz "ozw. &gt;v/zr rsss

AZ I PIUT 5  
 June. Jui lle t 9 SS  
 2t,3 t 4  
 Strie rit lle et re canot ituec

FILTRE DE BUYS.BEL LOT ÄÊ Ä I Û

Ö// \_\*Serie complète (t 2t3+4) rit elle et rconstituÉe



Sinusoïde d'ajustement

$$y = \bar{y} + R \cos 2\pi \frac{t - \theta}{T}$$

$T = 24$   $\theta =$  heure du sommet par rapport  
 au début de la série

$\theta =$  heure T.U. du sommet

$q =$  nombre de cycles

	q	$\bar{y}$	R	$\delta$	$\delta'$
Azimuts observés	30	149,96	5,832	4°54	16°54
Azimuts reconstitués	30	149,93	5,957	4°48	16°48

N.B.:1) Les azimuts reconstitués sont ceux correspondant à la somme de 13 ondes élémentaires des marées déterminées par la méthode des moindres carrés.

2) Le temps sidéral indiqué est celui correspondant au jour moyen, soit le 22 juin 1955

3) L'échelle des azimuts reconstitués a été décalée de manière à ce que les deux sinusoïdes d'ajustement aient même axe.

GrnpAique MZf

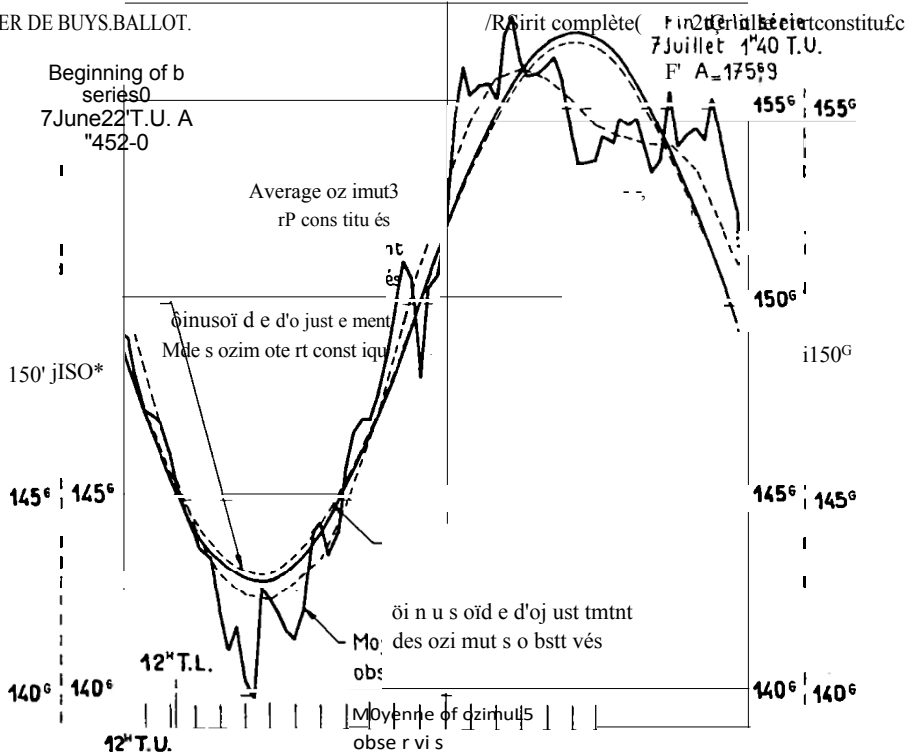
ANALYSE HARMONIQUE

xwxi rsr wxe  
"iow'pvz

25<sup>H</sup>  
AZIPIUTS  
June, July 195 5  
tg 2y 3t S  
Série réelle et reconstituée

Dz zx Jzwicæoz zoiw.>oiziz rsss

FILTER DE BUYS.BALLOT.



Sinusoid e d'ajustement

$y = y + R \cos 2\pi \frac{t - \theta}{T}$

0 "T. U. du 22 June

12 "T.U

	q	ÿ	R	0	6'
Azimuts obse r vis	28	149°80	7°00?	18"11	1s "58
Azimut s re t onstitvi	2 8	14 9°92	6'766	18"12	15"59

T\_Z § 6\_time of t,ommeË p0r 'A PP r!

N.B.: 1) Les ~~obs~~ reconstitués sont ceux correspondant à la somme de 7 ondes élémentaires des marées déterminées par la méthode des moindres carrés.

2) Le temps lunaire indiqué est celui correspondant au jour moyen, soit le 22 Juin 1955 (12" T.L. correspond au passage de la lune au méridien)

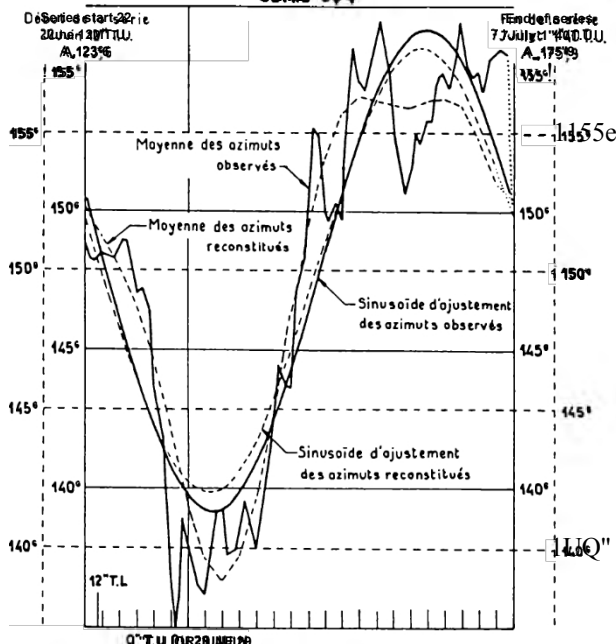
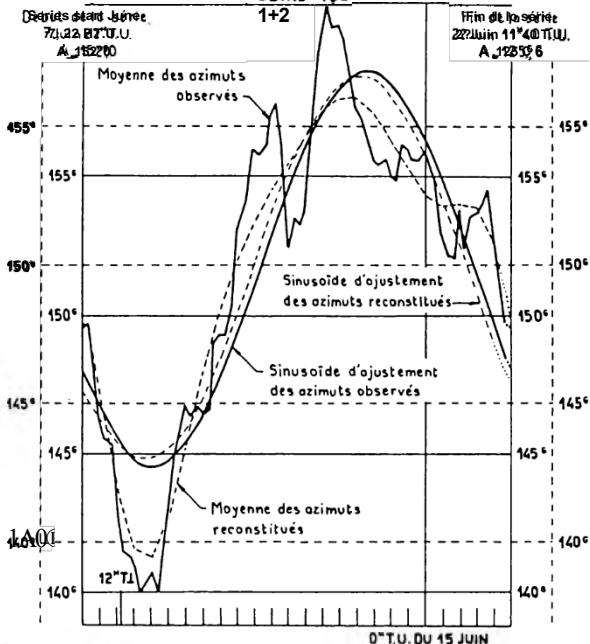
*Source* : Ma Note du 20 noJembre 1958 (see *Tobteau lily*)

FILTRE DE COURTE PERIODE DE 25 JOURS

25"  
AZIMUTS  
Série reconstituée et reconstruite  
1.2 3.4  
Temps lunaires

## SÉRIE 1,2

## SÉRIE 3,4



SINUSOIDE D'AJUSTEMENT

3.2 + R. 2.1

T = 25

0 heure du sommet par rapport au début de la série

0 heure du sommet par rapport au début de la série et de la lune au méridien (considérer l'axe de la lune)

Reconstituted azimuths are those corresponding to the sum of 13 elementary modes determined by a least-squares method.

N.B. 2) Les azimuths reconstitués sont ceux correspondant au jour moyen de 25 jours (série 1,2) et de 29 jours (série 3,4) d'azimuts observés.

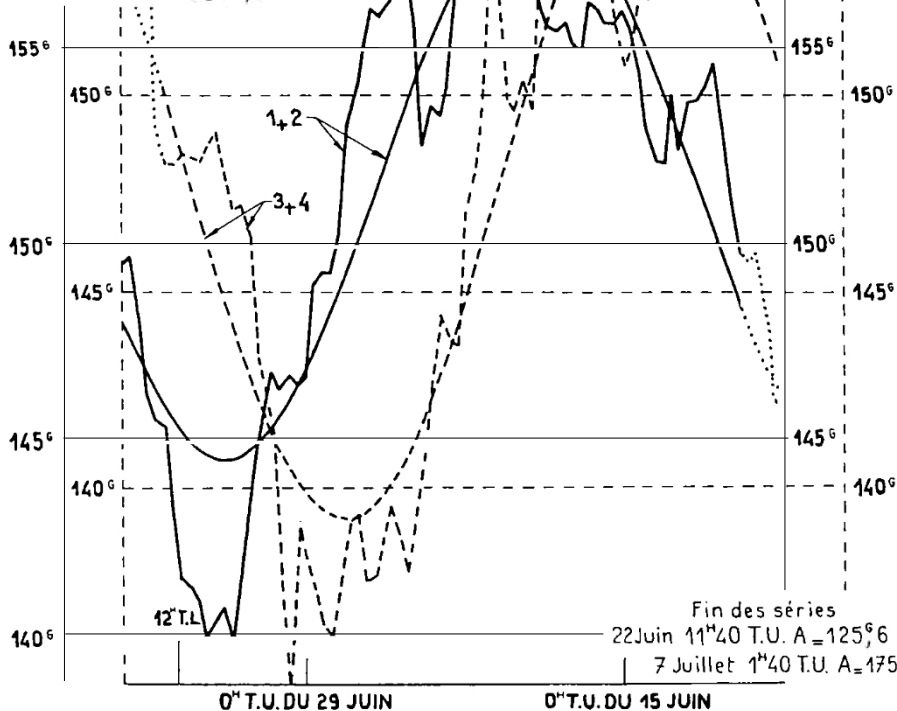
2) Le temps lunaire indiqué est celui correspondant au jour moyen, soit le 15 Juin 1955 (série 1,2) et 29 Juin 1955 (série 3,4). 12<sup>h</sup> T.L. correspond au passage de la lune au méridien.

Source : Ma Note of November 20, 1958 (see Table III)

Source : Ma Note du 20 novembre 1958 (voir Tableau III)



## ANALYSE HARMONIQUE

ANALYSE HARMONIQUE DE LA SÉRIE VI  
DE JUIN - JUILLET 1955FILTRE DE BUYS-BALLOT. PÉRIODE DE 25"  
Deux séries de 14 jours de 25" (1+2 et 3+4)Début des séries  
7 Juin 22" T.U. A=152°0  
22 Juin 12" T.U. A=123°6

Sinusoïde d'ajustement

$$y = \bar{y} + R \cos \frac{t - \theta}{T}$$

 $T = 25$   $\theta$  = heure du sommet par rapport  
au début de la série

 $\theta'$  = heure du sommet par rapport à l'heure de passage de la lune  $q$  = nombre de cycles  
au méridien du jour moyen (considérée en temps moyen)

Période		-	R	$\theta$	$\theta'$
1 + 2	14	151°71	7,171	16°36	14°25
3 + 4	14	147°89	8,693	19°59	5°42

N.B.: 1) Le temps lunaire indiqué est celui correspondant au jour moyen, soit le 15 Juin 1955  
(série 1+2) et 29 Juin 1955 (série 3+4). (12" T.L. correspond au passage de la lune au méridien)

*Applying the generalization of the Ischuster Test to the case of autocorrelated time series*

3- Any discrete series of  $N = 2n + 1$  numbers  $x_j$  can be represented by the sum of a constant and  $n$  sinusoids. Obtaining a sinusoid of a given period by any method of harmonic analysis can therefore only have real meaning if its relative amplitude is sufficiently large.

In the case where the  $x_j$  are *independent*, Schuster established a test of significance that has become classic, but *this test is inapplicable in the case where the  $x_j$  are autocorrelated*. In 1957, I showed how Schuster's test could be generalized and specified the conditions for a periodicity test applicable to autocorrelated series <sup>10</sup>.

- Suppose, for example, that we consider a series of  $N$  observations  $x_i$  and that the application of the Buys-Ballot filter for a period  $T$  determines a sinusoid

$$(1) \quad X = R \sin(t - \theta) \quad \sigma = 2x/T$$

and let  $P_p$  be the probability that  $R$  has a value greater than or equal to  $\bar{a}$

(10) See my *Notes* of May 13 and December 23, 1957 to the Académie des Sciences *Tesi de periodicitt. Tãntrolimtion du test de Schucter on c'is de striec temporelles 'iu - foerrééz"* and "*Application du test de Ischnever généralisé à l'onolyse harmon ique dec mimuts du pendule parotonique*". These *notes* were presented by Jean-Marie Kampé de Fériet, member of the Académie des Sciences.

In 1961 I presented a detailed demonstration of this test at the 33rd Session of the International Statistical Institute, with an application to the case of the Bougival parabolic pendulum observations in 1958 (see Section C below) in *Tã8 Communication: "Test de periodicitt. Tãntroicoftion du Ceci de Schucier au cas dt eë - riec htm mettes aufocorrèltic dont fũypotũde d'un pœessus de perturbations otJo-toires d'un eycfeme cfible"* (Bulletin de l'Institut International de Statistique, 1962, Tome 39, 2ème livraison, p. 1d3-194). This paper is reproduced as *Appendix D* in the second issue of this issue (see Contents above, p. 31).

a given value of  $R_p$ , assuming that the series under consideration is devoid of any periodicity. The formulation I've determined makes it easy to calculate the probability  $P_p$  <sup>11</sup>.

Thus, for example, the Buys-Ballot filter applied to the series of chained observations of *jitin-July 1955* and the 25 h. wave gave  $R = 7.0$  grades <sup>12</sup>. For this value we find  $P = 0.48 \cdot 10^{-5}$  <sup>13</sup>. This means that in the !XR !hese of the absence of any periodicity in the streak under consideration, there is less than one chance in 100,000 of obtaining an  $R$  value greater than or equal to 7.0 grades by analysis of the Buys-Bollot filter.

From the point of view of statistical analysis, the significance level is therefore 0.00048 <sup>9</sup>-. We can conclude that the existence of the 25-hour wave is virtually certain.

(11) We have

$$(1) \quad P_p = \text{Prob}2(ji) = e^{-\lambda}$$

$$(2) \quad \mu_{\omega} = \frac{I_{\omega}}{E[I_{\omega}]} = \frac{N I_{\omega}}{4 \sigma^2 k_{\omega}} \quad \ddot{o}' \cdot \left(1 - \frac{1}{N}\right) s^2 \quad \circ = 2wp$$

$$(3) \quad I_{\omega} = A_{\omega}^2 + B_{\omega}^2 = R_{\omega}^2 \quad A_{\omega} = \sum_{s=0}^{N-1} e^{i s \omega} \cos a \quad m_{By} = \sum_{s=0}^{N-1} x_{s+1} \sin s \omega$$

$$(4) \quad \begin{aligned} &v=h \\ &= 1 + 2 \sum_{v=1}^{N-1} \left(1 - \frac{v}{N}\right) r_v \cos v \omega \end{aligned}$$

$p$  is the period. Frequency  $m$  is equal to  $8 N/p$ .  $s^{\circ}$  is the variance of  $x_i$  and  $h$  is the value of  $v$  above which the autocorrelation coefficient  $r_p$  no longer significantly differs from zero.

f12) § A5.2 above, *Graphigut Y*, p. 100.

(13) My note of December 23 1957 to the Academie des Sciences (note 8 above).

In the caa of the June-July 1955 series we have with the notations of note (11) below above :  $N = 2161$ ,  $\sigma = 20.24$  grades ; and for  $T = 25$  h. we have  $p = 25 \cdot 3 = 75$ ,  $k_{tt} = 5.292$ . We have  $= 7.01$  grades (*ffiraph Y* in § A.5.2 above). We then have  $\log = 7.01'$ ,  $eC$

$$\log = (2161 \cdot 7.01') / (4.20,24' \cdot 5,292) = 12, \quad 25P_p = e^{-\lambda} = 0,481 \cdot 10^{-5}$$



- A titre d'illustration le *Graphique VI* représente le fréquences-gramme de la série de *nombre-décembre 1954* de 721 valeurs horaires  
 nVic ile lines d'égale signification statistique  $1 <$ .

We can see that the 24 h. 50 min. wave (i.e. 24.84 hrs.) with a n amplitude of around  $2R = 11$  grades has a significance level of  $P = 0.062$  No 15. There is **therefore** *less than one chance in a thousand* of such an amplitude being observed in a series devoid of any real periodicity.

(14) This Graphic is the photographic reproduction of the *Graphique VI C J* of my Conference of November 7, 1959, "*Fond-il reconsidérer les lois de la gravitation?*" Nouveets *résultats, Bilon et Perspectives*

This frequenciesgram can be usefully compared with the frequenciesgram of the July 1958 monthly series from Bougival (Gropffique OVI in § C.2.4 below, p. 154).

These two graphs show significant periods corresponding to harmonics characterized by values  $m = 29, 30, 31, 32, 33,$  and  $34$  corresponding to periods ( $p = 721/m$ ): 24.86 h ; 24.8 h ; 23.26 h ; 22.53 h ; 21.84 h ; and 21.21 h.

I eign that periods of 23.09 h ; 22.31 h ; and 21.67 h as well as periods of voisines apparaissent dans les analyses de la théorie des marches schurman, 1941, *Manual of Harmonic Analysis and Prediction of fides*, p. 164-165).

To simplify, the analyses in this work focus mainly on the 24 h 50 mn (24.84 h); 24 h; 12 h 25 mn (12.42 h); and 12 h periods, and tout port icufic rement on the 24 h 50 mn période.

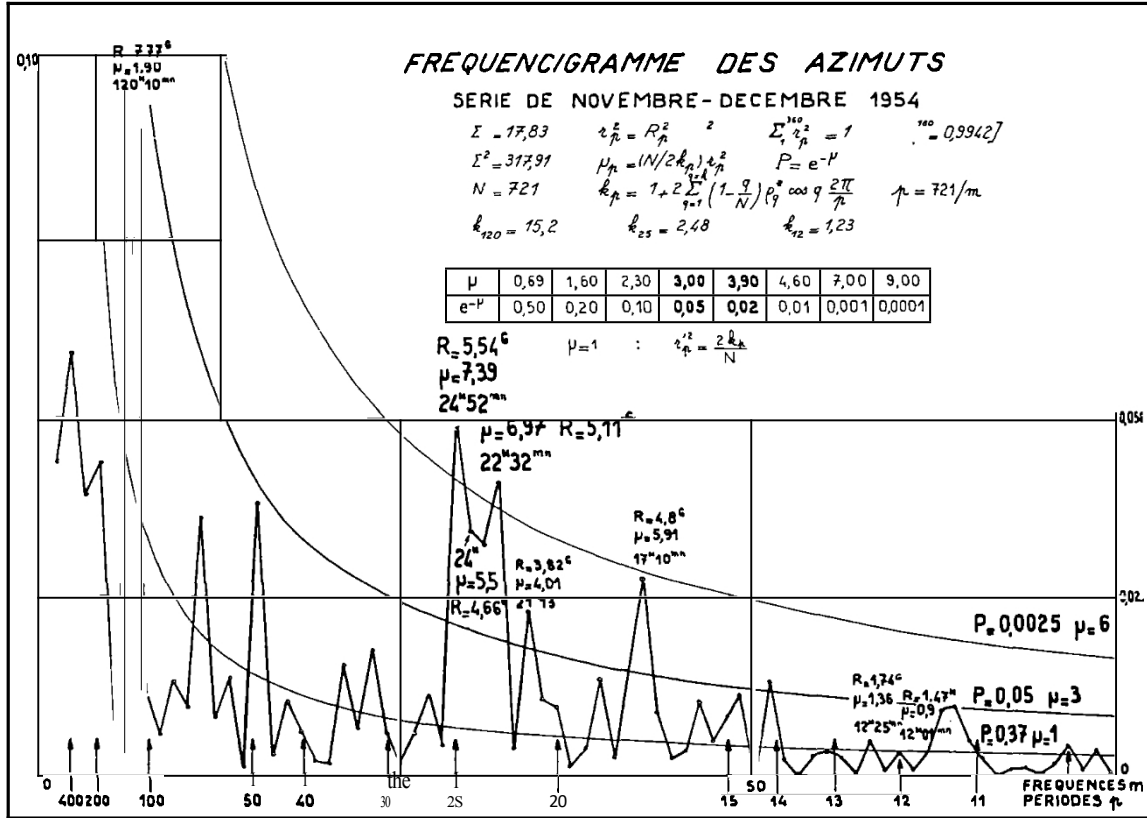
(15) The frequenciesgram considered corresponds to  $N = 721$  hourly values. For 17iarmonic corresponding to  $C m = 29$  we have the period  $p = 721/29 = 24.86$  h = 24 h 52 mn, and an amplitude  $2R = 11.08$  grades.

For this period we have (*Graphique XI*):  $\zeta = 7.39$ ;  $P = e - P = 0.062\%$ .

For  $m = 30$  we have the period  $p = 721/30 = 24.03$  h = 24 h 2 mn. with  $R = 4.66$  grades ;  $tt = 5.5$ ;  $P = eE = 0.4\%$ .

The choice of the value  $N = 721$  hourly values corresponds to the condition that elle allows to repair the amplitudes of the two periods of 2d h and 24 h 50 mn.

We have:  $721 / 24 = 30.04$ ;  $721 / 24.8d = 29.03$ . These values are not very different from whole numbers.



Caption: The formulation of the test is deduced from my Note of December 1, 1957 to the Academie des Sciences. See also my memoir of 1961. "L'essai de l'iriodicite". *Wri Jrolixat tori du Tesi de Sch u'fer ou ca" de "triee temporelle auf x-ort]]Je" dans l'hypothe "e d'un processus de perturbations aleatoires d'un systeme stable.*  
 Source: *Craphique II C 1 de ma Conference du 7 novembre 1959*

*Absolute certainty of the lunisolar effects observed*

4 From the three preceding analyses, that of chained observations, that of the comparison of the periodic structures of the observed series and the calculated aeri- als, and that corresponding to t h e application of the *Generalization of the Ischuster test*, we can conclude *with complete certainty that the diurnal lunisolar effects observed, and especially the lunar p̄eriodicilē of 24 h 50 mn, really do exist.*

*The first question /on- domentole at the beginning of this Section must therefore be answered in the affirmative. The considered monthly series of chained observations of the azimuths of the paraco- nic pendulum do indeed contain sta- tistically significant lunisolar periodic terms, and this with complete certainty.*

(16) Il e'ngit naturellement d'une certil nde promène et non d'une certitude mélaph-y

## 2-- Z.eø e 'eds čunzæzfažreø oðøœués et fa čhëor-še oczueffe zfe fògrtzø'zYofžon

In view of the observed amplitudes of lunisolar effects in the case of the anisotropic support, it is easy to verify that the effects observed are *totally inexplicable* within the framework of current theories of gravitation, whether Newtonian or relativistic<sup>4</sup>.

*Observed and calculated values of lunar influence on the azimuth of the paraconic pendulum with anisotropic support*

1- If we consider, for example, the action of the Moon on the pendulum during a 14-minute experiment, we can estimate that, to a first approximation, according to the theory currently accepted, it translates into an average azimuth variation of less than  $10^{-13}$  radians per second<sup>1</sup>.

In fact, the average amplitude of 5.3 grades found for the 25 h. wave for the seven series of monthly observations carried out, corresponds to an average variation of about  $0.19 \cdot 10^{-8}$  radians per second<sup>2</sup>. The observed effect is therefore 18 million times greater than the calculated effect!

*Extremely small lunisolar influence on the motion of paraconic pendulums with anisotropic supports*

2- In reality, the theoretical effects of lunisolar influence on pendulum movement are so small that none of the *XIXth century authors who worked on pendulum theory, some of whom were excellent mathematicians, attempted to calculate them.*

The extraordinary smallness of the calculated effects is easily explained if we consider that to obtain the effective gradient of the Sun's or Moon's attraction at a point on the Earth's surface, we need to take the difference between the gradients at this point and at the center of the Earth.

(1) *Tobleou Vii*, relation (8), p. 129 above.

(2) G A.5.3, note 8, p. 98 ci-desaus.

(3)  $0,106 \cdot 10^{-8} / 10^{-1} = 1,06 \cdot 10^{-7}$

What's more, the pendulum's oscillation plane can only rotate under the influence of the lunar attraction as the gradient varies around the point under consideration. We must therefore consider the difference in this gradient between its value in the pendulum's mean position and its value at a nearby point\*.

The comparison of observed and calculated values of the influence of the Moon and the Sun on the pendulum's movement is so important for the purposes of this book that I felt it necessary to present at the four Toöfenex ZV è Off the principles of calculations allowing this confrontation 5t Ü

*Table IV - Classical theory of Foucault pendulum motion for straight or elliptical paths*

3- *Tobleou IV* briefly recalls the results of the the- rique analysis of pendulum motion under the action of terrestrial rotation, *as presented in all the Treatises*.

(4) This is the gradq  $U_i$  - fat  $U_i$  difference from relationship (5) in Z'nöleou Y below, p. 127.

(5) To simplify the presentation, the following calculations take no account of the compound centrifugal force corresponding to the Earth's rotation and its variations in space fialay4 by the pendulum. The following presentation only aims to enlcute in first *approximation the effele of the tunisotory attraction zur mouue- ment du pendule pernconique*, and not eeux de te rotntion terrestre.

To take into account, as a first approximation, the compound centrifugal force due to the Earth's rotation in the following formulas, simply replace the g acceleration of gravity due to the Earth's attraction by the corresponding  $g = s - P\text{g} \frac{Q^2}{L}$ . This is what all Trait4s do.

(jp @ 2 yg 2 L rt = (0.729 10\* cos 48.9 °) \* 6.3712 10 = 1.46 a fraction of approximately 1.5 10\* of gravity.

For a complete calculation, see Allais, 1956, TA4orie du *Pendule poroconique*, Partie VA, *Thorie dec mouvements reteni/s* (p. V2-V28) et ifouement du *Pendule conique nous l'in/fluence de to rolotion terrestre* fp. Y29-Y46). VOURE gelement Allais, 13 mars 1958, *Application du TMorème de Bour ou ces des mouomezts terrestres dans le cos le*

(6) *A detailed study of the influence of stars on the motion of the paraconic pendulum with anisotropic and isotropic supports is given in the second volume of this work (Chapters I and ii, Sections B, p. 28 above).*

Given the ellipticity  $b$  of the trajectory, the variation  $\xi'$  of the azimuth of the plane of oscillation can be written as

$$(1) \quad \xi' = -\omega \sin L + (3/8) p e \quad b p = g d / 1$$

$\omega$  is the Earth's rotation speed,  $L$  is the latitude of the location, and  $a$  and  $b$  are the major and minor axes in radians of the elliptical path described by the pendulum. The second component of  $\xi'$  corresponds to Airy precession. The plane of oscillation rotates *in the same direction* as its elliptical trajectory is described by pendulum 7.

*Table V - Forces acting on the Foucault pendulum relative to axes linked to the Earth under the action of gravity, terrestrial rotation and the attraction of stars according to the current theory of gravitation.*

4- Toöfeeu Y shows how the acceleration of the pendulum's center of gravity relative to axes  $S x y z$  lids **the Earth**, where  $S$  corresponds to the pendulum's suspension point, is determined **from** the fundamental relation

$$(2) \quad \mathbf{F} = M \mathbf{Y}$$

which, according to the theory of gravitation, *is applicable* (apart from uniform translation) *only in relation to the Copernican trihedron*  $S' x' y' z'$ , whose origin is the center of the Sun and whose axes pass through three fixed stars.

Relation (3) in *Table Y* gives the acceleration expression absolute 7 of the gravity  $\mathbf{G}_{\text{center}}$  as a function of the acceleration of  $\mathbf{G}$  relative to the Earth, the driving acceleration, and the Coriolis acceleration.

An *essential* fact is that we have

$$(3) \quad \text{grad}_y U; - \text{grad}_T U, = (\text{grad}_S U; - \text{grad}_y U;) + (\text{grad}_y U; - \text{grad}_S U;)$$

(7) The ellipse is described in the direct or retrograde direction, depending on whether  $\theta$  is positive or negative.

(8) The bold notation  $U_i$  means that the gradient of the gravitational potential of the other  $i$  is considered at point  $G$ .

*The first term, which corresponds to the deviation of the vertical due to the other i and which is independent of the center of gravity G of the pendulum, has no influence on the pendulum's movement.*

*The only factor involved in this motion is the difference between the action of the star i at point G and at point IS, the pendulum's support point. This difference is naturally extremely peLite\*.*

*Table VI - Influence of the sun and moon on the movement of the paratonic pendulum u n d e r current gravitational theory*

5- Table M shows how the second members of the differential equations in  $m = x / l$  and  $n = y / l$ , which depend on t h e action of the other i, are determined. The influence of the other i on the motion of the paratonic pendulum is determined by the relationships <sup>10</sup>

$$(4) \quad m'' + (g / l) m = m K_i (m \cos 2A_i + n \sin 2A_i)$$

$$(5) \quad n'' + (g / l) n = K_i (m \sin 2A_i - n \cos 2A_i)$$

$$(6) \quad K_i = 3 C_i \sin^2 z_i \frac{M_i}{M_T} \frac{r_T^2}{d_i^3} g$$

where  $z_i$  and  $A_i$  represent the zenith distance and azimuth of the other i,  $M_i$  and  $M_T$  the masses of the other i and the Earth,  $r_T$  the radius of the Earth, and  $d_i$  the distance of star i from the center of the Earth.

(9) Its order of magnitude is given by relationship (10a) of T'otileou V.

In fact, all the authors who have most thoroughly applied the theory of relative motion, such as Bour and Gilbert, have assumed that gravitational forces are constant throughout the space swept by the pendulum. *ile support must que le de xième term of the second member of the relation (3) ci-dessus (p. 120) est nul.*

<sup>10</sup> T'otileou V, *Annales de l'Observatoire de Paris, Mémoires*, Cinquième Partie, Influence de la rotation terrestre, p. V.1 - V.28. See also Allais, 13 mars 1958, *Revue de l'Observatoire de Paris*, 32 p. This memoir extends Bour's results to the most general case, where we take into account the variation of the gravitational field in the space swept by the system in movement that we consider.

On relative motion, see E. Bour, *Mémoire sur les mouvements relatifs*, Journal de Mathématiques pures et appliquées, Tome VIII, 1863, pp. 1-51. See also Gilbert, *Mémoire sur l'application de la méthode de Regroupement des perturbations au mouvement relatif*, Gauthiers-Villars, Paris 1889, 197 p. Gilbert relies on Bour's 1863 dissertation.

(10) These relationships are demonstrated from the serial development of expression (2) of T'otileou Y.T. The second members of relations (4) and (5) above are considered as perturbations.

Based on relations (4) and (5), the narration method used de-constants allows us to deduce that in the first approximation, the influence of the other  $i$  can be represented by the two relations <sup>11</sup>

$$(7) \quad \xi' = -\tan L + 3/8 p \cos Q$$

$$(8) \quad \beta' = \frac{\alpha}{2p} K_i \sin 2(A_i - \phi)$$

Naturally, if there are several stars, there are <sup>12</sup>.

(11) In fact, it is easy to show that for the values of  $\theta$  observed during each 14-minute experience, i.e.  $8d0''$ , ( $|\xi| < 0.001$ ) the direct effect (relation 6 of Tableou YC below).

$$(i) \quad \phi' = \frac{1}{\alpha^2 - \beta^2} X_j \cos 2(A_j - S)$$

is much smaller than the indirect effect corresponding to the relationship (relationship 7 of the Tableou M below).

$$(2) \quad S' = \frac{\alpha}{2p} K_i \sin 2(A_j - S)$$

In fact, the order of magnitude  $e_2$  of the direct effect is (relation 6 in Table Yi below)

$$(3) \quad e_1 = \frac{1}{\alpha} \frac{\beta}{\alpha} K_i$$

whereas the order of magnitude of the indirect effect is (relation 6 of Table'iu Yii below)

$$(4) \quad e_2 = \left(\frac{3}{8} p \alpha\right) \left(\frac{\alpha}{2p}\right)^{-At} \\ = \frac{3}{16} \alpha^2 \frac{\Delta t}{2} \Gamma_i$$

hence

$$(5) \quad \frac{e_2}{e_1} = \left(\frac{3}{16} \alpha^2 \frac{\Delta t}{2} K_i\right) / \left(\frac{1}{p} \frac{\beta}{\alpha} K_i\right) = \frac{3}{16} p \frac{\alpha^3}{\beta} \frac{840}{2}$$

i.e. for  $|\xi| < 0, i, \alpha \approx 1/10, p = 3.44$

$$(6) \quad z_3 \approx 44 \quad 2 = 270,9$$

This is a very common circumstance for all disturbances, where the indirect effect outweighs the direct effect.

According to trophics iii and 2V of G A4 above (p. 94-95) and 2 "oöteou X of the § E.3 ci-deasoua (p. 180) we can certainly take  $b$  (in cm)  $< 0.1$ , hence for  $l' = 105$  cm (§ A 1.2, note 6, above):  $\xi < 0.001$ .

On this question, see my Note b l'Académie des Sciences of December 16, 1957, 2 "Adorie du pendule poroconique tt in/l'uence üniaoöire, note 3. In this Note I had indicated for the ratio  $e_2 / e_1$  and the Minimum Value of 130. £ new estimate *eat* better.

(12) In fact, and at first *approx:emotion*, the little *ttfuz s'quement*.



