Maurice ALLAIS

L SPACE SOTROPY

LA NECESSAIRE REVISION

DE CERTAINS POSTULATS DES THEORIES CONTEMPORAINES

*

LES DONNEES DE L'EXPERIENCE

CLÉ$IENT.JUt.lAH

Editions juridiques et économiques

 ISBN 2-908735-

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 euery suggestion, but determined to judge for himself. He should not be biased by appearances ; haue no favorite hypothesis ; be of no school ; in doctrine haue 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 pMnomenon. If the uërification doesn't happen, it's because there's something unexpected*, something extraordi- nary: it's because we've found* f'inconntz 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 c o n v e n t i o n a l conformism, finds it impossible t o have his ideas accepted, no matter how rigorous the arguments that formally demonstrate their accuracy. ..

The cort i q/uste of the nouateurs, the rnéconnaisaisaisance and f'ouòfi *of their 'euvres, the iniquitous judgments which one pronounces against them, the persecutions even which one inflicts to them, are the* ***rule****, many sauonts 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*****

* **Citd by H.C. Dudley, Neui *Printiplea in* Quontum 3fecfionic, 1959, Exposition Press,** New **York, p. 6.**

--Vilfredo Pareto, 1917, 'ProifJ dr *Hociologie* GénJrofe, Payot, p. 320.

--- **Henri Poincaré,** 1906, m Science *et l'H ypotMzt,* Plammarion, **1927, p.** 178.

'* kouis de Broglie, 1953, Zo P/tysigue guantï@ue *reatera-I-ells Indétsrminisle,* **Gauthier-Villaró, p. 22.**

* **-*Auguste Lumitre, 1942, *Z-ca Foazoytura du Pmgrèa, Lee 3fondorina tontrt lca Pionniere de lo Scienze,* Imprimerie Léon Sézanne, Lyon, p. X, III, and XYI.**

SPACE ANISOTROPY

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(Mémoiresdedécembre1958)

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(Mdmoire de 1961)

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(Memoir of 1982)

f' - Ser fa *distribution* iiormufe deø modern ò cms citstozits régu-fifirement espocés 'f'une sonime 'fe øinusoiï2eø
(Note by Maurice Allais presented4 by Robert Fortet, CRAS, May 30, 1983)

G - C! itsiźférofione str fes łhéories pfiysi9ues
(Excerpts from my 1983 dissertation: *Les fondements de la théorie de l'utilitë et du risque)*

Zf - *hea* expézfienc *eв by Damon C. MÛler 1925-1926 and the Z7séoz-ie*

(Memoir published in "Ln ufatine *et la Rouge",* revue polyteeh-nicienne, September 1996, pp. 29-37).

I - Zteø rêguIaMt:ê's trimti,g'*nifîc'M:rues in leø obsertiaM* s *in* e *ê-*romètr *new de Dafon C. Miller J92S-192õ*
(Comptes Rendus de l'Acadèmie des Slciences, March 1997).

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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 *mur l'existence d'une Composante Périodique de Période Voisine de 24h CO' dans les Mouvements du Pendule Paraconique à Slupport 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 Peri- dicities 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 Peri-dicities by Considering Corrêlogrammes*
IRSID, November 26, 1959, 9 p.

Eclipse de soleil

1959 *Mechanics - Movements of the Paraconic Pendulum el Total Sloleil 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 t h e 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 Ani-sotropic Support and Small Oscillations - First and Second Appro- ximat ion*
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 o f a support as iso-tropic 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 t h e Azimuth.*
IRSID, November 10, 1959, 7 p.

C-éocféoie et Optique - ***Search ExpérimenfaJec***

1968 *Anomalies in Triangulation and Levelling Operations - Possible Explanation and Comparison with Experience*
IRSID, May 21, 1958, 5 p.

IŒÖ *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 moitde of physics...

"It must call upon a new gran- deur, accept a new hy pothesis 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 enri- chir the physi que of a new quantity, to complicate it with a new hypothesis.*

"These pri nci pes are the very esse rice of our physical theories".

M.P. Duhem *

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

Max Born **

"There's always a nutsiòfes *te ndance in letting* opt ntons *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 ouvrages* 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 cause* de *the truth.*

"In the su jets of philosophy and sctence, *outoritt has always been the great adversary of vêrité. Despotic calm is usually the triumph o f error .*

"En sctence *et* en pAtfosop/ite rten ne *doit être tenu 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 1

(-) M .P. Duhem, 1899, *des* théories *Electriques de* -f. Clerl 2'faxwelf. *Etude hisse* - ri9ue ef critique, Annales de la Socièt4 Scientifique de Bruxelles, 24*^ annèe, 1899- 1900, p. 2d5.

(- -) Max Born,l920,Lo tAdorie de fn refotiuitd *d'Einstein* et *ses* boses *physiques,* Gauthier-Villars, 1923, p. 291.

(---) Stanley **Jevons,** 1888, *Théorie* de l'économie f-oïitique, 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/ïcotions *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, Atierrntion *Problemc. A Discussion* concerncng' the mo- *tion of fée Eorth, ond* concerning the *connection bel* ween *Ether ond Gross* 2'fofter, ioitfi some nez Axperiments. Philosophical Transactions of the Royal Society of London, 1894, Vol. 184, p. 727-8%.
Edmund Whittaker, *History o[the theories o(Aether and* Electricity, Tome I, 'Flic Cloasicof 7'heories, 1951 YO*F especially Chapter XIII, *Clœsical* **7'/seory** in ï/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 relativit4 theory in literature2,

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 o *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* ïJ, 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'Esclangon 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* 'ï "fieories, 1953; see especially Chapter II, the 2telotiuity *o[Theory of Poincoré end* Z-orentz, pp. 27-77, and Chapter *W ,* Crrovitotion*, pp.* 144-146; and *Chopitree IV, VI,* and *VII* below.
Wfiittaker 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 construc- tion *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 appro[ondissement prêola ble of their structure and regularities* that conditions any overall theoretical construction [3].

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 inïerim 2teport on o 2temeni o/ the ***Allais Exp****erimenl* Robert Latham (Imperial College of Science and Technology, 70 p.) **écrit (p. 5) :**

> *'Indeed in all hic trorh there is a* complete absence *of any delailed al* tempt ct explnnntion, *coupled* However with very greet cere *end fho- roughnesc in the conducl oj"* tùe ezperiments".

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 onisotro- *pie de l'espott.*

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 Relativity4 , 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* t e s t *patterns and collimotors* in 1959 *(Chapter III);* and the analyses I have c a r r i e d 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* Y, VZ, *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* informa- üon6 *mq* on the very foundations of con- temporal physical theories *(Chapter VIII).*

My 1958 memoir

2 - In the magazine *"Perspectives X"* of the Ecole Polytechnique in 195 1$, I published an overview of my work on the anisotropic paraconic pendulum from 1954 to 1957, under the title *"Doit-on Reconsidérer les Z.ois de la Gravitation î"*. The English version of this thesis was published in 1959 by the American journal *"Aero-IS pa ce Engi neeri ng"* of the institute *of the Aeronautical lsciences* under the title: *"Slhould the Laws of Gravitation be Reconsidered î"* *.

This dissertation was followed by a two-page * *Note Complémentaire* in which I reported on *crucial experiments* carried out simultaneously with identical dis- positives 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, *"Compltmenlz expérimentaux et théoriques" (* see Contents above, p. 31).

(2) *Aero-S!poee* Engineering, September 1959, ri° 9, p. 46-52 ; October 1959, ri° 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, di - recteur de la ***Net** ionol* Aemnn utics end Spnce *Administration.*

(3) This *Supplementary* Note*, which will be* sent *to* the *Perspectioez X* Editorial Office *at a later date, is subject to the following conditions*
/e8 ezp4rïence8 ***cruciales*** les *de* 'Juiîîet *1958,* a ét4 simplement en car t4e sans pagination dans cette publication after proofreading.

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

(4) I-e first volume will be published sous8 le titre: *"Rechercher* €z#4rimento/e8 *el ' héoriques sur* les Théories *Phyaiguez 19S3-1960"*. It will include my main publications and dissertations (see the *Rtftrencec* 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 Poroconique"* and *"Anol yee s Compltmenfoirez des Données de l'Obzeruofion"*.

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 pa- raconic 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 [1].

(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 9uorf of all my time on my theoretical and experimental research into physical theo riee.

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 7'roité *d'économie* Pure, 1994, *T'roiaiàm-* Introdtzction, p. 19 and 26).

A conviction

2- I've **always been convinced** that the propagation **of** gra- **vitation and electromagnetic actions takes** place from *near to near, and that it implies* the existence of an intermediary medium, the *"ethe r"* of Fresnel and the physicists of the XIX* century, without, however, being able to **consider,** *as was generally accepted in the* ***XIX! c e n t u r y*** *,* that the "ethe *r" of Fresnel* and the physicists of the XIX* century was the *"ethe r" 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** [2]

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 [3] It was the study of these *completely unforeseen* anomalies that formed the essential object of my experiments from 1954 to 1960.

(2) See *Chopitre* Yí 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 *di[ficult at louc pointc de uue.*

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 chomps that can be made* [5].

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"* [6].

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. l).

(5) So, in 1989, I gave up suggesting any new experiments on the action of 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 l95d 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 rec7uercAes eqpéz€mezufo£es sur flej>9nô'uZe poxoconJqane 7954-7960,
e7 sin" Ms déuicuNona opfi#ues des uBées sur zmMes *J9S8*

The existence o f 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 ma- gnetic 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 [1]. 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 4$ 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 torres-pondance* 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 [5].

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 trionguloiion 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) Cfiopitre JJ, Section B, below.

Anomalies observed during the two total solar tclipses 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 anisol ropy direction of uarioble space with time deduced from observations of the paraco- nic 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 tin *isol rope support in* order to be able, by a new me- thod o f analyBe, to *determine at each instant the anisolropic direction o f 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 o f spatial a ni sot ropy (Chapter II).*

(7J Cùepitre J, Section D, below.

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* evi- dence of the anomalies of the paraconic pendulum and their periodic struc- ture 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 *n o one had even suspected t h e i r existence.*

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

(81 See for example Dejean de Fonroque, July 1879, *Du Pendule,* 7'héorie de *ses* un- rintions, Chamerot, 32 p., and *Note* du 14 avril 1979 à l'Académie des Sciences, présen t4e par M. A. Cornu, *Sur* diverses *expdrÎences* /oites ouec un *pendule oscillont* ouec *de grandes* emplit *udts.* (see a detailed analysis of Dejean de Fon roque's memoirs in the *second volume of* this work, Chapter Cf, Section C, above, p. 28).

bc mJmoires *de Dejean de Fonroque* are only *quolitotives ef they* canuenï *foire l'objet d '* **une onnlyse** *quonliloti ae.*

(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- c£ttioZl statistiqtle 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 - *Ptriodicitt Test - Cenerology of the Ptriodicitt Test S!thuster au* cos *de séries femporellec auto-* ***torreltes***

Note of November 13, 1957 - *Observation of the movements of the poroco pendulum* nique.

Note from November 2S 1957 -*Harmonic analysis of pendulum movements* ***paraconique.***

Note dated December 4, 1957 *Movement of* the *paraconic* pendulum *and eclipse of the* sun *on June 30, 19S4.*

Note of December 16, 1957 - *Poroconic pendulum theory and influence* ***lunisoloire***

Note of December 23, 1957 - *Application of the* ***Generalized*** *S!chuster Test ö dec azimuth harmonic onolysis of the paraconic pendulum.*

Note of November 3, 1958 - *Noncelles experiments* on *the poroconic pendulum* onisotropic *support.*

Note to December 22, 1958 - *Periodic slrizclure of pendulum* ***movements*** *poroconic with onisotropic support ö Botigiuol et* Soinï-Germain *in July 1958.*

Note of January 19, 1959 *Experimental determination of the influence of the* inclination *of the load-bearing* surface *on the motion of a paraconic pendulum with* ***onisotropic*** *support.*

Note dated February 9, 1959 - *Experimental determination of the in 'luence of support onisotropy on poroconigue pendulum motion.*

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.

Sn addition to their publication in the Comptes Rendus de l'Académie des Sciences, these various Notes were published aèpardment by Gauth ier-Villara in two **Fascicules** entitled: *"S!lrucf ure périodig tie dec mou sementc du pendult poroconique à cuc - pension anisotrope el* in/Iueztce Iz/nisoIaire. *Rêsullals 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 195g [11],

Mes Confêrences

9.- My work has been the subject o f three Conferences organized by the *Certle Alexandre Du our :*

- the first, *"Faut-il reconsidérer les lois de la Gravitation? Slur* une *nouvelle expérience de Mêtanique",* on Saturday February 22, 1958, in the Henri Poincaré amphitheatre a t Ecole Polytechnique [12].

- the second, *"Faut -il reconsidêrer les lois de la Gravitation? Noueaux résultats, bilan et perspectives",* on Saturday November 7, 1959, at the Société des Ingénieurs Civils de France [lÖ]

- the third, *"Lees përiodicitès constatëes dans le mouvement du pendule paraconique sont-elles réelles* on non? *Generalisation du test de Slchuster an cas de séries temporelles autocorrélèes",* 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 (§ A2.2 above).

(13) 13 'tableaux muraiix ; 50 p[0 'ȷections ; stenotypie de la Con férence et de la Discusson, 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 st4notypies 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 [1'], 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 [17], [1] .

(15) The dissertation presented to the Soeidtd Pronçoise d'Astronoutique was entitled *'Rechercher théoriques* et *expérimertto/es* rtouue/les *sur lo firouitoliozz'* (December 1958, 21 p.).

Attached to this memoir are my memoir of May 13, 1958 "Anomoiies *du mou-* ue ment *du pendule poroconique à cupport 'anisotrope"* (68 p.), my first eight Notes è 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. Grovity *Research Foundot ion* in MRI was entitled: *"Neur Theoreticol ond* Azperimentof 2teseorcfi *Word on Crranit y"* (January 1959, 9 p.).

(17) Whereas in 1868 Van der Willingen was able to write (de Pendule *de Foucault ou* ñfusde 7'eyïer, Arch. Musée Teyler, I, 1868, p. 342)

> *In* the experiments *on* the Poueouït pendulum, in terms of ezpèri- menant or *moina,* we *stopped precisely at the point* where *utri - lablea difficulties* **began".**

in 1959, the responsible French scientific authorities deliberately put an end to my work. ezpé nothing these on the pendulum paraconique *clore that* porodozofemenï *the difficulties* essen-
tiellea believe ttt aurmonttes.

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 was4 *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 e r n i e " ihéo'-ivres, 9so- s9s

Analysis of paraconic pendulum movements

1 - Since 1953, I've never stopped working on the analysis and si- gnification of paraconic pendulum movements and the op- tical deviations I've associated with them, particularly as regards their rela- tionships 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 [1].

Two major di//iculties

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, etzctzn *author had not calculated the lunisolar influence on the motion of the Foucault pendulum.* In view of the *fundamental* importance of the theo- rique estimation of this influence, I was obliged to develop a complete theory.
paraconic pendulum movements [2].

(I) §A2.3.

(2) See my Mé moire 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 ?\fotes intended for the Aeadémie des Sciences but which could not be published (see above p. 33).

Ayplicolion dti 'Ffiéoréme *de Bour ou c'is des* mouvements terrestres *dons le* cos le *plzzs* general, March 14, 1958, 9 p.

Equolione du mouvement *du pendule parocon ique b support onisotrope b* petites *oscillolions.* **Premiére** et *deuxième approximation,* August 18, 1958, 7 p.

- Yoriotions des *poromètres ostuloteurs de* /'etïipse *décrite dons* ïe *mouvement du* pendule *poroconique à support anisotrope* et d petites osciffoïions. *Première et deux ième opprosimol ion,* 18 aoùt 1958, 6 p.
- Corrections of first *opproximotion* dti pendulum poroconi9ue d'*support onisotrope* et d'petites *osciffotions,* August 18, 1958, 4 p.
- Second *opproximotion* corrections *of the isotropic supported poroconic pendulum el à pelites 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 diffi- cult4 by developing a test generalizing the *Slchuster 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 gënëralisation of the Schuster test nu *cos de stries temporelles o ufocorrtltes* was the subject of my two Notes è l'Académie des Sciences of May 13 and November 13, 1957, and of my overall dissertation *"Test de périodicité. Généralisation du test de Sthuster ou tes de ctries temporelles outotorréltes dons l'hypothèse d'un pro:icessus de perturbe lions oltoloirez d'un système s fable",* prësent4 en 1961 à l'I nstitut International de Statistique. This text is reproduced in *Appendix* D of the second volume o f this book.
See *Chapter* ï 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*

$$\frac{1}{\sqrt{|g|}} \partial_i (\sqrt{|g|}\, g^{ij} \partial_j \varphi) - \frac{1}{c^2} \frac{\partial^2 \varphi}{\partial t^2} - \frac{2 k_0}{c} \frac{\partial \varphi}{\partial t} - k2\ Q + 4 \text{ x } K\ 6 = 0$$

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

- *Sur une interpréfntion* possible *des anomalies de la grauitè et ses applications,* November 5 1957, Sd p.
- *Interpretation of gravity anomalies* as *a screen effect of group ections,* March 1960, 29 p.

On my dissertation on the speed of light of November d, 1957, see below *Chapter I,* § G.5.2, p. 226; *Chapter VI,* § C.1, p. 511-51d; 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 obser- vations 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 Gëntrale* *.

(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 *Wotes à l'Atadtmie des Slciences* de 1957-1959, et tout particulièrement dans mon mémoire d'ensemble de 1958, *"Doit -ori 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 [3].

Consistent anomalies

4- In fact, the analysis of the observations of the paraconic pendulum with anisotropic sup- port 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 Esclangon'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 o f a

nnisotropy of space,
the determination of this anisotropy o f space by *astronomical in- fluences,*
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 Relativit4 Theory, Albert Caquot never ceased to urge m e 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 I* below).

It seems preferable to me today to free myself *completely* from any con Crainte 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 concep- tions, but it does allow us to assert that Motif *behaves as if* these three guiding *concep-* tions *ef/ectively* corresponded *to a reala-*

In any case, *the validity, coherence* and *properties of the observations analyzed in the first* ciii9 *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 t o precise laws and a general theory.

Only one certainty is currently emerging. Non-new phenomena have emerged that currently accepted theories a r e unable to explain.

I have therefore limited myself in this work *to only the data from observation, systematically avoiding any theoretical synthesis* [7]

(4) See Chapter YC below.

(5) *En fort l'onisotropie* de *l'espèce el so déterminer ion por des* in/tuences astrono- *mics are not* hypotheses. They *are observational data.*

(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 exphq* ner /oci1emen ï à porttr de f fiypotfiêse d'une anisotropie de f'espece *d'inertia* (see below, Chapter f, § F.3, p. 206-212, and *Chapter* ff, Section I, p. 320-325).

La Théorie de la Relatiuitë

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 prete ndu nêgati ca rattère 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) *Lettrt dt Ftnelon, 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 e¢ 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.* T hese 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* compidtement *que celle de l'oct ion mm nelle de deux corps".*

(André-Marie Ampère, 1826, *Théorie mothémotique 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* prësent4s in this ouwage.

(6) André-Marie Ampère, 1826, 7'/*Mathematical* theory *of electro-dy- phenomena namigues* uni9uezzienï *deduced from experience, p.* 118.

Relatively large-scale effects

3.- If one considers contemporary research, the consi- deable 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 paraco- nic 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 ef- fects observed, particularly for the paraco- nic 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 sexagtsimal 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* ^{105} *rodians 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* rad/sec = lo ' (. 60 . 60) (100 . 365.25 . 24 . 60 . 60) seconds per century

= 6.51 10 seconds per century

(9) Some may wonder why I waited so long to publish this book. The reasons are simple. In the soi xante years, the hostility that was mani - fest4e 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 proba bili t4s, all of which gave rise to numerous publications on my part (see Allais, 1989, *Autoportroits,* p. 121-144).

As Max Planck [10] 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 [11]:

> *"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 lstiente el l'H ypothèse,* Flammarion, 1927, p. 178.

D

SOIMIENSFFOPPOSIMONS

• I owe *a debt* of *gratitude to* Emmanuel André-Martin, Pierre Ricard, Albert Caquot and René Dugas 1$, 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 2.

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, whose efficiency, professionalism and sense of-
sion, intelligence and dedication were exemplary, that my experiments could be carried out with full success 4

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

(2) On the premises of Compagnie Clem ençon, 34 rue Milton, Paris, whose president was André-Martin withl 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 was4 astonishing

I have read our 1943 economics book. I consider it to be absolutely fundamental and comparable to Lagrange's "Métonique Analytique" for economics. 'Je *fois nous donc con mnce. What do you* nine-if?
"ñfof/ieureusement *je pars demoin oux Etats -Unis, mois adressez nous de m'*i *part ou Directeur de* f'ïnsfif 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.

(41 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 ö* l'Académie des Sciences sur le pendule paraconique '. 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 was4 that of February 9, **1959** [6]

I am indebted to Ztené *Dugas* [7] 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.
nique des azimuts du pendule paraconique [8]

(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. 5l).

(6) From that date onwards, a powerful cabal was launched against both my work in physics and my liberal positions in economics.
See below, Cfinpii re f, § G.5, p. 225-230, and *Chapter X, §* B.2, p. 685-686. See also Louis Rougier, Juillet 1959, Scendnte d *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'lngres, the analysis of the foundations of Mechanics, and he published two fundamental works: *ffistoire de la Mtçonique* (1950), and *Lo Mtcanique au Vlllème 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 ***paraconique sera*** *pendulumin tour les obserualoires 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 *Comitê d'Action lstient ifique de la Défense Nat ionale* and by the *Centre National de la Recherche lScientifique,* 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 Recherthe 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 Slociété Française d'Astronautique,* and a 1959 award from the *Gravit y Research Foundation!*

These experiments in July 1958 were accompanied at IRSID by sighting experiments on test patterns, which gave *quite de- cisive* 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, CAopitre *III,* Section B (see p. 29 **above).**
It's thanks to Guy Berthault that we'll be able to publish the present xc - lume 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 ScieRceS by Albt rt Cäquot and Joseph Kampé de Fériet [1].

On February 22, 1958, I presented a summary of my results at the of my Conference, *"Faut-il reconsidérer les lois de la gravitation î Slur* une *nouvelle expérience de mécanique",* chaired by Albert Caquot, in the Henri Poincaré amphitheatre at Ecole Polytechnique [2]

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 î".* The English translation was published in 1959 by the American journal *Aero-IS pace Engineering,* under the title *"!Should the Laws of Gravitation be Reconsidered î"* *.

(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, £e pendule /nto1 eux lois de to méconi9ue, Le P-igaro Littéraire, January 18 1968; René Sudre, L'#nigme de to *grayitotion,* Revue des Deux Mondes, February 1 e* 1958; Henri François, f'-out-iÎ reconsidérer les fois de to grouitotion?, Le Monde, February 22, 1958; Pierre Devaux, ñfoni/este *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, Donation Cot, Jean Coulomb, An dré Dan ion , Georges Darmois, Joseph Kampé de Fériet, André Léauté, Albert Përard, Joseph Pérès, René Perrin, Maurice Roy, Pierre Tardi, René Thiry.

(4) Vokci above§A.2.2, p.42.

All in all, my experiments have aroused considerable interest o n all sides, in France and abroad 5

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

IS) I received a huge amount of mail from all over the world.
For example, j u s t recently, on August 1 -* 1996, I received a letter from a phys'ici'in Chinese S.W. Zhon of the Department of Physies, *Huazhong* €fniuersity *of Slctence ond 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 anomaliec of the paraconic pendulum during the tclipse 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 § **I1.3** above, p. 39). See also the assessment made in May 1959 by General Paul Bergeron, Chairman of the Comitë d'Action Scientifique de la Défense Nationale, in his letter to Werner von Braun, Cfinpitre f, § G.6, note 2, below, p. 231.

(7) Following the publication in September **1996** of my article **"Les** *exptrientes de Doyton C. Miller et lo Thtorie de l'*i *relatiuité"* in the Revue Polytechnicienne, *Lo Jaune et la* Bou,ge, one of my 1958 correspondents, Paul Ernest de Montaigne, a former s t u d e n t of the Eeole Polytechnique, wrote to me in the following terms

> "-f'oi suivi *ouec un tres Rif inltret vos conftrences sur le pendule porc - conique* .
> "J'oi *éf I, font- il le dire, un peu {m ztrt de veus uoir Prim Nobel d'Economie.* Je nous *oltendois douontoge Prim Nobel de Physique.* Yous Voici *reuerin à lo Physique, et com ment! I hope you won't obondonn it again".*

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

> *"Plocse* occept iny *most heort y congrotulations*
> *'II is a pit y, ctiente being uihat* it is, ifiot you con'I ***get a si miler recogni*** *- lion for the pendulum worn. I Sinew I cm in o minorit y but my personal Grew is that it is just cc important, and will be ccii nowledged os such in due tourze".*

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. *esloblishmenls"* of all kinds who dominate [1].

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 cru- ciole experiments and their significance *.*

(1) My first two Notes to the Académie des Sciences, "3fouuements pdrio- *digues du pendule poroconique" eï "Anolyse harmonique des mouvements du pendule* pnroconique", presented by Albert Caquot in 1956 were initially ët4 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 Acodtmie des Sciences.

(2) In a recent book, *"L'uniuers de lo relatiuitt gtntrale",* 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 gtntro- lement hostiles. Yet his orgumentot ion étoif solide...*
>
> 'Lo *trifique qui a Ht forte - en 1958 - des résultats de Meurtre Allois est dtpouruue d'objettiuité*

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* Neu/ton*'s faith. Many experiments show anomalies. If there were only one, one could doubt it; but their number and rtpttition should have induced the scientific community to analyze the* results *and* take them *into account. However, their publication* only aroused *izzdiffiéreme* ef /'7zosfi/ifé. A lo udrit4*,* les arguments ïnuogués *por tes* oduersoires *des thèses de Meurtre Allois sont contradictoires. Les ans - por* exemple - confestenï *la périodicité des courbes représentoliues* des *récul tot s obzeruJs, clore que d'entrer l'odmef tent, mois prétendent* que *lo Ioi* de 7Veu/ton *sufj'iI à* les *expliquer.*
> *'Mois il y o pire que l'indi{férente; le silence. In the specialized press, one would expect to find an orderly critique of the results of Meurtre* AI1oi#, and *in the general press, editors would be pleased with the noueouté of the reszzllots obtained, equalling* the *argztmenls of their opponents. However, nothing of the sort happened, and in J 958, 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 dogmotiques*, 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* /enefÎque *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 *rë used to prevent me and they never will prevent me from defendingR: cR: who I pensSR: to be the verilë* S.

(4) The most formidable and treacherous tactic against new ideas is the conspiracy o f silence, against which n o 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 dep0utised of it, and William 'lames w a s right to write (Allais, 1966, L'ñconomique en tent que *isciencel*

> *Tome doctrine trouerse trois êtots: on l'attaque d'abord, en la déclo - ront 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 retonned in(in so mérite ble importance and its ad uersoires reuendiquent mort l'honneur d e l'oooir découverte".*

(5) N o n e t h e l e s s , I felt that *t h e* rejection by the Acadèmie des Sciences of the publication of my *Hole of 23 [éurier 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 IRSID and, for lack of funds, n juin 1960 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 Portie* of this work Chapter VIH, see above p. 30).