If we limit this to the influence of the Sun and **Moon**, we have

$$(10) \quad \beta' = \left[ \frac{\alpha}{2p} \quad \text{em2 (A, -g)} + \quad \text{sn2 (A-}$$

where # represents the azimuth of the pendulum's plane of oscillation, and A and Aj lea

**azimuths of the Sun and Moon.**

$$(11) \quad K_s = \frac{3}{2} C_s \sin^2 \alpha, \quad C_s = \frac{a}{M_T} \frac{g}{d_s^3} g$$

$$(12) \quad K_j = \frac{3}{2} C_j \sin^2 \alpha_j, \quad C_j = \frac{M_j}{M_T} \frac{g}{d_j^3} g$$

$$(13) \quad C = 0.396 \cdot 10^{-5} \quad C_j = 0.862 \cdot 10^{-5}$$

Remarkably, C and Cj are of the same order of magnitude.  $C_j / C = 2.177$ .

*Table VII - Observed and calculated values of the Moon's influence on the movement of the paraconic pendulum for the 24 h 50 min periodic component.*

6- Formulas (7) and (8) above can be used to determine the theoretical influence of the Moon on the pendulum's movement. *Table YES* shows that for the *approximate mean value*

$$(14) \quad 2R = 5.3 \text{ grades} = 0.0833 \text{ radians}$$

of the 24h 50mn wave, the average variation observed  $\beta'$  corresponds to a variation  $A = 0.0833$  radians in  $24.84 / 2$  hours, hence the average value *observed* (relation 1 in *Table Yff* above).

$$(15) \quad Q\} = 0.186 \cdot 10^{-5} \text{ radians per second.}$$

(13) **The expressions for and K<sub>j</sub>** are quite identical to those given in **note (3)** of my *2<sup>e</sup> fete t o t h e Académie des Sciences* of December 16 1957 *iThorie du Pndult Poroconi9 ne tt Influence L.unisoloire, note 3), except that I have erroneously included in the ez- pre88ion8 of K" and K<sub>j</sub> the multiplying factor 1 + k - h* corresponding to the deviation from the vertical. In fact, the SG cat distance is *totally independent* of luni8o-deformation.

**laire du sol.**

In fact, the factor  $1 + k - h$  (which can be taken as approximately equal to  $2/3$ ) only applies to the deviation from la vertical, whose expression is given by re- laUon (6) of Z'o6/eou Y, p. 127 **below** (see also § F'.1.3 and its note 6 below,

La valeur théorique calculée de la variation  $\Delta\phi'_t$  due à la Lune devant la théorie actuelle de la gravitation se déduit de deux relations deux relations

$$(16) \quad \Delta\phi'_t = \frac{3}{8} p \alpha \Delta \beta$$

où  $\Delta \beta$  (16) représente l'accroissement moyen  $\beta$  du demi petit axe de l'ellipse décrite par le pendule correspondant à la relation

$$(17) \quad \Delta \beta = \beta \Delta t = \beta(t) \frac{\Delta t}{K_t} < \frac{\Delta t}{2p} K_1 \frac{\Delta t}{K_t}$$

déduite de la relation (8) ci-dessus.

deduced from relation (8) above.

Pour une durée  $\Delta t = 14$  minutes = 840 secondes on en déduit (Tableau VII relation 8) 14 minutes = 840 seconds, we deduce

$$(18) \quad |\phi'_t| < 10^{-13} \text{ radians par seconde}$$

(18)  $I < 10^{-13}$  radians per second  
d'où pour le rapport de la valeur observée à la valeur théorique la relation (relation 9 du Tableau VII ci-dessus) to the theoretical value, the relationship

$$(19) \quad |\phi'_o / \phi'_t| > 18,3 \cdot 10^6$$

$$(19) \quad I \gg |S| > 18,3 \cdot 10^6$$

Dans le cas considéré de la périodicité de 24 h. 50 mn. la valeur observée est d'ordre de 180 millions de fois plus grande que la valeur calculée 14 > 15 greater than the calculated value 14 > 15.

(14) Ce chiffre est plus faible que celui indiqué de 50 millions indiqué en 1911 dans ma Note du 19 décembre 1957 à l'Académie des Sciences: *Théorie du Pendule Paragonique et Influence des satellites* (8) (9) mais il reste du même ordre de grandeur.

La différence résulte essentiellement de la considération pour l'anneau de 5R = 0. de diamètre 2R = 10.46 grammes de juin-juin 1955 93ans ma Note du 5.5 décembre 1957 (Tableau I du § A.5.3 ainsi que de 59) p. au lieu de la amplitude moyenne de 5.8. degrés considérée ici (Tableau I du § A.3.1, p. 92).

(15) Les estimations astronomiques considérées dans les calculs des Tableaux VI et VII sont les suivantes en unités CGS: (*Southsonian Physical Tables* in 1936 voir les séries 1951 et 1952, *Annals of the Astrophysical Observatory* 7:29, 730, 731, 734) :

$$\begin{aligned} M_T &= 5,975 \cdot 10^{27} & r_T &= 6,3712 \cdot 10^8 \\ M_S &= 1,987 \cdot 10^{33} & d_S &= 149,5 \cdot 10^{11} \\ M_1 &= 7,343 \cdot 10^{25} & d_1 &= 384,401 \cdot 10^8 \\ \mu &= 6,670 \cdot 10^{-8} & g &= \mu \frac{M_T}{r_T^2} = 981,6 \end{aligned}$$

$$C_s = \frac{M_s}{M_T} \frac{r_T^2}{d_s^3} \quad g = 0,396 \cdot 10^{-13}$$

$$C_1 = \frac{M_1}{M_T} \frac{r_T^2}{d_1^3} \quad g = 0,862 \cdot 10^{-13}$$

Et est constant dimension T<sup>2</sup> (le carré d'un temps).

*An indisputable impossibility*

7- The second fundamental question posed at the beginning of this Section must therefore be answered with absolute *certainty* in the negative. *It is totally impossible to explain within the framework of the currently accepted theory the diurnal lunisolar effects observed, and especially the amplitude  $2 R \text{ constat}4e$ , of the order of 5 grades, of the lunar periodicity of 24 h. 50 mn* <sup>16</sup>.

The theoretical lunisolar influences on the azimuths of the paraconic pendulum according to the current theory of gravitation are *so small*, and the observed influences relatively *so large*, that during visits to my laboratories in Saint-Germain and Bougival, no specialist in the theories of Mechanics and Astronomy contested the impossibility of the paraconic pendulum's azimuth. of such an explanation <sup>17</sup>,

(16) The correction given by the theory of general relativity is either negligible.

The correction  $\tilde{a}$  made to the Wtonian ne potential according to Schwarzschild's formulation corresponds to the coefficient

$$k = 1 - \frac{2\mu M_i}{c^2 d_i}$$

where  $i$  is the coefficient of universal gravitation,  $M_i$  the mass of the Moon,  $c$  the speed of light, and  $d_i$  the distance from the Moon to the Earth (Darmois, 2-o tudorie einsteinnienne de ie ø revitotion. *Let uérifications ezpdrimentofes*, Hermann, 1932, p. 13). We have (for  $c = 3 \cdot 10^{10}$ )

$$\frac{2\mu M_i}{c^2 d_i} = \frac{2 \cdot 6 \cdot 6710^* \cdot 7 \cdot \cancel{E} \cdot 32\cancel{5} \cdot 0}{9 \cdot 10^{20} \cdot 384,41 \cdot 10^8} = 2,83 \cdot 10^{-13}$$

(17) The above calculations are based on Lagrange's method of varying constants. The motion of the paraconic pendulum can be considered as an elliptical motion disturbed by various disturbing forces. In my 1956 general theory of the paraconic pendulum, the method I found most convenient and fastest was that used by Lagrange, which leads to a system of differential equations that can be integrated by successive approximations.

On this method, see Allais, 1956, *Théorie du Pendule Poreconique*, Première Partie, Section D, p. 1 53-1 9B). See also and notably Tisaerand, *l'FroitJ de Wiconiq we Céieste*, Tome 1, *Perturbations det plonëiez d'oprèz la mdl hode de la uorio- tion dev wanstofes orbitraires*, Gauthier-Villars, 1889, p. 173-1881.

FORCES ACTING ON THE FOUCAULT PENDULUM  
RELATIVE TO AXES LINKED TO THE EARTH

classical theory  
 dana le eoa d'une frctieetoire renôligne ! elliptical \*

m = Earth's rotation speed S:  
 suspension point  
 S x y z: Earth-related axes

L: latitude of observation site G:  
 pendulum center of gravity Axis  
 orientation : Sx: South; Sy: East;  
 Sz: Zenith

l = length of simple equivalent pendulum

$$m = x \quad / \ln = y / 1$$

$$p = 2a / T = gil \quad l = SG$$

UT: Earth's gravitational potential N:

$$g = fat \quad UT$$

wire tension

M: mass of the  
 pendulum

$$(1) \quad F = M y$$

$$(2) \quad F = M \text{ gra } UT + N$$

$$(3) \quad \vec{\gamma} = \frac{d^2 \vec{SG}}{dt^2} + 2w \wedge \frac{d \vec{SG}}{dt}$$

hence (1)

$$(4) \quad \frac{d^2 \vec{SG}}{dt^2} = -\vec{g} - 2 \omega \wedge \frac{d \vec{SG}}{dt} + (N/M) \quad g = \text{grady} > T$$

Relation (1) is - *opposed to* be valid only with respect to the Copernican axes, except for a uniform translation.

$$(5) \quad m'' + m = -2m \sin L n'$$

$$(6) \quad n'' + n = 2 \tan L m'$$

Wu- of its de i'a : imm t ce pendue 'dedum de 's) i '6"

$$(7) \quad \theta' = - \tan L \quad (\text{straight path})^1$$

$$(8) \quad \phi' = - m \sin L + \frac{3}{8} p o 9 \quad (\text{elliptical trajectory})^2$$

(IJ Paul Appell, 1953, Traite de 2'fëennique RoionneJie. Gauthier-Vîll&T8, Toire I I, p. 293- 296 , G. Bz-uliât and A. f'och, 1967, 3fdconîgue , Masæen, p.153-156.

The two equations (5) and (6J) result from the fact that, in the first *opprozimiton*, one canpzendzeN-g, a-1 (AppeH1d "p.293)

Q) H. Resal 3Yoiz4 de ñf4cnni9ue Gëndrofe, 1895, Gauthier-Villars, Tome I, p. 130.

See also Jules Haag, *Les mouvements uibrofoirez*, Volume II, Presses Universitaires de France, 1955, § 194, p.194-196. Precession (3/8) p o 8 is due \$ Airy

FORCES ACTING ON FOUCAULT'S PENDULE RELATIVE  
TO LATERAEAXES UNDER THE ACTION OF  
LASTER ATTRACTION i

$M_i$ : masae of the other

$i$ : center of the other

$i$

$U_i$ : attraction potential of the other  $i$

$d_i = SI$

$l = SG$

$T$  = Earth center

$g$ : universal gravitation coefficient

$r_T$  = radius of the Earth

$N$  = force exerted by the support on the pendulum

(1)  $F = My$

(2)  $F = Mgr_a \quad U_T = Mgr_a U_i + N$

(3)  $\vec{d} \frac{d^2 \vec{SG}}{dt^2} = -2m \frac{d \vec{SG}}{dt} + gra \& p U_i$

hence

(4)  $-\frac{SG}{2} = -\vec{g} - 2\vec{\omega} \wedge \frac{d \vec{SG}}{dt} + fat U_i - grad_j U_i + (N/M)$

(5)  $\frac{d^2 \vec{SG}}{dt^2} = -\vec{g} - 2m \wedge \frac{d \vec{SG}}{dt} + (fat U_i; - grad_j U_i) + (fat U_i; - fat U_i; *) \quad (N/M)$

(6)  $\text{bold } U_i; - \text{bold } U_i; = \text{deviation from vertical}$   
*(This term is not involved in the movement of the pendulum)*

(7)  $\text{fat } U_i; - \text{grad}_S U_i; = \text{effective acceleration exerted on the center of gravity } G \text{ of the pendulum}$

Orzfre &-grzuu2eur '2e f'zee4fèrtiozt onguofoire e:ereée pur f'oafre i car fe,penzfufe

(8)  $\vec{\gamma}_i / l = (\text{grad}_G U_i - \text{grad}_S U_i) / l = \mu \frac{M_i}{l} \left[ \frac{GI}{GI^3} - \frac{SI}{SI^3} \right]$

(9)  $|\vec{\gamma}_i / l| \sim \mu \frac{M_i}{d} \quad g = \mu \frac{M_T}{r_T^2}$

(10)  $|\vec{\gamma}_i / l| - C_i = \frac{M_i}{M_T} \frac{-^*}{d_i^3} g$

Sun:  $C = 0.396 \cdot 10^{-6}$

Moon:  $C_j = 0.862 \cdot 10^{-6}$

(1) " On relations (1), (2), and f3) see the references in note (1) of Tobleou 2V.

SUN AND MOON INFLUENCES  
ON THE MOVEMENT OF THE PARACONIC PENDULUM

*Aération anyuf ire due ö r'istre i rTable V)*

$$(1) \quad \vec{\gamma}_i / l = \frac{M_i}{M_T} \left[ \frac{GI}{GI^3} - \frac{SI}{SI^3} \right] \quad SI = d_i$$

$$(2) \quad \vec{\gamma}_i / l = \frac{M_i}{M_T} \frac{r_T^2}{d_i^3} \frac{g}{l} \left[ \left( \frac{SI}{GI} \right)^S \frac{+}{GI} - \frac{+}{SI} \right]$$

The influence of the other  $i$  on the pendulum's motion can be calculated by in-Producing in the second members of relations (5) and (6) of *Tobleou IV* the terms corresponding to the horizontal projections of  $y ; /l$  (relation 2), hence, as a first approximation

$$(3) \quad m'' + \frac{g}{l} m = -2 \text{ tu sin } L \text{ n}' + K, \quad (m \cos 2 A_i + r_i \sin 2 A_i)$$

$$(4) \quad n'' + \frac{g}{l} n = 2 \text{ msn } L \text{ m}' + \quad (m \sin 2 \quad - \text{ncos} 2 \quad )$$

$$(5) \quad K_i = \frac{3}{2} C_i \sin^2 z_i \quad C_i = \frac{M_i}{M_T} \frac{r_T^2}{d_i^3} g$$

$$z_i, A_i : \text{zenith distance and azimuth of the other } i \quad C = 0.39B \cdot 10' = 0.862 \cdot 10^{-13}$$

Given relationship (8) in *Table IV*, we derive from (3) and (4)

$$(6) \quad \beta' = -m \sin L + 8 p r_i Q + \frac{1}{p} \frac{\alpha \beta}{Q^2} K_i \cos 2 (A_i - \beta)$$

$$(7) \quad \beta' = \frac{\alpha}{2p} K_i \sin 2 (A_i - \phi)$$

In view of the orders of magnitude and the fact that in each 14-minute experiment we have  $g < U1000$ , relations (6) and (7) give as a first approximation (note 10 of § 2.5 above)

$$(8) \quad \beta' = -m \sin L + 8 p r_i g$$

$$(9) \quad \beta' = \frac{\alpha}{2p} K_i \sin 2 (A_i - \beta)$$

Sources for T'nötenux Y et Yi : Allais, September 1966, T'fidorie *du pendule p'roconique* and, May 1996, *Sur les y riodicit'c lunicolairec dti pendule paraconique*.

The  $K_i$  terms of relations (6) and (7) are deduced from the  $K_i$  terms of relations f3) and f4) from 2'ntifenu II of *Anneze I* of my *Théorie du Pendule Paraconiquet* de September 1986 (my Note of May 1996, § 9, p. 12).

**CALCULATED AND OBSERVED VALUES  
OF THE INFLUENCE OF [A LUNE  
ON THE MOVEMENT OF A PARACONIC PENDULUM  
pour la période lunaire diurne  $T = 24,84 \text{ h} = 24\text{h } 50\text{mn}$**

According to Table i (§A.3) for the period  $T = 25 \text{ h}$ . representative of the period  $T = 24\text{h } 50\text{mn}$  the mean amplitude is

$$2R = 5.3 \text{ gradea} = 5.3 \text{ z} / 200 = 0.0833 \text{ radians}$$

The corresponding average variation observed over a half-period  $24.84 / 2 = 12.42 \text{ h}$  is as follows

$$(1) \quad \ddot{\xi} = 0.0833 / 12.42 \cdot 3600 = 0.186 \cdot 10^{-5} \text{ radiana per second}$$

We have (7) obfeou M, relations 8 and

$$(2) \quad 9' = -m \sin L + \frac{3}{8} \beta \quad (p = gd/l = 3.4d)$$

$$(3) \quad Q'_{=KG} \sin 2(A_u - \xi)$$

After an experiment lasting  $Et = 14 \text{ minutes} = 840 \text{ seconds}$ , we have  $^2$

$$(4) \quad \xi - \frac{\alpha}{2p} K_1 \Delta t \frac{\sin 2(A_1 - \phi)}{2}$$

so that for the mean value  $9$  of  $\xi$  ( $\bar{\xi}$  = mean value of  $\xi$ ) we have

$$(5) \quad |\beta| \sim \frac{\alpha}{2p} K_1 \frac{At}{2} \sin 2((\bar{\xi} - 9)) \Gamma < \frac{\alpha}{2p} K_1 \frac{At}{2}$$

For the theoretical mean value  $J \xi$  of the Moon's influence, we have

$$(6) \quad |\xi'| < \left( \frac{3}{8} p \bar{\alpha} \left( \frac{\alpha}{2p} K_1 \frac{\Delta t}{2} \right) \right) = \frac{3}{16} \bar{\alpha} \frac{At}{2}$$

BOlt QOlr ii - U10

$$(7) \quad \xi \{ < 0.788 \quad X_j X_j = \frac{3}{2} \sin^2 \delta C_j \quad C_j = 0,862 \cdot 10^{-1}$$

or

$$(8) \quad \xi \{ < 0.788 \frac{3}{2} C_j = 1.18 \cdot 0.862 \cdot 10^{-1} = 1.018 \cdot 10^{-1}$$

**Value oâarc'ée / Value tlafoz-fique \***

$$(9) \quad \frac{\phi'_o}{\phi'_t} > \frac{0,186 \cdot 10^{-5}}{1,018 \cdot 10^{-1}} = 18,3 \cdot 10^{-5}$$

(1) \*The index  $l$  corresponds to the Moon.

(2) The average value of a quantity  $h$  over the 14 minutes of movement of the pendulum is denoted by the notation  $\bar{h}$ . The absolute value of  $h$  is designated by the notation  $|h|$ .

(3) Note that  $r_T / l = 6.37 \cdot 10^6 / 83 = 7.67 \cdot 10^4$  where  $r$  is the radius of the Earth and  $l$  the length of the equivalent simple pendulum (§ A.1.1 above).

J.- Oee compoo nfe pdrzoóiques ron expfícoÓJev por deap 4noménee **as**

If the observation s6riesa of the paraconic pendulum contain effective diurnal p6riodic componentsa of p6riodes of 24 h. and 24 h. 50 mn. *stotistiquiernent significatives*, et si les effets p6riodiques ainsi constat6a *ne peuvent elre consid4r4s* comme rdaultant de la th6orie actuelle de la gravitation, *est-ce que ces effets p4riodiques peuvent 4tre at- tribu6c 6 une in/fuence directe ou indirecte d'un phnombne p4riodique* connu. This is the third fundamental question.

### *Comparative harmonic analysis of different phenomena*

1- To answer this question, I carried out a harmonic analysis for the same periods of 24 h. and 24 h. 50 mn. of the following phenomena: Temperature at the laboratory and at Le Bourget, Atmospheric pressure at the la- boratory and at Le Bourget, Magnetic declination, K-numbers of Bartels magne- tism, and Wolf numbers (solar activity), and I compared the results of this analysis with those corresponding to the azimuth of the paraconic pendulum, both from the point of view of amplitude and phaaea.

If one o f these phenomena could be considered as the cause of the observed movements of the paraconic pendulum, we would observe: 1° an at least approximate *concordance of phoses* between cause and effect; 2° the *same periodic structure*, and in particular the same ratio of amplitudes for the 24 h. period and the 24 h. 50 min. period, for which the 25 h. period can be validly substituted as a first approximation, for ease of calculation.

*However, this double circumstance does not apply to any of the phenomena analyzed.* It should also be pointed out that the graphs representing these phenomena show no visible con- nexion with the graphs representing the azimuths of the oscillation plane of the paraconic pendulum.



By way of illustration, *Graphs XII and XfZf* show, for the period June-July 1955, the comparative results of Buya-Ballot's analysis for 24- and 25-hour periods, for the azimuth of the paraconic pendulum and *the temperature measured at Bourget*<sup>1</sup>, on the one hand, and for the paraconic pendulum and *the magnetic declination, on the other*.

registered in ChamboT1-lEt-ForÔt<sup>1</sup>.

*Graphs XII* show, firstly, that there are phase differences between the adjustment sinuoids for the 24- and 25-hour periods of azimuth and temperature, and, secondly and more importantly, that *the 25-hour wave has a much greater relative amplitude point in azimuth of the paraconic pendulum than for the temperature*.

*Graphs XIII* show a remarkable phase correlation between pendulum azimuth and magnetic declination for both the 25-hour and 24-hour periods. However, for both periods, *the relative amplitudes are very different*. *T'nffeou VIII* shows that the same is true for the November-December 1954 period<sup>2</sup>. Such phase concordances cannot be attributed to chance\*.

In fact, and for at least one of the phenomena examined, the lunar component of période 25 h., representative of the wave of 24 h. 50 m., has an amplitude comparable to that of the 24 h. wave\*. *This periodic structure is a characteristic of the azimuths of the paraconic pendulum and is a confirmation of the hypothesis of the existence of a periodicity in the variation of the magnetic declination*.

- (1) This is the closest observatory to the IRSID laboratory in Saint-Germain.
  - (2) Allais, 1958, *Anomalies du mouvement de la pendule paraconique*, p. 31-32.
  - (3) It is very remarkable that this phase concordance is not found for fortnights and weeks, as one would expect in the hypothesis that this phase concordance would correspond to a phenomenon real, as the different relative amplitudes of the waves correspond to azimuths and to the magnetic declination for 24 h. **§25h.** should give phase differences when the Buys-BalloC harmonic analysis covers periods too short for these waves to be separated.
  - (4) See in particular *Table I* in § A.3.1. above. For the seven monthly series, the average  $R_{q5} / R_{q4}$  ratio is equal to 1.39.
- ( Voir ma Note du 16 décembre 1957 à l'Académie des Sciences, "Périodicité de la pendule paraconique et l'influence lunisolaire".  
 Voir tout particulièrement mon mémoire du 21 avril 1958, *Anomalies du mouvement de la pendule paraconique*, p. 23-33.

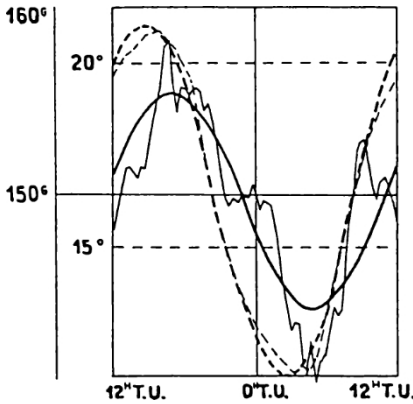
Graphique XII

COMPARAISON DES RESULTATS  
D'ANALYSE HARMONIQUE

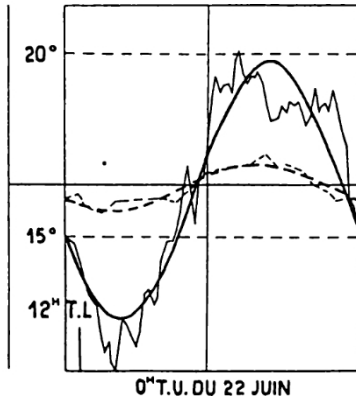
24<sup>h</sup> Azimut  
AZIMUTH AND TEMPERATURE  
June/July 1955

COMPARAISON/SON RESULTATS/AVEC/LES/DE/LE/LOT  
BAI/OU/POUR/LE/24/ET/25/ET/TEMPERATURE/ET  
POUR/LES/PERIODES/DE/24/ET/25

24<sup>h</sup>



25<sup>h</sup>



Légende

- Moyenne des observations
  - Ajustement sinusoidal
  - Azimuts
  - Temperature
- B: Le Bourget observations  
B: observations du Bourget

Source: Chart VA 3 a from my Conference of February 22, 1958.  
Source: Graphique VA 3 a de ma Conférence du 22 février 1958.

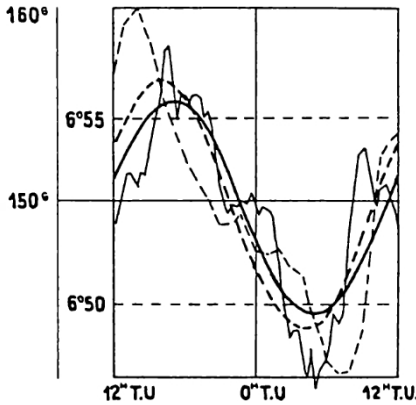
Graphique XIIKIII

COMPARAISON DES RESULTATS  
D'UN AZIMUTH ET D'UNE DECLINAISON

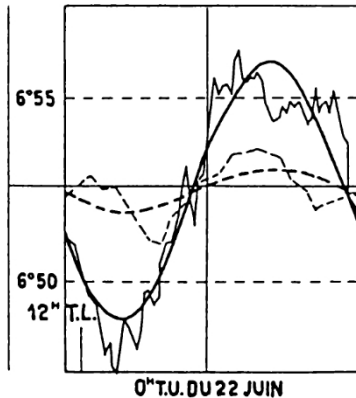
24<sup>h</sup> AZ 285h  
AZIMUTH AND MAGNETIC DECLINATION (G)  
JUNE 24 JULY 1955

COMPARAISON DES RESULTATS DU FILTRE DE BUTYLS BALIOT  
POUR L'AZIMUTH ET LA DECLINAISON MAGNETIQUE  
MAGNETIQUE POUR LES PERIODES DE 24 ET 25

24<sup>h</sup>



25<sup>M</sup>



Legende

- Moyenne des observations
  - Ajustement sinusoïdal
  - Azimuts
  - Déclinaison magnétique
- Observations de Chantou de Foch

## PARACONIC PENDULUM AZIMUTHS AND MAGNETIC DECLINATION

OBæwafþons deJvouemóre-Zàécemóre 1954 eç de ' uin-J'uçffeç 19fi5 *Pézfiodm::ítéþ*  
*de M et 2'-fi læmz-ee*

Periods		Azimuths in grades			Sexagesimal minutes declination			$\frac{2R}{D'} / \frac{2R}{D}$	H-H'
		2R	2MD	H	2K	2RYD	H'		
28h.	Nov-Dec 1954	12,87	0,080	5h 52 mn	0,578	0,037	4h 33 mn	0,4B1	1h 19 mn
	June - July 1955	14,01	0,155	15h 58 mn	1,205	0,056	16h 02 mn	0,362	- 0h 04 mn
24 h.	Nov. - Döc. 1954	10,34	0,064	13h 17 mn	2,67	0,17	11h 36 mn	2,65	1h 41 mn
	June - July 1955	11,66	0,129	16h 54 mn	6.Çi	0,32	15h 56 mn	2,51	0h 58 mn

*Agenda:* 2n = amplitudes of periodic components in grades; 2R' = amplitudes of periodic components in sexagesimal minutes.

D and D' = differences between the largest and smallest values of the series considered.

H and H' represent the times of the wave peaks in lunar and solar time. See *Grophiquee XIII*

*Source:* My memoir of April 21, 1958 (revised January 28, 1960), *Anomalies in the motion of the paraconic pendulum with anisotropic support*, p. 32.

*lunisolar variations of the vertical and the movement of the paraconic pendulum*

2- Lunisolar variations in vertical <sup>6</sup> have often been put forward as a possible explanation for the constated effects. In fact, this is not the case. *By themselves*, and as I have already indicated, *variations in the vertical*

(6) At the suspension point S the component of the vertical direction corresponding to a star i is represented by the difference

$$(1) \quad \vec{f}_i = \text{grad}_S U_i - \text{grad}_T U_i$$

of the gradients of the potential  $U_i$  at points S and at the center of the Earth (relation 6 of *Tableou Y* above). Assuming the Earth to be inddformable, we demonstrate that the horizontal  $f_{ij}$ t and vertical  $f_{ij}$ p components of  $f_j$  have the following expressions:

$$(2) \quad f_{ih} = \frac{3}{2} \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} g \sin 2q$$

$$(3) \quad f_{ig} = 3 \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} g \left( \cos^2 z_i - \frac{1}{3} \right)$$

$M_i$  and  $M_T$  are the masses of the celestial body i and the Earth.  $r_T$  is the radius of the Earth and  $d_i$  is the distance from the Earth to the celestial body i.  $g$  is the acceleration of gravity and  $z_i$  is the zdnithal distance from the other i.

If we take into account the Earth's deformation, the deviation from the vertical is equal to the ratio

$$(4) \quad \delta_i = \frac{f_{ih}}{g} - \frac{3}{2} (1 + k - h) \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} \sin 2q \quad z_i | 1 + k - h - \frac{2}{3}$$

The coefficients  $k$  and  $h$  are Love numbers corresponding to the deformation of the Earth under the action of star i. Thus

$$\frac{\delta_i}{\sin 2q} = \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} \left( C_i - \frac{r_T}{g} \right) \quad C_i = \frac{M_i}{M_T} \frac{r_T^2}{d_i^3} g$$

The values of  $C_i / \sin 2z_j$  for the Sun and Moon are in rodions and sezogdsimoid secondes (§ 2.5 and note ld to § 2.6 above).

$$(6) \quad \frac{\delta_s}{\sin 2q} = C_o \frac{^oz}{g} = \frac{0,862 \cdot 10^{-13} \cdot 6371 \cdot 10^3}{981,8} = 2,570 \cdot 10^{-10} \text{ "d. - s, s0 lo}^{-3} \text{ "}$$

$$(ir) \quad \frac{\delta_l}{\sin 2q} = C_l \frac{r_T}{g} = \frac{0,862 \cdot 10^{-13} \cdot 6371 \cdot 10^3}{981,8} = 5,590 \cdot 10^{-10} \text{ rad.} = 11,5d \cdot 10^{-10} \text{ "}$$

See in particular Schureman, 1941, *Mon nel of iformonie Arret ycic ond Prediction of Tides*, p. 14, and Stoyko, 1947, *L'otraction lunisoloire ct lec pendulec*, Bulletin astronomique, Tome XII I, (p. 1-36), p. 3, relation 2, p. 6, and p. 30.

It is worth noting that for the Sun and Moon, the agreement between the values observed and calculated deviations § from the vertical is Pres remorquoëte (see in particular Stoyko, 1947, id., p. 31).

*have no influence on variations in the oscillation azimuth of the paraconic pendulum!*

*variations in the horizontality of the support and the  
movement of the paraconic pendulum*

3- At the beginning of May 1957, it was argued that the horizontal position of the surface supporting the pendulum could change during the course of the experiments, due in particular to the thermal deformation of the building as a result of changes in sunlight during the day, and that if the surface of the support took on a slight inclination, the pendulum's plane of oscillation would return to the position of the vertical plane containing the line of greatest slope of the surface supporting the pendulum.

On May 2, 1957, one of my correspondents, who was highly competent in this field, wrote to me: "*How can we not envisage that the variation in the position of your support will not reach one minute, given the realistic installation?*"

In fact, eighteen months earlier, on December 15 and 16 1955, I had already carried out experiments on the effects of support inclination on the movement of the paraconic pendulum, showing that these effects were quite negligible. In view of the quality of my correspondent, I **immediately** carried out new control experiments on **the effects of variations in** the horizontality of the support, in **May** and June 1957, w h i c h confirmed the results of my 1955 experiments.

- To determine the influence of support inclination and to eliminate any systematic periodic influence, I carried out successive experiments with the surface of the support alternately horizontal and inclined.

(7) See above § B.2.t, p. 120-121.

t8) Remarkably, the pendulum depends on the outside world only through *its suspension* by the supporting **surface**. The influence of the outside world **can** therefore only agzr by the inclination of this surface.

t9) See my very detailed *Nett* of 19 'anvigr 1959 to the Académie des Sciences "O4termtnoñion *expérimentale de l'influeitce ae l'intlinaison de lo surfote pononte sur le mouvement du pendule paraconique à support anisotrope*".

Let  $i$  be the inclination of the support in sexagesimal seconds and  $J$  the angle made with the north-south vector by the downward projection of the line of greatest slope. The azimuth of the oscillation plane is denoted by  $\xi$  (*Graph XIV,2*).

*Graphs XTV,I ; XV,III ; and XV,TP* represent the observations made on December 15 and 16, 1955 with the paraconic pendulum <sup>10</sup> for 48 14-minute experiments, the odd-numbered experiments corresponding to a horizontal surface and the even-numbered experiments to an inclined surface, the balls being changed

after each experiment, for inclination  $i = 2064''$  and angular deviation  $\xi - J = 50$  gr.

Although the individual curves are significantly different, mainly due to the disruptive influence of the beads (see *Graph XTV,I* representing the results corresponding to experiments 5 to 12), the average curves for the 24 odd and 24 even experiments are not very different (*Graph XV,III*). The same is true of the averages calculated for December 15 and 16 (*Charts XV,TVA and RGB*).

However, if we consider the mean curves of all the morning and evening experiments without taking inclination into account, we find two very different mean curves (*Graph XV,IVC*).

Similar results were obtained on May 22 and 24, 1957 for  $i = 1032''$  and  $J = 25$  grades.

These results show that the systematic influence of the epoch, a few hours away, on the movement is much greater than the influence due to an inclination of the order of 2000 or 1000 sexagesimal seconds. Remarkably, in all cases, the tangents at the start of the mean curves correspond exactly to the Foucault effect.

- In addition, from June 19 to 23, 1957, I carried out a continuous series, day and night, of *double-chained* experiments lasting 14 minutes. For the odd-numbered experiments, the surface was horizontal; for the even-numbered experiments,  $i = 1032''$ ,  $J = 396$  gr. The balls were changed for each experiment. Each odd-numbered 14-minute experiment began in the azimuth at which

(10) Pendulum used during the continuous experiments of November-December 1854 and June-July 1855.

the previous odd-numbered experiment, and similarly for even-numbered experiments. In this way, we had two independent aerial observations (*Graph XP, V*). The differences in azimuths are due to a slight systematic influence of the surface inclination (1.72 grades for 1032" for  $\delta - J = -29.7$  gr) and to a preponderant accidental effect of the balls.

For these two series, the Buys-Ballot filter (*Graph IV*, Yf9 for 24 h gives two curves with the same appearance, the effect of the balls being practically eliminated and the amplitude of the periodic effect increasing with the inclination.

In addition, eight double-chained experiments, each lasting 10 h, carried out in mini-yuin 1957, showed a systematic influence of 2.77 gr for a double inclination of 2064" and  $\delta - J =$

- 33.77 gr.

From all these experiments, and taking accidental errors into account, we can conclude that the influence of inclination is approximately proportional to  $i$  and about 1.5 gr per 1000", for  $\delta - J$  little different from - 30 gr.

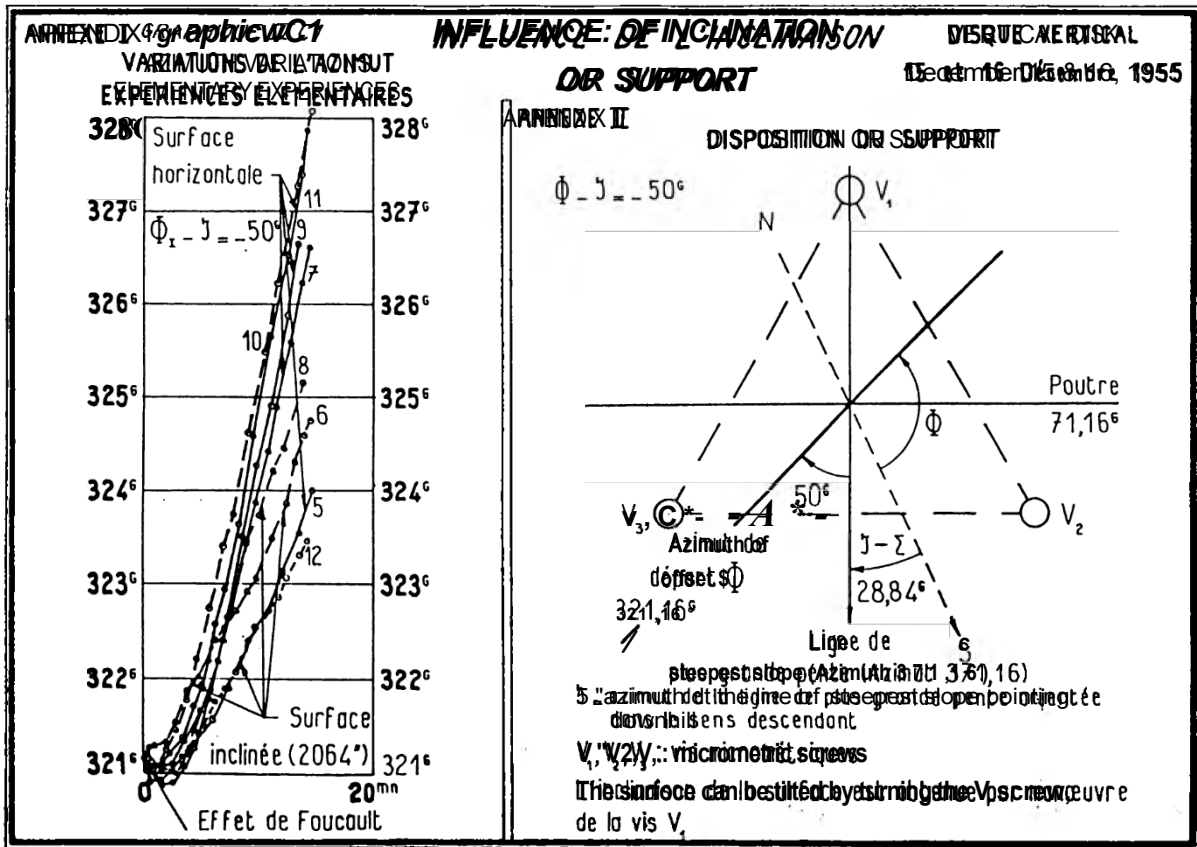
- These numbers can be usefully compared with the results obtained in Bougival and Saint-Germain in July 1958. The 2R amplitudes of the effects corresponding to the 24 h 50 min filter were of the order of 2.15 grades, while the daily variations in inclinations never exceeded 8" aexagesimal seconds 11, 12

- From all these indications, it follows that the comparison of experimental results obtained by varying the inclination of the support surface of the ball of the paraconic pendulum, either in the course of Idchera in a given azimuth, or in the course of doubly chained experiments, shows that *the periodic variations in azimuth over time cannot be considered as resulting from variations in the inclination of the support with respect to the vertical.*

(11) **Au cours** de ces expériences en Bougival et Saint-Germain en juillet 1958 (voir **Section C** ci-dessous, p. 142-161) les inclinaisons de surface ont été relevées avec une précision de dix minutes par des niveaux de précision de l'Institut géographique national.

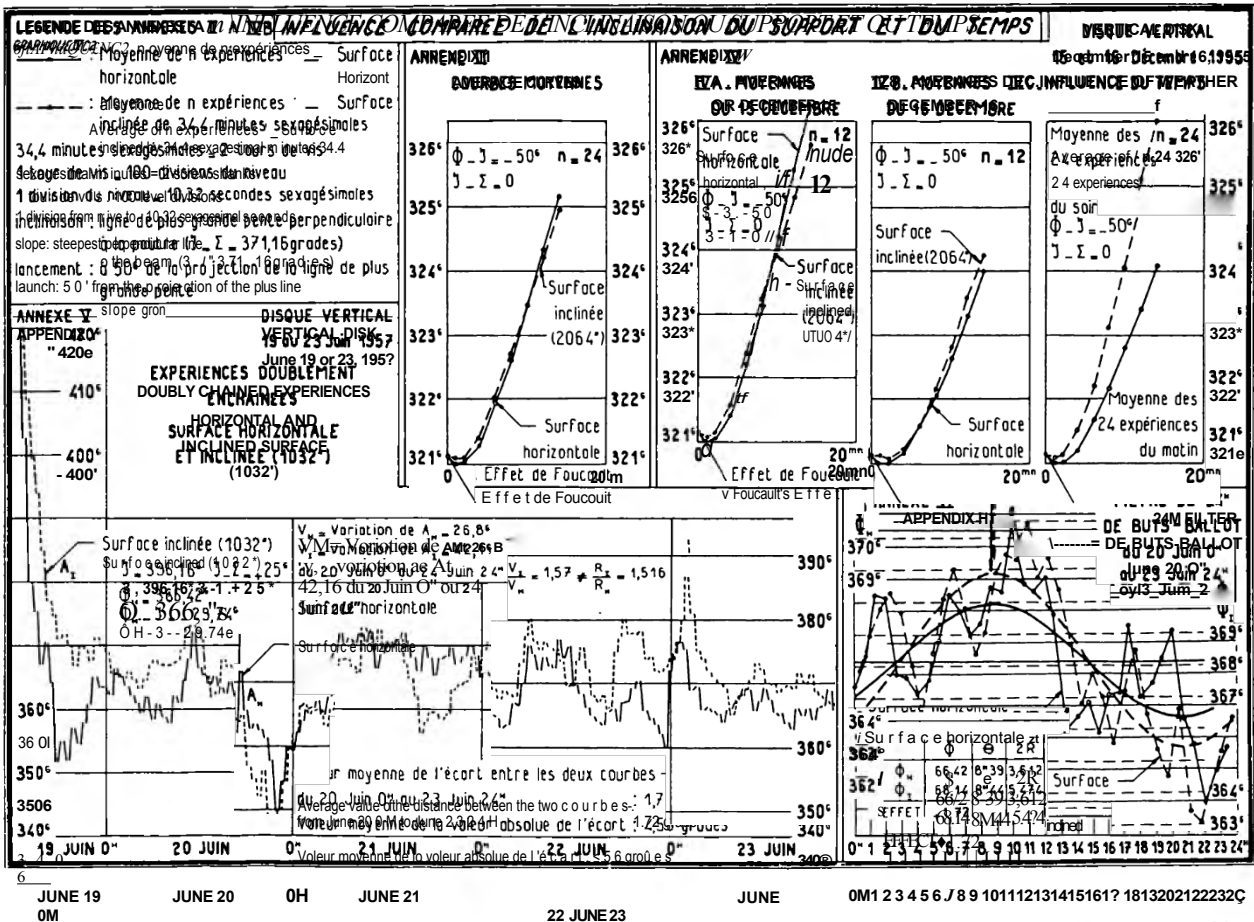
(12) Rappelons que les variations théoriques de la verticale due à l'action lunaire sont de l'ordre du centième de seconde sexagésimale (§ 3.2 ci-dessus).





Source: Ma Note du 19 janvier 1959, Comptes Rendus de l'Académie des Sciences, T. 248, p. 359-362 (photographic reproduction).

Source: Ma Note du 19 janvier 1959, Comptes Rendus de l'Académie des Sciences, T. 248, p. 359-362 (reproduction photographique).



Source : Ma Note du 19 janvier 1959, Comptes Rendus de l'Académie des Sciences, T. 248, p. 359-362 (reproduction photographique).

*Source: Ma Note du 19 janvier 1959, Comptes Rendus de l'Académie des Sciences, T. 248, p. 359-362 (photographic reproduction).*

*No direct relationship between the azimuths of the paraconique pendulum and known phenomena*

4- All in all, we must conclude that the **diurnal** lunisolar components of 24 h. and 24 h. 50 œzt., **and especially the former**, whose existence must be considered as *very real*, and which are *totally inexplicable* in the case of the current theory of gravitation, *cannot be considered as the result of t h e direct or indi- rect action of a known phenomenon, and this in all certainty.*

The third fundamental question posed at the start of this Section B<sup>1</sup> must **therefore** be answered with *absolute certainty* in the negative. *This is a new phenomenon, completely inexplicable within the framework of the currently accepted theory of gravitation.*

## C

THE CRUCIFIED EXPERIENCES OF JUNE-JULY 1958  
**DE SAINT-GERMAIN-BOUGIVAL**

*1-- Leø cšezzx faóraVočres de žšaznf-Germažn ez čfe Aougiuaf*

At the end of 1957, in agreement with Albert Caquot and Pierre Tardi, I drew up a research program for the first semester of 1958, with a view to obtaining a grant from the Centre National de la Recherche Scientifique.

This program included the repetition of experiments on the paraconic pendulum in a laboratory at a certain depth below the soil, either in an underground quarry or in an underground mine, so as to avoid any imperfect wind or temperature disturbance.

Finally, in early 1958, a site was found in a disused underground quarry in Bougival, the "*Blanc Minéral*" quarry.

*The set of experiments planned was crucial in confirming or refuting the existence of a lunar component with a period of 24 h. 50 min. in the ozimuths of the paraconic pendulum with anisotropic support, of an amplitude inexplicable within the framework of gravitational theory.*

Following this project, and with the help of the *Centre National de la Recherche Scientifique* (CNRS) and the *Comitt d'Action Scientifique de la Défense Nationale* (CASDN), in June-July 1958 I carried out two series of *simultaneous continuous* experiments with two *identical* devices in my laboratory in Saint-Germain and in a new laboratory in Bougival, installed in a gallery in a part of the Parisian town of Saint-Germain.

**of the then-abandoned "Ble nc Minëra l" underground quarry**, devoid of any previous development, with 57 m. of clay and chalk overburden, and about 6.5 km. from Saint-Germain. The distance to the free surface (counted **horizontally**) was around 800m\*.

After a one-month trial period in June <sup>2</sup>, two simultaneous continuous experiments were carried out in the two laboratories at Bougival and Saint-Germain, lasting 30 days from July 2 0 h TU to July 31 23 h 40mn TU 1958 <sup>3</sup>

These two *crucial* *fout à doit* experiments showed that the periodic anomalies previously highlighted *were observed again*, and that, in both laboratories, they presented remarkably corresponding periodic structures\*.

(1) The installation of the BOU V81 ~~été~~ laboratory was remarkably carried out by the IRSID workshop with pdfabHquës materials and with the installation of a rigid metal pendulum support identical to that at IRSID (see § E.3 below). Both pendulums were *identical* to the one described in § A.1 above.

The support was installed on massive reinforced cement pillars interwoven by a system of metal girders.

The anisotropy of the substrate proved to be *comparable* to that of the IRSID (see § E.3.3 and 'Pot'Rou X below, p. 180).

(2) In particular, it was necessary to organize two teams of seven observers working in day and night in both laboratories.

(3) It was not without a certain apprehension that I approached these two crucial experiments, for as much as I was absolutely certain of the existence at IRSID § 2 meters below ground of a diurnal lunar periodicity of 24 h. 50 min. of an amplitude incompatible with the accepted theory of gravitation, it was impossible for me to predict what effects would be observed at a depth of 57 m.

(4) Readers may find it useful to refer to the following two *notes* I have prepared for you.

tées § l'Académie des Sciences on November 3 and December 22, 1958.

- *Nouvelles expériences sur le Pendule Poroconid ue b support anisotrope.*  
CR. CS., T. 247, 1958 p. 1428-1431.

- *Structure périodique nes mouvements du Pendule Poroconi9ue d Bougiuol et Saint-Germain est Jui/let J 958.* C.R.A.S., T. 24 7, 1958, pp. 2284-2287.

2-- *la afin otzaz epézfiodiqzze dizzi ne dee des séries d'obseroatlozzs enchcfiz fier che &ouguiauf and che fiafnY-Germain*

*Charts XVI to XXIII and Table IX* below represent the essential results of the harmonic analysis of the two series of chained observations at Bougival and Saint-Germain, as published in my two *Notes* of November 3 and December 22, 1958 to the Académie des Sciences.

*The periodic component of 2d h. 50 mn*

1 - *Graphs XVI and XVII* show the results of harmonic analysis obtained using the Buys-Ballot filter method for periods of 25 h. and 24 h. 50 min from continuous chained observations made *simultaneously* in both laboratories during one month, day and night, from July 2 0h. UT to July 31 23 h. 40 mn UT, under *identical* conditions to those of my previous experiences in June-July 1955 1 2

*Graphs XVI and XVII* compare the results obtained in the two laboratories at Saint-Germain and Bougival. Whether using the 25 h. filter or the 24 h. 50 min. filter, the two adjustments have very little difference in amplitudes, and *very remarkable* phase concordances.

In fact, these concordances are on the whole *more marked* for *Graph AVII*, corresponding to a period of 24 h. 50 min than for *Graph XVI*, corresponding to a period of 25 h.

(1) § A. 1 above.

(2) TU notation corresponds to universal time. The notation TL corresponds to lunar. The passage of the moon over the meridian corresponds to 12 hours TL.

*Le isrophique XXI* a été publié dans ma *Note du 3 Novembre 1958* b l'Académie des Sciences. **Les résultats** correspondant au *Grophiqque XVII* ont été publiés dans ma *Note du 22 de cembre 1958*, ainsi que dans la *Note Com plém ento ire* de mon Mémoire de 1958, "*Doit-on Reconsidérer les fois de lo Gravitation?*"

I think it's important to point out that, back then, we didn't have any of the computing resources available today. Calculations were made by hand, using an **electric** machine.

Naturally, it was much quicker to apply the Buys-Ballot filter with data from 20 minutes to 20 minutes for a 25-hour period. In my *memo of November 3, 1958*, I immediately presented the results for a 25-hour period.

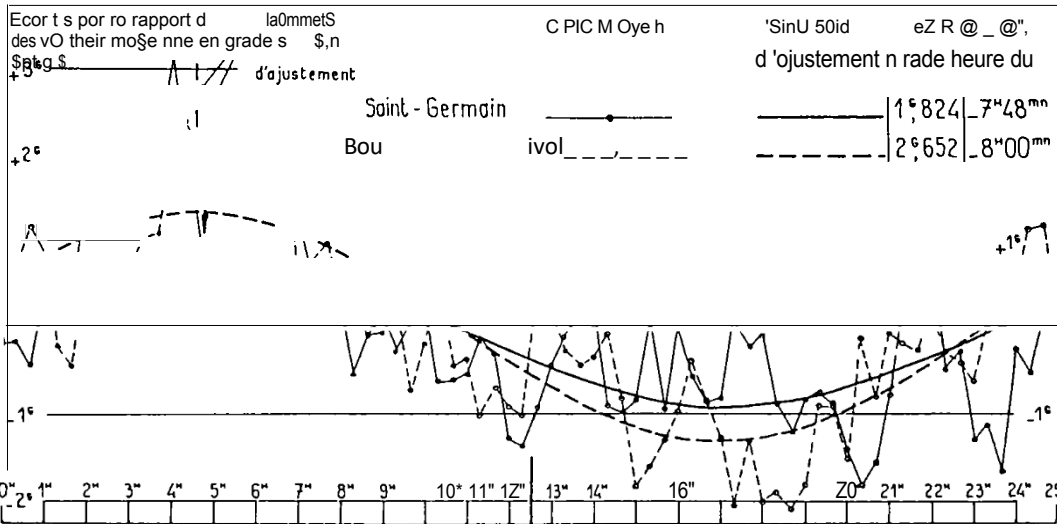
**Dana** the following weeks I had the Buys-Ballot filter applied for a period of 24 h. 50 min by interpolating the data from 10 m to 10 min.

*The comparison of results obtained in the two calculations is all the more Entificotiué.* The concordance of the results is better when the period of 24h. 50mn instead of the approx imative period of 25 h.

# COMPARAISON DES RESULTATS D'ANALYSE HARMONIQUE DU FILTRE DE BUYS - BALLOT

EXPERIENCES DE JUILLET 1958  
LABORATOIRES DE S'GERMAIN ET BOUGIVAL

f d'c' Z y "uille 0 ^FU au 37 /tzi7lel Z3'40'''TU)



*Passage de la lune au méridien du jour moyen: 12<sup>m</sup> TL*

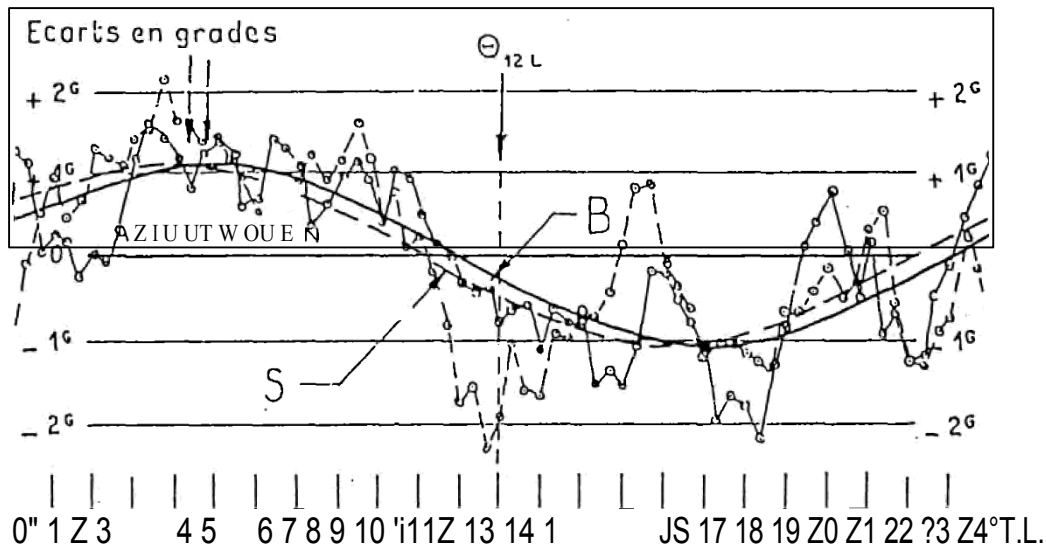
Source : Note du 3 novembre 1958 d'Atad6mie des Sciences, Nou uelles expriences sur le pendule poroconique  
b support onisoirope, CRAS, T. 247, p. 1429



EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAIN

Boys-BoJlot filter "reeuttat" for id filter - 50 mn.-.

&



L'égtnde

BOUGIVAL	2,17	- 7 h. 23 mn.
SAINTE-GERMAIN	2,10	- 7 h. 55 mn.

———— DOUGIVAL  
 Fine lines: medium cycles

----- SAINT-GERMAIN  
 Traits fins: Sincis"les l'a jcisimnt

Source: Allais, 1958, Doit-on recon sidérer fes fois de to gmUifofion, Nort Complémentaire annexée.

HANGER

I.C.

*The periodic structure of the two series of enchanted observations  
from Bougival and Saint-Germain*

2- Table X gives the overall numerical results of the analyses carried out. They are shown in *graphs XVI to XXIII*.

The notations B and S correspond respectively to the two pendulums at Bougival and Saint-Germain. The notations B + S and B - S correspond to the half-sum and half-difference of the observed azimuths.

The 30 *days of observation* were divided into four elementary periods of 7, 8, 8 and 7 days, designated by the numbers 1, 2, 3 and 4. The notations 1 + 2 and 3 + 4 designate the first and second fortnights of the month, and the notation 1 + 2 + 3 the second fortnight.  
+ 4 the whole month.

8 represents the time of the apex of the adjustment sinusoid in universal time; 0 - 012L represents the time of the apex in lunar time, taking as its origin the time at which the Moon passed over the meridian 0 i2L-.

It is very remarkable that, on average, the amplitude of the 24 h. 50 min. is approximately double that of the 24 h. wave at Bougival and Saint-Germain éfn òfent

*During the month of July 1958, the effects of the 24 h CO mn period are more or less the same in both laboratories. However, the effects of the 24 h and 12 h periods were of opposite sign.*

*Graphs XIX to XXIII* show that this structure is also observed in each fortnight considered in isolation 4t 5t

(3) The average of the two ratios  $2.174 / 1.394 = 1.559$  and  $2.106 / 0.776 = 2.714$ , i.e. 2.136, is only - sibly equal to the ratio  $C_j / C. = 2.177$  (§ B.2.5 above, p. 123).

(4) According to *Table iX*, if we consider the 24 h. 50 min wave, the half-sum  $(B + S) / 2$  has an amplitude of 2.140 grades, while the half-difference  $(B - S) / 2$  has an amplitude of 0.034 grade.

For the 24 h. wave, half-difference  $(B - S) / 2$  and half-sum  $(B + S) / 2$  have amplitudes of 1.086 and 0.310 grades respectively.

It can be seen that the *half-com mc tB + SA 1 2* contains virtually no compounds with a period of 24 h. whereas the half-difference  $(B - S) / 2$  contains virtually no compounds with a period of 24 h. 50 min.

Applying the 25 h Buys-Ballot filter to the half-difference  $(B - S) / 2$  is equivalent to application of f' a 25 h. filter to a 24 h. wave.

( OR One circumstance remains totally *unexplained* to this day: *phosis* oppositions for 24 h. and 12 h. waves at Saint-Germain and d Bougival.

f6) These *average* concordances are all the more remarkable in that the day curves *nolières* azimuths at Bougival and Saint-Germain *differed* quite *significantly*.

*Que d'ensemble*

3- These results, *essential as they are*, show that ...:

- n. that the existence of a 24 h 50 min periodicity *is not due to an accidental cause*;
- b. that it is also true when there is practically *no thermal variation*, as was the case in Bougival;
- c. that it can also be seen when the laboratory's external structure *is sheltered from all external disturbances*, as was the case in Bougival.

*These two series of crucial and fundamental experiments totally swept aside all previous objections to my experiments with the paraconic pendulum as to the existence of a diurnal lunar periodicity of 24h 50mn of an amplitude totally inexplicable within the framework of the current theory of gravitation!*

*They have fully confirmed that there are anomalies of a periodic nature in the motion of the dissymmetrical paraconic pendulum with an anisotropic support, which are inexplicable within the framework of the currently accepted theory of gravitation, and which, in the present state of discussion, cannot be linked to any known phenomenon.*

( For an illustration, see Jean Goguel's *note* à l'Académie des Sciences "*Observationz à propos du pendule paraconique*", (CRAS, Tome 246, n° 16, April 21, 1958, p. 23d0 to 23d2).

These *Notes* essentially attributing the effects observed to *bits* - oscillations. This is typical of the *many* *grounded* objections I've been<sup>4</sup> presented with.

It is based on vaguely formulated hypotheses, the precise meaning of which is difficult to grasp. Moreover, there are no theoretical calculations or numerical applications to justify them.

But this is just one of *many* examples. On December 3, 1956, Henri Villat (1879-1972), professor of fluid mechanics at the Faculté des Sciences and president of the Section de Mécanique at the Académie des Sciences, told me: "*Replace doc billes d'acier p'ir dec billes d'ogale, et nous verrez tout*

However, when I *changed* to visit my laboratory at IRSID, he replied - *invitation* dait: "Yppez-nous .... Quefte *invitation* it pour moi un *dplacement* d Saint -Cremain, cor Je ne suic pas un *experimental*teur".

EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAIN

overall results

PERIOD FILTER	PENOUTE	PERIODE TEMP3	2R	$\ominus$	$\ominus - \ominus_{12L}$	PERIOD FILTER	ASK	PERIODE TEMP3	2R	$\ominus$	$\ominus - \ominus_{12L}$
24 <sup>h</sup> 50 <sup>mn</sup>	B S	1 Z3+ 4	2,174 2,106		- 7 <sup>h</sup> 23 - 7 <sup>h</sup> 55	15'	B + S	1 Z 3 4	1,804 2,880		- 7 <sup>h</sup> 45 8'01
ZS'	B S	1 + 2 + 3 + 4 "	2,662 1,840		- 8 <sup>h</sup> 01 - 7 <sup>h</sup> 47	24 <sup>h</sup>	B - S "	1 + 2 3 + 4	0,502 1,708	16 1? 13'59	
24	B S	1 t Z+ 3+ 4 ,	1,394 0,776	13 <sup>h</sup> 56 11 <sup>h</sup> 40		12 <sup>h</sup>	B - S "	1 + 2 3 + 4	1,502 1,986	5 <sup>h</sup> 26 6 <sup>h</sup> 31	
Z*	B S	1 t Z 3 A ,	1,890 1,54 30	5'L1 06		24	B S B S)	1 2 3 + 4	1,388 1,880		7 <sup>h</sup> 23 7 <sup>h</sup> 54

Legend: 8 -- time of the apex of the adjustment sinusoid in the middle of the period. 0t2t,  
= time when the Moon crosses the meridian in the middle of the period.

In Bougival we have:  $2R(24\text{ h }50\text{ mn}) / 2R(24\text{ h}) = 2,174 / 1,394 = 1,563$

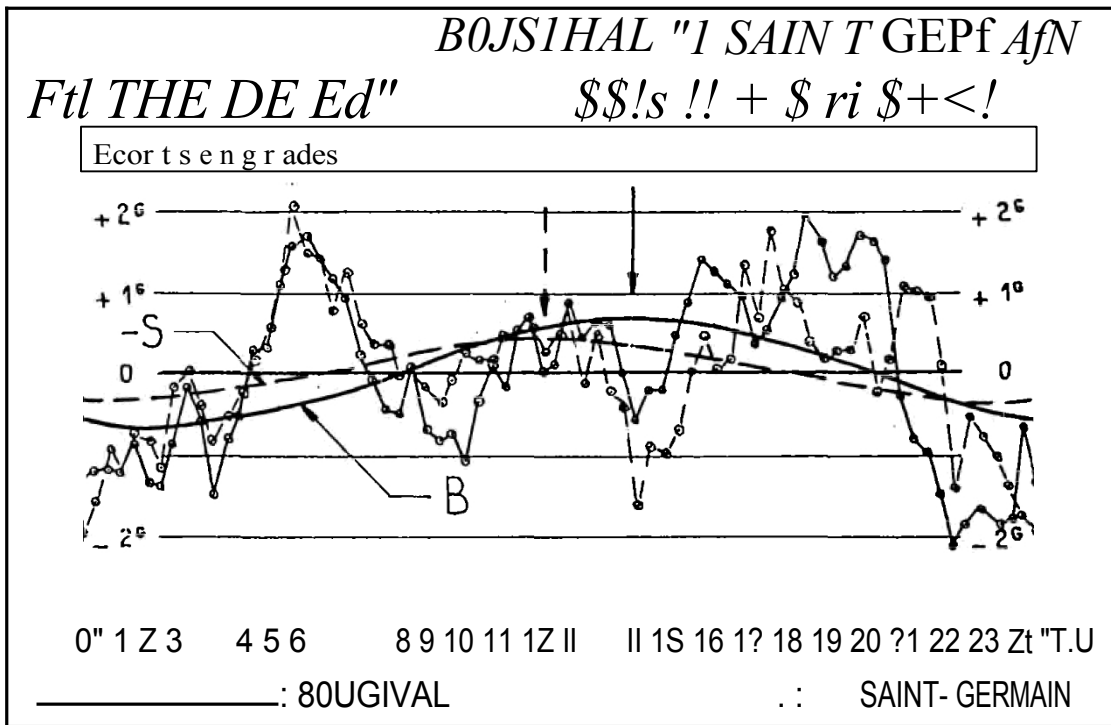
At Saint-Germain we have:  $R(24\text{ h }50\text{ mn}) / 2R(24\text{ h}) = 2,106 / 0,776 = 2,714$

The average amplitude ratio is thus:  $(1,563 + 2,714) / 2 = 2,138$

Source : Note b l'Académie des Sciences du 22 décembre 1958, *Structure périodique des mouvements du pendule protonique d Bougival et de Saint-Germain in July 1958*, C RAfi, T. 247, 1958, p. 2285.

EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAE

Boys-Ballot Filter Resuyats for id hour filter

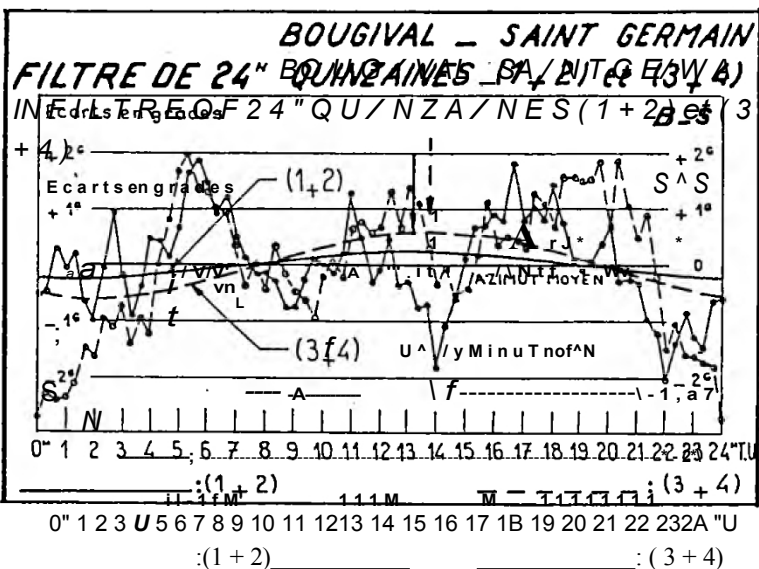
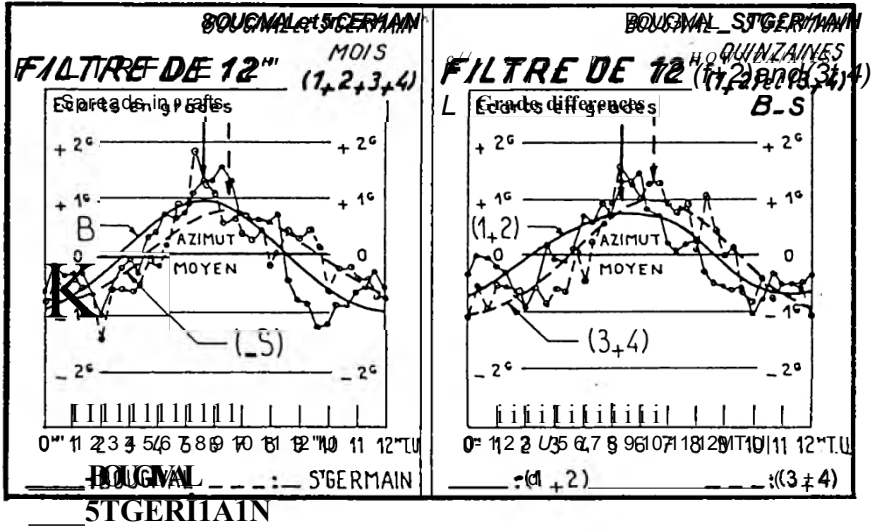


Source : Note h l'Académie des Sciences, Sruclure périodique des mouvements du pendule paraconique à support anisotrope à Bougiuol et ô Soinl- Germain en juillet 1958, CRAS, 'l'. 247, December 22, 1958, p. 2285.