Que *of*ficielle *mondarins de lo science of ficielle pu participer à ceSte cobale n'en chonge unfortunately nothing lo n'iture. 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 ä* mon Unité *d'économie Pure,* § 3d, p. 124-126, and Louis Rougier, July 1959, *"lscondole à Pol ytechnique".* Louis Routier's memoir is reproduced in Appendix *I.D* in the *Second Volume o f* 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 standardsela , is not mine. But **would** that **b e** a major reason to **keep quiet** 6 7

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

(6) In my *Con[trence* of February 28, 1958, at the Ecole Polytechnique and bored the ex - crucial events of July 1958 I was already saying

> *"Il* est fiors de *doute* d'*il i* y*a a* pour *moi de très grands risques, étant un économiste, à [aire* des recherches *de physique, et d'encourir le risque de* me tromper. *You can forgive* a pro/essionneJ to *be wrong, but* you can't *pordon* nero connors d *someone* who ri'est *pos de lo* pro;fession, 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 récfoc¢ïôn 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 [1],

The first *volume*

2- In what follows, I examine [2] :

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

My experiments with the paraconic pendulum on an 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 nOm s.

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)* [4].

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 out
leader ori,ginoiu, et non *pos aun: commentires de seconde main.*

(4) See Sommnire above, p. 28-33.

(5) See in particular Cfiopiire 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 [1]. But today, as in 1958 [2], 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, bias 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 Scien ces (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 ob- servations 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* o6figured *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 n o t 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 obs- tacles 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 motte */ondnmenfnf of my approach is to express what I believe to be vêritê.*

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

MIsx&exæxwCESsURU&PENDuc PARACONIQUE ASUPPORTANISOTROPE

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* orats *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* utuement *surexitte ; 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*

Me6 Expériences sur II pendtlle par£fco£llque à Stlpport anisotrope [1] se poursuivies de 1954 è 1960. They gave rise to ten Notes to the Académie des Sciences in 1957, 1958 and 1959 [2] 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 Du[our: "Faut -il reconsidérer les lois de la Gravitation î lsur une nouvelle expérience de Mécanique"*, February 22, 1958; *"Faut -il re- consider* fes lots *de la Gravitation? Nouveaux résultats, bilan el perspec- tives"*, November 7, 1959; and *"Les périodicités constatées dans le mouvement- ment du pendule paraconique soul -elles réelles ou non î Crénéralisation du test de lschuster au cas de sêries temporelles autocorrélêes",* March 18, 1Œi7 .

In view of the very conception of the present work, the presentation The following discussion of anisotropic paraconic pendulum anomalies will *necessarily* be *confined to the essentials* [6]

* 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, fntreduction, § A.2.2, p. 42.

(4) See above, *Inlrodutlion,* § 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).

CARACIESESGENNRAUKDESEXPEWŒNCES
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.

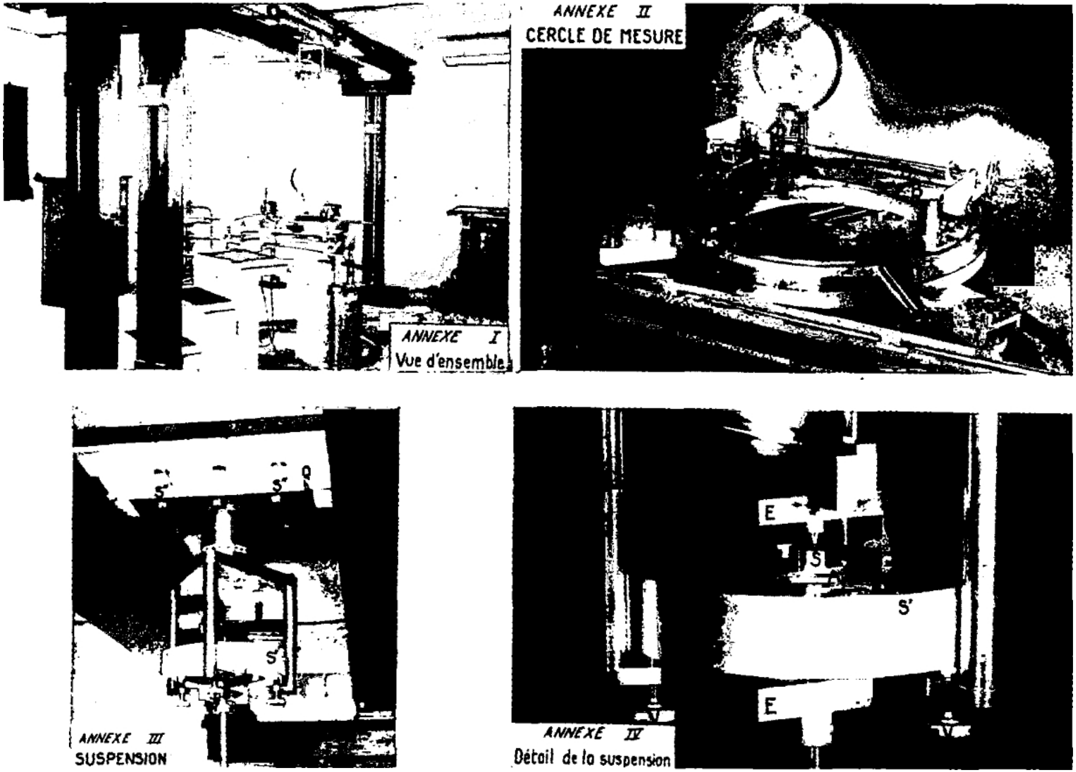
This surface in turn rested on a 4.5 cm thick, circular, hollowed-out S' aluminum support with an A appendage. The recess allowed the pendulum to rotate through a total angle of 210 degrees. This support S' was supported by three screws mi-
V attached to a support S" bolted to a beam, itself clamped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

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

(2) These four photos grapnıes sc h' have reproductions of nnzes *I to IV* of my *Note* from November 13, 1957 l'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.



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

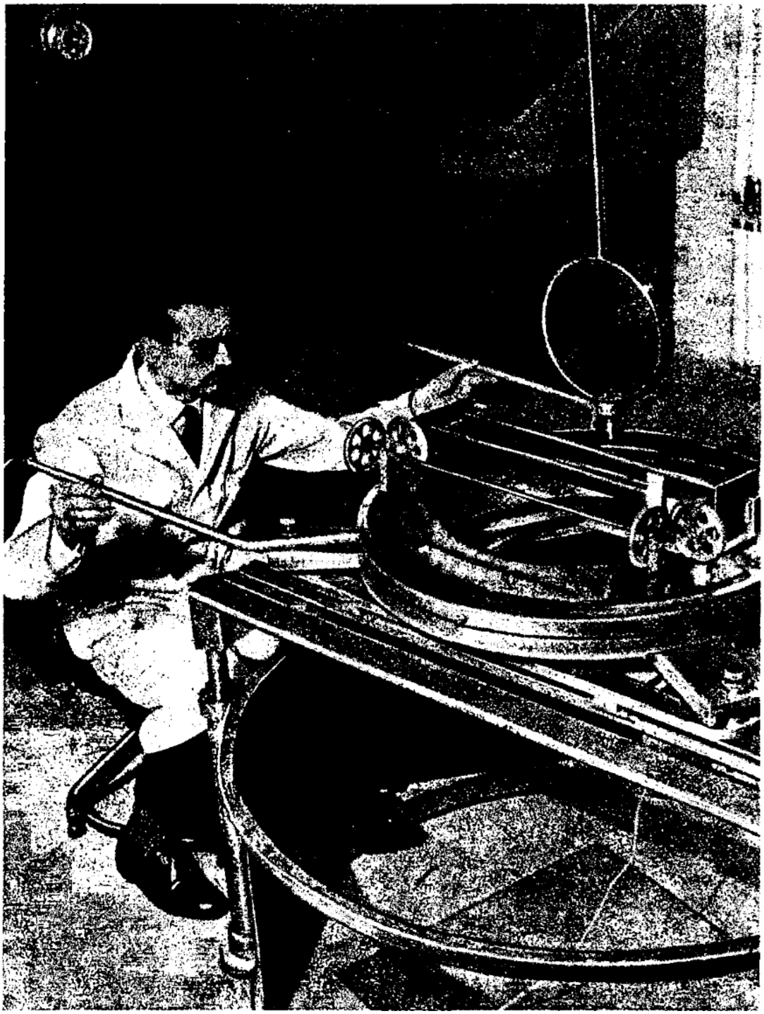


Photo of my lab manager Jacques
Bourgeot

Georges Lacoste, *progress in our ph ysiq nal knowledge of the fields orurend the drowning à de passionnonles 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 pen- dule 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 la- boratory 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 dis-
105 cm [6].

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 nrec ti oscillation plane with *an accuracy of the order of a tenth of a grade.*

(4) The corresponding pendulum period T = 2 z lig 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 coe;f/icient *dt dissymtfrie b* -- 2(B-A) / (B+A) was thus 0.269.10 '2. The moment of inertia C of the pendulum about its vertical axis was 270.10 '. £r *coefficient* gyrostoticifd= 2C/(A+B) was y = 0.325.10 '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 in-
Yi£O0 Û CT0.

(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 [7].

To avoid any systematic influence, the ball supporting the pen- dule 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 /otbfe 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 w a s released in a different plane.

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

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

(9) This influence of the support has been precisely determined by lflooring experiments in different azimuths, eliminating the in8uence of time by a random choice o f 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 [10]

To my knowledge, *this is the only example in the literature of$_R$ observations of continuous moniëre 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 fCfiopitre iY, § B.2, below) continued for around a year, but averaged only around 15 observa - tions 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).

fl2) Some experiments were carried out in 1954 using a long pendulum suspended by a grñee 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 enchaînées - 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.*

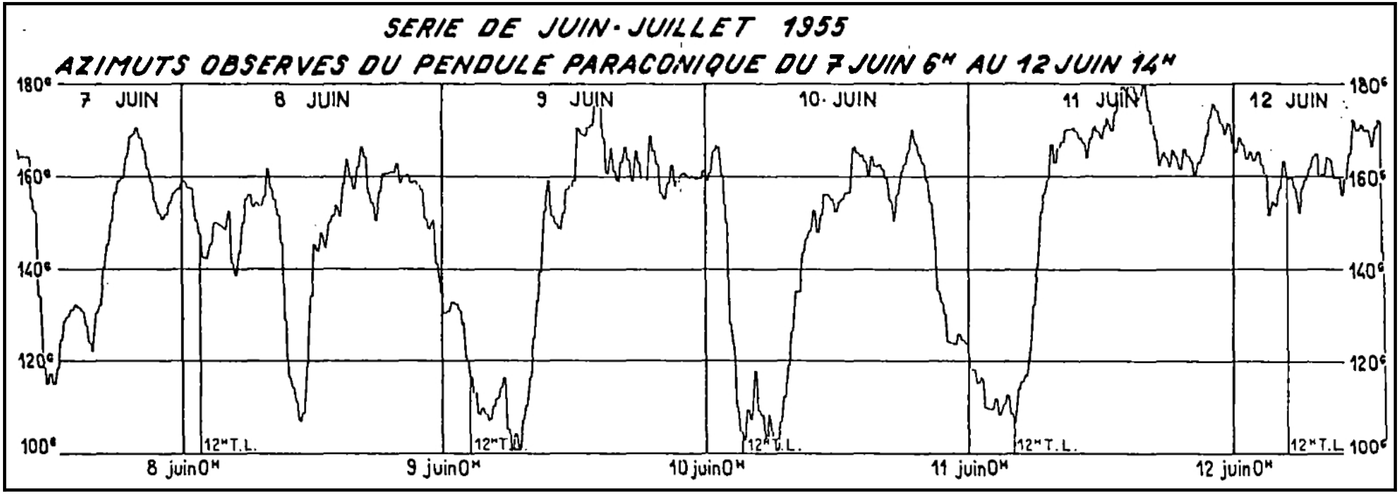
Graph J shows the chained azimuth observations of the June 7 12 h. to June 12 14 h. The times of the Moon's transits at midday are indicated by 12 o'clock TL [2]. *Graph II* represents the azimuth variations for the entire period from June 7, 12 p.m. to July 7, 12 p.m. . Similar azimuth variations were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

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

t2) The indication 12 h. TL means 12 hours in lunar time.