Graphique XIX, XX, XXI

EXPERIENCES DE JUULIET 1958 BOUGIVAL ET SAINT GERMAIN

Résultats des filtres de Bougival et de Saint Germain pendant 24 heures



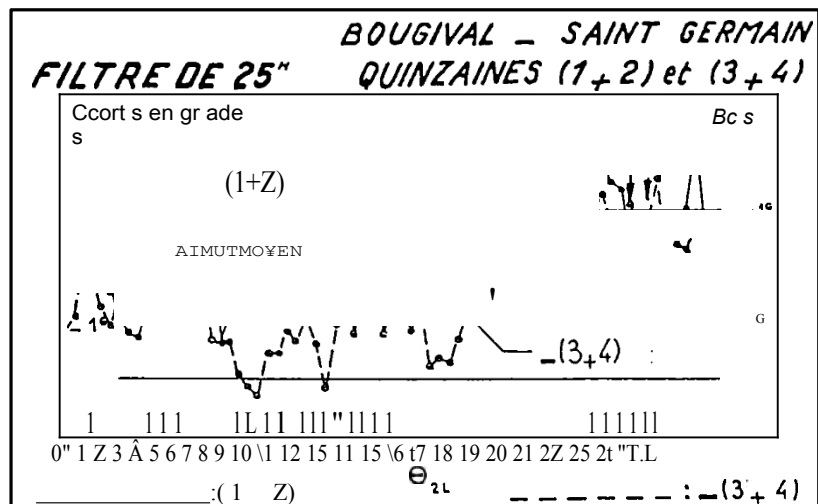
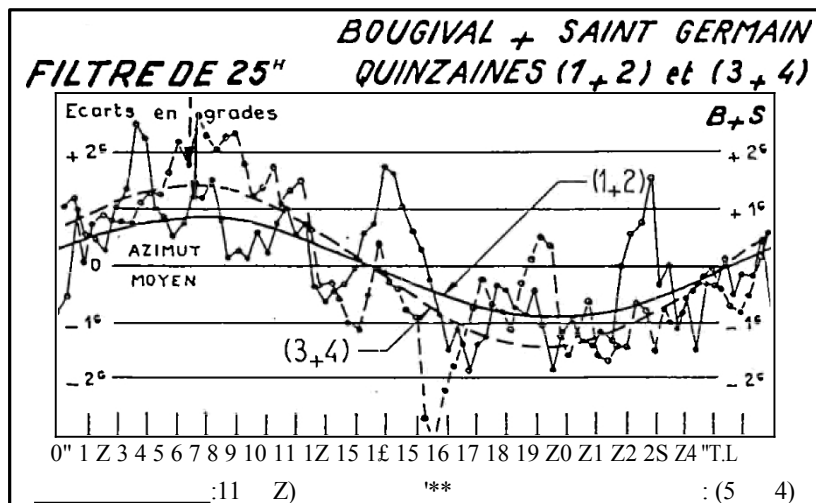
Source : Note à l'Académie des Sciences du 22 décembre 1958, Structure périodique des mouvements du pendule paraconique à support anisotrope à Bougival et à Saint-Germain en juillet 1958, CRAS, T. 247, p. 2286.

*Source: Note à l'Académie des Sciences, December 22, 1958, Structure périodique des mouvements du pendule paraconique à support anisotrope à Bougival et à Saint-Germain en juillet 1958, CRAS, T. 247, p. 2286.*



**Graphiques XXII et XXIII**

EXPERIENCES OF JULY 1958 AT BOIJGIVAL AND SAINT-GERMAIN

*Aésu\*fars du /iffre de Guys- altot pozzz- urxÇfifz-e de 2fi etzz-es*

*isource : Note b l'Académie des Sciences du 22 décembre 1958, Htruture périodique des mouvements du pendule paraconique ô support anisotrope ô Bougival ef ô J oirtt-Cermoitz en /ui//ez 958, CRAS, T'. 247, p. 3286.*

*P'réquencigramme de la série de Bougival de juillet 1958*

4 Consideration of Bougival's monthly série fréquencigramme is particularly significant, and *fully* confirms the above conclusions 7,

*Graph XXTV* shows the series of 721 hourly values obtained for the azimuth of the paraconic pendulum in Bougival from July 2 at 0 h. TU to July 31 at 23 h. TU. UT to July 31 à 23 h. 40 min. TU . This graph looks very similar to *Graph II*, which shows the azimuths for June-July 1971 and 1955\*.

*Graph XXV* shows the 24 h. 50 min. cycle obtained by Buys-Ballot analysis from the hourly values of *Graph XXTV* with amplitude  $2R = 2.13$  grades.

*Graph XXVf* shows the frequencigram of the series of 721 hourly values from *Graph UV*, with lines of equal significance represented. The significance level of the 24 h. 50 min. is 0.07 No corresponding to a probability of less than one chance in 1000 11,

The 24 h. 50 min. has an amplitude around 1.68 times greater than that of the 24 h. period, whose significance level is only around 14 No. 12.

(7) This analysis was presented as an illustration in my 1961 communication to Institut iniernotionat *de Statistique* (note 10 in § B.1.3 above, p. 113).

(8J) in neze *II B b* ma Communicatio n of 196 1 at the Institut iniernotionot *de*

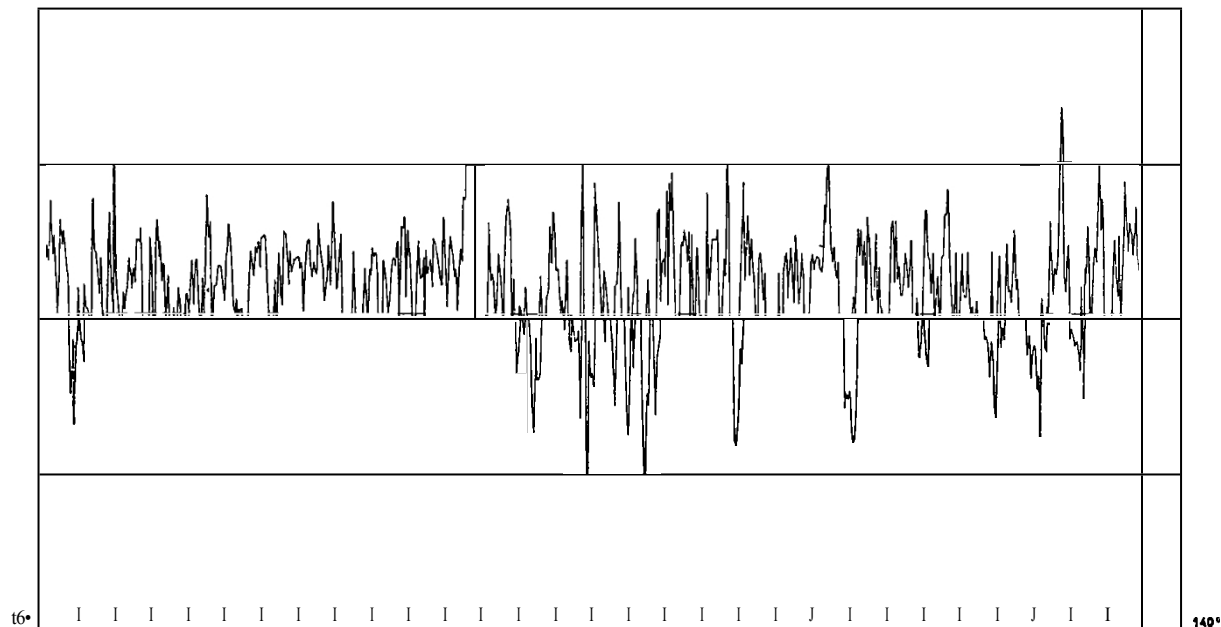
(9) Grop5Ígue 77 of § A.2 above8, p. 89.

(10) *Annex:e III C ä* ma Communication de 19 61 à l'nfstif ut *Iniernotionol de Islolistigue*.

(1D) My 1961 communication, § 18. The value of ti corresponding to the period of 2d h. 50 mn. is 7.28 with  $P = e^{-7.28} = 0.0007$  See § B.1.3 above.

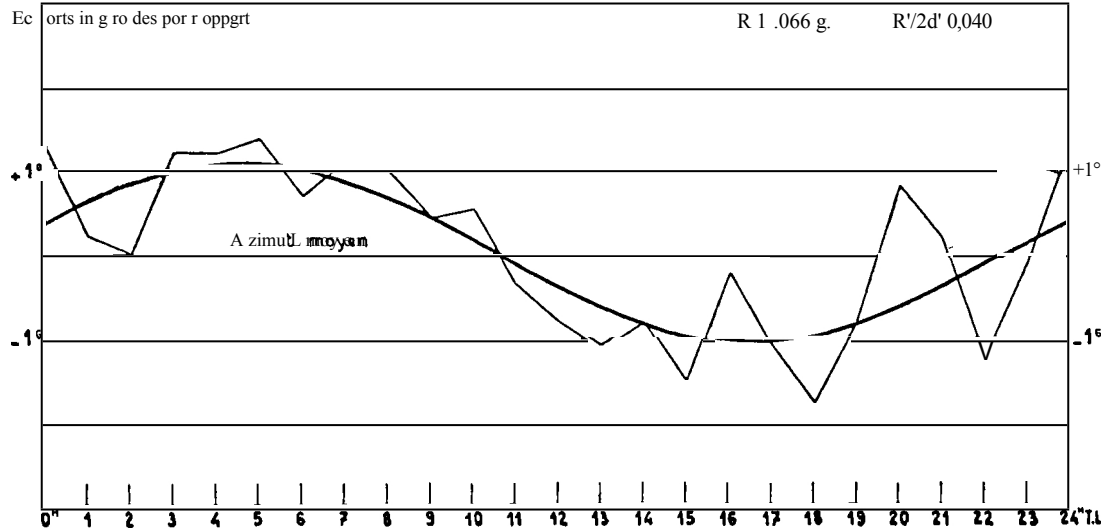
(12) For comparison, see the chart of the 721 ho - values series. raires of November-December 1954 (*Crrophuf ue XI* of § B.1.3 above).

## BOUGIVAL'S JULY 1958 OBSERVATIONS

*Acimut:e de plan d'ocillation**N -- 721 hourly thieves*

Source: Appendix II B of my 1961 paper for the Institut International de Stntsiiqitr, "i "esi de ptriodicitè. Généralisation du test de ischuster ou cas de sJries temporelles outocorrélées dans l'hypothèse d'un processus de perturbations aléatoires d'un système stable".

The 721 hourly azimuth values of the Bougival paraconic pendulum are shown in Anneze II A of my Commu nization of the International Institute of Isiatisties in 1961.

*cinute du plan d'ocillation*

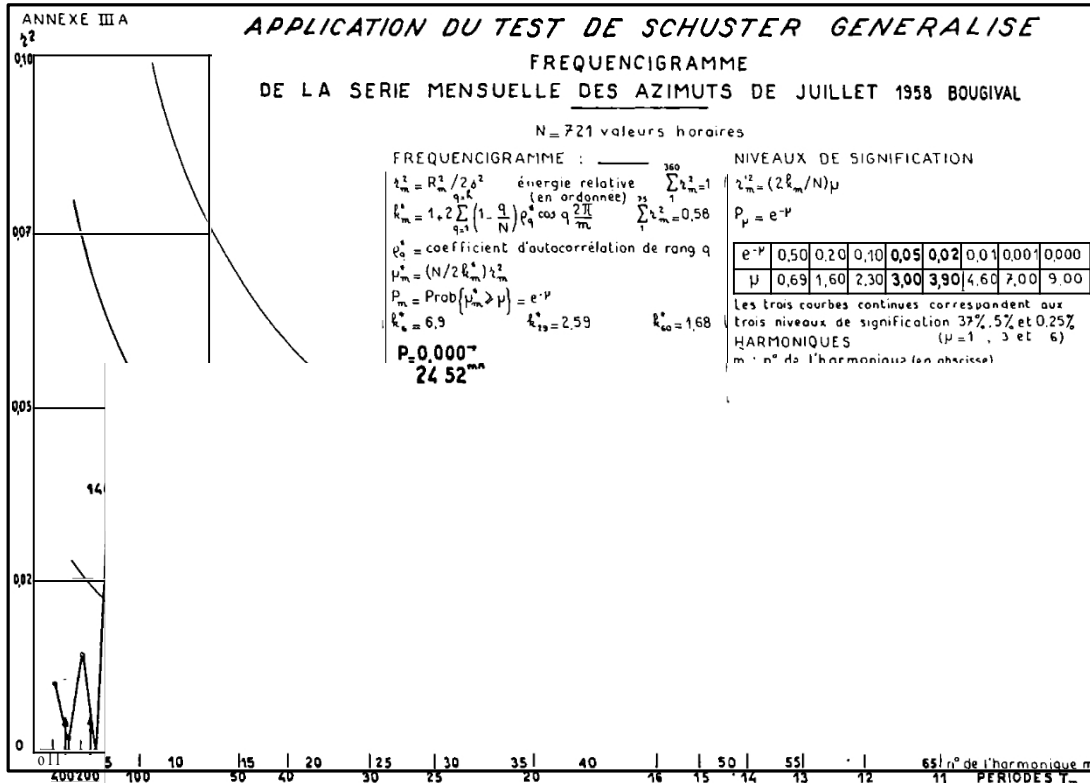
Mgende ——— observed

————— valuessinusoid of adjustment

}t 1,()f-i6 g.

R' / 2 o' = 0,040

isource : Annese III C de ma Communication de 1961 à l'Institut International de islaistiqu, ""i "est dr pèriodiciit. Oèndrolisotio du item de Schester au cos de séries iempottlez out' orH1Je# dan" l'AypoNse dt un processus dt perturbations oltatoires d'un système siabl



Legend: On test formulation, see § B.1.3 above and Legend to Graph XI.

isouret: Annex III A of my 1961 Communication to the Institut Interriotoriol de istatistiqu (see Source of Graph XXI).

*Graphs XXVII and XXVIII* show the adjustments half-sums  $(B+S)/2$  and half-differences  $(B-S)/2$  of means, *daily* azimuths B and S of the paraconic pendulum in Saint-Germain and Bougival on 31 July 1958<sup>1</sup> taking into account both linear trends<sup>2</sup> and sinusoids with a period of 27.322 days *equal to the sidereal period of the moon!* -\*.

We can see that the two adjustments in *Graphs XXVII and XXVIII* are practically in phase. The  $(S+B)/2$  adjustment has its summit on July 24, 8 p.m., and the  $(S-B)/2$  adjustment has its peak on July 24, 0 a.m. The two trends of - 0.149 and - 0.147 grades per day in *Charts XXVII and XXVIII* are virtually identical.

(1) In contrast to *the coitus that precedes*, the adjustments that follow have been effective. (see also recently in journal J996).

On the *XXVII* graph, azimuths are counted in degrees from North. *direct meaning.*

Let me remind you that we have

$$(1) \quad f/W = 1 - R$$

The difference  $1 - R^*$  represents so the fraction of variance not explained by the multiple correlation (see *ci-dessus*, p. 101, note 12).

(2) These linear trends correspond to longer-period fluctuations, especially fluctuations with a period of six months (see *Ghoptre V*, Section B, below).

(3) In fact, the moon's sidereal and synodic periods give almost exactly the same correlation coefficients. The reason for this is that for a total duration of 30 days (i.e. 720 hourly values), both sidereal and synodic periods are *indiscernible*.

I have considered the *sidereal period* to facilitate any comparisons. with Esclançon's and Miller's results corresponding to sidereal time.

In fact, This choice was fully justified by the analyses of paraconic pendulum observations series with isotropic support (see Chapter II, §). E.2 and F.3).

(4) Between the sidereal period  $T_1$  and the synodic period  $T_2$  of the moon, and the annual period  $T$  of the Earth, expressed in mean days, we have the relationship

$$(1) \quad \frac{1}{T_1} + \frac{1}{T_2} = \frac{1}{T}$$

with :  $T_1 = 27.32166$

$T_2 = 29,53059$

$T = 365,25636$

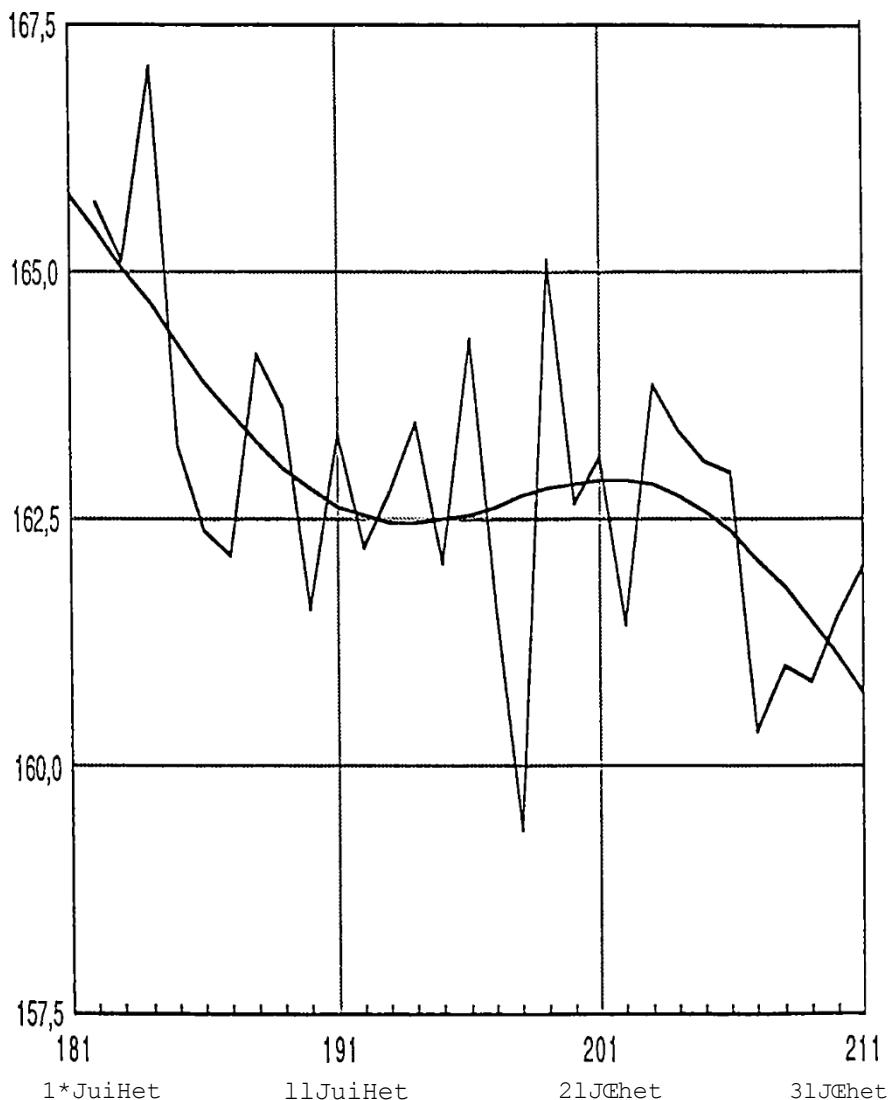
HALF-SUMMARY (S + B' / 2 OF AVERAGE DAY VALUES

RES

DES AZIMUTS IN BOUGIVAL AND SAINT-GERMAIN

A, precisely according to the moon's sidereal period of 27.322 days, taking into account a linear trend

July 2, 12 a.m. - July 31, 12 a.m. 1958



*Ligende* :  $Z = 1.61$  ;  $R = 0.629$  ,  $1-R = 0.60d$  ;  $m = 162.9$  grades ,  $\text{trend} = -0.149$  grade per day ,  $r = 1.02$  grade ,  $\sigma = 1.25$  grade ;  $N = 30$  days.

$Z$  = standard deviation of ;  $R$  = multiple correlation coefficient:  $r$  = half fit;  $r_i$  = data sinusoid amplitude ;  $\sigma$  = standard deviation of residuals.

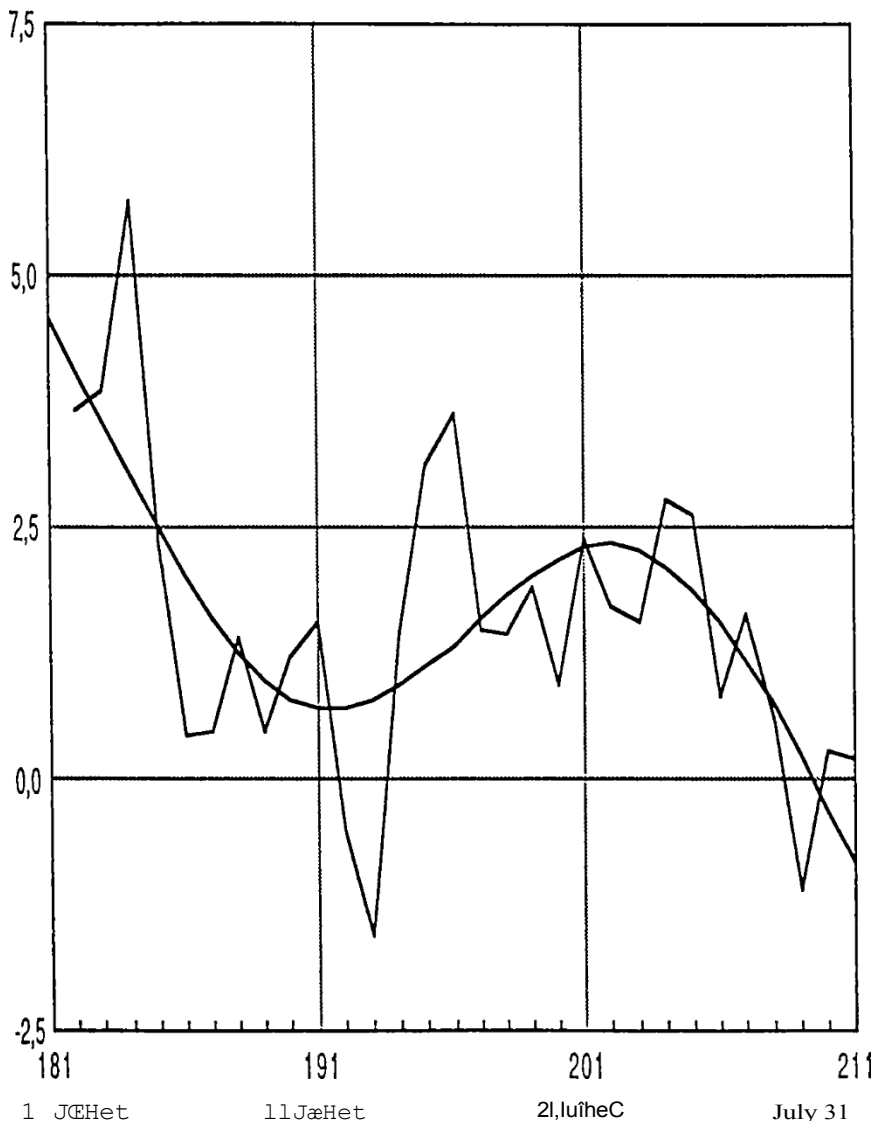
Sinusoid maximum and minimum dates: July 24, 8 p.m.

( $s = 204.85$ ) and July 11 4 h.  $ls_j = 191.19$ ). Dates  $s$  are counted in days from 1<sup>st</sup>-January 1958 0 h.

HALF-DIFFERENCE (S - B) / 2 OF DAILY AVERAGE AZIMUTH VALUES AT BOUGIVAL AND SAINT-GERMAIN

Adjustment for the moon's sidereal period of 27.322 days, based on a linear trend

July 2, 12 a.m. - July 31, 12 a.m. 1958



Legend:  $Z = 1.51$ ;  $R = 0.680$ ,  $l-R = 0.537$ ;  $m = 1.55$  grade, trend =  $-0.147$  grade per day,  $r = 1.70$  grade,  $r_i = 1$ ,  $l_i$  grade;  $N = 30$  days (see Graph XXVII legend).

Dates of sinusoid maxim nm and minimum: July 24, 0 h.

( $s = 204.03$ ) and July 10 8 a.m. ( $s_j = 190.37$ ). Dates s are counted in days to

From January 1, 1958, 0 h.



## 4.-Vtred'exentdfé

The *crucial* experiments carried out at *the same time and under the same conditions* in Saint-Germain and Bougival added *decisive* elements to my earlier analyses from 1954 to 1957, eliminating *with complete certainty all the* pseudo-explanations put forward by my opponents of the periodic effects observed. For example:

- The *practical* identity of the periodic components of 24 h. 50 mn. components observed at Saint-Germain and Bougival is *sufficient to eliminate any causal explanation*.  
/ortutte.
- Similarly, the *practically* *invariable* temperature conditions achieved in the Bougival laboratory *eliminate any thermal effect*.
- The *parallelism* of the periodic effects observed at Bougival and Saint-Germain *means that any influence based on the influence of the building at the IRSID or on that of any superficial cause can be ruled out*.

In fact, these two crucial experiments *ostly swept away all the objections previously presented* on my experiments with the *pendulum*.

This is particularly true of the 24-hour diurnal periodicity. 50 min <sup>1</sup> of *totally unexploited* amplitude within the framework of gravitational theory.

(1) In my November 7, 1959 lecture at the Cercle Alexandre Dufour, I reiterated what had been said to me before:

*I've been told for months: "Let's have experiments at some moment in different places, and if we obtain similar results, it will be decisive. The question will be heard, it will be judged, and it seems to me that we will be able to see it". A very tame staff member even said to me: "It's not even necessary that there should be the same results. If your pendulum had some movements, that would be a considerable flow".*

*En fait, l'identité des composantes périodiques de 24 h. 50 mn. est bien au-delà de ce qui, dans les expériences de juillet 1958 à Bougival et à Saint-Germain, était considéré comme décisif.*

That the scientific authorities responsible did not give me the support that would have enabled me to find the necessary financial resources to continue my experiments remains *totally incomprehensible* to me today, *so scientifically aberrant was their attitude* \*.

(2) See section G of this chapter for an *overview*.)

In fact, opinions were very divided. *Neu/* membres ou *moins* de *l'Académie des Sciences* pensaient *que* mes *expériences* devaient être *pourchassées*, mais *opposément* ils étaient *minoritaires* (§ G.6 ci-dessous, note 3, p. 232).

**DFVIAJIONS OBRDRVEEELORSDEDFUKECLIPEES  
TOTALISDEMUBL**

During the two total solar eclipses of June 30, 1954 and October 20, 1959, an anomalous lunisolar influence manifested itself in the form of remarkable perturbations in the azimuthal motion of the paraconic pendulum.

During the 32-day series of chained observations in June-July 1954 and at the time of the total solar eclipse on June 30, 1954, the oscillation plane of the paraconic pendulum abruptly shifted by about 15 grades.

*Graphs XXCE and XXX* show the angular displacement of the oscillation plane port4 as a function of time. Each point represents the starting azimuth for each 14-minute observation period, equal to the azimuth of the oscillation plane after 14 minutes in the previous experiment.

(1) In pendulum consisted of a vertical bronze disk and two disks **hori-**  
(see § n.3.1 above).

(2) See my Note of November 18, 1957: "3'fouvements du pendule paratonique ci Ecfipsr totole de aoteil du 30 iuin 1954", C RAS, T. 245, December 4, 19ü7, p. 2001-2003.  
The displacement of the asimut of the sifuJ pendulum's ground-wheel oscillation plane

(§ A.1.2 above) was so **noisy**, that u'it came as a complete surprise to the observer, Jacques **Bou eot** (my lab manager), and that il called me right away on the t6l6phone. Il iave never observed6 such a location before.

*Graph XXIX* shows the azimuth curve observed from June 28, 1954, 8 p.m., to July 1, 1954, 4 a.m., as well as the symmetrical curve of the left part of the azimuth curve with respect to the vertical line corresponding to June 30, 0 a.m., *Graph XKX* shows the detailed azimuth curve observed from June 30, 9 a.m., to June 30, 3 p.m., (universal time).

The eclipse began at 11 h. 21 mn. and ended at 13 h. 55 min. At the *precise* moment of the eclipse's onset, the azimuth of the oscillation plane abruptly rose by 5 degrees from the trend that had previously characterized its movement. Twenty minutes before the eclipse maximum at 12 h. 40 min. this deviation reached a maximum of 15 grades, then decreased progressively, but more abruptly than during the ascent, dropping to 1 grade 20 centesimal minutes before the end of the eclipse.

As far as we can tell, the movement of the oscillation plane resumed a CD movement after the eclipse, similar to the AB movement observed before the start of the eclipse (*trophic XXIX*).

*Graph XKIX* shows an approximate symmetry of the azimuth curve with respect to the vertical line corresponding to June 30 0 h. This symmetry, which can be attributed to the almost periodic azimuth<sup>3</sup> is observed for about 28 h. on either side of the the axis of symmetry. Assuming, as is likely, that this symmetry corresponds to a physical reality independent of the perturbations caused by contact between the ball and the surface, it is notable that nothing in the part of the azimuth curve prior to the time of the center of symmetry corresponds to the very strong deviation observed during the eclipse.

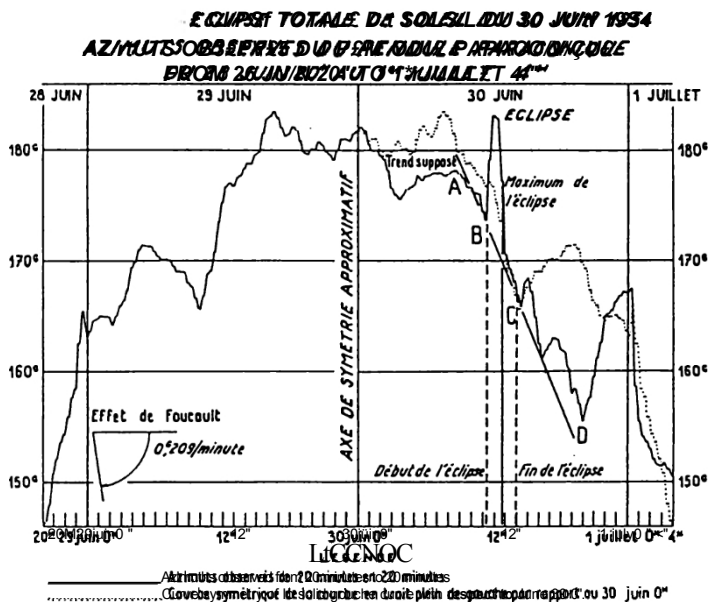
(3) On the almost periodic structure of a series, see § A5.4 above, p. 101.

It should also be pointed out that during all the periods of continuous observation, I have never **observed** any variation in the azimuth curve analogous to the BC part corresponding to the solar eclipse of June 30, 1954\*.

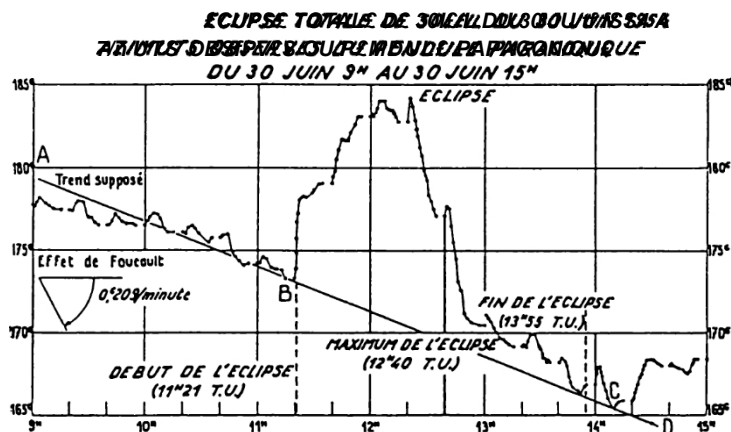
Finally, we can see that the rapid azimuth variations observed from 11 h. 20 min. to 12 h. and from 12 h. 20 mn. to 13 h. correspond to angular velocities of the order of  $0.62 \cdot 10^{-4}$  and  $0.79 \cdot 10^{-4}$  radian per seconde, i.e. respectively **1.13** and 1.43 times the Foucault effect (i.e.  $0.55 \cdot 10^{-4}$  radian per second at the latitude of the Saint-Germain laboratory). The forces involved in the observed disturbances are therefore of the same order of magnitude as those involved in the Foucault effect.

(4) It is remarkable that the maximum apparent deviation due to the eclipse took place 20 minutes before the maximum of the eclipse. There was therefore a certain asymmetry in the effect observed. A similar asymmetry has been observed, but in the opposite direction, with the maximum of the effect being later than the maximum of the eclipse, for terrestrial magnetism (Lion, *Comptes rendus*, 33, 1851, p. 202; 34, 1852, p. 207; Lion and Muller, *Comptes rendus*, 14, 1874, p. 199) and for the terrestrial electric field (Nordmann, *Comptes rendus*, 1874, p. 199).  
*rendus*, 1t2, 1906, p. 40 Chevrier, *Comptes rendus*, 197, 1933, p. 1143; Rouch, *Comptes rendus*, 239, 193d, p. 465).

## Graphique XXIX



## Graphique XXX



## **2.- L'éclipse totale de soleil du 2 octobre 1959**

A similar disturbance, with an amplitude of around 10 grades, occurred during the total solar eclipse of October 2, 1959, *which was only partially visible in Paris.*

The movement of the paraconic pendulum was observed over a 3-day period from September 30, 8 pm to October 4, 4 am. U.T. The circumstances of the movement are shown in *Graphs XXXI and XXXfi.*

The experimental conditions (laboratory, support, pendulum, use of the same ball for each series of observations) were *exactly the same as for the eclipse of June 30, 1954.*

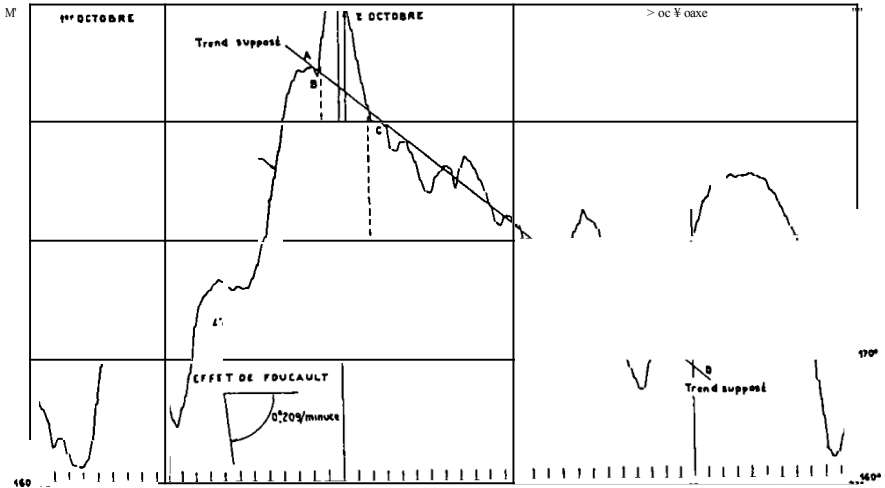
Whereas on June 30, 1954, the eclipse took place on a steadily descending part of the azimuth curve, and the observed deviation presented a shape never seen before, the deviation observed on October 2, 1959, if there is any deviation at all, takes place in the vicinity of a som- met, its shape is nothing exceptional, and a definite interpretation therefore becomes difficult.

However, if we assume that, in the absence of the eclipse, the general movement observed would have been represented by the line A B C D, the deviation corresponding to the eclipse can be determined as shown in *Graph XXXII \**.

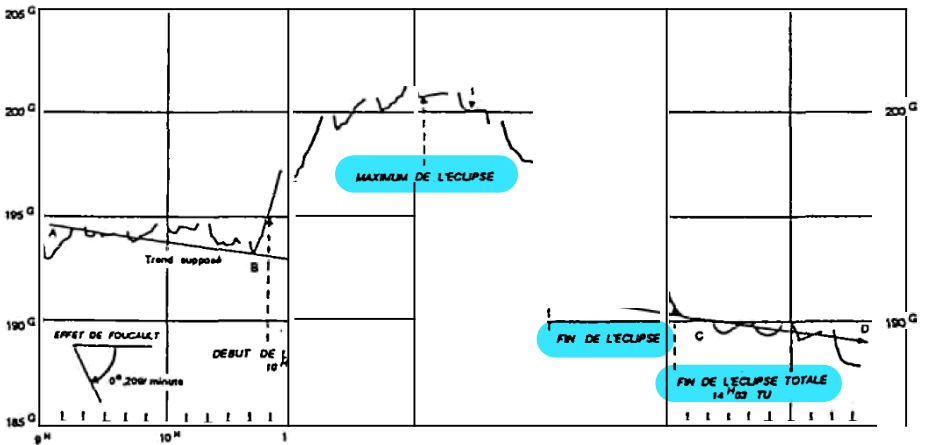
(1) See my unpublished note of November 10, 1959: "*Surprises of the porc - conique pendulum and Eclipse totob de coleil of October 2, 1959.*"

(2) On the influence of the total solar eclipse of October 2, 1959 on the motion of the isotropic suspension paraconic pendulum, see below, *Chapter II, Section H.*

TOTAL SUN ECUPSE OF OCTOBER 2, 1959  
 aZIM'ITs PxRAcONioiJE PENDULE OBSERVATIONS  
 yes ocroaRe is^du s ocroane z:fi



TOTAL SUN ECUPSE OF OCTOBER 2 1959 AZIUUTS  
 oBSERVES DU PENDULE n R CoNi'o'IE  
 DIJ 2 OCTOBRE ghyij y g ceuane is h,s,j



Source: Allaia, Note, non publie, du 10 novembre 1959, 3'fouvement du Pendule pnuconi9ur tt E-cmps tototr de zofr'd du 2'ctobre 1959 (reproduction photographique).



g.- Comparaisoizz chesperfzrôafiozss oBsemées lors desdezxx éolipeee totafiee de  
:ifiaie "a au 3o)uin 19ifi el du s iab -e 19s9

If we plot on the same graph for both eclipses the observed deviations from the assumed trends in the period of time surrounding the eclipse, if we take the maximum of the total eclipse as the common origin, and if we take abscissa scales such that the two durations of 3 h. 14 mn. for 1959 and 2 h. 51 mn. for 1954 are represented by the same length, the disturbances assumed in the two cases appear very similar (*graph XXXIII*).

Although these are only two experiments, and although the assumed tendencies of the pendulum's plane of oscillation before and after the eclipses are not absolutely certain, the perturbations considered in both cases present the same *similitude assez remarquable*.

In both cases, the angular deviations observed per unit time are of the same order of magnitude as those corresponding to the eddy current effect. We can deduce *that the forces involved in the observed perturbations are of the same order of magnitude as those involved in the Foucault effect.*

*In both cases, the deviations brought the pendulum's plane of oscillation closer to the méridian. These deviations are totally inexplicable within the framework of current gravitational theory.*

(1) It should be noted that this similarity only exists if we consider the beginnings and ends of the total eclipse for all latitudes given by the Knowledge of Times, and not the beginnings and ends observed locally in Paris (on the graphs in my 1957 *Note to the Académie des Sciences* on the 1954 eclipse, the various times indicated relate to the eclipse observed in Paris).

This interpretation seems justified by the fact that the amplitudes of the deviations assumed in both cases are of the same order of magnitude, whereas the portion of the surface eclipsed by *Ports* in 1959 represented only 36.8% of the surface eclipsed in 1954. The slightly greater deviation observed in 1954 can be explained by the fact that, at the start of the eclipse, the pendulum was approximately 27 degrees from the North-South azimuth (to which the plane of oscillation appears to be attracted at the moment of eclipse), whereas it was only 7 degrees from this azimuth at the corresponding moment in 1959.

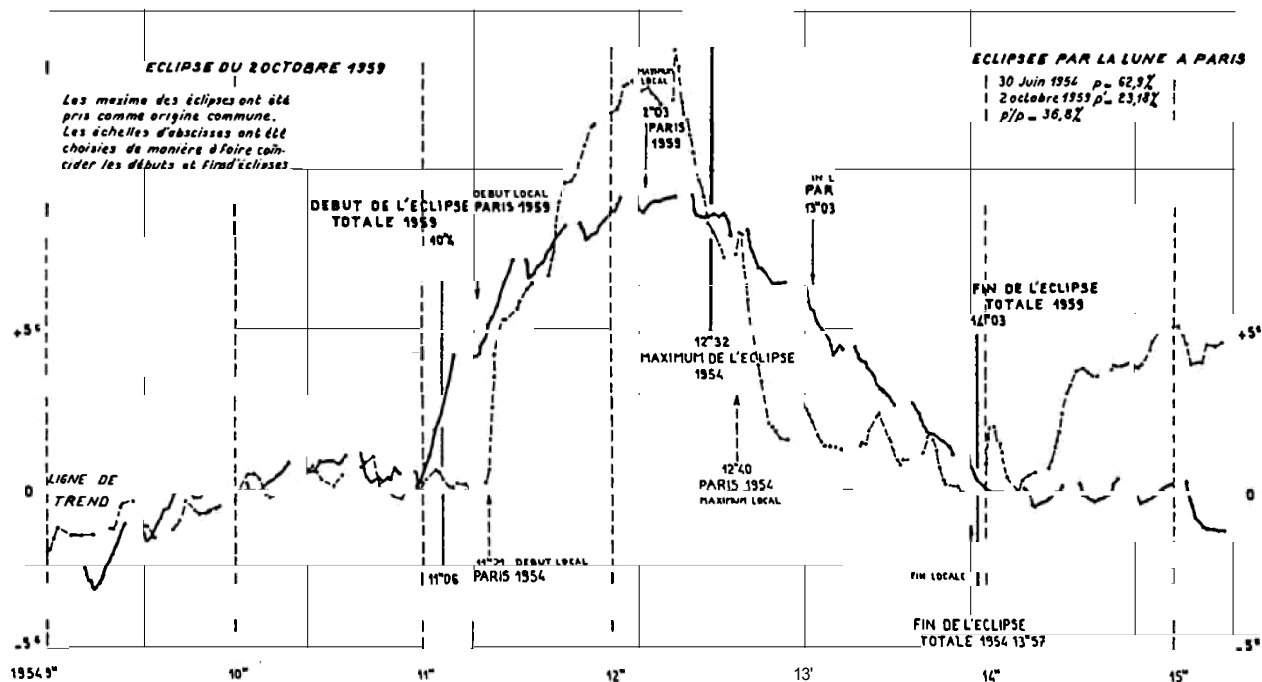
Due to lack of funds, similar observations could not be made in my Bougival laboratory, which had to be decommissioned in 1960.

*Whatever the intrinsic scientific importance of the anomalies of the paraconic pendulum corresponding to the eclipses considered, their relative scientific significance is in fact quite small compared to that of the periodic lunisolar anomalies observed, the existence of which is fully established by the hundreds of thousands of observations that have been made and from which they have been deduced\*.*

(2) Since these anomalies are totally inexplicable in the context of *In théorie* currently admired by the community.

(3) In fact, the anomalies observed were confirmed by experiments by Saxl and Allen during the solar eclipse of March 7, 1970 (1970, *isolate Eclipse ne been by a Torsion Pendulum*, Erwin J. Saxl and Mildred Allen, *Physical Review*, D, vol. 3, Number 4, 15 February 1971). Saxl and Allen's article refers expressly to my own experiments.

COMPARAISON DES AZIMUTS OBSERVES  
PENDANT LES DEUX ECLIPSES



Source: Allais, Note non publiée du 10 novembre 1959, étonnement du pendule puraconique et Eclipse totale du Soleil du 2 octobre 1959 (reproduction photographique).

## E

KEY MOVEMENT FACTORS  
PARACONIC PENDULUM WITH ANISOTROPIC SUPPORT

This movement is extremely important

All the experiments I carried out from 1954 to 1960 demonstrated that the movement of the anisotropic-supported paraconic pendulum is an *extremely complex* phenomenon, *very difficult to analyze*, and one that for months kept me asking a great many questions.

*In fact, it was only in September 1955, following the third monthly series of observations in June-July 1955, that I came to a certainty as to the actual existence of a diurnal lunar component of 24 h. 50 mn. in the monthly series of observations of the pendulum po- raconique, an existence totally inexplicable as to its amplitude in the framework of traditional theories!*

It was then that I decided to submit my results to the scrutiny of outside personalities. In the three years that followed, 127 personalities, including *more than fifty specialists in Mechanics and Gtophysics*, came to visit my laboratory at IRSID, and later at Bougival.

(1) See my *Nett zommoire cur ltd mouutment du ptndult paraconique*, September 1955, 18 p.

- **In December 1955 I had drawn up a first draft** of the **theory** of the <sup>||</sup> conical pendulum, and **by September 1956** I had completed a comprehensive essay on the theory of the \*- \* pa- raconique pendulum.

*As no test for autocorrelated time series existed in the literature, I had to develop one. After months of work, I arrived in February 1957 <sup>5 Q</sup> at my *Genenrolization of the Test de**

*Slcluster* which was the subject of my *Note* of May 13 1957 to the Académie des Sciences \*. This test *fully* confirmed my earlier conclusions as to the *real* existence of the 24-hour lunar periodicity.

*50 min. of an amplitude totally inexplicable within the framework of the current theory of gravitation.*

- It is obviously impossible to give an exhaustive analysis of all the factors that determine the motion of a paraconic pendulum with anisotropic suspension. I shall therefore confine myself in this section to examining *the four factors that are really quite essential*: the Foucault effect, the anisotropy of the support, the random influence of the balls, and the periodic influences exerted on the pendulum.

I'll round off this analysis with an illustrative examination of some purported explanations for the phenomena observed.

Finally, I'll briefly present some observations on the existence of a time-varying limit plane, towards which the pendulum's plane of oscillation tends during each 14 - minute experiment.

(2) *Fôéorie du pendule conique* (rédaction provisoires), December 1955, 50 p.

*t81 Tfi4orie du pendule poroconique*, æptembre 1956, 441 p.

(4) This theory has enabled me to calculate the exact lunis- lair influence on the motion of the paraconic pendulum (see § B.2 above).

f5) *Note on the interpretation of paratonic pendulum experiments*, April 25, 1957, 18 pp.

(6) See § B.1.3 above.

*Paraconic pendulum and Foucault effect*

1- It is very significant that *while the pendulum oscillation remains plane, the displacement in azimuth of the plane of oscillation of the para-conical pendulum leads to the Foucault effect in all my experiments!*

D results from 1A that mea experiments are in no way in contra- diction, as has been all too suggested, with the general result of Foucault's experiment. *They fully include Foucault's effect.*

*Eddy effect and ellipse generation*

2 - Any cause, other than the Foucault effect, acting on the pendulum may act either *directly*, by modifying the speed of rotation  $\dot{\phi}$  of the azimuth  $\phi$  of the pendulum, or *indirectly*, by creating an ellipse which, through the Airy <sup>2</sup> effect, causes precession.

$$\dot{\phi} = (3/8) p \text{ l x b} \qquad \mathbf{p=2x/T}$$

of the pendulum's plane of oscillation, with o and Q representing the major axis and minor axis in radians of the pendulum's elliptical trajectory, and T its period of oscillation.

As the Foucault effect is always observed when the oscillation is plane, we can conclude, *at least as a first approximation, that any cause other than the Foucault effect acting on the pendulum inter- comes indirectly through the creation of ellipses.*

(1) §A4above, p. 93-95.

At IRSID latitude ( $L = 48.90^\circ$ ), the rotation speed of the plane of oscillation corresponding to the eddy current effect is  $- \sin L = - 0.550.10^{-1} \text{ rad./sec.}$  where m represents the Earth's rotation speed ( $m = 0.729.10^{-1} \text{ rad}^2\text{sec.}$ ).

If no other effect occurs, the pendulum's oscillation plane will make one revolution.  
complete in  $2\pi / m \sin L = 11.42 \cdot 10^6 \text{ seconds, or } 31.76 \text{ hours.}$

(2) § B.2.3 above, p. 120, and § B.2.5, note 11, p. 122.

*Classical experiments with the Foucault pendulum and the paraconic pendulum*

3 - It's true that Foucault's experiment gave rise to some spectacular demonstrations, such as the one carried out in 1852 at the Panthéon in Paris with a pendulum 67 metres long and weighing 28 kg. The oscillation remained almost rectilinear, with an amplitude of 0.06 radiana\*.

Let me emphasize here *the essential differences between the* experimental conditions of the paraconic pendulum and those of Foucault's classical experiments.

1 - The paraconic pendulum used is a *short pendulum* of the order of a metre in length, as opposed to several metres, or even tens of metres, in the experiments of Foucault and his successors.

In fact, it's well known that it's very difficult to obtain the Foucault effect with short pendulums. There are almost always anomalies\*.

f3J See the Second **f**ertic of this book (Chapter ii, Section C) for an overall analysis of the numerous experiments on the Foucault Pendulum.

The bibliography of experimental research on the Foucault Pendulum is extensive, but two facts strike us: - the scarcity<sup>4</sup> of serious work; - the paucity of numerical data on the **results** obtained.

On the Foucault Pendulum experiments, see in particular the *Bibliographie du Pendule*, très Etendue, published in 1889 by the *ffocift tronçaise dePhysique* (Collection de mémoires relatifs à la Physique, Tome IV, Gauthier-Villars), and my 1958 me- moire, "*Doit-on reconsidérer les lois de la grouillation ?*", p.99-100.

(4) In an interesting article (On the *irregularities of Motion of tht Foucault Ptdulum*, The Phyeicel Review, April 1919, Vol. XIII, n-4, pp. 241-258) A. C. Longden writes:

*"Mort Thon o 8core of uittl-hnouin phy#itiata ond oafronomera ort on rtcord esaffirmin that the Foucault Pendulum muet be utry long ond very /ieovoy in ordtr to give satisfactory results".*





- 2 - The paraconic pendulum used *can rotate on itself*, whereas the Foucault pendulum is tied to the wire that supports it \*- \*.
- 3 - The movement of the paraconic pendulum used was observed *without euctine dtscontintiitd*, day and night, for periods of the order of a month. This has *never been the case in previous experiments with the Foucault pendulum*.

From all these indications, it is clear that *nothing in the results of my experiments contradicts those previously obtained*. On the contrary, all previous results have consistently mon- trö the existence o f anomalies, th e study of which has certainly been neglected until now <sup>7</sup>.

f5) The only experimenter I know of to have tried a ball suspension is Longden (see note 4 above). He states in the following lapidary terms that he has eliminated this tp suspension. 249)

*"I decided not to use the ball end pfone support on account of his tendency to rotate and the upper surface of IM bell*

The sphere was fixed and it was the surface that rolled on the sphere.

(6) It's worth pointing out here that n o dissertation on pendulum theory studies the motion of the pendulum's central inertial t r i h e d r o n when the pendulum is suspended by a point or a ball.

I filled this gap in my 1956 dissertation, 'Théorie *du pendule porconical* (note 3 in § E. I above).

In the Second Part of this work iChapter f, Section B) I will set out the essential elements of this movement.

(7) Among the anomalies observed, *the most curious* is undoubtedly that reported by Abbé Panisetti: *a pendulum sets itself in motion in an east-west direction*. His experiments were carried out with pendulums measuring 1 to 16 m. tRevue Cosmos, 1856, Tome VIII, p. 503-504, and 1857, Tome IX, p. 638-639).

These experiments can be compared with similar experiments by General Louis Pasteur (see my 1958 dissertation, "*Do we need to reconsider the laws of lo grouifofon?*" p. 101).

### 3 - Anisotropy of the support

#### *Experimental process*

1- To demonstrate the influence of support anisotropy on the motion of the paraconic pendulum in an experiment lasting  $\theta = 14$  min, I carried out successive releases at equidistant azimuths of  $q$  grades as follows <sup>1</sup>.

In each azimuth  $p$  releases are made;  $N = (200/q) p$  experiments are thus carried out. To eliminate any systematic influence over time, the order of the starting azimuths is determined using a table of random numbers.

In the course of a given experiment lasting  $\theta = 14$  minutes, the plane of oscillation of the paraconic pendulum moves through an angle  $\Delta\phi$  from the initial azimuth  $Q$  considered. Plotting the  $\phi$  on the x-axis and the mean variations *in grades per minute* on the y-axis

$$(1) \quad \phi' = \Delta\phi / \theta$$

we obtain a correlation graph between the average angular displacement speed per minute and the starting azimuth.

#### *Empirical representation*

2- If, for each azimuth, we average the  $\phi'$  we obtain a sinusoidal curve with a period equal to 200 grades. The least-squares method can then be used to calculate the sinusoidal fit

$$(2) \quad \phi' = a \sin 2(\phi - Z)$$

that best represents all observations.

(1) Voir ma *Noie* du 9 février 1959 à l'Académie des Sciences, *Détermination expérimentale de l'influence de l'anisotropie du support sur le mouvement du pendule paraconique.*

We then observe that the correlation residuals still have a sinu-soidal shape with a period twice as small, and we are finally led to the *empirical representation*

$$(3) \quad \xi' = ap + a_1 \sin 2(Q - Z_1) + a_2 \sin 4(\xi - )$$

We can also observe the value in centimetres at the end of 14 minutes of the minor axis  $2b$  of the ellipse described by the tip of the needle placed at the inferior part of the pendulum, and the correlation obtained can also be represented by an expression of the type <sup>2</sup>

$$(4) \quad 2b = 2Q + 2b_1 \sin 2(\xi - Z_1) + 2b_2 \sin 2(\xi - )$$

*Expressions (3) and (4) represent the combined action of the support and the eddy current effect.*

### *Estimating the effects of substrate anisotropy*

3 - *Tobleou X* shows the results obtained for different values of  $p$  and  $q$  with the two identical paraconic pendulums  $P_1$  and  $P_2$  and the two virtually identical suspensions @ and  $S_q$  that I used in my two laboratories at Saint-Germain and Bougival during the experiments of 1955, 1956 and 1958 <sup>3</sup>.

The two *graphs XXXTV* represent for  $q = 10$  grades and  $p = 5$  the results obtained for angular variation and minor axis  $2b$  for the pendulum and suspension at the Saint-Germain laboratory from March 4 to 10, 1955.

The angles  $Z_j$  and  $Z_1$  on the one hand and  $Z_2$  and  $Z_j$  on the other correspond remarkably well. The azimuths  $j_j$  are very close to the azimuth of the perpendicular to the support, equal to 371.16 grades, which can thus be considered to represent the support's anisotropic azimuth at IRSID.

The theoretical displacement corresponding to the eddy current effect is shown in *Graph XXXTV* for the displacement of the oscillation plane.

(2) To obtain the minor axis in radians, divide its measurement in cm by the distance  $l' = 105$  cm from the tip of the pendulum needle to the center of the ball (see note 6 in § A.1.2 above),

(3) Some of the Pohteu slips in my Note of February 9, 1959, have been corrected on the 2 "ehteou X below.

A comparison of the results of the August 13th 1958 correlations for Bougival and Saint-Germain (*Table X*) shows that in both laboratories, the two supports exerted practically *the same influence* on the movements of the two pendulums.

If we limit ourselves to the main effect in azimuth represented by relationship (2), we have the following average in *grades per minute*

$$(5) \quad g_{\theta} = -0.127 - 0.677 \sin^2(g-372.11)$$

and in *radians per second*

$$(6) \quad = 0.262 \cdot 10^{-3} = -0.8 \cdot 10^{-3} - 1.772 \cdot 10^{-3} \sin^2(g-372.11)$$

*I- the effect of substrate anisotropy is thus of the same order of magnitude as the eddy current effect equal to -0.550.10<sup>-3</sup> radian per second, i.e. -0.21 grades per minute 5, 6,*

#### *Linked observations*

4 - This shows that when, in the course of a series of observations, the azimuth of the pendulum settles permanently in an azimuth far from the azimuth  $Z = 371$  grades, *it is because a cause C* is acting which counterbalances the restoring effect of the suspension. As shown by the experiments analysed above, this restoring effect would be to rapidly bring the plane of oscillation back to the direction of the Z plane, which, given the combined influence of the support and the balls, constitutes a stable direction of equilibrium. For a deviation of 50 grades, cause C is equivalent to around three times the Foucault force <sup>7</sup>.

*Ives fluctuations due to cause C of the equilibrium position of the pendulum's oscillation plane, correspond to the anomalies of the paraconic pendulum with anisotropic support.*

(4)  $(a/200)/60 = 2,618.10^{-3}$ . The constant term -0.127 is of the order of half the Foucault effect equal to -0.21 grade per minute.

(5) Note (1) in § 2.1 above, p. 173.

(6) Assuming, with the notations of Z'ouéou W (p. 128)

$$f' = \sin^2(E - \theta)$$

we have (*Table X*, p. 180f.

$$\tan = (0.174/2)/(103.840) = 1.01 \cdot 10^{-3} \text{ rad./sec.}$$

(7)  $1,772 \cdot 10^{-3} / 0,550 \cdot 10^{-3} = 3,22$ .

*Other observations*

5 - *Graphs XXXV* compare the **results** obtained by the same analysis at *different times and for different pendulum types*. The second *graph (XXXV)* shows observations from March 4 to 10, 1955 for Teòfeoti X.

The *ogèbric* mean of the four series of observations gives in *grades per minute*

$$(7) \quad = -0.047 - 0.897 \sin 2 (\$ - 374.95)$$

and in *radians per second*

$$(8) \quad = -0.123.10 - 2.348.10 \sin 2 (\$ - 374.95)$$

With regard to the amplitude and phase of the adjustment sinusoid, these results are exactly the same as those shown above g- \*.

*Effect of substrate anisotropy*

6- In total, the anisotropy of the support exerts a restoring effect towards the azimuth of 371 grades (count4 from the South in the direct direction, or 171 gradea counting azimuths in the direct direction from the North) corresponding to the perpendicular to the beam, support of the pendulum.

The result is that for *the seven monthly series* from 1954 to 1960, the azimuths of the oscillation plane (*measured directly from the north*) were constantly between 93 and 268 degrees. The

azimuths) of the aept monthly series were consistently between 150 and 174 grades. The mean value of mean azimuthsa

§ was4 about 164 grades 10

(81 On *ZXXV trophics*, the quantity represented is  $y = y_p + a \sin 2 (\$p - \$)$ , while on DIV tJrepAi9ues, the quantity represented is  $\$' = ap + at \sin 2(\$ - \$t)$ . Corresponding notations are  $y_p = a$ ,  $a = -a$ .

(9) In the August 1954 experiments,  $y_p = 0.18$  grades/minute, a positive value, whereas all other values are negative. In this series of experiments, the weight of the pendulum was much higher (see § A.3.1 above).

(10) 7'nòIenu f of § A3.1 above.

In the 7'ùJorie GènJrnie *du f-endule* that I **drew up in 1956**, I counted the angles *positiuemf in the direct direction from the South*, but for calcula of later application it seemed simpler to calculate them from *the North in Je zenc direct*.

**CARACTERISTIQUES DE LA CORRELATION  
DU MOUVEMENT AVEC L'AZIMUT DE DEPART**

5U5FC75ION	7EfidULC q		5	NOMBRE TOTAL D'EXPERIENCES	IERIOSCS D'OBSERVATION	DEPLACEMENT EN AZIMUT EN GRADES/MINUTE					SMALL AXES OF THE CLIPSE EN FIN 05%PENIECE GN EN				
	UTILISE EN GRADES					$\phi' = a_0 + a_1 \sin 2(\phi - \Sigma_1) + a_2 \sin 4(\phi - \Sigma_1)$					$2b_0 + 2b_1 \sin 2(\phi - \Sigma_1) + 2b_2 \sin 4(\phi - \Sigma_1)$				
						$a_0$	$a_1$	$\Sigma_1$	$a_2$	$\Sigma_2$	$2b_0$	$2b_1$	$\Sigma_1'$	$2b_2$	$\Sigma_2'$
Saint Germain	P,	10		100	4 ou 10 mars 1955	-0,114	-0,236	370,67	+0,089	8' 00"	+0,001	-0,166	370,90	+0,015	375,78
"	P,	20	2	20	4 janvier 1956	-0,131	-0,607	383,58	-0,069	397,71	0,030	-0,141	388,95	-0,07	391,24
"	P,	20	2	20	21 mai 1958	-0,157	0,781	371,34	0,152	573,08	+0,016	0,200	372,74	0,065	373,05
-	P,	20	2	20	13 août 1958	-0,153	-0,541	365,51	0,166	390,02	+0,010	0,177	371,69	-0,065	40/63
8-ti-l	P,	20	Z.	20	13 août 1958	-0,078	-0,616	369,24	0,087	382,09	0,000	-0,188	369,19	0,07	80,61
<b>MOYENNES</b>						-0,127	-0,677	372,11	+0,015	385,38	+0,017	-0,174	374,70	+0,003	384,66

PENOULES

8. : et ngyle utiil# t dens le t xpi rie nc es c ntinue s d e t  
mois en Nov.-Déc. 1954, Juin- Juillet 1955 à S'- Germain  
et Juin- Juillet 1958 à Bougival

P: pendulum utaisi in June - July 1956 à 5'. 6er moir

**EFFET DE FOUCAULT**

0.21 grade/oinuLr

t " ti lliSTI(UÉS Ofi s 5U5PFsSIOHS iitIL lf l(S

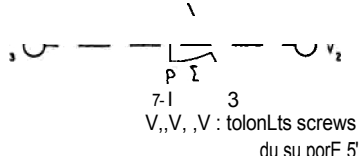
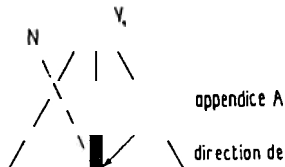
AliNut dt lo perpen0itulu0ir e ou suppor L I 371.16 grades

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N0sst N" 1Z kgs.

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par rayporL cu center de  $B \_ \text{g3, } j1 \text{ } 10'$   
lo bille de suspension  $\_ \text{0,3M. } 10'$

Ptriode T . 1,85 seconat  
Dist#nct du c0ntrz 0s grovit  $\text{OG t . } 83 \text{ tm t}$   
or tenLre de lo bille  
R0yon de lo bille  $\_ \text{0,3Z5 to}$

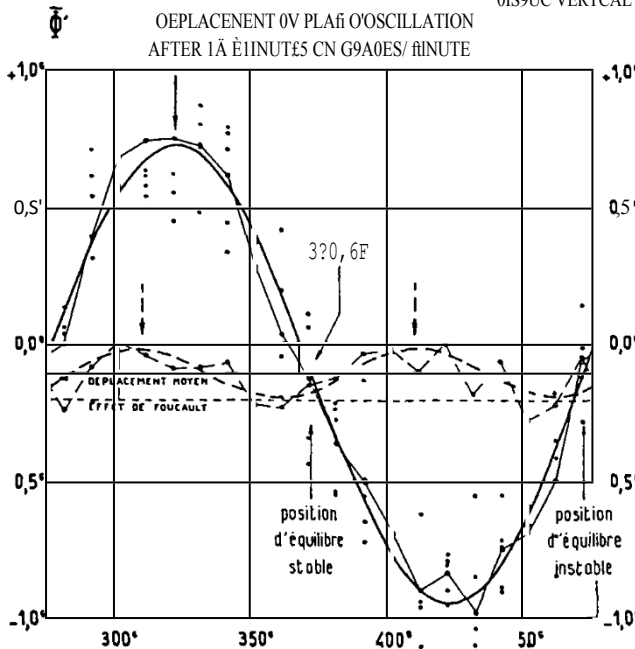


Source: Graph IV.B.2 of my Conference of November 7, 1959, and my *Noie* of February 9, 1957 at the Académie des Sciences, Détermination of the influence of the anisotropy of the support on the motion of the paraconic pendulum.

**CORRELATION DU MOUVEMENT DU PENDULE PARACONIQUE  
AVEC L'AZIMUT DE DEPART**

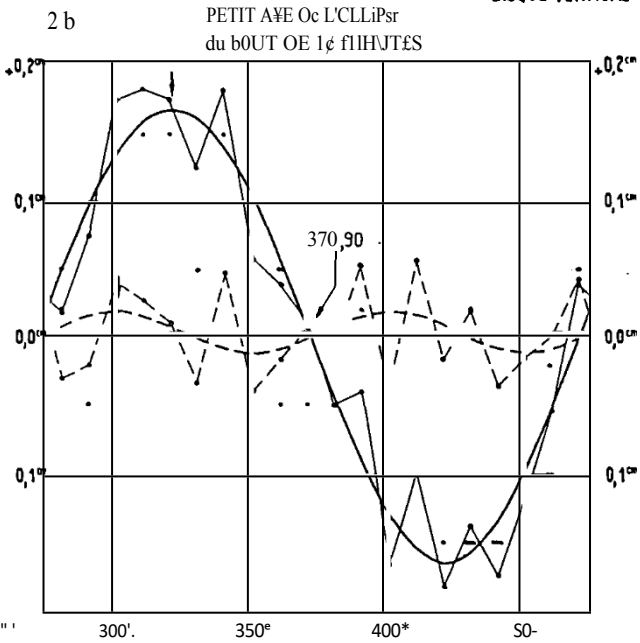
TO 10 MARS 1955

PENDULE P  
DISQUE VERTICAL



4 AU 10 MARS 1955

PENDULE P  
DISQUE VERTICAL



Observations Alimentaires

— average curve  
— sinusoid of a justerint

— average curve bts r t sidus  
— sinusoid of o justement dçs ri si dus

No a : L z s icarts por ra part aux cowbts ngmms  
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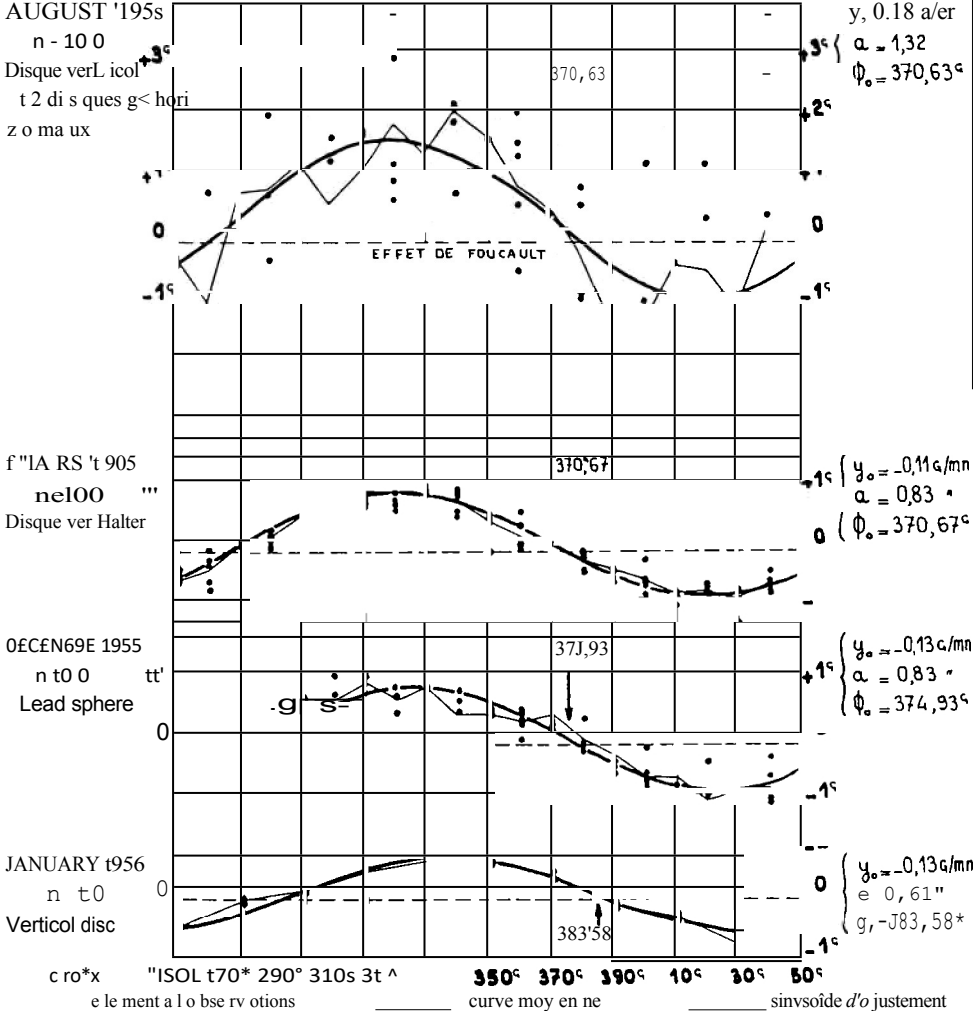
Source: *Trophique IV.B.1* of my Conference of November 7, 1959, and my *Note* of February 9, 1957 to the Académie des Sciences, *Détermination expérimentale de l'influence de l'anisotropie du support sur le mouvement du pendule paraconique.*

**ANISOTROPIE DU SUPPORT**

**SUSPENSION SUD**  
 August 1954s l "Iars 't959  
 Dù c "mbra 1950\_ Janvier t956

**CORRELATION DU DEPLACEMENT ELEMENTAIRE DU PLAN  
 AVEC SA POSITION INITIALE  
 TION LAD'OSCIL**

**VARIATION EN GRADE PAR MINUTE APRES 14 MINUTES D'EXPERIENCE**



Ugende: The eddy current effect corresponds to - 0.21 grades per minute.  
 Source: *Efraphig ue IV.B.2* of my Conference of February 22, 1958.