131All graphics drawn from 1954 to 1960 are reproduced by photography. *without any change.*

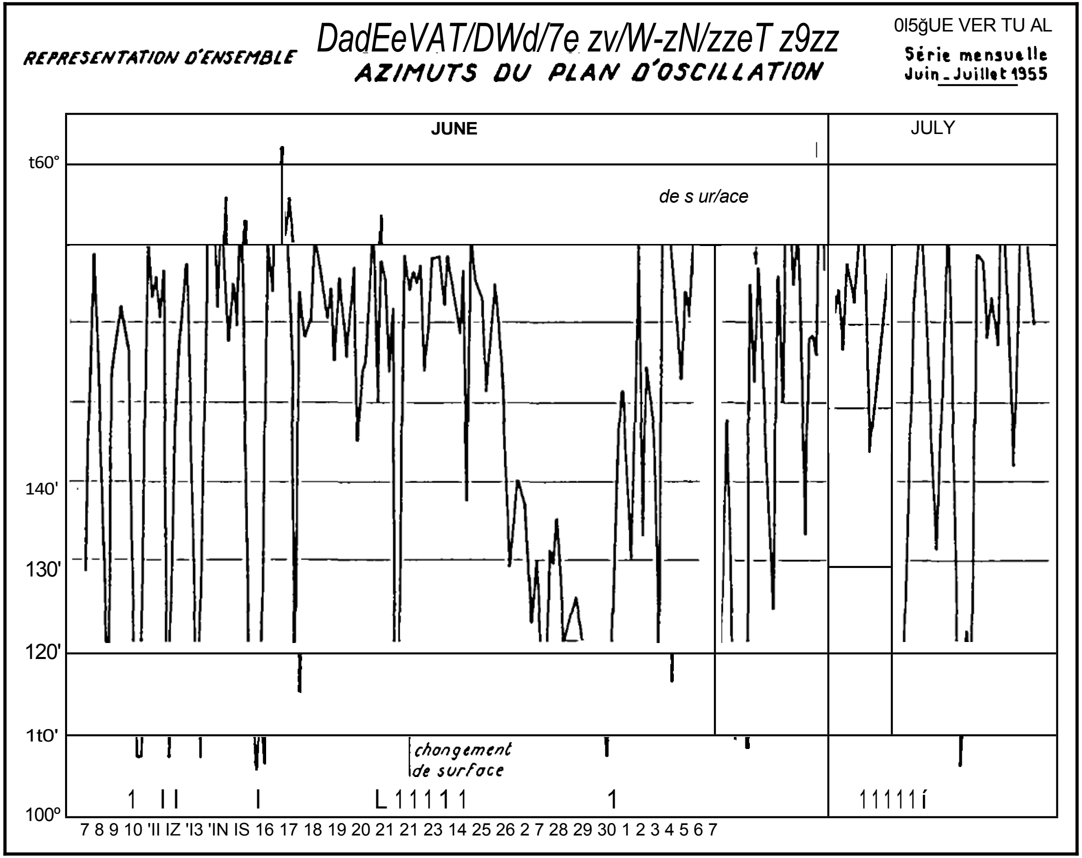
(4) For illustration, see GrepAi9ue XXff corresponding to the monthly series of chained observations from Bougival in July 1958 (§ C.2.4 below).



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.

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

HANGER



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.

S!ource: 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 were4 carried out.

The T'oöfeou *I* opposite shows for each monthly series the duration in days, the mean azimuth $, the minimum azimuth $", maximum azimuth

mum $_{JM}$, their mean ($", + $M) / 2 , the ratio (+ $_{QM}$) / 2 $, the total azimuth varia- tion D = $M - Qp , the amplitude $_{2R2}$ of the 24 h wave , the amplitude $_{2R25}$ of the 25 h wave , the ratio $@_5$ / @q , and the ratios / D and / D [1], These values are presented *in* both *grades and de-.* *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. th mn, 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).

19S5* except for the June-July 1954 series, where the pendulum consisted of one vertical and two horizontal bronze disks [5],

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 [6],

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 pen- dule constituted by the bronze disk [7].

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, **Ide juir** 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 Yofume 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 = Azimuth of support onisotropy -- *171.16 grodes -- 154.04 degrees*

Periods	Duration in days (1)	Average date (2)	$\overline{\phi}$ (3)	ϕ_m (4)	ϕ_M (5)	$\frac{\phi_m + \phi_M}{2}$ (6)	$\phi_m + \phi_M$ 2 (7)	D $= \phi_M - \phi_m$ (g)	2 (9)	2 (10)	'!2s' R2g (11)	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)	Sg (84)	253 1228)	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	Œ	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)	I25 (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* ^II^ *Jonaier 1954.*

4.- $p and sM denote the minimum and maximum values of the ostillation plane azimuth. $ represents the mean values of the azimuths §.

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* [1] *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 [2].

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 [3].

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 *.

(l) At Saint-Germain, the angular rotation of the Poucault effect - m sin L is
- 0.55.10 radian/sec., which in ï4 *minutes* corresponds to an angular displacement of

- 0.55. 1060 . 14 = - 2.94 grades in 14 minutes

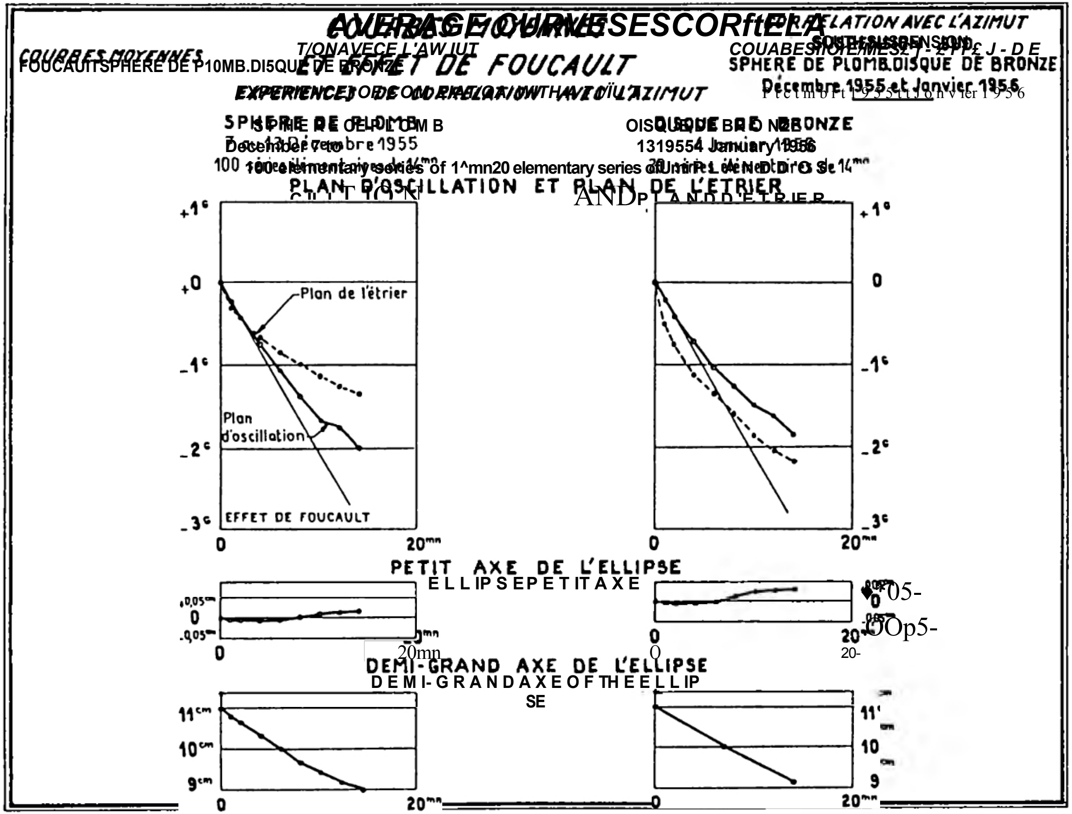
(2) Choosing the lead sphere corresponding to a symmetrical pendulum (B = A)**a** has the advantage of better demonstrating the movement of the inertial trihedron than in the case of the asymmetrical pendulum (Bw A) made up of a disk.

(3) *Trophies III and ïV* show that over the course of a 14-minute experiment, the amplitude decreases from 11 cm to around 9 cm, i.e., for 1' = 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 l'support aniso trope asymmetrical paraconic pendulum is presented in the Second Volume of this book, *Chapter I,* Section

B (see above, p. 28).

Graphique III

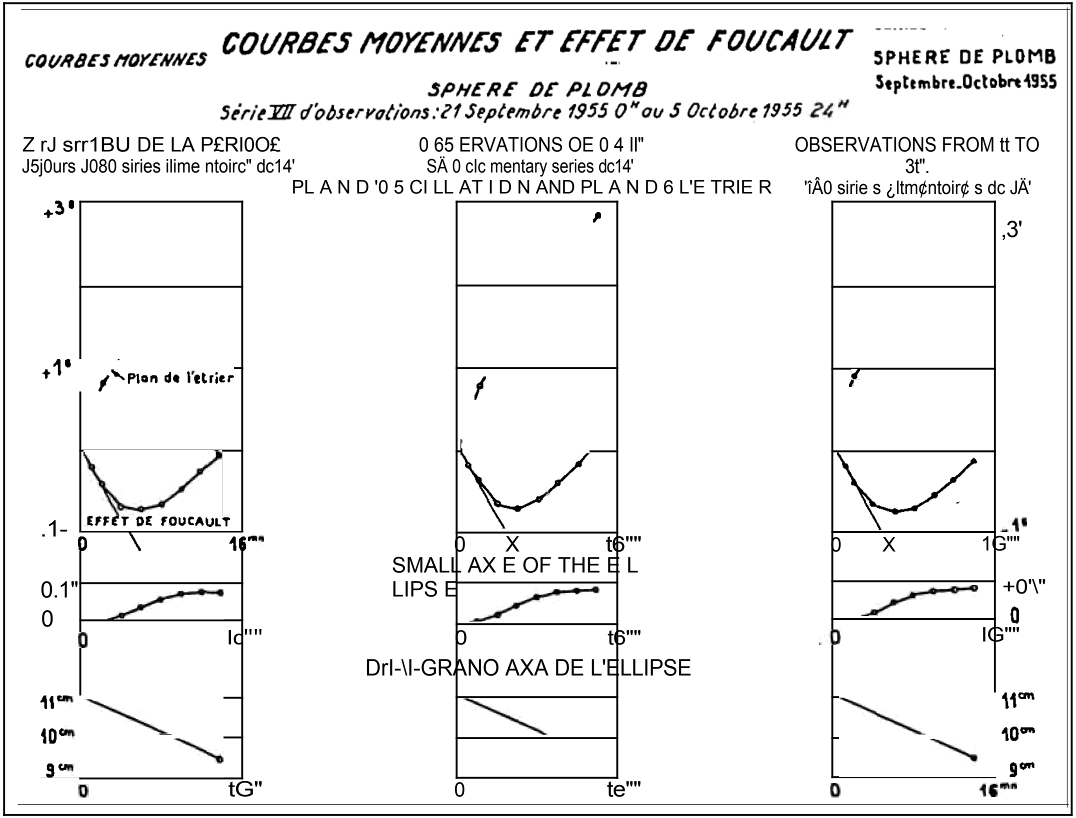


Légende : azimut du plan d'oscillation — azimut du trièdre principal d'inertie

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

Source : Graphique III A 2 de ma Conférence du 22 février 1958.

Source: Graph I I IA 2 **from my Conference of February** 22, **1958.**



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

Source: Graph III ñ 1 of my Conf4rente of February 22, 1958.

S.- Zo afructurepériödi9ue fierre '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 2

(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¡ 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 :	xj	"2	x_q

The trend may be eliminated by considering a (q + 1)*^-column whose mean is xqqj . The trend is defined by the ratio Cxqq - xq) / q .

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

the concordance of the phaaes for the original adrie and for the sum of its periodic compoBents for each of the two seriesa of 15 jourB into which a series of 30 can be decomposed
jours [3] ;

The quality of the fits is characterized by a low dispersion of the pointsa around thefitting sinusoida.

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 em:holmes obsen'ations of June-July J955

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 [5] *('Pobleau 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 *(Z'obleou ff).*

The Ki (T = **23.93** h.) and Mi (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.

1 - **1** – 1

24.8412 h = 24 h.50mn 28" (24.8d12 / 2d = 1.03505)

oiï 29.5S05 days represents the Moon's synodic period.

(5) The anal **ilyse de cette ser** d and atmospheric pressure analyses were carried out by the *Sltraice* ***Hydrographique*** *Nat ional dt Ports* and by 1 ***Institut*** *Hydrograph iqut dt Hamburg.*

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 compoaantea [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] The amp1itude of this pćriodic component is 14 grades.

Orders of magnitude

3 - The 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 0.37.10 * rad./sec., i.e. approximately one-fifteenth of the Foucault effect equal to **0.55 10-°** 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'oôîeou 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 ca1cuI shows that if we analyze a wave with a period T = 24 h. 50 min. with At a period T2 = 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, ———— d6= 0.367 10 rad/sec. 0.55 10 / 0.367 10 -5q 15.0

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

ϕ' = 10.46 0.3fi7 10 = 0.186 10 rad/sec.

which corresponds to approximately one thirtieth of the eddy current effect (0.186 10 / 0.55 10 = 1/ 29.6). t9) 67.04 /10.46 = 6.41 .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 *poraconique'.*

Tableau II

PARACONIC PENDULUM AZIMUTHS AND ATMOSPHERIC PRESSURE

Annual series *June-July* ***1955***

Ajuotemento à 13 pêrîodez de lo théorie dee morêeo

iservice Hydrographique de Poris el Institut Hydrographique de Hambourg

		2R wave diameter													
SeMBg	kB¢iB tl0EI total observed	I¢t 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	î\/Lt 6* 21	**' 6* 10	TOTAL
Azimuths 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 va	riation	total	observe	2R/D			**TOTAL B**
Azimuts		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

I.A.5

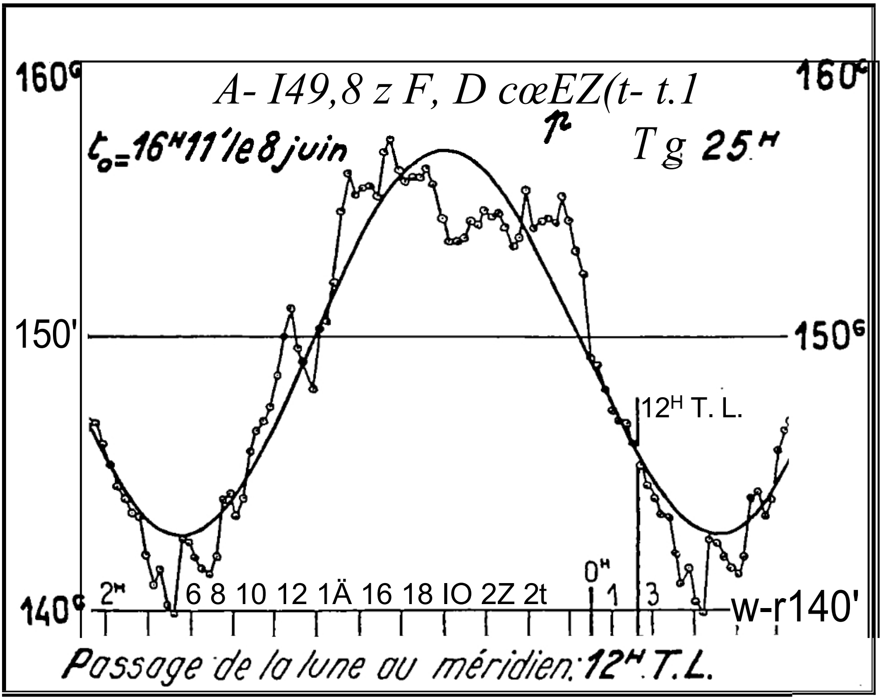
Tourr*:es: Noie* du 25 novembre 1957 a l'Atadémie des Sciences, *Anolyze harmonique des mouvements du pendule poroconique,* and *Tableau III A* de ma Conférence du 22 février 1958.