#### 4 - Influence of beads

*The third influence on the movement of the paraconic pendulum is that of the balls.*

Neither the beads nor the bearing surfaces can be considered perfect <sup>1</sup>. In fact, experiments have shown that for each elementary 14-minute experiment, the beads have an influence on the bearing surfaces. average randomness of 2.5 grades, of the same order of magnitude as the Foucault effect <sup>2</sup>, **but which cannot explain the variations observed d'azimut as demonstrated by the triple-mentally chained experiments carried out in May 1957** <sup>3</sup>

In any case, *the random effects of the beads cannot explain the highly significant periods observed, especially the diurnal lunar period of 24 h. 50 min.*

(1) It was therefore in my interest to change the beads as often as possible, in order to eliminate their effects through the play of averages, an interest that had not occurred to me in my first experiments in June-July 1954.

In fact, from November 1954 onwards, the logs were changed at the beginning of each experiment, and on average the bearing surfaces were changed every week.

(2) See § B.1.1 above.

An influence of 2.5 grades for 14 minutes = 840 seconds is equivalent to an average influence of  $2.5 \text{ grades} \cdot 840 \text{ s} = 0.47 \text{ } 10^{-4} \text{ rad./sec.}$  whereas the eddy current effect is  $0.5 \text{ } 10^{-4} \text{ rad./sec.}$  Both effects are therefore of the same order of magnitude.

(3) § B.1.1 above.

### 5.- *Influences astronomiques périodiques*

*The diurnal and semi-diurnal lunisolar periodic frequencies which, in terms of amplitude, represent one of the most remarkable aspects of the paraconic pendulum's motion, have been highlighted in all the time series constituted by the azimuths **observed** during **the** month-long **observation** periods by various harmonic analysis techniques whose results have been remarkably concordant: Buy8-Ballot filter, Darwin or least-squares fit to a given group of waves, periodogram and correlogram.*

*The existence of these periodic tn/Itierices, and especially of the diurnal lunar periodic component of 24 h. 50 min., is a certi-*

*toy structures almost periodic ions*

1- In fact, all the graphs we can deduce from the series of observations show a *large number of* morphological similarities, symmetries and double symmetries, and local periodicities, and these sequences have *all the characteristics of almost periodic functions\**.

In view of their periodic components, as identified, which are relatively numerous and of incommensurable periods, it is certain that the azimuth series of the paraconic pendulum are *indeed* almost periodic functions to which are added random components due essentially to the influence of the balls.

(1) This is not, of course, a metaphysical certainty that would require an infinite series of observations, but a practical, human certainty or sense of *probability*.

(2) See § A.5.4 above, p. 101.

### *Orders of magnitude*

2- *The angular velocities of azimuth narration* corresponding to the amplitudes of the two largest periodic components detected in the seven monthly series of observations of the paraconic pendulum with anisotropic support, whose periods are respectively 24 h. and 24 h. 50 min, are each of an order of magnitude equal to the thirtieth of the Foucault effect. The sum of the amplitudes of all the periodic components appears to be of the order of the Foucault effect.

*In [cit, no experimenter has detected any lunisolar effects* in the movement of the Foucault\* pendulum, no doubt due to the low angular amplitudes of the oscillations and the generally long pendulums used, but also and above all due to the always *very limited* duration of pendulum experiments.

*In theory*, no author has really studied the lunisolar action on pendulum motion. Indeed, all authors considered such an action to be so small that it was experimentally undetectable, and therefore useless to calculate.

### *Lunisolar effects and coefficients of the general theory of Newtonian potentials*

3- In view of the general theory of the Newtonian lunisolar potential in the literature <sup>5</sup>, it is essential to compare the orders of magnitude *redrawn* from the various periodic lunisolar components corresponding to the paraconic pendulum and to the gravitational forces deduced from the lunisolar potentials.

(3) \*\* The lunar diurnal period of 24 h. 50 min. corresponds to a variation of the order of  $0.2 \cdot 10'$  radians per second, whereas the Foucault effect is  $0.550 \cdot 10'$  radians per second. We thus have  $2 \cdot 10' / 0.550 \cdot 10' = 0.36 \cdot 10^{-1} = 1 / 27.8$  (§ A5.3. above).

(4) As far as I know, Dejean de Fonroque is the only experimenter to have made lunisolar effects. In 1879, well before Miller (*Chapter IV* above), he even pointed out that below, Sections C and F) existence of an orbital effect due to the Earth's motion on its orbit and an effect corresponding to the movement of the solar system towards the Constellation of Hercules. However, his observations are only *quasi-observations* and really *little* conclusive. They are practically unusable (see note 7 in § B.2.6, p. 50 of the above introduction).

A detailed analysis of Dejean de Fonroque's memoirs can be found in the second section of this book (*Chapitre II*, Section C).

(5) See especially: Paul Schureman, *Moon and Harmonic Analysis and Prediction of Tides* U.S. Department of Commerce, Washington, 1941.

For simplicity's sake, I'll limit myself here to lunisolar diurnal effects. *Table XI* presents the analysis of these effects for the four most important diurnal waves Ki (23.93 h.), Mb (24.84 h.), Oi (25.82 h.), et 9i (26.87 h.)<sup>6</sup>.

We can see that the *relative* structure of amplitudes is *totally dif- fërente* for the paraconic pendulum and for the gravitational theory of lunisolar forces. Thus, for example, the lunar Mi component of 24 h. 50 mn. (24.84 h.) is *relatively four times greater* for the paraconic pendulum than for the tidal theory, while the 24-hour solar component is *relatively twice as small* as for the gravitational force, *giving a relative difference of 1 to 8*.

It is this *completely fõndamental* structural difference that allows us to assert that the anomalies of the paracic pendulum constitute an *entirely noiirenuous phtnomenon*, quite dis- tinct from the phenomena deduced from the Newtonian theory of gravita- tion. *In fact, the periodic structure observed cannot be considered as deriving from any of the phenomena resulting from the graui- tational potential of Lttnë and lsoleil.*

(6) A detailed analysis is presented in the second *volume* of this book. (*Chapter Yi*, Section A, see above, p. 30).

The coefficients deduced from the theory of Newtonian potentials apply equally well to deviations from the vertical and to the theory of tides (see note 5 above).

It is curious, to say the least, to note how the orders of magnitude of the coefficients of the lunisolar potential developments f7'õbteor Xfj are *largely* ignored in scientific circles, even among specialists in geo-phy- sics.

DIURNAL LUNISOLAR EFFECTS  
PARACONIC PENDULUM  
AND LUNISOLAR FORCE COEFFICIENTS

Periodicals	K and P 24 h.	My 24,84 h.	Ot <b>25,82</b> h.	Qt 26,87 h.	Total amplitudes
		Pendulum paraconique Amplitude de l'onde 2R en grades			
June-July 1954	2,34	4,12	2,56	6,76	15,98
Nov.-Zac. 1954 <sup>1</sup>	11,26	11,54	4,64	6,18	33,62
June-July 1955 <sup>3</sup>	13,00	10,46	4,78	7,78	36,02
Averages	8,87	8,71	3,99	6,91	<b>28,54</b>
Relative values: a	31,08	<b>30,52</b>	13,98	24,21	100
		This theory of forces lunisolaires			
Coefficients	0,7060	0,0977	0,3771	0,0730	1,2g38
Relative values: b	<b>56,31</b>	7,79	30,08	5,82	100
		Import of their relatives			
a/b ratios	0,552	3,92	<b>0,465</b>	4,16	1

*Ggende:* 1) series of 721 hourly values centered on June 23, 1954, 12 h. 20 min.  
2) series of 721 hourly values centered on December 3, 1954, 12 h. 20 min.  
3) series of 721 hourly values centered on June 22, 1955, 12 h. 20 min.

*sources:* 1) *Pendule Poroconique*: Calculations by the Hamburg Hydrographic Institute  
ma Note du 4 décembre 1956, *Analyse harmonique des mozzuements dzz  
pendzle poroconiqzze. Compléments* (3 p.).

2) *Coefficient s de lo théorie des /orces lunisolaires*: Schureman, *Manual of  
Harmonic Analysis end Prediction of Aides*, 1941, p. 16d-165.  
The amplitudes of very similar periods are grouped together.

6 - *Aazfiz-es fanteura*

**In view of** the periodicities observed, and in particular their amplitudes, so-called explanations and objections have been put forward again and again. I believe it is necessary to examine a few of these by *way of illustration*.

*The devices will be imperfect and therefore we will not to draw any valid conclusions from my experiences.*

1- It's a fact that no matter how precisely the appliances were built they have construction defects.

**For example, the pendulum's center of gravity may be slightly off-center** <sup>pdf rã rapport à l'Éxt priflCfpäl d'Értie passant pÉfr II Cynthia dt la bflle</sup><sup>3</sup>, or the metal parts may not be perfectly homogeneous. **As for** balls and bearing surfaces, **they** are not perfect **and are** more or less deformable.

But, *whatever these construction defects may be, they can in no way lead to the appearance of real periodic effects.*

*The effects observed are due to the magnetism of the small steel truncated cone (about two hundred grams) linked to the pendulum and in which the ball is embedded.*

2- This magnetic action probably exists, but it is *very weak*, and without any calculation we can say that magnetism would have the effect of orienting the pendulum in an azimuth varying by only a few centesimal minutes per day, *which is not the case*.

(1) See my Note of April 25, 1957, *Interpretation to be given to all experiments on the poroconic pendulum* (18 p.)

(2) All the pendulums used were built to an accuracy of one hundredth of a millimeter (see my Note of January 15, 1957, *Note sommaire sur les recherches sur le pezdute poroconique*, 13 p., p. 5).

(3) It is in fact particularly difficult to achieve a perfectly straight stem with a material such as brass, which was chosen for its non-magnetic qualities.

*The effects observed are due to eddy currents generated in the pendulum by its movement in the magnetic field.*

3- If such an effect existed, it would have the effect of orienting the pendulum in a privileged direction that would only vary by a few centennial minutes per day, *which is not the case.*

*Shifts in the plane of oscillation are due to air movements in the laboratory*

4- If such an action were the explanation we were looking for, the pendulum's movement would be the result of a series of random actions. In the harmonic analysis of azimuths, therefore, no *statistically significant sinriodicities* would appear.

*The periodicities observed would be real, but they would be due to the elastic reactions of the building.*

5- However, the building itself cannot exert any periodic action. If it does exert an action, it is in fact a transmitted action, in which case the observed phenomena would be due to the amplification of an already known geophysical phenomenon. But in fact, all known geophysical phenomena have periodical structures *very different* from those of the paraconic pendulum <sup>4</sup>.

*The equations of motion of the paraconic pendulum would have periodic solutions with periods precisely around 2d h. and 2d h. 50 min.*

6- It should first be pointed out that such an argument completely overlooks the other periodicity observed <sup>5</sup>

In any case, the argument would only be valid if the paraconic pendulum oscillated continuously.

(4) \*\*\* See § B.3.1 and B.3.3 above.

(5) See *Tobleou II* of § A.S. 2 above and *Toëleou Xi* of § 5 above.

In fact, as the pendulum is restarted every 20 minutes from a position of rest, its movement should only depend on **the azimuth of departure**, and it should always be the same. However, we observe that for all azimuths in the interval (370 + 50 grades), the azimuth variation during a given 14-minute experiment is sometimes positive, sometimes negative, depending on the epoch.

In any case, periodicities of 24 h. or 24 h. 50 min. would not be possible in 14-minute experiments.

*The distribution of anomalies according to the normal distribution would show that they are of a random nature.*

7- Certainly, chance can imitate periodicity, but periodicity can also imitate chance.

In fact, as early as 1954, I was able to verify that the sum of the ordinates of 13 sinusoids obtained in the analysis of my numerical series by the least-squares method *was distributed according to normal* faith. However, it is indeed the sum of 13 sinusoids, and therefore a clearly defined, *non-random* quantity. As a result, the *randomness* of a numerical series in no way excludes it from representing an almost periodic non-random phenomenon.

It was this observation that led me in 1981 to demonstrate a theorem I called *Theorem T<sup>6</sup>*.

*No similar phenomenon has been observed in gravitation experiments.*

8- In fact, such an argument is worthless on its own. The

16) Voir mon mémoire de 1982, *Fréquence, Probabilité et Hasard*, avec deux Appendices : 1.-fréquences empiriques et fréquences mathématiques - Illustration, 2 de Théorème T - L'o sim motion du hasard por des ouchions presque périodiques. This memoir is appended as *Appendix E* to the *Second Name* of this work.

In fact, if a phenomenon is the result of a sufficiently large number of incommensurable periodic influences, its values over time will follow a normal distribution.



*The very essence of the discovery of any new phenomenon is that it hasn't been highlighted before.*

*A priori assertions*

9- On numerous occasions from December 1956 onwards, Henri Villat, President of the Section de Mécanique at the Académie des Sciences, constantly asserted that I hadn't theorized the device, and that if I had done it correctly, everything would be explained.

In his letter of March 26, 1956, he wrote to me:

*"But the anomalies, or rather what you consider to be anomalies in your experiments, will be explained in the simplest possible way as soon as you've made the necessary calculations".*

and in his letter of June 4, 1958 to our mutual friend G. Varlan il wrote:

*"Once the (device) equations have been integrated, or at least properly interpreted, t h e r e will be no more myst ère".*

In fact, on December 6 1956 I had sent him my geTleral dissertaTion of s£1 sE1ptE1mbre 1956 "*ThêoTie du pendule paraconique*"<sup>8</sup>.

But that wasn't the real issue. It lay in the orders of magnitude. As I pointed out to Henri Villat in my letter of **July 24, 1958** :

*"I feel I must draw your attention to the fact that the periodic lunisolar ef- fects on the motion of the paraconic pendulum that can be calculated from the classical theory of gravitation are of the order of 10! and therefore inappreciable.*

(7) In reality, my opponents were driven by a single idea. The accepted theories, perfectly verified, could not be challenged. It was totally impossible for a non-professional to have done so, and his experiences could therefore have no value.

(8) This 441-page memoir set out the general theory of the paraconic pendulum. To determine the elliptical trajectory of the paraconic pendulum this memoir mdtuait la **variation** these constants does Logron,ge and he indicated in his Seventh **Partie** of synthesis (p. VI I.1 è VII.29) all the /orm mes of first approximation corresponding - pondotif or movement of the poroconic petitude and to all the /ncteurs intervening in this movement.

On the influence of the experimental set-up, see § 6.1 or 6.6 above.

*"So the rigorous theoretical calculation of motion seems to me to be of purely academic interest, since the order of magnitude of the effects calculated in this way is about a hundred million months smaller than the order of magnitude of the effects observed?"*

*"L to and only there seems to me to be the important question. I have observed effects of rotation of the oscillation plane of a periodic nature, of the order of magnitude of 10 radians per second, whereas if the observed movements are due to lunisolar action, the theory can only explain effects of the order of 10E-10 radians per second. It is only from this angle that the theory of the device seems interesting and indispensable to me..."*

*At the end of our letter of June 4, 1958, we all said that as soon as the calculations had been made and correctly interpreted, there would be no more mystery.*

*"I don't quite understand your point of view. It's easy to see that the effects of rotation due to the lunisolar influence are of the order of 10E-10 radians per second \* while the effects observed are of the order of 10<sup>5</sup> radians per second. The difference in orders of magnitude is such that an explanation of the effects observed by the classical lunisolar influence seems impossible to me.*

- To my knowledge, this order of magnitude has not been contested by anyone who takes the currently accepted theory of relative motion as a starting point.

In fact, Henri Villat never replied to my letter of July 24, 1958 9, 10

(9) In view of the impact of Henri Villat's *a priori* affirmations within the Académie des Sciences, I reproduce in the *Deuzième Volume* of this work, in *1958*, the entire correspondence relating to Henri Villat's incessantly repeated assertions.

In fact, Henri Villat always refused to visit my laboratory at IRSID, and I was only able to meet him once, on December 3, 1956 (see above § C.2.3, note 7, p. 148).

(10) In a letter dated 1<sup>st</sup> June 1960, Jean Leray, member of the Section de Mécanique de l'Académie des Sciences, wrote to me

*Yos considérationz l'horiquec ... n'aboutiscenf poc à des conclusionz numdri9ues; vou4 ne confrontez pes les grondeurs que uouc ouez nit mesures d l'turc uoleurs colculéec fndori9uement. Veus n'ouez donc pes [nit lo th orie de Notre pendule".*

Jean Leray was thus repeating word for word the arguments presented two years earlier by Henri Villat. And, like Villat, he took no account of my September 1956 dissertation "2<sup>e</sup> *théorie du pendule paraconique*", which I had sent to him, nor of the orders of magnitude, *font ó /oit essentiels en l'espèce*.

Visit fait, Jean Leray ignored the crucial experiences of July 1958 section C above.

In fact, what observation shows is that during each 14-minute experiment, there is a limit plane, resulting in particular from the combined action of the support and astronomical influences, such as the lunisolar action, towards which the pendulum's oscillation plane tends. *This limit plane varies constantly over time.*

*Empirical representation*

1- **In view of the results obtained in the analysis of** the influence of supply 1\$ anisotropy, we can validly assume that *a course of*

*For each 14-minute experiment, the mean value \$ ' of the variation in azimuth per unit time is as follows*

$$(i) \quad Q' = -tu \sin L + k \sin 2(X - Q) + K \sin 2(Z - \phi) + C$$

with 2

$$(2) \quad k \sin 2(X - Q) = \sin 2(Z - \phi) - C$$

In these relationships  $-tu \sin L$  represents the Poucault effect,  $Z$  the direction of support anisotropy,  $X$ ; mean azimuth corresponding to astronomical influence *during the 14-minute period under consideration*, and coefficients  $k_j$  and  $k$  are time-varying coefficients. The  $C$  term represents the random influence of the beads.

represents the mean azimuth of the pendulum's plane of oscillation at during the 14-minute experiment under consideration, and  $\phi'$  represents its average variation per unit of time over this period, with  $\phi' = d\phi/dt$ .

(1) \* § E.3 above, p. 176.

(2) Naturally, we have

$$(1) \quad k \sin 2X = Z k_j \sin 2X, \quad k \cos 2X = Z k, \cos 2X_j$$

$$(2) \quad k = \frac{Z + 2Z \int \cos 2 \hat{\phi}_j}{\int \cos 2 \hat{\phi}_j}$$

$$(3) \quad \tan 2X = (E k_j \sin 2) / (E k_j \cos 2 \hat{\phi}_j)$$

*expression of the azimuth of the boundary plane*

2- Relationship (1) can also be written as

$$(3) \quad \xi' = -c' \sin L + f \sin^2 (Y - \xi) + c$$

by setting  $\xi = \xi'$

$$(4) \quad f \sin^2 (Y - \xi) = k \sin^2 (X - \theta) + K \sin^2 (Z - \zeta)$$

Y represents the mean azimuth of the boundary plane towards which the plane of oscillation of the paraconic pendulum tends *during* the 14-minute experiment under consideration, *if f has a sufficiently high value.*

Naturally

$$(5) \quad Y = Y(t) \quad X = X(t) \quad k = k(t) \quad f = f(t) \quad c = c(t)$$

The Y direction corresponds to the overall effect of the astromonics of mean X direction and the influence of Z direction support anisotropy.

f3i Naturally we have

$$(1) \quad f \sin^2 \nu = k \sin^2 X + X \sin^2 Z \quad f \cos^2 Y = k \cos^2 X + K \cos^2 Z$$

$$(2) \quad f = \frac{k^2}{2k \cos^2(X - \theta)}$$

$$\frac{k \sin^2 X + X \sin^2 Z}{k \cos^2 X + K \cos^2 Z}$$

We can also write

$$(4) \quad \operatorname{tg}^2 (Y - E) = \frac{k \sin^2 (X - E)}{K + k \cos^2 (X - Z)}$$

*Limit plan and experience*

3- Relationship (3) corresponds exactly to what is suggested by the chained troia aérica experiment carried out from May 6 to 10, 1957.

Naturally, what we observe is the azimuth  $\xi$  of the pendulum at the **cOtlPB of** a chained experiment, and this azimuth is in relation with both the  $Z$  direction of the support's anisotropy, and the  $X$  direction representative of all astronomical influences.

In representation (1), the attraction of the  $\xi$  oscillation plane to the  $Z$  direction is greater the greater the coefficient  $k$ , the more pronounced the  $X$  direction, and the greater the coefficient  $K$ , the greater the attraction of the oscillation plane to the  $Z$  anisotropy direction of the support.

As far as we can tell, the fact that the plane of oscillation **constantly deviates from the  $Z$  direction of support aniaotropy** shows that the coefficient  $k$  is of *an order of magnitude comparable to* that of the coefficient  $K$  <sup>5</sup>.

*Determining the  $X$  direction of anisotropy in space*

4- Determining the direction of X-ray anisotropy in space due to astronomical influences is naturally of *considerable* theoretical and practical interest.

*This involves the creation of an isotropic support* for which the  $K$  coefficient in relation (1) is zero, and is the subject of *Chapter II* below.

(4) § B.1.1. above, p. 103.

(5) This deduction appears to be confirmed by analysis of the series run from September 28 to October 4, 1959, using the paraconic pendulum and isotropic support (see Section *H* of *Chapter II*).

## 8.- Street ç'ezzeemôfe

Empirical and theoretical analysis of the observations performed on the movement of the paraconic pendulum has shown that the azimuthal movement of the paraconic pendulum with anisotropic suspension is essentially the result of four combined effects: the eddy effect, a restoring effect of the anisotropic suspension, the random influence of the balls, and periodic influences of astronomical origin.

*To a first approximation, whether we're talking about lunisolar effects or the effects of substrate anisotropy, the effects observed result from the effectiveness of trajectories generated on the one hand by substrate anisotropy and on the other by astronomical influences.*

*In fact, while the oscillation of the paraconic pendulum remains plane, there is no detectable effect of support anisotropy or lunisolar periodic influence, and the motion of the paraconic pendulum is reduced to the Foucault effect.*

At each instant there is a limiting plane, the result of the Foucault effect and the combined action of the support and periodic astronomical influences, towards which the pendulum's oscillation plane tends. This limiting plane **varies** over time due to astronomical periodic influences.

## THE INERTIA SPACE HYPOTHESIS COPY OF INERTIA SPACE

1-- Z-o fhcorze ej f'expsrzence

*Incompatibility of the results of observations of the parabolic pendulum with anisotropic support with the classical theory of mechanics*

1 - All the foregoing analyses lead to one *eöso- fumentally indisputable* conclusion. The amplitudes of the periodic luniso- lair components of the motion of the paraconic pendulum ä anisotropic support are *totally inexplicable* within the framework of the current theory of gravita- tion.

For the 24 h 50 min periodicity in particular, this theory leads to diurnal variations in the azimuth of the oacillation plane of the paraconic pendulum of the order of  $10^{13}$  pg g2ys per second, whereas the effects observed are of the order of at least  $10^{15}$  pg pjjs per second in the case of anisotropic suspension \*.

La différence des ordres de grandeur entre les effets observés et calculés est telle *qu'aucune des personnalités* qui viennent visiter mon laboratoire de l'IRSID n'a contesté *l'impossibilit totale* d'expliquer les mouvements obser-vnés du pendule paraconique dans le cadre de la thño- rie actuelle de la gravitation et des movemenŁs relatifs.

f) See above § I.B.2.5, p. 121-123.

Les effets obser-vés sont d'au moins de l'ordre de  $10^{15}$  radiana par seconde dans le cas of isotropic suapenaion (*Chapifre ff*, § F.2).

*The postulates of current gravitation theory*

2 - Since the current theory of mechanics leads to orders of magnitude that are *completely incompatible with observational data* in the case of the *periodic lunisolar components of the motion of the anisotropically supported paraconic pendulum*, we need to *re-examine the assumptions on the basis of which the theoretical orders of magnitude were obtained*. These assumptions are essentially the following:

- 1- *Gravitational forces are assumed to be inversely proportional to distance and proportional to mass*. The coefficient of proportionality is assumed to be the same whatever the masses and distances.
- 2- *The transmission of gravitational forces is assumed to be instantiated and to take place in a straight line in a space assumed to be Euclidean and isotropic.*

- 3- The force exerted on a point M whose inertial mass  $m$  is assumed to follow the law

$$(1) \quad \vec{F} = m \vec{T}$$

It is therefore assumed to be proportional to the inertia mass.

- 4- *The mass corresponding to gravitational forces is assumed to be equal to the mass of inertia.*

- 5 - The law  $\vec{F} = m \vec{y}$  is assumed to be applicable only with respect to a system of axes  $O' x' y' z'$ , called *Galilée axes*, animated by a uniform translational motion relative to the system of axes, called *Copernicus axes*, whose origin is the solar system's center of gravity and whose directions join this center of gravity to three fixed stars.

- 6 - Let  $O x y z$  be a system of axes linked to the earth. We have

$$(2) \quad \vec{T} = \gamma \cdot \vec{T}_r \cdot \vec{T}_t$$

$$(3) \quad \gamma = \frac{r}{c}$$



$a_2$ , denotes the relative acceleration of point M with respect to the referential **Oxyz** linked to the **Earth**;  $a_y$  is the drag acceleration due to the motion of the **reference system**  $Oxyz$

with respect to the reference system  $O'x'y'z'$ ; and  $a_y$  designates the complementary acceleration, known as the Coriolis acceleration.

**Ona**

$$T \quad \gamma, = 2 \quad m \quad v,$$

where  $v$  denotes the velocity of point M relative to the axes  $Oxyz$ , and  $\gamma$  and the instantaneous rotation of the reference frame  $Oxyz$  with respect to the galilean reference frame  $O'x'y'z'$ .

Relation (2) is a pure mathematical identity.

7 - We have

$$(5) \quad F = F' + f_2$$

$F_1$  **represents** the force of attraction of **the Earth** and the other stars, and  $F_2$  the resultant of the other applied forces. The force  $F_1$  is assumed to be proportional to the peaante mass of point M.

In the case of the Foucault pendulum,  $F_2$  represents the tension  $N$  of the suspension wire. Dana le caa du pendule paraconique  $F_2$  représente la force  $N$  exée par le support aur la bille du pendule.

8- It follows in particular from these assumptions that the space corresponding to Copernicus' reference frame is considered to be *par- ticularly Euclidean and isotropic everywhere*.

Alla these hypotheses are classic \*, mara il convient ici de lea rappeler. In view of the observed motion of the paraconic pendulum, *some of these hypotheses are certainly invalidated by experience.*

*Eeo v 'rififications of the current theory of gravitation*

3 - In fact, the current theory of gravitation appears to be well verified:

in the case of astrea motion (celestial mechanics) \*, in the case of falling bodies on the Earth's surface \*, in the case of Foucault's pendulum for rectilinear oscillation \*, in the case of lunisolar deviations from the vertical if we takes into account the deformation of the terrestrial apheroid under lunisolar action 6,

On the other hand, the theory of gravitation eat *mise en tchec* when applied to the caa of the influence of the Sun's and Moon's attraction aur the motion of the paraconic pendulum, whether agiaaea of the amplitudes of the luniaolar periodic componentsa or of thea anomalies conatat4ea lora dea eclipaes total de Soleil.

(2) See for example Paul Appell, 1953, *Trois J de Mtonique* 2tionnetle, Tome II, Chapter XXII, p. 267-302; A. Foch, 1967, 3fJceni9ue, Masaon, p. 149-156.

(3) In his *Coure de 3fttaniq c fGauthier-Villaró*, 1930, Tome I, p. 387) Paul Painlevé éwit

*'Par del approximatonz zuccezzuez, les èquationz qui done le tae nit troie corpa scüèment a'ottireni dorment fes soüitions du problhme del trets corpz permeltent de tolculer les èp?idmJridcs pour un long interueiè 'fr temps i fes toblez de W Verrier les dorment pour trets z 'lta fet ouzzi pour frois si2cies en orrihrej. Hi l'on compore fes position- toltulèez onet lee positionz oòseruJes, l'oteord cat une confirmotio Jcïotonte del lors de Newfon : i'oztre qui s'èeerte fe pins dr iè position prdvuc est to Lune qui posse tnnfôt en ovonce, tontôt en retard por lo poaitio colculèe en ua que ce retard ou cette evence fpozze une seconde de temps en un zihcle".*

f4) See for example Foch, 1967, id. p. 151-152.

(SI See for example Foch, 1967, id., pp. 155-156, and § A4 above.

(6) See in particular M.N. Stoyko, *L'extraction üniaofoirr et ièc penduie#*, Bulletin astronomique. Tome XIII, 1947, p. 6, 29-31, 46. The oòseruJe deviation from the vertical is equal to aa value calculated by assuming that the terrestrial spheroid is undeformable, multiplied by 1e coefficient  $1 + k - h = 0.667$ , where k and h are Love numbers corresponding to the deformation of the terrestrial apheroid under lunar action.

On lunisolar deviation from the vertical, see above § B.2.4 and 2'ebteeu Y B.2.4, p. 127. See also note 6, p. 135.

-f riaiozi 'Zr fr u4rī 'zñion 'Zen lits defogruu'ttofon

In view of thea *major* anomalies of the paraconic pendulum, we need to examine how accurately thea loa of gravitation are verified both in astronomy and on the Earth's surface\*.

Surprising as it may seem, all the treatises on Mechanics and Astronomy are remarkably silent on this fundamental question. This is an essential gap and an obvious deficiency from the point of view of scientific discipline. In fact, a law of any kind has no meaning whatsoever if we don't know the degree of precision with which it has been öt4 verified.

*Precise astronomical verification of mechanical postulates*

1- As the fundamental laws<sup>8</sup> of mechanics at the earth's surface are the result of an extrapolation of the results obtained in astronomy, it is of interest to examine the extent to which these laws have actually **been verified** in this field.

Unfortunately, this discussion is nowhere given, as **Newton's laws are assumed to be rigorously verified. However, without going into a detailed discussion that would be beyond the scope of this book,** it is relatively easy to ae the order of magnitude of this precision.

Consideration of the residuals of the least-squares adjustments used to draw up the tables currently used in astronomy shows that the order of magnitude of a

f) Allais, 1958, *Do we need to reconsider the feu lois <sup>gravitation</sup>*, p. 101-102. See also lément Allais, April 21, 1957, *Anomofie- du tOUU\*mCE1 ! du paroconiqua pendulum ä #up-eitotropic port* (71 p.), p. 51-56.

Voir également ma Troi-itme *Conférence du 18 mon J967*, Bulletin n- 121 du Cercle Alexandre Dufour, mai 1967, p. 114-118 (§ B.2.9 de *1 Introduction* ci-deesue).

constet4s deviations for angular displacements between observation and theory is at least of the order of 1 sexagdsimal second of arc <sup>2</sup> of which gives a relative error of the order of

$$(1) \quad 1 / 90.60.60 \times 3.10 -$$

Readers may wish to consult H. Spencer Jones's seminal article, *The rotation of the earth and the secular accelerations of the finn, Moon and Planets!*

Ⓔ Amn. deFob.dePæu:Mmoee, TomeIY (taTene) ;Y (Mecue) YlMa8  
\*tYtnœü, KXlY Satume) XXYIIt (UzanusandNeptuzne), XXXl (Jupit).

(3) Monthly Notices of the Roy. AsCr. Soc. vol. 99, 1939, 541-558. A good summary of this study is given in Danjon, *Astronomie Générale*, Sennac, Paris, 1952-1953, pp. 120 à 126.

The graphs on pp. 36 and 37 of this study show the following times  
Newtoniana determined using the orbital motions of the Moon, S01e1, Mercury and Venus only agree to within 2 arcseconds due to irregular fluctuations, taking as unit the arcseconds corresponding to the mean motion of the Moon. These8 accidental deviations can even reach 75 seconds for Venu8. If we con8ider that the Sun, Mercury and Venua travel respectively 1/13.37, 1/3.22 and 1/8.20 arcseconds on their trajectories when the Moon travels 1 arcsecond on its, we come to the conclusion that the poaition8 on the orbits are certainly not determined at a prtciaion greater than 1 arcsecond.

This is the conclusion we arrive at when we compare the forecasts made by various8 tables. For example, "Connaissance des tempe J 957" and "Z/ie American Spfiemeris 2857" forecast January 1, 1957 at 0 h. U.T.

	RIGHT ASCENSION			
	C.D. T .	A. E.	difference in second <del>de tempo</del> C. D. T - NE .	gap in second bow
<b>Soleil</b>	18h44m53s, 89	18h44m54s, 02	- 0,13	- 1,95
3f Mercury	19 57 46, 40	19 57 46, 41	- 0,01	- 0,15
WJnu*	16 56 13,30	16 56 13,22	+ 0,08	+ 1,20
<b>Mara</b>	0 49 57,52	0 49 57,35	+ 0,17	+ 2,55
-tupiter	12 7 17,48	12 7 17,47	+ 0,01	+ 0,15
Saturræ	16 31 32,96	16 31 32, 01	- 0,0E	- 0,75
ffronua	8 33 58,63	8 33 58, 63	0.00	0.0E1
ñ/eptune	14 2 51,75	14 2 51,71	+ 0. X	+ 0,60

M average thief obmlues ecorts is about 0.92 ceconde d'ora.