AZIMUTH OF THE PARACONIC PENDULUM

Monthly serie of jciin-JtiiIfeP *1955*

JJtistemeiit by fr Buys-Belloc method at a

fine orb of *25h.*



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

An almost periodic structure

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

They feature numerousø aymmetries or double symmetriesø with respect to certainø datesø, numerousuaea øimilitudes by translation, and local periodicit4s *1.

Ajustements par les moindres carrts

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 1°.

(10) Une fonction preв9ue pJriodíque est une вomme de composantes sinusoïdales de përiodes incommensurables (Voir *l'Appendice* £ de la *DeuxiAme* Porłie de cet outrage, Allais 1983, "Préquence, ProßoßititJ, el *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. *Appendite II, §* P.9, *R4gułoritts łocalec des fonttionc prepue ptriodiquec.*

(12) I think it's worth recalling here very briefly the principle o f these cal- culs in the case of a simple corrëlation.

Consider two functions zfx) and y(x) where zfx) is assumed to depend linearly on y(x) in the first approximation, and for which we have n pairs of observations (zø, y ø), functions of xø . The eвtimation of the correlation between z and y re - comes I to determine the function

II) z-fx)= a yIx) + b

where a and b are constants, so we have

(2) Úi0=z*)+tx

and such that for the n pairs of observations available, the sum Z E* of the squares of the

* °'d*® *n ß0İt minimum.

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

$$(2) \qquad 1 - R^2 = \sigma^2 / \Sigma^2$$

where Z and o respectively represent the standard deviations of zfx) and Б(x).

In principle, for all correlations in this volume, I indicate thea values of R, **Σ, et σ.**

On Yes calcul de corrélation e eet leur signification see Harald Cramer's remarkable ñfotAemoticat *Wethodc of* Stofisficø, Princeton University Preвв, 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 relativity4 , 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 phe-nomene?*

A la premińre question fondamenŁale l'expćrience permet de donner une r6ponse *totalement affirmative pour trois raisons tout ó foit eo-sentielles :* the observation of triply enchain4ea sćries, the compar6e structure of the sćriea observed and reconstitućes ń from their pćriodic compo- santea d6terminóes by harmonic analysis, and the application of the *G4n4rolization of the lschuster Test to the can of autotorrelte striae.*

Triply 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.

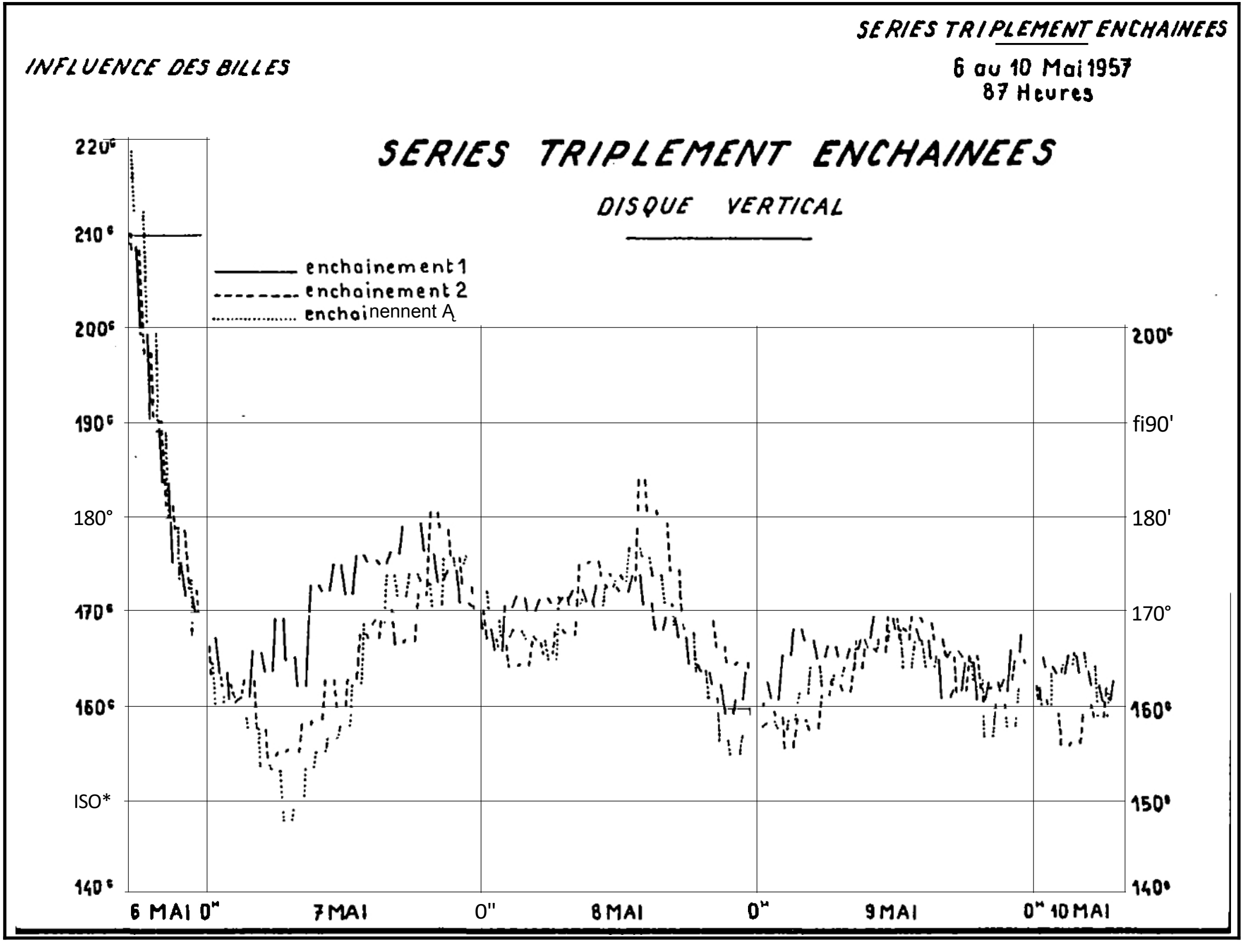
In fact, three series of triple-chained 14-minute experiments carried out from May 5 to 10, 1957, for 87 hoursa, showed that the oscillation plane movements in the three series were *similar (Graph VI).*

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 ä 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 2S novembre 1957 è l'Académie des Sciences, Anofyte *harmonique* der mouuementr *du pendule* pororoni9ue.



Source: *Cropńipuc* IV A 2 of my Conference of 22 fćwier 1958.

Hëries obsenite- et -êries 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 <i 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 o f 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) 7'oô/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* [5]

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.*

B y 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 series5!*

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 min. and + 2 h. 20 mn. respectively, the H - H' differences remain relatively small.
The 'ï "obleou ïïï 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 per-turbations 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 Yïï Gropfii9ue and the 24 h. wave, we have H - H' = 4 h. 54 mn. - 4 h. 48 mn. 6 min. and on the Yffï 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**a** obtained in an operation covering the whole month.

(8) We have fGrophi9ue D

19 h 59 mn - 16 h 36 mn = 3 h 23 mn

et (Graphiques IX)

19 h 59 mn - 16 h 36 mn = 3 h 23 mn
19 h 41 mn - 16 h 16 mn = 3 h 15 mn

(9) The preceding analysis, the 'Peblenu ffJ, and the C-rephi9ues YES, YïfJ, fX, et X have öt4 preaent4a dana ma ***Nett*** unpublished, but very widely circulated, of November 20, **1958**: *"Slur l'exietenct d'une composante ptriodiq 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*

ooz-nespom xazat ù f'azzof de's aérîes

obæruées et calculées jzar fa mé7fzode cie Zïuy-Zïaffot

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""	- Œ 01
1 + 2 3 + 4	- i* o2mn - 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	$\overline{H - H'}$		$\overline{\mid H - H' \mid}$	
month fortnight week	3 = - 15 - 15--'		15mn 37mn 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 l + 2 + 3 + 4 the whole moia.

For example, for the first fortnight of June-July 1955 and for the 25 h fGrnpùi9ues fH period: 16 h 36 mn - 16 h 16 mn = 20 mn.

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

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

Source :

Ma *Nale* ü *au* 20 novembre 1958, Jur *l'*existezrce *d'une composante pério-digue voisine de 24 h. 50 mn. in the movements of the* anisotropically supported *poroconigue pendulum.*

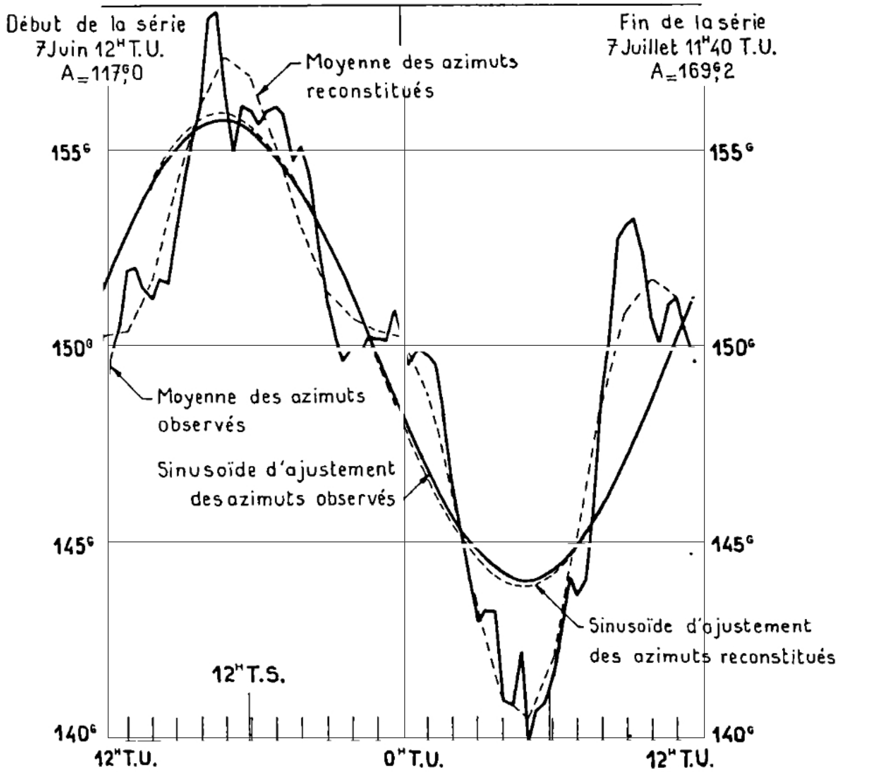
ANALYSE HARMONIQUE

AZ I P1UT 5
June. Jui lle t t9 SS
2t,3 t 4
Strie rit lle et re canot ituec

xwxzrze wxemowzeae

pu zx szp/r œgz "ozw. >v/zzr rsss

FILTRE DE BUY5.BEL LOT ÄÊ Ä I Û Ö// _*Serie complète (t 2t3+4) rit elle et rtconstituÉe



Sinusoïde d'ajustement

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

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

θ' = heure T.U. du sommet

	q	$\bar{y}$	R	*θ*	*θ'*
Azimuts observés	30	149,96	5,832	4ᴴ54	16ᴴ54
Azimuts reconstitués	30	149,93	5,957	4ᴴ48	16ᴴ48

q = nombre de cycles

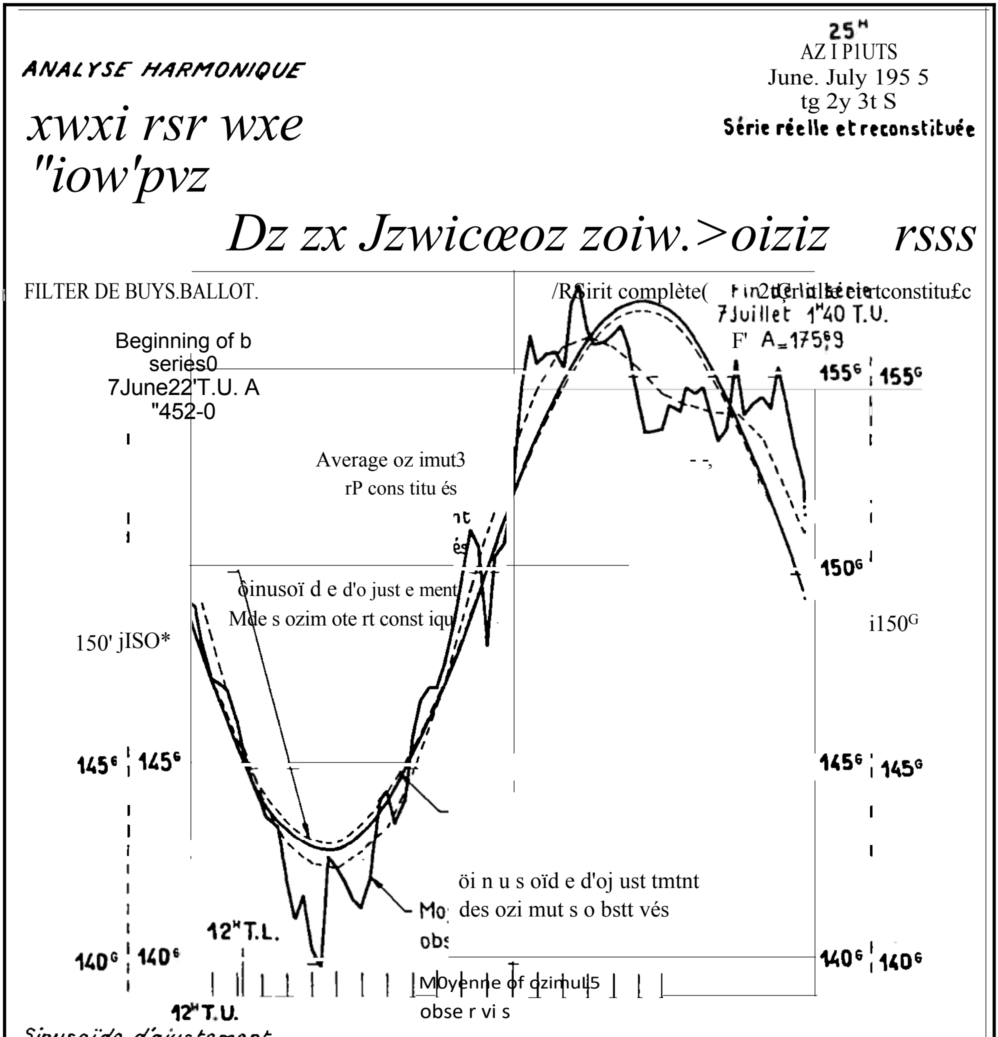
N.B.: 1) Les azimuts reconstitués sont ceux correspondant à la somme de 13 ondes élementaires des marées déterminées par la méthode des moindres carrés.

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

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

Source - My note of November 20, 1958 (see *Table III)*

GrnpAique MZf



0 "T. U.du Z2June 12 "T.U

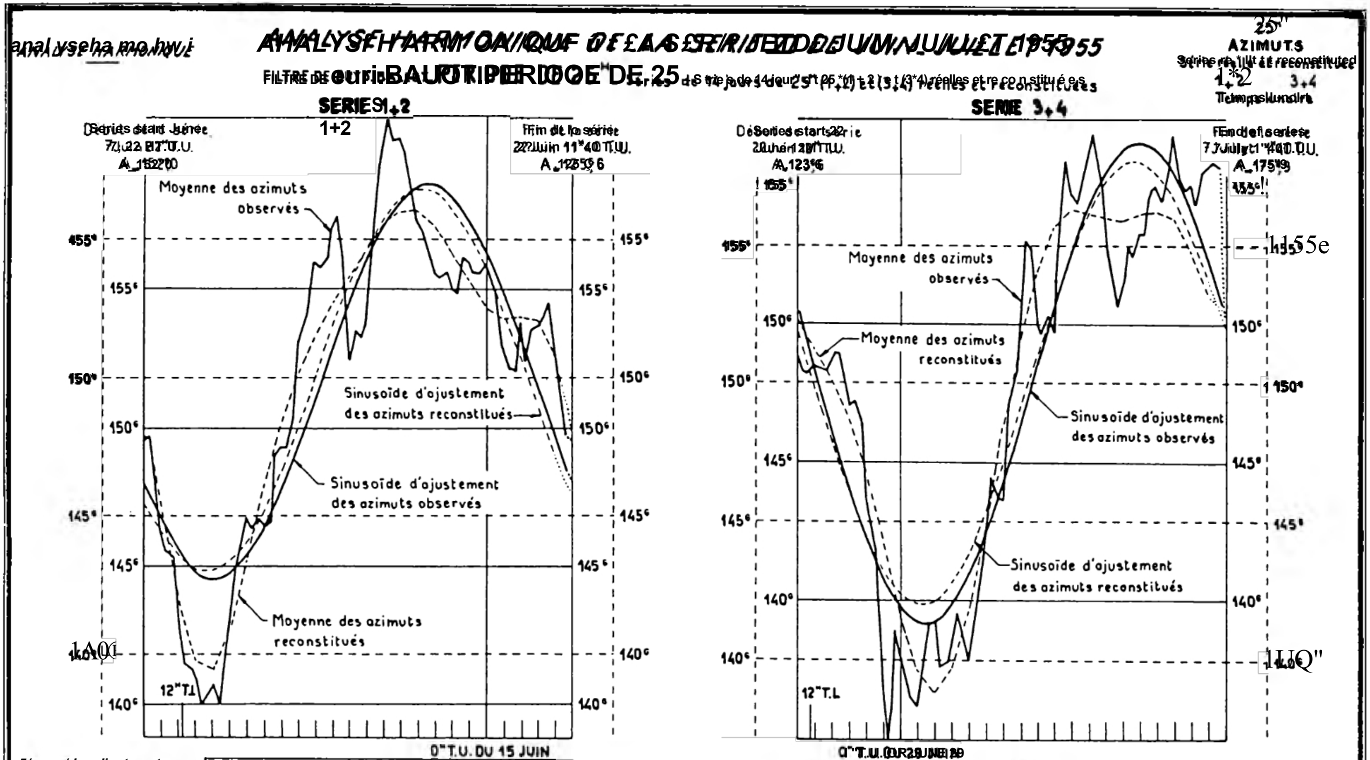
	q	ÿ	R	0	6'
Azimuts obse r vis	28	149*80	7*00?	18"11	1s "58
Azimut s re t onstitvi s	2 8	14 9*92	6'766	18"12	15"59

T_ Z § 6_ time of t,ommeÊ p0r '^ PP r! or st rie goal

N.B. : 1) Les azimuts reconstitués sont ceux correspondant à la somme de 13 ondes élementaires 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 (12ᴴT.L. correspond au passage de la lune au méridien)

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



Sinusoïde d'ajustement

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

T = 25

θ = heure du sommet par rapport au début de la série

θ′ = heure du sommet par rapport à l'heure du passage de la lune au méridien du jour moyen (considéré en temps moyen)

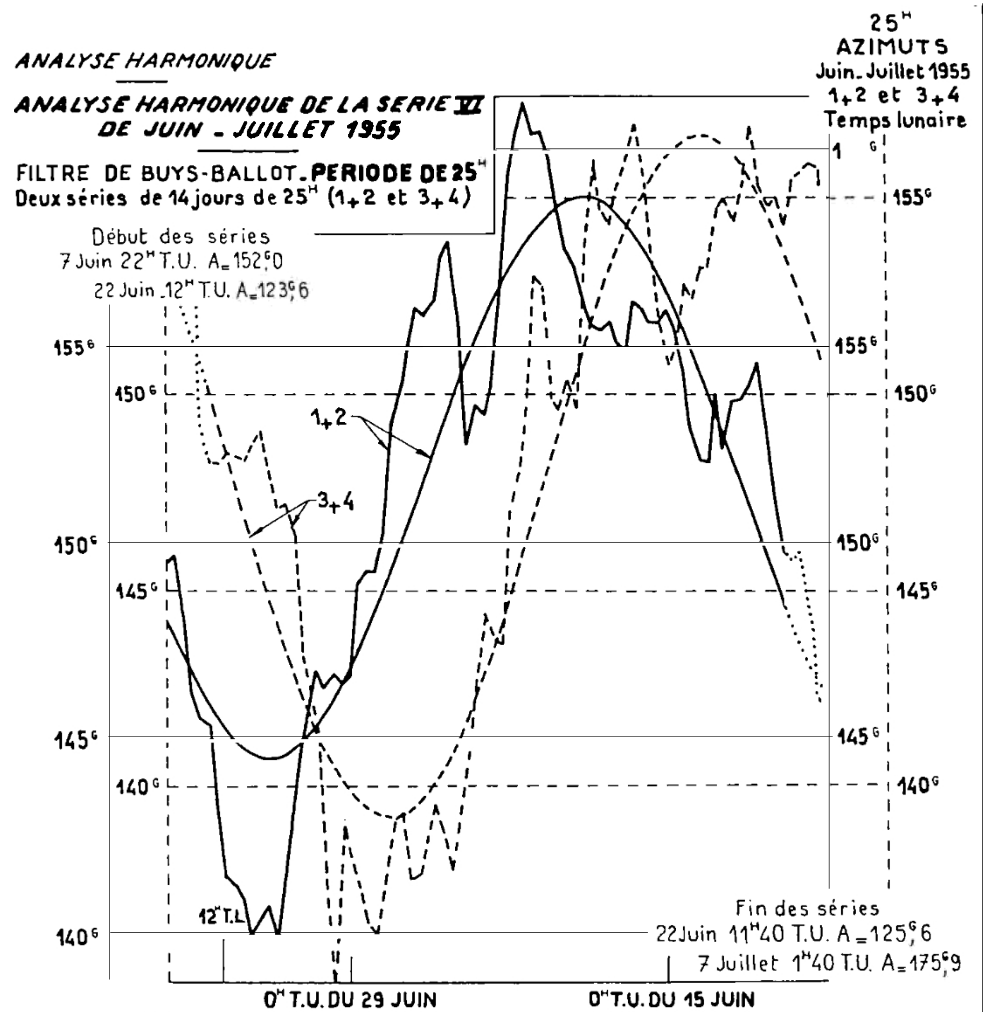
q = nombre de cycles

Période 1+2	q	$\bar{y}$	R	θ	θ′
Azimuts observés	14	151°71	7,171	16ʰ36	14ʰ25
Azimuts reconstitués	14	149°65	6,887	16ʰ16	14ʰ05

Période 3+4	q	$\bar{y}$	R	θ	θ′
Azimuts observés	14	147°89	8,691	19ʰ59	5ʰ42
Azimuts reconstitués	14	149°99	8,033	19ʰ41	5 57

N.B : 1) Les azimuts reconstitués sont ceux correspondant à la somme de 13 ondes élémentaires déterminées par la méthode des moindres carrés.
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). 12ʰT.L. correspond au passage de la lune au méridien).

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



Sinusoïde d'ajustement

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

Période		–	R	θ	θ'
1 + 2	14	151,71	7,171	16H36	14H25
3 + 4	14	147,89	8,693	19H59	-5H42

T = 25 θ = heure du sommet par rapport au début de la série

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

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). (12H T.L. correspond au passage de la lune au méridien)

S'iiirce. Ma Note du 20 novembre 1!l58 l*o'r *Tahleou III)*

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

3- Any discrete series of N = 2n + 1 numbers x_i 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 ampli- tude is sufficiently large.

In the case where the x_i are *independent,* Schuster established a test of significance that has become classic, but *this test is inapplicable in the case where the x_i 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 [10].

• Suppose, for example, that we consider a series of N observa- tions xi and that the application of the Buys-Ballot filter for a period T determines a sinusoid

(1) X = R sin to (t - @) o=2x/T

and let Pp be the probability that R has a value greater than or equal to ä

(10) See my *Notes* of May 13 and December 23, 1957 to the Académie des Sciences *Tesi de ptriodicitt. Tdntrolimtion du tect de Slchucter on c'is de striec temporelles 'iu - foeorréléez"* and *"Application du test de lschnever généralisé ä l'onolyse hormon 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 paraconic pendulum obser- nations in 1958 (see Section C below) in Tt\8 Communication: *"Test de ptriodicift. Tëntrolicofion du Ceci de Slchucier au cas dt* eë - *riec htm mettes aufocorrèlttc dont* f'ùypotùdse *d'un* prœessus *de* perturbations otJo-***toires*** *d'*un *eycfème cf'ible"* (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 Rp, *assuming that the series under consideration is devoid of any ptriodicitt.* The formulation I've determined makes it easy to calculate the probability Pp [11].

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 [12], For this value we find P = 0.48.10-5 [13], *This means that in the !XR !hëse 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 9-. We can conclude that the *existence of the 25-hour wave is virtually certain.*

(11) We have

(1) Pp = Prob2 ji) = e'>

(2) $\mu_\omega = \frac{I_\omega}{E[I_\omega]} = \frac{N I_\omega}{4 \sigma^2 k_\omega}$. ö' . (i- $\frac{1}{N}$) s^2 o=2wp

(3) $I_\omega = A_\omega^2 + B_\omega^2 = R_\omega^2$ $A_\omega = \frac{2}{N} \sum_{s=0}^{N-1}$ ¢* cos a mBy= $\frac{2}{N} \sum_{s=0}^{N-1} x_{s+1} \sin s\omega$

(4) $= 1 + 2 \sum_{v=1}^{v=h} (1 - \frac{v}{N})\, r_v \cos v\omega$

p is the period. Frequency m is equal to 8 N/p. s° is the variance ofa xi and h is the value of v above which the autocorrelation coefficient rp no longer significantly differs from zero.