Note that it would be a mistake to take the denominator of relation (1) to be the angle covered during the total duration of the observations, which is of the order of a aitle, because the unknown constants of the movement are precisely determined so that on average there is no systematic stcular deviation between the observed and calculated azimuths.

This has led to the conclusion that Newton's laws have only been verified in astronomy with a relative precision of  $3 \cdot 10^{-6}$ , which is actually quite remarkable <sup>\*</sup>.

It's a conclusion that goes against the grain of conventional wisdom, without any real discussion in fact, but it seems to be the obvious one.

It is therefore appropriate to consider

that the current laws of gravitation are in no way the perfectly verified, definitive and immutable laws on which one bases the assertion that my experimental results are contrary to all the experience acquired in the field of astronomy. These laws, like all experimental laws, are only verified with a certain approximation.

*Prétention des vérifications à la surface de la terre des postulats de la mécanique*

2- The most accurate experiments in mechanics at the earth's surface have been those involving the second pendulum. These experiments show the well-known relationship :

$$T = 2 \cdot \sqrt{\frac{I}{Mg}}$$

deduced from the postulates of mechanics. I represents the moment of inertia of the pendulum. The quotient  $I / MI$  is calculated from length measurements; T is measured; and g is deduced. Experiments carried out by Volet at the Pavillon de Breteuil in Sèvres, enabling g to be measured directly by photographing the fall of invar rulers, confirmed the values deduced from the pendulum's observations to within  $10^{-5}$ . This is the order of magnitude of the precision with which the principles of mechanics appear to be verified at the earth's surface.

(4) On the peculiar acceleration of the Moon's motion, see in particular P. Tisserand, *Traité de Mécanique Céleste*, Tome III, chapitre de l'ensemble des I Jones relatives au mouvement de la Lune, Gauthiers-Villars, 1894, Chapitres XIII, XVIII et XII- Voir également W.M. Smart, *Celestial Mechanics*, Longmans, 1953, Chapitres 17, 18 et 19.

(5) These laws are so perfect that Hegel felt he had to give a demonstration of them. taphysique, *Philosophie der Natur*, trans. Vera, I, p. 293, Parag. 270. Voir les commentaires qu'en donne Pareto, *Unité de Sociologie*, Y.I., p. 269).

*Order of magnitude of the anomalies observed in the motion of  
the paraconic pendulum ä anisotropic support*

3- These figures are interesting to compare with the order of magnitude of the anomalies I've conat4ed. This order of magnitude is that of thirtieth of the Foucault effect <sup>6</sup>, and the latter is of the order of three millionths of the pea@jjq- <sup>7</sup>,

*The effects observed are therefore of an order of magnitude less than or equal to the order of magnitude by which the principles of mechanics can be considered to be true on the earth's surface and in astronomy.*

f6) § CS.3 above, p. 98.

faith The two equations that determine Foucault precession are (§ B.2.3, *Tobleou 2V*, p. 126)

$$m'' + p m = -2 m \sin L n'$$

$$2 \_ \quad l = 981 \ 83 = 11,82$$

$$n'' + p^* n = 2 tu \sin L m' \text{ As we}$$

have

$$\hat{U} = M 2$$

and } has components  $lm''$ ,  $ln''$ , we see that Foucault's disturbance force

$$-2 m \sin L M l n'$$

$$2 m \sin L M l m'$$

whose order of magnitude is

$$2 u' \sin L M l p o$$

$$p = 2 n / T = 9 \text{ g/l}$$

where  $r_i$  is the amplitude. Thus we have

$$\frac{\text{Eddy force } p \ 2 \ tu \ \sin L \ M \ p \ l \ r_i}{\text{PecanteurM}} \quad \frac{2 \ ni \ \sin L \ p \ l \ o}{g} \quad \mathbf{g}$$

According to my experiments of June-July 1955 (§ A1 and A4 above)  $m \sin L$

$$= 0.55 \ 10'$$

$$= 83 \ \text{em}$$

$$= 3.44$$

$$= 0.1$$

$$\frac{\text{Eddy force}}{\text{Pecanteur}} \quad \frac{2 \cdot 0.55 \ 10'}{\text{"}} \quad \frac{3.44 \cdot 83 \cdot 0.1}{\%1}$$

$$i20 \ 10'$$

As a result, the order of magnitude of the anomalies of the paraconic pendulum with anisotropic support, equal to one thirtieth of the Foucault effect, is *about three-m'dito* - *môme dr ta pesonttur*.

It **should** also be pointed out that, due to their periodic structure, the B conatat4ea anomalies are *zero on average*. So, if new forces are to be considered, the corresponding diurnal lunar anomalies only come into play within the framework of the solar day, the sideral day or the lunar day. From the astronomical point of view of planetary motion, we need to match them with forces whose integrals are zero along a planet's trajectory. Their order of magnitude is therefore comparable to the order of **magnitude with which** we can **think** that Kepler's laws will be verified in the course of a planetary revolution<sup>(8)</sup>.

*As a result, the anomalies highlighted are in no way in contradiction with previous experimental data, either on the earth's surface or in the field of astronomy.*

(8) In other words, if, in addition to Newtonian equities, we were to play with equities that were 10<sup>1</sup> times smaller and zero on average during a planet's revolution, they **would** probably be indistinguishable.

R- L'h32x'f8è'æ 'le ĩzi rompre de f'eej>zee 'fiziez-tie ef æo impfirofione

*Paratonic pendulum anomalies explained by inertial space anisotropy*

1- *As early as 1955, I was able to show that a difference of the order of  $10^{-6}$  in inertial mass in two rectangular directions could explain the effects observed*<sup>4a</sup> \*. In fact, because of their small size, the effects of such an anisotropy of inertial space may well have escaped the observation of experimentalists until now.

If I did not mention this hypothesis in my *No I es* to the Académie des Sciences of 1957 to 1959, it was to avoid arousing the general opposition of all the **supporters** of the Theory of relativit§ 2, I mistakenly thought that the anomalies in the movement of the paracenic pendulum, *as revealed* by my experiments, would be sufficient in themselves, following Planck's principle:

*"The scientific value of precise experiments is independent of their theoretical interpretation".*

In fact, if we consider that the precision with which the active laws of gravitation are verified, i.e. with a relative error of the order of  $10^{-6}$ , *the hypothesis of the anisotropy of inertial space is compatible with all observational data* \*.

*equations of motion of the paracenic pendulum assuming anisotropy of the inertial species*

2- We can indeed explain the order of magnitude of the periodic lunisolar compo- santes constet4es dana the pendulum's motion.

(1) Allais, August 12 1955, *ñfouvemcmts Périodiques du Pendule Conique*, t12 p. with four Appendices), p. 7.

(2) See note (3) in § C.1.3 of l'ozzoduc7iozs, p. 57.

(3) P.Janck, /zsi7iotiõns ô lo Z'hy8igue, 1941, Flammarion, p. 256.

(4) See § E'.1.3 below.



paraconic with anisotropic support by an anisotropy of the order of  $10^{-6}$  of the space of inertia generated by the influence of the ætreø >.

• *Table XII* below shows the essential relationships relating to the influence of inertial space anisotropy caused by the influence of **stars**.

It is assumed that the inertial mass  $M_i$  of the pendulum relative to its weight  $Mç$  is relatively increased by  $1 + G$  in the direction of the celestial body  $i$ . Coefficient  $G_i$  represents the influence of celestial body  $i$ .

Relationships (1) and (2) in *Table Off* show what becomes of *let the first members* of relations (5) and (6) of the *TV Table* of the Section B.2 above in the case where the  $O_m$  axis is oriented<sup>4</sup> towards star  $i$ . *Ceø* relations Equivalent to relations (3) and (4) in *Table XII*, where the second member of equation (3) can be considered as a *perturbation*.

Equations (3) and (4) correspond to the trajectory of the pendulum paraconic an ellipse for which relations (5) and (6) define the variations  $\xi'$  of the azimuth and  $\eta'$  of the minor axis of ellipse <sup>6</sup>.

For any orientation of star  $i$ 's azimuth  $X_i$ , relations (5) and (6) are replaced by relations (7) and (8).

Other  $i$  exerts a *direct* influence on  $\xi'$  represented by relation (7) and an *indirect* influence on  $\xi'$  via  $\eta'$  and Airy's prediction. In fact, direct influence is *relatively negligible* compared to indirect influence <sup>7</sup>,

*Formally*, relations (7) and (8) are Lout ä analogous to the expressions for  $\emptyset'$  and  $[\eta']$  corresponding to current gravitational theory. *lseuh feeffix:ient-diffèrent*.

(5) The same explanation applies to the paraconic pendulum with isotropic support (see *Chapitri* if, Section I).

(6) Allais, 1956, *TMorie du Pendule Poroconi9ue*, Fascicule III, n- 1103, Tnøïenu if, p. AI,6.

(D) See note (11) in § B.2.5 above, p. 122.

$\emptyset'$  xcl (s' du r "si" xri.

Naturally we can define a **directi** on average **anisotropy of the inertia space** in poaant

$$(1) \quad c \sin 2(X - \xi) = Z T_i \sin 2(X_i - \xi)$$

• *Table ZONE gives the first  $a_R$  proximation of the expres- sions of  $\xi'$  and  $Q'$  taking into account a foia the Foucault effect,  $A_i 10^6$ 's **preeeasion** of the anisotropy of the  $8\text{supp} 11\$$  and the anisotropy of the inertia **space** generated by the different stars.*

The expressions (1) and (2) for  $9'$  and  $9'$  in *Table Pfff* can be used to estimate the order of magnitude of the variation in  $\xi'$  corresponding to the influence of the Moon (relations 3 and 4).

The order of magnitude of the observed value  $\xi\xi$  of  $g'$  a ddjà dt4 calculated in the case of lunar periodicity of 24 h. 50 min and the anisotropic supported pa- raconique pendulum 12

From the equality  $i|' =$  we deduce the estimate  $c_l = 0.20.10^6$  of  $t_i$  (*Table Xfff*, relation 6).

We can thus see that *the order of magnitude of the lunisolar components in the motion of the parabolic pendulum can be effectively explained by an anisotropy of the order of ION of the inertial space for the influence of each star.*

### *Effects of inertia space ani-otropy*

3- The more pronounced the anisotropy, the more the plane of oacillation of the pen- dule tends to ae closer to the direction of the star in question, the effect of the other being to increase inertia in the direction of this star.

(9) On the implications of such a relationship, see the analyzed case of relationship (2) of the § E.7.1 above, p. 193.

(10) § B.2.3 above, p. 120.

(11) Allais, 1956, *TMorie du Pndult Paraconique*, Table T1 in *Nett Appendix 28*.

I.F.3

(12) HYPOTHESIS OF INERTIA SPACE ANISOTROPY  
§ B.2.6, p. 123.

*An action at a distance is thus replaced by lo-anisotropy. inertia space wedge!!.*

- If we compare the equations of pendulum motion corresponding to the anisotropy of inertial space with the equations corresponding to classical theory, we see that the effect corresponding to the anisotropy of inertial space is proportional to the square of the amplitude and inversely proportional to the length of the pendulum, whereas the effect corresponding to the current theory of gravitation is proportional to the square of the amplitude. amplitude, but is independent of pendulum length  $l^*$ .

*Thus, according to the current theory of gravitation, the influence of the stars is independent of the length of the pendulum, whereas in the hypothesis of the anisotropy of the space of inertia, the corresponding effect is all the more marked the shorter the pendulum!*<sup>5-16</sup>.

(13) In reality, the postulate of instantaneous propagation of gravitational forces at a distance cannot be accepted.

Since Maxwell's representation of local actions by Thinkers, we have generally come to believe that gravitational actions propagate, and that they can be represented by local properties of space (see Section YC below).

(14) According to the 'Poöfeou *Hh* (relation 7), the effect corresponding to the anisotropy of inertial space is proportional to  $\alpha^2/l$  while the effect corresponding to the actual theory of gravitation is proportional to  $\alpha^2/l$  (Pohfenu XiZi, note 2).

This explains why the longer the pendulum, the less the eddy effect is disturbed. In the case of the Foucault pendulum in the Panthéon in 1852, the length of the pendulum was 67 meters and the amplitude of the oscillations was 0,06 radians (§ E.2.3 ci-des-sus). On the assumption of the anisotropy of the inertia species, the theoretical effect was about 220 times smaller than in the case of the paraconic pendulum. We have in fact

$$(0, 2/67) / (0, 1^*/0, 83) = 1/224$$

In the case considered by Dejean de Fonroque's introduction, § B.2.6, note 7 below, the pendulum was suspended by a wire about 1 metre long, and the initial amplitude of the oscillations was of the order of 45°, or about 0.785 radians, whereas in the case of the paraconic pendulum we have  $l = 83$  cm and  $t_i = 0.1$  radian (about 6°). The  $\alpha^2/l$  ratio was thus around 50 times higher than in my own experiments  $(0.78^2 / 100) / (0.1 / 83) = 51.2$ .

(1ö) From relations (1) and (2) of the 'Poéleou *XIII* and by posing  $k = 3 \pi r_i / 8$ ,  $tg = p' x r_i / 4$ ,  $p_j = p t_j / 2$  we deduce as a first approximation the relation

$$(1) \quad \ddot{\theta} = k [ \sin^2(Z - \theta) + Z \sin^2(X_j - \theta) ]$$

Very interesting implications of this second-order differential equation will be examined in the Second part of this book, Chapter Z.

*Determining anisotropy coefficients  $z_i$*

4 - Given the *very limited* data currently available, the precise determination of  $\epsilon_i$  anisotropy coefficients as a function of the astrea's characteristic parameters and their variations over time is quite straightforward.

impossible \*\*.

(continued from

note 15)

Relationship (1) can be written as

$$(2) \quad \#'' = k f \sin^2(Y - \delta) \quad X = (3/8) p a - (3/8) . 3,4d . 0,1 = 0,129$$

where  $Y$  is the azimuth of the boundary plane.

In the case where the azimuth  $\delta$  of the plane of oscillation is close to the azimuth of the limiting plane  $Y$  resulting from the combined action of the support and the stars, equation (1) reduces to the linear differential equation

$$(3) \quad \#'' + 2 k f \delta = 2 k f Y$$

If  $Y(t)$  varies relatively little over an experiment lasting 14 minutes = 840 seconds, the general integral of (3) is

$$(4) \quad \# = Y + A \cos Ct + B \sin Ct \quad r_i = \sqrt{9 2 k f}$$

For initial conditions

$$(5) \quad \# = \delta \delta \quad \dot{\#} = - tu \sin L \quad \text{for } t = 0$$

equation (4) is written

$$(6) \quad \# = Y + (J - Y_c) \cos f_2 t - \frac{\sin L}{\Omega} \sin n t$$

Taking into account the restoring effect of the anisotropic suspension, for which we have as order of magnitude  $jq = 10$  (Note 6, p. 178, of § E.3.3 above) we can estimate the order of magnitude of this work, Chapter f, Section B) that we have approximately:  $p = j p g = 1.41 \cdot 10^{-1}$ , and therefore

$$(7) \quad r_i = 0.611 \quad \sigma^* e^{-s} / n = 10284'' - 2,86 \text{ heure B}$$

As a result, the azimuth  $\delta$  oscillates around the limit plane with a period of 09 h 00 min. For experiments lasting 14 minutes such oscillation does not occur and the azimuth of the plane of oscillation approaches the azimuth of the boundary plane during each experience of 14 minutes.

(See also the calculation in note 2 of § II.1.2 and the 'Potifeno X' in Chapter ii, p. 324-325). During a 14-minute experiment, the third term of relation (6) corresponding to the eddy current effect gives an average effect of

$$\dot{\#} = - tu \sin L \cos n t$$

As the average  $\cos f_2 t$  over a 14-minute experiment is smaller than

At any time we have  $|\dot{\#}| < tu \sin L$ .

(16) On the effects of the anisotropy of inertial space in the case of the paraconic pendulum with isotropic support, see Chapter ff, Section I, **below**, pp. 320-325.

(17) If we consider the respective actions of the Sun and Moon on the diurnal components of the azimuths of the plane of oscillation of the anisotropically supported paraconic pendulum, and the empirical data available for the Sun and Moon in § A.5 above, we can assume that the anisotropy coefficients of the Sun and Moon are of the same order of magnitude as the coefficients  $C_g$  and  $C_j$  corresponding to the plane of oscillation of the paraconic pendulum. (above), we can assume that the

**210** isotropy coefficients of the Sun and Moon are of the same order of magnitude as the coefficients  $C_g$  and  $C_j$  corresponding to the current theory of gravitation (§ B.2.5 above, p. 123). **E.F.3**

DMFLUENCE DTTN ASTRE i SUR LAZDMUT ET LE PETIT AXE  
DE LA TRAJECTOIRE ELLETTIQUE DU PENDULE  
PARACOIQUE DANS L'THYPTHESE DE L' SOTROPIE DE  
L'ESPACE DINERTIE

*Notation*

Om axis = direction of greatest inertia <sup>1</sup>

q = coefficient of the anisotropy induced by the other i <sup>2</sup>

*Differential equations in m and ri*

$$(1) \quad (1+j)\epsilon + p2m=0 \quad p^2 = g/l$$

$$(2) \quad n + 2n=0 \quad \epsilon_1 \ll 1$$

*As a first approximation*

$$(3) \quad \epsilon + p2 = 0$$

$$(4) \quad n + 2n = 0$$

$$(5) \quad \phi'_i = \epsilon_1 p \frac{\alpha \beta}{\alpha^2 - \beta^2} \cos 2$$

$$(6) \quad \theta = -q \frac{p}{2} \sin 2\theta$$

*Any axes Om , On ; Solution*

$$(7) \quad \phi'_i = \epsilon_1 \frac{p}{\alpha^2 - \beta^2} \cos 2(X_j - \phi) \quad \text{Ofj} = \text{azimuth of the other f}$$

$$r^* = - \frac{1}{2} \ln 2(X - \phi)$$

(1) Rotations in § B.2.3 above and the corresponding 'Foölenu 2V, p. 126.

(2) The anisotropy coefficient  $\epsilon_1$  is a *dimensionless* coefficient.

(3) These equations are to be compared with the equations deduced from the current theory of the universal attraction and corresponding to the attraction of a star (IG B.2.5, Z'ööféou W, above, p. 128) .

$$(1) \quad \phi' = \frac{1}{p} K_1 \frac{\alpha \beta}{\alpha^2 - \beta^2} \cos 2(X, - \phi)$$

$$(2) \quad \zeta = \frac{1}{2} \ln 2(X - \phi) - g$$

EQUATIONS OF MOTION OF THE PARACONIC PENDULUM IN THE ANISOTROPIC MEDIUM OF THE INERTIAL SPACE AND IN THE CASE OF THE ANISOTROPIC SUPPORT

Equation of motion (as a first approximation)

$$(1) \quad \ddot{\varphi} = -m \sin L + \frac{3}{8} p \alpha \beta \quad p = \sqrt{g/L} = 3,44$$

$$(2) \quad \dot{\varphi} = \frac{1}{4} p' a' \sin 2(Z - \varphi) + Z \frac{p}{2} \alpha \beta \sin 2(\varphi - \#)$$

Orders of magnitude of  $e$  - Illustration in the context of the L.une - Periodicity of 24h. 50mn.

Theoretical robbery of average longitude during an experience of duration  $\Delta t = 14 \text{ mn} = p \Delta t$ ,

$$(3) \quad \bar{\varphi}' = \frac{3}{8} p \bar{\alpha} \bar{\beta} \quad a=0,1$$

$$= \frac{1}{8} \int_0^{\Delta t} \frac{p \bar{\alpha}}{2} \sin 2(\varphi - g) dt$$

$$= 9,32 \text{ zj size } 2 (X_j - \varphi)$$

$$(4) \quad \text{Order of magnitude of } \#; = 9,32 \text{ e rad./aec.}$$

Order of magnitude of average thief  $\varphi_g$  (§ B.2.6, p. 123)

$$(5) \quad \varphi_g = t3.186.10' \text{ rad./aec.}$$

Corresponding order of magnitude of  $q$  from (4) and (5)

$$(6) \quad e; = 0,186.10' / 9,32 = 0,20.10$$

According to (1) and (2), the effect of the anisotropy of the inertia space zone  $\varphi_j$  and  $p; j; 2$

$$(n) \quad \left( \frac{3}{8} p \bar{\alpha} \right) \left( \frac{p}{2} \bar{\alpha} \varepsilon_1 \right) \frac{\Delta t}{2} \sin 2(\# - g) = \frac{3}{161} \frac{g}{\#} \varepsilon_1 \frac{\Delta t}{2} \sin 2(X_1 - \varphi)$$

(1) They indicate **quilsa t des** the mean values over the duration  $\Delta t = 14$  minutes = 840 seconds of the experiment.

**O)** According to the current theory of gravitation, the effect of universal attraction on  $\varphi$  is proportional  $\varepsilon$

$$\left( \frac{3}{8} p \bar{\alpha} \right) \left( \frac{1}{2p} \bar{\alpha} K_1 \right) = \frac{3}{16} \varepsilon_1$$

(2'ođienü Off, note 31, and according to the relationship f71)

$$\left( \frac{3}{16} p^2 \bar{\alpha}^2 \varepsilon_1 \right) / \left( \frac{3}{16} \bar{\alpha}^2 K_1 \right) = \frac{g}{1} \frac{\varepsilon_1}{K_1}$$



## ON MY EXPERIENCES ON        PARACONIC PENDULUM ANISOTROPIC SUPPORT

### ***1.- Signification et portée des expériences sur le pendule paraconique à support***

The essence of the *necessarily very brief* analysis I have just presented of the anomalies of the paraconic pendulum with an anisotropic support can be summarized as follows:

1) The motion of the anisotropically supported asymmetrical paraconic pendulum includes periodic components with a *statistically significant* amplitude, particularly periodic components, with periods of around 24 h and 24 h 50 min, of the order of one-thirtieth of the Foucault effect.

2) In particular, the diurnal lunar component of 24 h. 50 min cannot be identified with the periodic diurnal lunar component resulting from the theory of gravitation *as calculated* from the double principle of inertia and universal attraction, and from the theory of relative motion, *whose amplitude is about twenty million months smaller* in the case of the paraconic pendulum with anisotropic support.

3) The *very particular* periodic structure of the observed phenomena, resulting in particular from the relative importance of the amplitude of the lunar periodicity of 24 h. 50 min, *totally rules out* any explanation based on any of the already known periodic phenomena that have been envisaged as being able to explain the amplitude of the observed periodicity.

4) My analyses from 1954 to 1957 were *vividly and spectacularly* confirmed by *similar* results obtained during two *crucial* experiments carried out in July 1958 at IRSID and Bougival, in an underground quarry with a 57-metre cover 6.5 km from Saint-Germain.

5) In fact, constat4ea periodicities, and especially the lunar component of 24 h 50 min, *really do exist*. They are *totally inexplicable* within the framework of current gravitation theory. *They cannot be considered as resulting from t h e direct or indirect action of any known phenomenon.*

6) Anomalies were observed during the *two total solar tclipses* of June 30, 1954 and October 2, 1959. They are *totally inexplicable* within the framework of current gravitational theory.

7) *The observed effects, whose order of magnitude is of the order of a millionth of a gravitational force, are in fact not incompatible with any of the experimental results previously obtained, since the precision with which these results were obtained is precisely a few millionths.*

8) *In the current state of discussion, the anomalies observed can only be explained by considering the existence of complementary terms in the gravitational actions. The simplest hypothesis is that of anisotropy in the space of inertia.*

Of course, the fact that the anomalies of the paraconic pendulum can be explained by anisotropy in the inertial space does *not prove the actual existence* of this anisotropy, but it does prove that *everything happens as if the inertial space were anisotropic.*

9) These anomalies are not isolated occurrences. Numerous anomalies have been observed in many other geophysical phenomenaaa, and it seems probable, if not certain, that they all stem *from one and the same cause.*

fl) As it results from the calculation of the order of magnitude of "i (t \* 2.2 above) and from the calculation of note (7), p. 20d, of § F.2.3 ci-deaaus.

(2) See in particular the optical deviations of the sights on sights and neck - **limateurs, Esclançon's** optical observations, and the interferometric observations by Miller (**Chapitres** *Uf and TV* below).

See also the p ase concordances **de nase** highlighted in Cfiopitre Y, particularly in § E.1 of this Chapter.

*the publication of my work on the anisotropically supported  
paraconic pendulum*

1- The dissemination of my experimental results from 1956 onwards, and their publication in 1957, opened up a wide-ranging debate and enabled me to obtain the necessary funding to continue my experiments.

In particular, the impact of my February 22, 1958 conference at the Ecole Polytechnique, chaired by Albert Caquot, enabled me to finance the crucial July 1958 experiments at Bougival and Saint-Germain 1,

However, after my Note of February 9, 1959 à l'Académie des Sciences sur l'influence de l'anisotropie du support <sup>2</sup> it was dt4 impossible for me to publish any other Note, especially on the thdorie du paraconic pendulum. 11 a 6t4 notamment m'a impossible de publier cinq Notes que j'avais prdpardes, la premibre sur l'application du thñorbme de Bour au cas des mouvements terreatrea, et lee quatre autres sur l'application de la mdthode de variation des constantes de Lagrange au calcul de l'influence de tous lee facLeurs intervenant dans le mouvement du pendule paraconique \*.

(1) See in particular § B.2 and D.2 of the Introduction above.

I received a lot of criticism (from Henri Villat in particular) for giving my February 22, 1958 lecture at the Ecole Polytechnique, and in his letter of February 6, 1958, Henri Villat did not hesitate to write that the ensuing discussion would be "*obaurd*".

That my Conference was organized by the Cercfe Atezondre Du;four had even intolerable, and in my letter to Albert Caquot of February 18, 1958, I was4 led to conclude that eezire :

*'Au toura de tonueraotiona priuéea rtentes. te mnque d'objectiuit de mea  
odversoires eet deaenu incroyable. Ainsi, porolt, te oil que j'oi cc - ceptd de  
porfer dona te codre du Cercle Alezondre Dufour tendroit d com - promettre  
mea propres 7/s48ea éfont dozsr4 qu'il y o, parmi lea membres du Cercle Alexandre  
Dufour, aertoinea persozszse8 ozzti-reloitiuitea et jugées por lô-m5me onli-  
acientijfiquea.*

"It's like reuertu or Go/i/4e time".

(2) See the Introduction above, § B.2.7, note 8, p. 51.

(3) See introduction above, § B.3.2, note 2, p. 54.

Opposition to the publication of my work can be illustrated by the comments of Jean Leray, member of the Commaiaion de Mécanique de l'Académie des Sciences, in his letter of December 18.

*The non-publication of my results on the motion of an  
isotropically supported poroconic pendulum*

2 - Due to the closure of my IRSID laboratory in June 1960, the delays involved in processing the observations of November-December 1959 and March-April 1960, and the Academy's definitive refusal to publish any notes by me from February 23, 1960 <sup>5</sup>, I was *unfortunately* unable to publish any results corresponding to the *paraconic pendulum ä isotropic suspension* !- !.

(4) Letter from Jean Leray, December 18, 1960:

*'Je continue à regrener que l'Acodmie oil publié quelques de moe 2'fotez; elle ne peut, sons se détonzidtrer, pouveuire leur publico-tion : dec ricullote expèrimntnus, niJntoires perce que ie /rottement y joue un rdfe prpondronl, cons infrèt prnte que leur Théorie n'eet pos dJueloppée jusqu'à see toncluaionc num4riquec, vous d4duicez à coup de ctotictiques, des conclusions qui cemblenl, à pluc d'un de mes confrères et d moi-même, non justl/ides, mois qui vous poroissent ken -*

See § E.6, note 10, p. 192.

The reader will find in the Anneze IA of the *Deuzitme* volume of this work all my correspondence with Jean Leray.

(5) See *Introduction*, § D.3.2, note d, p. 71, and *Chapter III* below, § B.d, p. 331-3d0.

(6) See *Introdution*, § B.2.5, p. d9, and *Chapter II* below.

(D However, on November 5 and 10 1959, I wrote two *Novec* presented by René Thiry and André Léaut4 which unfortunately could not be published by the Académie dee Sciences: the first, *Pendule parotonique, flJoiiniion d'un support o u s i icotrope que possiöte* (10 p.); the second, *Pendule poroconique à aucpencion ise - Irope. Délermincline dee uarioliona ou cours du tempe des toroctériatiquee de lo corrè - lotion du mouvement ouec l'azimul* (7 p.) (see below, *Chapter II*, § B.1, note 1).

I attached your deuz Notes to my request for credit dated February 26, 1960, addressed to the Director of the CNRS.

In my new request of May 19, 1960 (reet4e cane cuite), I again mentioned these and the initial results of the analysis of the two a4riea o f observations made in November-December 1959 and March-April 1960 (see below Cüopitre ff, Section D).

- *From February 1954 to June 30 1960*, I continued my experiments on the paraconic pendulum with my two collaborators Jacques Bourgeot and Annie Rolland in my laboratory at IRSID.

It was thanks to the decision of Pierre Ricard, then *Chairman of Industries mtallurgiques et minières*, with his exceptional breadth of vision, that I was able to set up this laboratory. From the outset, I received particularly competent support from René Dugas, author of two remarkable works on the history of Mechanics <sup>1</sup>.

- To fully understand how my experiments unfolded, I feel it's best to present here a brief chronology from 1953 to 1960. It is divided into two periods: 1953-1959 and 1959-1960.

*Period October 1953- {turner 1959*

*July 1953*: IRSID decides to give me the resources I need for my research.

*October 16, 1953*: Installation of my IRSID laboratory.

*1\*"furiar 1954*: Start of experiments on the paraconic pendulum.

*June 4-July 9, 1954*: R4aliaation of the first series of monthly enchafnéea observations of the paraconic pendulum. Anomaly observed during the total solar eclipse of June 30 1954.

*November 16 - September 22 1954*: R4aliaation of the second series of monthly chained obaervationa of the paraco- nic pendulum.

*3yuin - 7ju'ület 1955*: Third series of chained menauellea observations with the paraconic pendulum.

*September 15, 1955*: I consider it *definitively established* the existence of the lunar component of 24 h. 50 min. with a *totally unexplained* amplitude within the framework of the currently accepted theory of gravitation.

(1) See *l'Introdu tion* above, § D.1, P. 6d-65.  
In July 1953 René Dugas was<sup>4</sup> appointed *Conseil acienti ut by*  
.RS.D

*From January 1956:* Numerous visits to the laboratory b y *outside perzonnolitya.*

*September 6, 1956:* Completion of my dissertation,  
*Theory of the paraconic pendulum* (4d1 p.).

*15 (évrier 1957 :* Elaboration du teat de p4riodicité4 pour lea **séries** autocorrelated.

*2S moi J957 :* *Note ä l'Académie dea Sciences sur la généralisation du test de Schuster au caa dea séries autz'corrëIées* <sup>2</sup>

*November 13 - December 23 1957:* Cinq *Noter ö l'Académie des Sciences sur mea expériences aur le pendule paraconique* <sup>2</sup>.

*February 2, 1958:* Lecture at the Ecole Polytechnique "*Font-il recon-sidérer Ier lois de lo Gravitation î. Sur une nouvelle ezpë rience de Métanique*", organized by Cercle Alexandre Duffy <sup>3</sup>

*mort-avril 1958:* *Installation of a second lobaratoire dana la car -rière aouterra.rue du Blanc Minéral de Bougivol* with 57 meters of overlap and 6.5 km distance from Saint-Germain with the help of the *Centre National de la Recherche Scientifique* and the *Comité d'Action Scientifique de la Défense Nationale.*

*2-30 June 1958:* Preliminary experiments in Bougival and Saint-Germain.

*July 2 - October <sup>1</sup>, 1958:* Simultaneous *cruciolea* experiments in Bougival and Saint-Germain. Monthly series o f anisotropic paraconic pendulum experiments, accompanied by optical sighting e x p e r i m e n t s in Saint-Germain.

*November 3 and December 22, 1958:* Two *notes 6 l'Académie dea Sciences aur lea expëriencea de Bougival et Saint-Germain* <sup>2</sup>

*January 19 and February 9, 1959:* Two *notes to the Académie de8 Sciences on the influence of the inclination of the bearing surface and the anisotropy of supra* <sup>2</sup>

### ***Période mars 1959-juin 1960***

*September 24-25, 1959:* First experiments with the paraconic pendulum in isotropic suspension.

(2) See *Introduction*, § B.2.7, p. 51.

(3) See *Introduction*, § B.2.9, p. 52.

*September 28 - October 4, 1959:* Parallel enclafnëea experiments with isotropic auspenaion and anisotropic auapenaion. Observations of the total solar eclipse of October 2, 1959.

*November 20 - September 15, 1959:* Two series of simultaneous experiments at IRSID with isotropic suspension and anisotropic auspenaion.

*7 nouemôre 1959:* Conference at the Société dea Ingénieurs Civila de France, "*Faut-il reconsidérer leo lois de la Grouiteton î Nouveaux réoultal:s, Bilon et perspeetiuez*", organized by the Cercle Alexandre

Our .

*P3 ;féurier 1960 :* Refua par l'Académie dea Sciences de publier ma *Note* aur lea résultats de mes observations aura lea déviations optiques des visées sur mire de juillet 1958 \*.

*March 16 - April 16, 1960:* Two simultaneous aerial experiments at IRSID with isotropic and anisotropic suspensions.

*June 30 1P60:* IRSID laboratory closes.

- From 1954 to 1960, I had to cope *alone* with an overwhelming workload. I had to design and direct the experiments; calculate the apparatus used; direct all the numerical analysis calculations and interpret them; acquaint myself with all the publications on experimental and theoretical research on the pendulum; develop a complete theory of paraconic pendulum movements, and in particular of their lunisolar compoaantea; analyze and extend alla contributions to the literature on the search fora periodicities and develop an appropriate test for autocorrelated time series; and at the same time ensure the financing of the experiments, and take multiple steps to this end.

(4) See above, *Introduction*, § D.3.2, pp. 70-72, and below, *CAopitre III*, § B.4, p. 339-340.

(5) In my "*Note commerce cur lez retherthes cur le pendule porotonique*" of January 15, 1957 (24 p.), I wrote (p. 18-19):

*'It would be a big mistake to underestimate your very big di 'fitullé dec poaés problems.*

*"Z. ffude f/s4ori@ue et ezp4rimento/e dec ph4nomà nec considérée néces site une connoicconce opprofondie dt dizciplineinec trhc diuercec imtconique, gJophy-cique, oztronomie, ctaticti9ue, el mohtmotiquesl, elle implique des expriencec dilicotec, They also require a great deal of care and attention to detail, and often very long colculc d'onolyce and toltulc numèriqutc, some of which would be inncessibtes si on ne dizposoit pom de mochinez à colcu ler 4eclroniqizea.*

*RecAercôdes de ce genre ntceccitent benucoup de patience, noire d'ocharnement, el de temps! So in such a field, nothing can pull [cit donc ta précipitation. If there's one thing I've learned from these three years of expfrmtc, it's tlui-lä".*

In particular, I had to face up to two gaps in the literature, which in this case were absolutely *essential*. Firstly, no author had calculated the influence of the Sun and Moon on pendulum motion. Secondly, no periodicity test was available for autocorrelated series. These were two *major* questions and that I had to resolve<sup>>-7</sup>.

- Following my lecture on February 22, 1958, and with the support of several members of the Académie des Sciences, including Albert Caquot, Pierre Tardi, and Marie-Joseph Kempé de Fériet, it was decided to carry out two crucial experiments observing the movements of two identical paraconic pendules, one at IRSID, the other at Bougival in an underground quarry with 57 meters of cover and 6.5 km apart.

These crucial experiments took place in July 1958 and *were a resounding success*.

The first period 1953-1958 thus culminated in the two crucial experiments of July 1958. *Paradoxically, and despite the resounding success of these two crucial experiments, I had to face growing hostility and major funding difficulties*. At the end of 1959, due to a lack of funds, the decision had to be taken to close the two laboratories at IRSID and Bougival in June 1960<sup>10</sup>

(6) See section B.3.2, p. 54-55, above.

(7) Throughout this period and in parallel, I taught economics at the Ecole Nationale Supérieure des Mines and the Institut de Statistique de l'Université de Paris, and published numerous memoirs on monetary dynamics, the Soviet economy, the European Community, and mining research (see Auiopo'Yr-ii-, 1989, p. 127, 128, 135, 138, 140, and 141).

(8) These experiments were funded 8 times by the *Comité d'Action Scientifique de l'Université de Paris* and by the *Centre National de la Recherche Scientifique*.

Previously, on October 9, 1956 and April 5, 1957, I had presented to the CNRS two financing requests that had been rejected.

(9) See above Section C, § 4, pp. 160-161.

(10) The untimely death of Pierre Ricard on April 4, 1956, and that of René Dugas on June 15, 1957, deprived us of two extremely valuable, and in truth *irreplaceable*, sources of support.

*Isan8 fe disparition prtmoturét de Pierre Ricard il eff certain qut mtv txpt -*



From January 1, 1954 to June 30, **1960**, i.e. for around **six** and a half **years**, the expenditure involved in the experiments was<sup>4</sup> relatively high.

Expenditure in *francs at the time* was approximately <sup>2</sup>: 1954-1957: 5 million per year; 1958: 8 million <sup>3</sup>; 1959-1960: 6 million. In all, from 1954 to 1960, the experiments cost around 34 million <sup>°- \*</sup>, of which around 50 & were spent on personnel <sup>\*</sup>.

(1) *A proximitiutmtnt un million froncc de 19s reprtaente cent mille /renccs de II 'aë* recalls that the changeover from the old francs<sup>8</sup> to the new francs in January 1959 was based on 1 new franc for 100 old francs<sup>8</sup>.  
*Dana ce qui auit lec dépener# de foncier 1959 ä juin J960 aout éooluétc tu onciena*

(2) The remuneration of my two collaborators, Jacques Bourgeot and Annie Rolland, amounted to approximately 2 million francs per year. Overtime pay for IRSI D aents taking part in ongoing experiments amounted to around 300,000 francs per month.

(3) Expenses corresponding to the two laboratories in Saint-Germain and Bougival.

(4) Of this total of 34 million, personnel costs accounted for around 17%. millions dont environ 13 millions **our mes** <sup>d u\*</sup> employees, and around 4 million for the remuneration of the othera **obuez' euze** during the series of continuous observations and for that of the vocations for mathematical work of some of my students at the Ecole des Mines and the Institut de Statistique de l'Université de Paris.

(5) All these expenses were financed by the Institut *de Rtcherche de lo Sidërgurgie* (IRSID) from January 1, **1953** to December 31, 1956 and from January 1, **1959** to June 30, 1960.

The Comitè *d'Action Scienti/ique de la flë/ense Netionale* (CASDN) covered expenses from January 1, 1957 to December 31, 1958.

The Centre Notionol *dt la Aecüerche Scienti/ique* (CNSS) contributed 3,500,000 francs to 1958 expenditure, and 2,500,000 francs to 1959 expenditure.

(6) The foregoing estimates of the coüt of exp riences **ériencesn** from my four Notes: of September 1955, *Nuit eommoirt sur lec mouvements du pendule toniqut* (18 p.), p. 6; of November 10, **1956**, *Noie Sommaire cur tes trouaux expdrimentous et l'hto- riquea e@ctuéus du ltr octobre 1953 ou ltr octobre 1956* dont le codre de l'Inrfitut de **Recherche de la Sidërgargis** (12 p.), p. 6-7; December 2, 1957, *Recherchersur les mou- uimentz du pendule poroconiqut* (6 p.), p. 4; and April 15, **1959**, *Note sur l'élol oct nel dtz recherches ef fé financemnt dre trououx* (3 p.), p. 2-3.

The total expenditure of 34 million from January 1, 1954 to June 30, 1960 was financed as follows:

IRSI	D18	52,9 %
million		29,4 %
CASDN	10 "	17,7 %
CNRS	6 "	

These expenses proved increasingly difficult to finance. Pierre Ricard's untimely death on April 4, 1956 deprived me of *essential* support, and from the end of 1956 IRSID stepped up its efforts to stop supporting me. From January 1, 1957 to December 31, 1958, the *Comité d'Action Scientifique de la Défense Nationale*, chaired by General Bergeron and later by General Guéri n, provided most of the funding for my experiments.

*Whatever the costs of my experiments may have been, their profitability or the regret of the eminent scientists was very limited.*

At the beginning of 1959, *and in view of the total success of the crucial experiments of July 1958*, the Comité d'Action Scientifique de la Défense Nationale (CASDN) *considered that the effective and indisputable existence of the phenomenon under discussion had been brought to light*, that its action should therefore be considered complete, and that it was consequently up to the CNRS to take charge of the continuation of the experiments<sup>®</sup>.

(7) In my October 1956 memoir (10 p.) to the CNRS, I wrote :

ç'n p/zYnom8ne noueou o 4t4 mçr en 4uzdence e7 i/ es7 indJn/06/e que /e4 00/t4dgUg0CçS du point de uue de zzo4 conceptiO/t4 7/s4origue4 du monde /t/t4igie /teuuen7 en fitre çrg4 cO/t4iddro6/es

"Z..dtizde syaticmatique del phdnom8nea noueoux o PIE dozt4 /e po44d a xtroordinoire tcondil4

'Nouc ne zourionc d'oilteur4 trop couligner que lo pourzueil dc cel rec/serc/ses ne pr4sen7e p/u4 Oct uellement de ri4gUe. Z'-exi4fence du phtnomlne 4t udi4 e- I en effet tertaine, et l'ordre de grondeur dc l'i di@4rence enfre les eff'ela obsemés et les eff'el8 co/cu/d4 eal non moins certaine . . .

'A tout prendre, le monianl **QfObOf** des crddi74 que noua demondon4 esf modest relocation ui4-d- t / t 4 **des** Ed4U/f its that e4comp7er.

Comme dO/ta tOu4 /e4 cas où il s'Ogtt d'un phénomène nouveau' la renfobilitt marginale des d4penzes que l'on peut e@ectueF e47 cerYainement **tr8a** firande, et il n'y a actuellement/cmçn7, zt notre ouc4, *Oucune* compo&t4O/**possible** entre /o rentabilit8 Scientifique dèpense de 25 mi/10/t4 concatr4t b l'4t ude del mouuemeni- du pendule porotonique et lo rentabilil4 scientifiqize d' une dèpense marginale dgale dont4 le domaine **atomique** .

(8) In my Mèmoire of April 15, 1959 I wrote to the CNRS :

"Dana l'étal achuel dc la discussion l'ouia général de 7ou7e4 les personno- /fit44 C0ztsu/t4e4 e47 §'u- tE4 P4\* A4W/t4'8 expdFtzzteztzo/es doiuen7 eme pour- sucuies . . .

"3fes recherche4 oni çtd /inonçder ju49ftfer :

a) par l'I stitut dc flecferrAes dc fo Sid4rurgie du 1-^ nto6re 196S or J January 1957,

b1 por le Comit4 d'Action Scientifique de la D4 ense Notionale du **1<sup>er</sup> janvier 1957 au 1<sup>er</sup> janvier 1959,**

c1 por le CNRIS b titrt d'appoint en 1958 pour "zoe aomme lotale de

On my request for support, the CNRS indicated on June 27, 1960 that it could not take over the financing of my research and referred me to another organization \*. In view of the closure of IRSID's laboratory on June 30, 1960, it was unfortunately only too obvious that the scientific backing of a Commission competent emanating from the Académie des Sciences et du Centre National de la Recherche Scientifique was a recourse of a nature of financial impossibility.

(following note 8)

*"L'intervention de l'IREID a été une intervention de débourrage. Qui n'est pas possible que grâce à la largesse de la part de M. Ritard, aujourd'hui décédé. Cette intervention ne pouvait se maintenir, car elle sortait du domaine de compétence normale de l'IRSID.*

*L'intervention du C.A.F.I.D.N. n'a eu lieu qu'à titre temporaire. En janvier 1956 le CNRS m'a permis tout ce que j'ai pu faire de cette situation. Le C.A.F.I.D.N. était intervenu sous la condition que l'aide soit limitée au financement de recherches normales pour prouver l'existence d'ions - libres du plasma.*

*Cette preuve a été apportée par mes expériences de juillet/août 1958 poursuivies simultanément dans deux laboratoires à Eoin-Oermoulval à Bougiua à 1 km de distance, le second étant à l'intérieur d'un corridor souterrain avec plus de 60 mètres de retour de terrain. Ces expériences ont montré en effet que dans les deux laboratoires les phénomènes tonitroïdes ont une structure périodique temporelle.*

*Cette preuve a été apportée par le CASD I à l'occasion d'une terminaison.*

*"The CNRS is the only one with the means to finance the continuation of my experiments".*

(9) In his letter of June 27, 1960, the Directeur général of CNTTS wrote to me:

*"La Commission de mandat que j'ai soumise à votre détermination est celle qui concerne ce problème, qui nécessitera de très grandes dépenses, à l'époque et vous suggère de nous adresser un Comité Interministériel de la Recherche Scientifique. 68 rue de Bellechasse (Paris 7ème)".*

(10) There's no doubt that a favorable scientific opinion would have enabled me to find in the industry the financial means necessary.

Of course, it's perfectly understandable that CNRS hasn't had all or part of the financial means to pursue research into the paraconic pendulum and optical sighting on test patterns, in one form or another, at IRSID or elsewhere, but it should have presented a well-founded *scientific opinion* on the merits of this pursuit.

If it was opposed, it should not have used the pretext of the scale of the financial resources involved and its inability to meet them; it should have *explicitly* justified its unfavorable opinion.

**In any case**, and as **expressly requested by** various members of the Académie des Sciences and the CNRS Commission de Mécanique, the CNRS should have set up and convened a Commission **tasked with** expressing an opinion, enlightened by all desirable auditions, including my own. Within such a Commission, its various members would have been able to express their respective positions *explicitly and in a reasoned manner*.<sup>1°</sup>

(11) In fact, and in view of the *approfondies* discussions that had been developing since the beginning of 1957, this would have been a relatively easy task (see below). § 3, p. 227-230).

## 5.- Des oppositions dogmatiques

### *Numerous objections and incessant rumour-mongering*

*I - In order to obtain the necessary resources to pursue my experiments, I had to constantly underline their great scientific interest, which was so obvious from 1956 onwards!*

I was constantly and strongly supported by certain members of the Académie des Sciences. But I have also had to face numerous objections, some of them totally unfounded, very powerful dogmatic opposition, plus or minus explicit, and the incessant propagation of rumors calling into question the validity of my experiments and mia r4sultäts 2. 3.

(1) **Dans** In my *ñfJ*moire of November 10, 1956, I wrote :

*M mire en dchec de lo théorie octueilement odmiae de lo grouilolion présente mont/estement un intérêt tonzidtroble.*

*Sons rien préjuger de l'explication qui pourro'/isolement htre donnée du pùdnom+ne, il y o incontestablement un p/tdnoméne nouveau non ezja/i/@u4 ef l'histoire tout entière de lo physique ect là pour témoigner de l'intrht que peut présenter un lei phénomène.*

*Cet in térhf est d'en font grand que jusqu'ici l'histoire de lo Mécanique Cdteste n'o comporté que d'tcla lanti auccéa, tes onomoties conrtotdes ne cons.Situent g.InJrnlement qu'une fraction négfgeohie des pùdzomèzte8*

*Un phénomène nouveou o été en évidence et il est indéniable que les conrdquencer du point de vue de nor conceptions t/idori9ue# du monde physique peuuent en Sire mis considérables ...*

*"L'èt ude sryzlt moique des/zdztom+ztes nouveoux o été dont le posté d'une ex.froordinoire ficondilt".*

(2) See for example § E.6 above, pp. 188-192.

My conratrieteur8 constantly displayed a combination of not - aive credulity with regard to established verit4a and aggressive skepticism with regard to my own work.

As I wrote in my 1958 memoir, Ooit-on reconsidérer tes tois de la grouilou? (p. 104) :

*"Je suis truc 'rappt du fait que mes aduereoirea ne sont d'accord qu'un point, souoir que)'oi tort. Moia cel accord n'est que global' et si mes ad - ueraoirez eptirintien en foit feurr points de une tia ne pourrait man- quer de remorquer que leurs poziliona sont contradictoires. To take just one example, such con8/dère that the existence o f an lu- niaolaire phenomenon ert intonteatoble, mara que ce phénomène est oisJment ex:pllicable dont le tadre de le théorie habituelle. Tel outre ou contraire retonn lt bien que lo structure pézftodigüe oð8eru4e, si elle dtoif r4e/è, seroif absolument inéplíceote, mois il conteste an riolité. L.c plus grand aeruice que pour - raient me rendre mes oduercoirea, te seroit de rJ':f uger is common lo réu - mmdes thses que j'ouonce. Je doute [ort q u'ifs can put themselves d/octord mr une rJ/motion commune".*

In fact, it was absolutely impoaaible for me to respond to arguments for *which there was no precise wording*. It was certainly very easy for some people to spread the word in the corridors that my results had no basis in fact, or that my interpretation of them was pure fantasy. *But they were careful not to make this point of view clear, unequivocal and public, which would have given me the opportunity to respond.*

*My work and relativity theory*

2- There's no doubt that my work has been interpreted as not being compatible with relativistic theory<sup>4</sup>.

- In December 1957 I gave Louis de Broglie my *Note* of November 4, 1957 on the interpretation of the consŁance of the speed of light\*, and on April 24, 1958 I asked him to publish it in the Comptes Rendus de l'Acadómie des Sciences.

This request was refused by the two permanent secretaries, R. Courier and Louis de Broglie, in their letter of May 5, 1958, on the following grounds

*"This note, which i s based on highly questionable work by M. Hël y, and which does not seem to conform to the well-established principles of relatiuitë theory, does not seem to us to be suitable for this presentation".*

f3) A recent letter dated September 24, 1996 sent to the Editor of "La Jaune et la Rouge" n following my September 1996 article, "Lee *exptience de DoytonC. Miller* J9£5-J926 et to Tfiëorie *de re/ntioit!*" shows that these *verye* Úiuoc rumors continue ô

(4) Voir l'7n7roduction **ci-dessus**, § B.3.3, note 3, p. 55, eC ci-dessous C/topitre Y/, 4 C.1, p. 510-514.

This Note généralisait l'équation d'Hdly

$$(1) \quad \Delta\varphi - \frac{1}{c^2} \frac{\partial^2 \varphi}{\partial t^2} - \frac{2\mathbf{k}}{c^2} \frac{\partial \varphi}{\partial \mathbf{a}} \cdot \mathbf{k} @ + 4\mathbf{xk}6 = 0$$

to the eas of space anisotropy.

This text *était* was, to say the least, *étonnant*, because as I had indicated in my letter of April 24: "*Les résultats dont fait état ma Note sont incontestablement nouveaux et leur exactitude mathématique est hors de doute*" >.

• In the *Résumé* attached to the Invitation to my Conference of February 22, 1958, I had written :

*"Zen anomalous lunisolar components of the paraconic pendulum motion seem to be connected with the difficulties or anomalies encountered in the study of numerous phenomena in mechanics, optics or electromagnetism and tending to show the existence of a certain anisotropy of space (experiments by Michelson, Morley and Miller in particular). If this connection were to be confirmed, it would obviously be of considerable importance, as it would provide Miller's work with direct support tending to confirm its validity".*

In view of the importance of the "*negative*" result of Michelson's experiment for the very foundation of relativistic theory > , my text on Miller's experiments was bound to arouse the hostility of all those members of the Académie de la Sciences who were convinced of the total validity of relativistic theory.

}ttt;y},d 7

### *Unscientific positions*

3- Dans toutes les discussions qui ont été poursuivies à partir de 1956 au sein de l'Académie des Sciences et du CNRS il était *tout à fait compréhensible*, et à vrai dire *tout à fait justifié*, que l'on soit sceptique *en premier abord* devant des expériences mettant en cause une théorie de la gravitation sans cesse vérifiée depuis des siècles.

(5) Louisa de Broglie était en fait *d'autant plus étonnée* qu'elle était en *contradiction flagrante* avec la citation d'un de ses écrits que j'ai placée en tête de ce volume avant non ref. p. 7 above **Sommaire**

In fact, the *my work* I use is *purely a tidortme of m'i IM.*  
moziqne8 donz /o uo/idifd dzoif imnte8lable.

(6) See *Cfinites* IV et YH ci-dessus. Voir également mon article d'août-septembre 1996 de la revue polytechnicienne, La Jaune, et la Rouge, Les Expériences de **Dayton C. Miller 1926-1926** et to *l'Histoire de la Relativité*. Cet article et mes réponses au *Courrier des Acteurs* sont reproduits dans l'Appendice H du Devisidme *Volume* de cet ouvrage (p. 31 ci-dessus).

(D See note 1 in § G.2.1 above, p. 215.

11 It was entirely understandable, and even legitimate, that when uncertainty arose about the reality<sup>4</sup> of the anomalies observed, the responsible scientific authorities were reluctant to fund costly experiments.

But it was *totally unacceptable* to deny everything en bloc *a priori*, to refuse to **examine present analyses** on the basis of preconceived judgments, prejudice, and "*established verities*".

In view of my experimental results, I reduced all discussions to three fundamental questions: - firstly, *did the evidenced funisolar irregularities really exist?* The second was *whether these periodicities and their amplitudes could be explained within the framework of the accepted theory of gravitation  $\hat{g}$ ; and the third was whether these periodicities could be explained on the basis of other known phenomena  $\hat{g}$ !*

In fact, no one has been able to present any valid objections to my three *totally affirmative answers* to these three fundamental questions: - *the periodicities considered really do exist*; - *they are totally inexplicable within the framework of the currently accepted theory of gravitation*; - *they cannot be translated into known phenomena*.

Just as the peremptory and wholesale challenge to the validity of my experiments was strong and constantly renewed, so my opponents proved incapable of presenting *precise, reasoned and binding* objections to these three fundamental questions\*.

What would have been reasonable would have been to examine objectively, *in the context of contradictory discussions*, the validity, or otherwise, of my argument on my three responses. But at no time was I given the possibility to be heard.

(8) Section B above, p. 102-141.

(9) The only exception is the note from **Jean Goel** dated 21 April 1958 à l'Académie des Sciences immédiatement réfuté par les expériences cruciales de juillet 1958 (voir ci-dessus § C.2.3, note 7, p. 148).



In 1959, the CNRS Mechanics Commission suggested to the CNITs Board that it should set up a Commission made up of independent and competent personalities. For me, this Commission would have had the immense advantage of constituting a tribunal that could finally endorse me and judge the validity or otherwise of my experiments on purely scientific criteria.

Jean Coulomb, Director General of the CNRS, sent me a letter on the 13th.

In May 1959, I received a letter advising me of the creation of this Commission <sup>10</sup>. In the end, to my knowledge and for reasons I do not know, this Commission was not constituted by the CNRS Board <sup>1\*</sup>.

(10) Here is the text of Jean Coulomb's letter of May 13, 1959:

"My Dear Colleague,  
 "Z-a Commission de ~~Mécanique~~ ue o txo mint uos demondee more **n a pas**  
 tru deuoir prendre nut décision immédiate. **She** demonized à etre **éclairée**  
 per a Commission de épJtioliste.  
 'Monsieur P.Jrøe m'o odreëé to mere dec members of cettt Commission  
 ande **Je fais** ou8içôt /e nécessaire #our /es conuoguer.  
 'Je regrette tom me none tes retorde ct soul prit de croire, mon Cher  
 Collègue, h mee sentiments déuouée.

(11) The best thing for me to do is to reproduce here the passage from my Conference of November 7, 1959, where I gave a few hints about the envisaged constitution of this Information Commission, the only ones I have received.

Inc dec more ct dee motsI distussions, ct ä to suite du Rapport d'un dec  
 membnee de to Commiseion de ñfëconique du C2JRE gut eel ter précent et  
 9ue je remercie d'ouoir bien voulu venir aujourd'hui, fe Commission de  
 ñfëconique o proposé en Øirectoire du CNIT S de concitituer une  
**Commission** formée de personnoitd# indé pendotec, en l'esptce lee membres  
 de l'Académie dec Sciences gut c inlëreccenl à cette question.

'Lo Commiseion de ñfJtonique du C2JRE tet iormée de perconnoitJe  
 comptentes. bthe rcnd compte de to gronde difficulty du sujet ...

'Je cuts reuenu dte Ernie-Unic quelquec jourc event to rtunion du  
 Diretoire. Je oi rencontrJ deux personno title dont le poicd ttoit d4tici danc  
 l'affoïre. L.'une qui a un poste élué done le C?JRE m'a dix : "J'oi ton - jourø  
 été port ison de cette Commis#ion". She sent me an oimoble letter in mbi.  
 l'ou7rea a 4/eude situation in the Uniuerieilé. '7'outeø two m'ont dix to veitle  
 de to rtunion du Directoire: "Nouc minus concitiluer èette Commission et to

**partici erait.**

cetLequestion gut o été dtothue o ft de sanoir qui y

be::demain use decision nëgotiue a été prise. A credit of 2,500,000  
 fronts was granted to me on the understanding that it would constitute the last  
 ;finance- ment of the CNITs".

In fact, as I pointed out in my lecture of November 7, 1959 :

*"The failure to set up this Commission is somewhat strange. It is a refusal to be informed. What would we think of a Court that would judge without appeal, without request, without investigation, and that would refuse any investigation?"*

*"Le dñi de jus- tice est ce qu'il y a de plus dieu:ile à supporter, et je protteste ici publiquement contre une **attitude** qui me paraît antiscienti- fique, sovoir le refus délibtré de s'informer".*

In any case, it would be unusual, to say the least, for notes by members of the Académie des Sciences to be rejected outright, without **the** reasons being explicitly **stated**.

## 6.- Une décision scientifiquement incompréhensible. L'arrêt total des expériences après le plein succès des expériences cruciales de juillet 1958

At the end of 1959, in the absence of any financial support, the decision was taken to close my IRSID laboratory on June 30, 1960.

*With the benefit of hindsight, this decision now appears scientifically incomprehensible, and à urai say quite inadmissible.*

A new phenomenon had been identified. To test its validity, it was decided to carry out two crucial experiments. *These crucial experiments confirmed the existence of the anomalies observed, which were totally inexplicable within the framework of conventional theories. admitted !- \**

(1) *Such a slow periodicty does not make it impossible to explain the amplitude of conatoltea ptiiodicitz, particularly that of 24 h. SO mn., therefore the framework of l'heoriea admiaea.*

To take just one example, during his visit to my IRSID laboratory in November 1956, Joseph Péres, a member of the Académie des Sciences, expressed his strong disapproval of this impossibility. As for the real existence of periodicity he had been very impressed by the results obtained (see the account of this visit in my Note of November 30, 1956. Note sur tes mouvement du pendule **paraconique** for 2<sup>f</sup>. Co9uot, f3 pages).

D'ailleurs, à aucun moment, outun spdcioliste de lo théorie de le grouitotton n'avait conte8t4 fe cofcuf de l'ordre de grandeur dea amplitudes dea composantes périodiques lunisolaires que j'avais publié dans ma Note du 16 décembre 1957 à l'Académie des Sciences, 'Püdorie du pendule poroconique et in;7uence funisofoire. represented respectively for the Moon and Sun, in unit48 C.G.S. by the coefficients

$$C_1 = g \frac{M_1}{M_t} \frac{r_t}{d_g} = -0,62 \cdot 10^{-13} \quad C_2 = g \frac{M_2}{M_t} \frac{r_t^2}{d_2^3} = 0,3\% \cdot 10^{-1*}$$

and yet my opponents hadn't lacked the desire to contradict me.

(2) In 8a May 1959 letter to Werner von Braun, Director of Netioriol Aeronoutics ond Spoce hminiatrotion, General Paul Bergeron, former Chairman of the Comité d'Action Istientifuf ue de lo **Æfenae** Hotionale, wrote

*"Avant de vous écrire, j'ai yugd nccsoire to make them both laboratoires du f ro:esseur Allais Idoni one located d 60 mdtres sous terres octompogné d'Jminent z apétiolitec - dont deux pro/essence d'l'école Polytechnique. A ft e r a diatucaton lasting several hours, no major errors were found, nor was any attempt m a d e to explain them to the onal yce.*

*"de crois d'oilleur devoir conc aignoler qu'on courc de ces deux der-nièrec onrites, pluc de dis membre de l'At mie des Serrures et plus dc trente personnalités éminentea, cpécialietea b des titres diuera de lo great - tation, aonl uenua utailer, aoit aon laboratoire de Saint-Germain, 8oif son toöorotoire aouferzoim de Bougiuo ival.*

*"Discucçione oppro(ondiec ont eu lieu, non 8eu/emen7 Ô ces occo-cieux, meta égolemt a pluaiuez reprises dans diuerc milieux ccienti- ( 2uea, notamment à l'Atod mie dec Scientec et ou Centre 2'fotioriol de la ReearcAe Scientifique. nucurif d'entre elles n'o pu jucqu'ici mettre en évidente une explication quelconque dont le cadre dec théories actuelle - ment odmiaea".*

These crucial experiments had swept away all pre-sent objections, and *unquestionably opened up new perspectives for Florent, as it had been definitively established that some of the fundamental postulates of the theory of gravitation had been invalidated by experimental data.*

However, instead of continuing with the research, it was decided to call it a day. Instead of continuing with the *now risk-free* experiments, since the **existence** of the phenomenon had been established, *the responsible scientific authorities* ceased all funding.

How can we explain such an *absurd* decision when in 1959, on the recommendation of Werner von Braun, Director of the National Aeronautics and **Space** Administration, the Institute of *Aeronautical Sciences* had decided to translate and publish in its journal *Aerospace Engineering* the English version of my 1958 dissertation, *Should the Case of Gravitation be Reconsidered?* and that same year the American *Gravity Research Foundation* had awarded me a prize for my experiments on the paraconic pendulum, and in **France** I had received the *Prix Galabert* from the Société française d'Astronautique!

*How can I explain such a decision when prominent members of the Académie des Sciences considered that my research should be pursued, and had never ceased to support me?*

- (3) Dane ma Note du 15 avril 1959 destinée au CNRS, Note sur l'octuel des re - tGrthez et le /inoncement de8 trououx du Profezreur Alloti, je pouvait écrire

*"M3f. Caguol, Cet, Dorrieuz, KompJ de FJriel, L,eoutJ. Perord, Roy, Tordi, Thiry, mtmbrez de l'AtodJmic des Sciences, pensent tout que mes rec rrcAer doivent être pourzuiuc. Il en ert de même des personnoifJ4 zcientifiquez trh# nombreuz qui ont uisitè mes foöorotoires et dom cer-linee feront horiz doute portie demoin de l'hcodémir des Sciences".*

*The reason for this is undoubtedly the omnipresent domination of obscure and fanatical forces, always so active, always so effective, always so incompetent \*, at all times to ensure the domination of "established truths" and oppose the progress of science - r).*

This opposition to all progress probably remains as strong today as it was yesterday <sup>7</sup> and I can have no illusions about how the present work will be received today, so much so that any expression of it will have a negative impact.

intolerable, as soon as it clashes with "established truths".  
blies" <sup>8</sup>y <sup>9</sup>

(4) In his aforementioned letter of 1 \* June 1960 (§ E.6.9, note 10, p. 192 above) Jean Leray stated doctorally:

*"Les pend mes de Foucouit modernes foncèionntnl d'oillturz donc frottement, sons tinison oucune: ce sont les satetfiter orti/cieifs".*

Such an assertion certainly deserves to feature prominently in an anthology of the aottiaiera of the mandarins of science.

One might well wonder by what aberration such a sommit4 could have been Blue ù la Section dt nñèconi9ue de l'Académie dea Sciences, a field in which it clearly had no competence.

(5) See CAapi7re /X below, p. 659-674.  
In July 1959, Louis Rougier was able to write about my experiments  
*i s t o n d o l t . à*  
Polytethniqut) :

*Ce qui l'on zubodort tu lui, ct conf des idées révolutionnaires, remet - tant en quction dtc poziuloiz concidérée comme dtc dogmec por certainr dtc membres de l'Acadm ie dtz Scientez".*

Louis Rougier's memoir is reproduced in Appendix i D of *Decrême volume de cet ouvrage*.

(6) The burial of my experiments on the anisotropic-supported paraconic pendulum was quite similar to the burial of Miller's 1925-1926 experiments and his 1933 dissertation (*Chapitrt NV* below) which I wrote about in my 1968 dissertation, *Do we need to reconsider fer lois de lo gr'ui lotion?* (p. 102, note 38) :

*"The outright burial of Miller's memoir (from 1933) porolts me un dec ccondolec dt la physique contemporaine".*

(7) As far as I'm concerned, in 1959 rumors were once again circulating within the Académie des Sciences about my experiments with the paraconic pendulum, tending to discredit them.

(8) On these dogmatic and blind positions see Cùopitre fX, p. 659-674, and the *Chapter Z*, pp. 685-689 below.

(9) The climate that existed in 1959 is now di@zci/ement imog'inoô/e.  
Before my November 7, 1959 lecture to the Société des Ingénieurs Civils de France, authorized and watchful personalities had told me: "Si : **le jugement** was good your dites lo uèritt, uouc htez perdu i vous des hostilitèc qui ne **allez susciter contre vous** démrmtront jomoic".

(My conference on November 7, 1959).

**Of course, there** have been *countless examples of mistakes made* by **scientific** authorities in all eras, **but in** the case of my **experiments** with the paraconic pendulum, crowned *in spectacular moni re* by the crucial experiments of July 1958, denying the obvious, *decisive and resounding* evidence, was and remains particularly cho- quant.

Today, **and after** some thirty-eight years since the publication in 1958 by *PersRectives X* of my memoir, "*Do we need to reconsider the laws of gravitation*", I can only confirm what I wrote at the end of that memoir, *written before the experiments that followed. crucial events of July 1958!*

*"My conclusions are reached in the full knowledge of the objections which have been explicitly and precisely expressed to me, and which I have examined in depth. To date, none of these ob- jections have been accepted.*

*"I fully understand that the facts I put forward and the interpretation I give to them raise a priori doubts and skepticism. I understand all the more all the reservations expressed about my results and conclusions, since I did not cease to make them to myself for three years, from 1953 to 1956. During these three years, I systematically refrained from reporting my results, even when they were particularly striking, as was the case for the solar eclipse of June 30, 1954.*

*"Skepticism is, I'm convinced, the only scientific stance that should be adopted when new rtsultots, resulting from limited experiments, tend to call into question the ua- liditt of principles that have constantly been con{irmed by innumerable previous observat ions.*

(10) p. 104. Mea comments from early 1958 bear witness to t h e hoaitilit4 I was confronted with from the moment I published my first ?\Notes to the Acad mie dea Sciences in 1957.

*"But if it's scientific to adopt a cautious and skeptical attitude, it's not scientific to condemn without hearing. Nor is it to condemn en bloc without saying where exactly we disagree ....*

*"Lee dogmatism and sectorism are not scientific positions. Facts and facts alone must decide on theories, not the other way round.*

In any **case, I'm not** one to bow to **ignorance** and fanaticism, and I can only heed Auguste Lumière's warning:

*"A new truth, whatever the convincing arguments on which it is based, is only accepted after a very long time, often twenty or thirty years.*

*"All those who have fought so hard for their ideas have had their conceptions forgotten".*

(11) Auguste Lumière, 1942, des *Foeyeurz du Progrès. L'ce Mondorine tout re lee Pionniers æe le Science*, p. 3d7.