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

(13) My note of December 23 1957 to the Acaddmie 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, o = 20.24 grades ; and for T = 25 h. we have p = 25 . 3 = 75, ktt = 5.292. We have = 7.01 grades *(ffiraph* Y in § A.5.2 above). We then have lyg = 7.01' , eC

jay = (2161 . 7,01*)/ (4.20,24' . 5,292) = 12, 25Pp = e" = 0,481.10

• A titre d'il1ustration le *Graphique* Vł repr6sente le frćquenci-gramme de la sńrie de *novembre-dtcembre 1954* de 721 valeurs horaires
nVic ile lines d'6gale 8ignification sŁati8tique 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 Gropfii9ue *VI C* J of my Conference of November 7, 1959, *"Fond -il reconsidérer les lois* de le *grauiiotion?* Nouveetts *rćsuftots, Bilon et* Perspectiues

This frequencigram can be usefully compared with the frequencigram of the July 1958 monthly series from Bougival fGropfiique OVf 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.Œ 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 voisinee annaraissent dans 1es analyses de la thćorie des marćes fsch ureman, 1941, ***Manual of Harmonic*** *Anol ysis and Prediction* o/ fides, p. 164-165).

To cimplify, 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 icufić rement on the 24 h 50 mn pćriode.

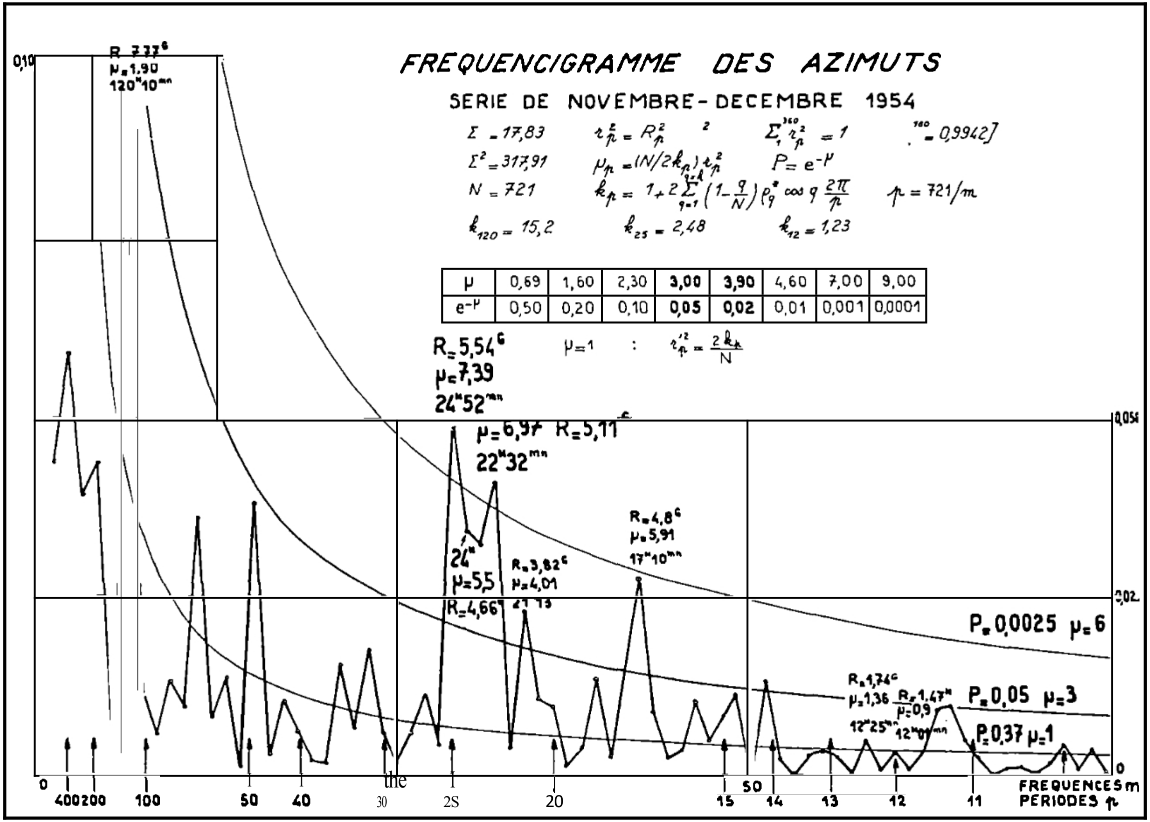
(15) The frequencigram considered corresponds to N = 721 hourly values. For I7iarmonic corresponding toC 6 m = 29 we have the period p = 721/29 = 24.86 h = 24 h 52 mn, and an amplitude 2R = 11.08 grades.

For this period we have *(Graphipue XI): ç* -- 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 = e€ = 0.4%.

The choice of the value N = 721 hourly values corresponds to the condition that e1le allows to repair the amplitudes of the two periods o f 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 Acaddmie des Sciences. See also my memoir **of 1961,** 'i "esf dr pJriodicitJ. *Wri Jrolixat tori du Tesi de* Sch *u f:er ou ca" de "triee temporellez auf*x-orrJlJe*" dans l'hypoihè "e* ***d'un processus de perturbations aléatoires d'un système stable.***

Source: Craphiqne II C 1 de *ma* Conférence du '/ novembre 19S9

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 aerials, and that corresponding to t h e application of the *Generalization of the lschuster test,* we can conclude *with complete certainty that the diurnal lunisolar effects observed, and especially the lunar përiodicilë 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 čunźæzfażreø oôøœués et fa** čhëor-še **oczueffe** zfe føgrтzø'zYofźon

In view of the observed amplitudes of lunisolar effects in the case of the aniøotropic support, it is easy to verify that the effects observed are *totally inexplicable* within the framework of current theories of gravitation, whether Newtonian or relativistic4.

Observed and calculated values of lunar influence on the azimuth of the paroconic 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 azİmut văriatioTl of less than 10-13 rădiăfl per Second 1.

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 0.19.10-* radians per second [2]. *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 XlXth 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* Vïï, relation (8), p. 129 above.

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

(3' o,iø6.io*/ io-'- = i8,s io-

What's more, the pendulum's oacillation 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 aur the four Toòfenux 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 theo- rique analysis of pendulum motion under the action of terrestrial rotation*, as presented in all the Treatises.*

(4) This is the gradq U¡ - fat Ui 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 cnlcute in first *opproximalion the effelc of the* tunisotory *ottraction zur mouue-* ment *du* pendule pernconíque, 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 - pg Q2 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 fraetion of approximately 1.5 10'* of gravity.

For a complete calculation, see Allais, 1956, TA4orie du *Pendule* poroconique, Partie VA, *Thtorie dec* mouuements reteti/s (p. V2-V28) et ifouuement *du* Pendule coni9ue *nous* l'in/fuence de to *rolotion terrestre* fp. Y29-Y46). VOuré gelement AJlais, 13 mars 1958, *Applicotion du TMorème de Bour ou* ces des mouuemeztts 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 §' of the azimuth of the plane o f oscillation can be written as

(1) §'= - tu sin L + (3/8) p e bp = gd/ 1

ni is the Earth's rotation speed, L is the latitude of the location, and o and 9 are the major and minor axes in radians of the elliptical path described by the pendulum. The second component of §' 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 t o axes linked to the Earth under the action of gravity, terrestrial rotation and the attraction of stars according to t h e current theory of gravitation.

4- Toöfeeu Y shows how t h e acceleration of the pendulum's center of gravity relative to axes S x y z lids ä **the Earth,** where S corresponds to the pendulum's suspension point, is determined **from** the fundamental relation

(2) F=My

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 t h e acceleration expression

absolute 7 of the gravity center G as a function of the acceleration of G relative to the Earth, t h e driving acceleration, and the Coriolis acceleration.

An *essential* fact is that we have

(3) grady U; - gradT U, = (grads U; - grady U;) + (grady U; - gras U;)

(7) The ellipse is described in the direct or retrograde direction, depending on whether 9 is positive or negative.

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

The first term, which corresponds to the dtuiation of the vertical due to the ostre 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 oction of the star i at point G and at point lS, the pendulum's support point. This diffćrence is naturally extremely peŁite *.

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

5- *Table M* shows how the second members of the differential equations in m = x /1 and n = y /1, which depend on t h e action of the other i, are determined. The influence of the other i on the
The motion of the paraconic pendulum is determined by the relationships [10]

(4) m'' + (g /1) mK¡ (m cos 2A; + n sin 2A;)

(5) n'' + (g / 1) n = K, (m sin 2A¡ - n cos 2A¡)

(6) K, = 3 C , ain° z¡ $\frac{M_i}{M_T}\frac{r_T^2}{d_i^3}g$

where z, and A¡ represent the zenith distance and azimuth of the other i, M, and M_T the masses of the other i and the Earth, r_T the radius of the Earth, and d¡ the distance of star i from the center of the Earth.

(9i 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 cons- tant throughout the eapaee swept6 by the pendulum. ile support musi que Ie de xićme term *of the second member of the* reiofion***(3) ci-dessus** (p. **120)** **est nul.***

k0iT' Allaia, aeptembre 1956, 'I ''hdorie ou *Pendule Paraconique,* Cinquibme Partie, Influenee de la rotation terrestre, p. V.1 - V.28. See also Allais, 13 mare 1958, pptieotion *du* 7'hdorćme *de Bour au can* des mouuements terrestrer *danc* Ie *can* Ie *pluc général,* 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 eonsidćre.

On relative motion, see E. Bour, ñfdmoire sur Ier mouvements reIoti;fs, Journal de Mathématiques pures et appliquées, Tome VIII, 1863, pp. 1-51. See also Gilbert, 3fdmoire *sur l'application de la méthode* de Regroupe d *diuera* proöfémez *du mouvement relatif,* Gauthiers-Villars, Paris 1889, 197 p. Gilbert relies on Bour's 1863 dissertation.

(10) These relationships ae demonstrated from the serial development o f expres- sion (2) of 7'o6feou YT. The second members of relations (4) and (5) above are consi- dćrćs a s perturbations.

Based on relations (4) and (5), *the narration method used de-constants* allows us to deduce that in the *first aoR *i* *ation*, the influence of the other i can be represented by the two relations [11]

(7) §' = - tu ain L + 3/8 p ce Q

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

Naturally, if there are several stars, there are [12].

(11) In fact, il is easy to show that for the values of 9 observed during each 14-minute ezpërience, i.e. 8d0", (1 §1 < 0.001) the *direct* effect (relation 6 of 7'nhteou YC below).

(;) $\phi' = \frac{1}{-}\frac{\alpha\beta}{\alpha^2 - \beta^2}$ Xj cos 2 (A{ - $)

is much smaller than the *indirtcl* effect corresponding to h the relationship (relationship 7 of the
Tobleou M below).

(2) $' = $\frac{\alpha}{2p}$ K_i sin 2 (Aj - $)

In fact, the order of magnitude e2 of the direct effect is (relation 6 in *Table* Yi below)
deasousi

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

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

(4) $e_2 = (\frac{3}{8} p\alpha) (\frac{\alpha}{2p}$ "-At 2)

$= \frac{3}{16} \alpha^2 \frac{\Delta t}{2}$ ri

hence

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

i.e. for I $I < 0,iAï1 ,ix= 1/10, p = 3.44

f6) z3 ,44 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 "oöïeou X of the § E.3 ci-deasoua fp. 180) we can certainly take b (in cm) < 0.1 , hence for l' = 105 cm (§ A 1.2, note 6, above): § < 0.001.
On this question, see my *Noie b* l'Académie des Sciences of December 16, 1957, 2 "Adorie *du* pendule poroconique *tt* in/fuence ïuniaoïoire, note 3. In this Note I had indicated for the ratio e2 / and the Minimum Value of 130. £ new estimate *eat* better.

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

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

(10) $\beta' = [\frac{\alpha}{2p}$ em2(A,-g)+ 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) $K_s = \frac{3}{2}C_s \sin^2 *$, $C_s = \frac{'a}{M_T} \frac{'T}{d_s^3} g$

12) $K_l \; \frac{3}{2} C_l \sin^2 z_l$ $C_l = \frac{M_l}{M_T} \frac{'T}{d_l^3} g$

(13) C = 0.396.10 ' Cj = 0.862.10' "

Remarkably, C and Cj are *of the same order of magnitude.* Cj / 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) 2 R = 5.3 grades = 0.0833 radians

of the 24h 50mn wave, the average variation observed §' 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) Q} = 0.186.10' 5 **radians** per second.

(13) **The expressions for and Kj** are quite identical to those given in **note (3)** of my *2\fete to the* Académie des Sciences of December 16 1957 *ïThtorie du Ptndult Poroconi9 ne tt Influence* ***L.unisoloire,*** **note 3),** ***except*** **that** I *have* erroneously included **in the ez-** pre88ion8 of K" and Kj 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 1a 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 suivant la théorie actuelle de la gravitation se déduit des deux relations

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

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

$$(17) \qquad \Delta\beta = \beta'_t\, \Delta t = \beta(t)\, \frac{\Delta t}{2} < \frac{\alpha}{2p}\, K_l\, \frac{\Delta t}{2}$$

déduite de la relation (8) ci-dessus.

Pour une durée Δt = 14 minutes = 840 secondes on en déduit (*Tableau VII* relation 8)

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

d'où pour le rapport de la valeur observée à la valeur théorique la relation (relation 9 du *Tableau VII* ci-dessous)

$$(19) \qquad |\phi'_o / \phi'_t| > 18{,}3.10^{6}$$

Dans le cas considéré de la périodicité de 24 h. 50 mn. la valeur observée est donc au moins 18 millions de fois plus grande que la valeur calculée[14, 15].

-----

(14) Ce chiffre est plus faible que celui indiqué de 50 millions (10^{-5} / $2\,10^{-13}$) dans ma *Note du 16 décembre 1957 à l'Académie des Sciences, Théorie du Pendule Paraconique et Influence Lunisolaire* (§ 3), *mais il reste du même ordre de grandeur.*

La différence résulte essentiellement de la considération pour l'onde de 24 h. 50 mn. de l'amplitude 2R = 10,46 grades de juin-juillet 1955 dans ma Note du 16 décembre 1957 (*Tableau II* du § A.5.3 ci-dessus, p. 99) au lieu de l'amplitude *moyenne* de 5,3 grades 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 (*Smithsonian Physical Tables*, ninth revised edition, 1956, The Smithsonian Institution, pp. 729, 730, 731, 734) :

$$M_T = 5{,}975\ 10^{27} \qquad r_T = 6{,}3712\ 10^{8}$$

$$M_s = 1{,}987\ 10^{33} \qquad d_s = 149{,}5\ 10^{11}$$

$$M_l = 7{,}343\ 10^{25} \qquad d_l = 384{,}4\ 10^{8}$$

$$\mu = 6{,}670\ 10^{-8} \qquad g = \mu\, \frac{M_T}{r_T^2} = 981{,}8$$

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

$$C_l = \frac{M_l}{M_T}\, \frac{r_T^2}{d_l^3}\, g = 0{,}862\ 10^{-13}$$

C_s et C_l ont pour dimension (T^{-2}) (l'inverse du 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 e[fets observed,* and especially the amplitude 2 R constat4e, of the order of 5 grades, of the lunar periodicit4 of 24 h. 50 mn [16].

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 contested4 the impossibility of the paraconic pendulum's azimuth.
of such an explanation [17],

(16) The correction given by the theory of general relativity is either negligible.
The correction ã made to the Wtonian ne potential according to Schwarzschild's formulation corresponds to the coefficient

$$k = 1 - \frac{2pM_l}{c^2 d_l}$$

where i is the coefficient of universal gravitation, M} the mass of the Moon, c the speed of light, and dj the distance from the Moon to the Earth (Darmois, 2-o tùdorie einsleinnienne *de* íe ørevitotion. *Let uérifications* ezpdrimentofes, Hermann, 1932, p. 13). We have (for c = 3 10)

$$\frac{2pM_l}{c^2 d_l} = \frac{2.6.6710*.7.Æ\ 3\ 25\ 0}{9\ 10^{20}.\ 384{,}41\ 10^{8}} = 2{,}83\ 10^{-13}$$

(17) The above calculations are based on Lagrange's method of varyingв 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 9ß). See also and notably Tisaerand, Ï°FroitJ *de Wtconiq we* Céíeste, Tome 1, *Perturbations* det *plonëiez d'oprèz la mdl hode de la uorio- tion dev вонятonfes orbitraireв,* Gauthier-Villars, 1889, p. 173-1881.

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

cJîcesi9ue 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 6quivalent pendulum
m = x /1n = y / 1
p = 2a / T = gil l = SG
UT: Earth's gravitational potential N: wire tension
g = fat UT
M: mass of the pendulum

(1) F = M y

(2) F= M gra UT+N

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

hence (1)

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

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

(5) m "+ m=-2msinLn'

(6) n" + i n = 2 tu sin L m '

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

(7) 9' = - tu sin L (straight path) [1]

(8) $' = - m sin L + $\frac{3}{8}$ p o 9 (el1iptical trajectory)[2]

(lJ Paul Appell, 1953, Traite *de* 2'fëennique RoiionneJïe. Gauthier-VÎll&T8, Toire I I, p. 293- 296 , G. Bz-uliat and A. £'och, 1967, 3fdconîgue , Masæn, p.153-156.
The two equations (5) and (6J) result from the fact that, in the first *opprozimotion,* 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

Mi: masae of the other i
I: center of the other i
Ui: attraction potential of the other i
d_i = SI l = SG
$\vec{N}$ = *force* exerted by the support on the pendulum

T = Earth center
tt: universal gravitation cœ&cient
r_T = radius of the Earth

(1) F= My

(2) $\vec{F}$= Mgra UT "Mgra U, +N

(3) $\vec{"} \frac{d^2 \vec{SG}}{=2"}$ 2 m $\frac{d\vec{SG}}{a}$ + gra&p U;

hence

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

(5) $\frac{æ2}{dt^2} = -g$ - 2 m $\wedge \frac{d\vec{SG}}{dt}$ + (fat U; - gradj- U_i)

+ (fat U; - fat U; *) $(\vec{N}/M)$

(6) bold U; - bold U; = deviation from vertical
(This term is not involved in the movement of the pendulum)

(7) fat U; - grads U; = effective acceleration exerted on the center of gravity G of the pendulum

Orzfre &-grzuu2eur **'2e f"zee4fèrutiozt ongufoire e:œreée** pur f'oafre i car fe,penzfufe

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

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

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

Sun: C = 0.396.10' " Moon: Cj = 0.862.10 *

(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 ïre *due ö* ï''istre** i *ïTable V)*

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

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

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 ; /1 (relation 2), hence, as a first approximation

(3) $m'' + \frac{g}{l} m = - 2$ tu sin L n' + K, (m cos 2 A¡ + ri sin 2 A¡)

(4) $n'' + \frac{g}{l}$ n=2msnLm'+ (msin2 -ncos2)

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

z¡ , A, : zenith distance and azimuth of the other i C =
0.39B.10' = 0.862.10' '3

Given relationship (8) in *Table IV,* we derive from (3) and (4)

(6) $' = - m sin L + 8 p ri Q + $\frac{1}{p\,Q2.}\frac{\alpha\beta}{2}$ K¡ cos 2 (A; - §)

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

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) $' = - m sin L + 8 p ri g

(9) $\beta' = \frac{\alpha}{2p} K_i$ sin 2 (A, - 9)

Sources for T'nötenux Y et Yi : Allais, September 1966, T'fidorie *du* pendule pnroconique and, May 1996, *Sur les y riodicitéc lunicolairec* dti pendule *paraconique.*

The K¡ terms of relations (6) and (7) are deduced from the K¡ terms of relations f3) and f4) from 2'ntifenu *II* of Anneze *I* of my *Théorie du Pendule Paraconiqut* 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 h = 24h 50mn

According to *Table* ï (§A.3) for the period T = 25 h. representative of the period T = 2dh 50mn the mean amplitude is

2 R = 5.3 gradea = 5.3 z / 200 = 0.0833 radians

The corresponding average variation observed over a half-period 24.84 / 2 =12.42 h is as follows

(1) $§ = 0.0833/ 12.42.3ŒD = 0.186.10 ^{5} radiana per second

We have *(7'obfeou M,* relations 8 and 9) [1]

(p = gd/ l = 3.4d)

(2) 9' = - m sin L + $\frac{3}{8}$ β

(3) Q' $_{=KG}$ sin 2 ($_{AU}$ - §)

After an experiment lasting Et = 14 minutes = 8d0 seconds, we have [2]

(4) $§ - \frac{\alpha}{2p} K_1 \Delta t \overline{\sin 2 (A_1 - \phi)}$

so that for the mean value 9 of § (ñ = mean value of ri) we have

(5) $|\beta| \sim \frac{\alpha}{2p} K_1 \frac{At}{2}$ i $\overline{\sin 2 ((@ - 9) 1}$ $< \frac{\alpha}{2p} K_1 \frac{At}{2}$

For the theoretical mean value J § { | of t h e Moon's influence, we have

(6) |$', $< (\frac{3}{8} p \bar{\alpha} (\frac{\alpha}{2p} K_1 \frac{\Delta t}{2}) = \frac{3}{16} \alpha^2 \frac{At}{2}$

BOlt QOlir ii - U10

(' 7}${ < 0.788 XjXj = $\frac{3}{2}$ sin^2 δ$_1$ C$_1$ Cj = 0 ,8fi2.10' "

or

(8) §{ < 0.788 $\frac{3}{2}$ Cj = 1.18 0.862 10' $^{1'}$ = 1.018 10' $^{'3}$

Value oôaerc'ée / **Value** *tlafioz-fique* *

(9) $\frac{\phi'_o}{\phi'_t} > \frac{0,186\ 10'^5}{1,018\ 10\text{-}1^*} = 18,3\ 10$

(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 h . The absolute value of h is designated by the notation l h l .

(3) Note that $_{rT}$ /l = 6.37 10' / 83 = 7.67 10' where T is the radius of the the Earth and l the length of the equivalent simple pendulum (§ A.1.1 above).

J.- Oee compoo nfe pdrźoóiques ron expficoÓJev por deap 4nomćnee **as**

If the observation s6riesa of the paraconic pendulum contain effective diurnal pńriodic componentsa of pćriodes of 24 h. and 24 h. 50 mn. *stotistiqiiernent significatives,* et si les effets póriodiques ainsi constatóa *ne peuvent elre consid4r4s* comme rdaultant de la thćorie actuelle de la gravitation, *est-ce que ces effets p4riodiques peuvent 4tre at- tribućc ó une* in/fuence *directe ou indirecte d'un phtnombne 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: 1º an at least approximate *concordance of phoses* between cause and effect; 2º 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* [1], on the one hand, and for the paraconic pendulum and *the magnetic declination, on the* other.
registered in ChamboT1-l£t-ForÔt [1].

Graphs XII show, firstly, that there are phaae differences between the adjustment sinuaoids for the 24- and 25-hour periods o f azimuth and temperature, and, secondly and more importantly, that *the 25-hour wave has a much greater relative amplitude pont* f'nzimut *of the paraconic pendulum than RouT 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'nf'feou VIII* shows that the s a m e is true for the November-December 1954 period [2]. Such phase concordances cannot be attributed to chance *.

In fact, and *for* atzcun of *the phtnombnes examined,* the lunar component of pćriode 25 h., reprdsenŁative of the wave of 24 h. 50 mu., has an amplitude comparable to that of the 24 h. wave. *. *This pt-structure riodique trbs particulibre des azimuts du pendule paraconique suffit pour tliminer comme e plication tous les* pfidnomónes *gtophysiques connus* [S]

(l) This is the closest observatory to the IRSID laboratory in Saint-Germain.

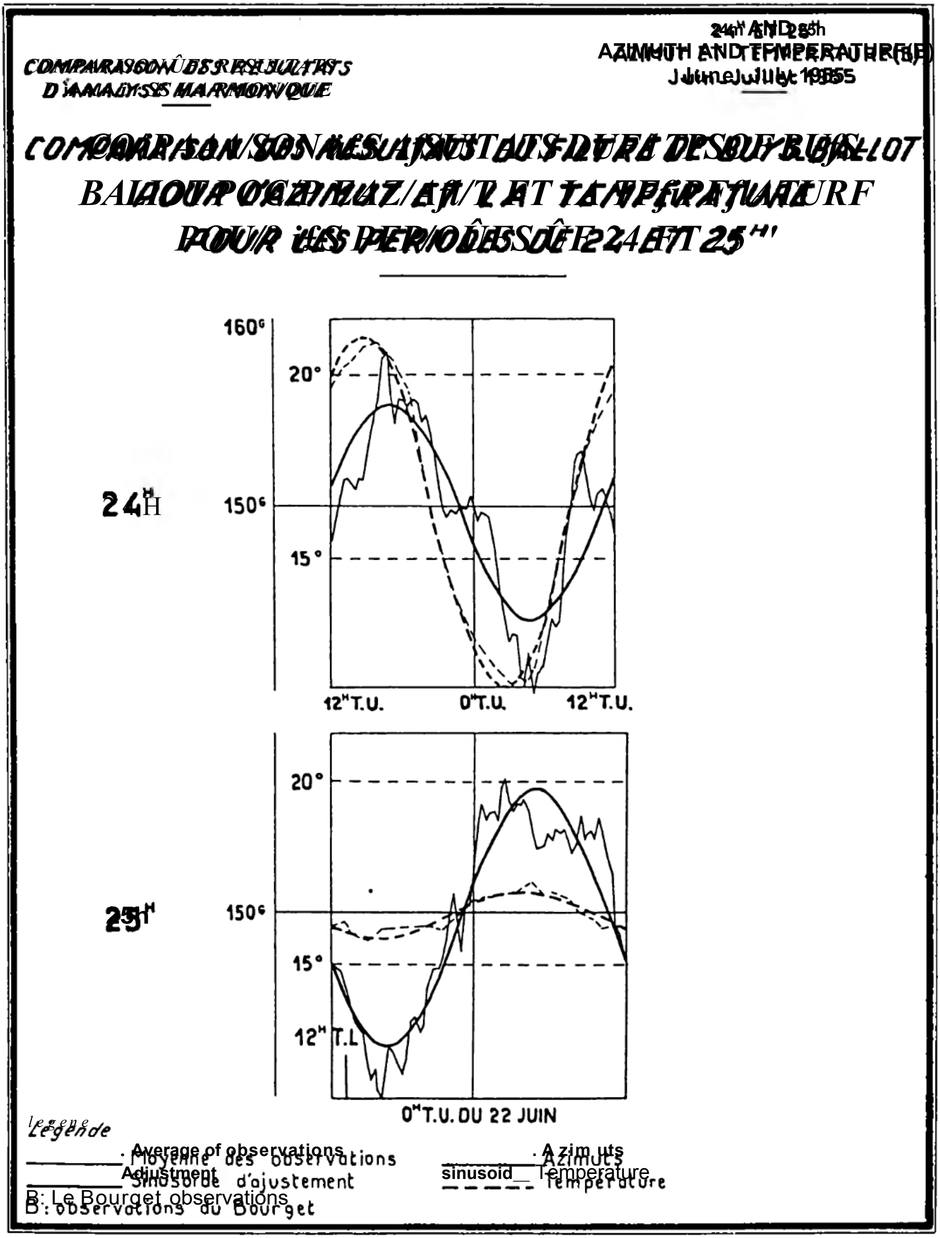
(2) Allais, 1958, Anomofier *du* mouvementmenf *du* pendule pnroconique, p. 31-32.

(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 con- cordanee would eorrespond ń a phćnombne rćel, as the dif- fńrent relative amplitudes of the waves eorrespond to azimuths and 8 the magnćtic dćclination 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* ï in § A.3.1. above. For the seven monthly series, the average R_{q5} / Rqq ratio is e q u a l t o 1.39.

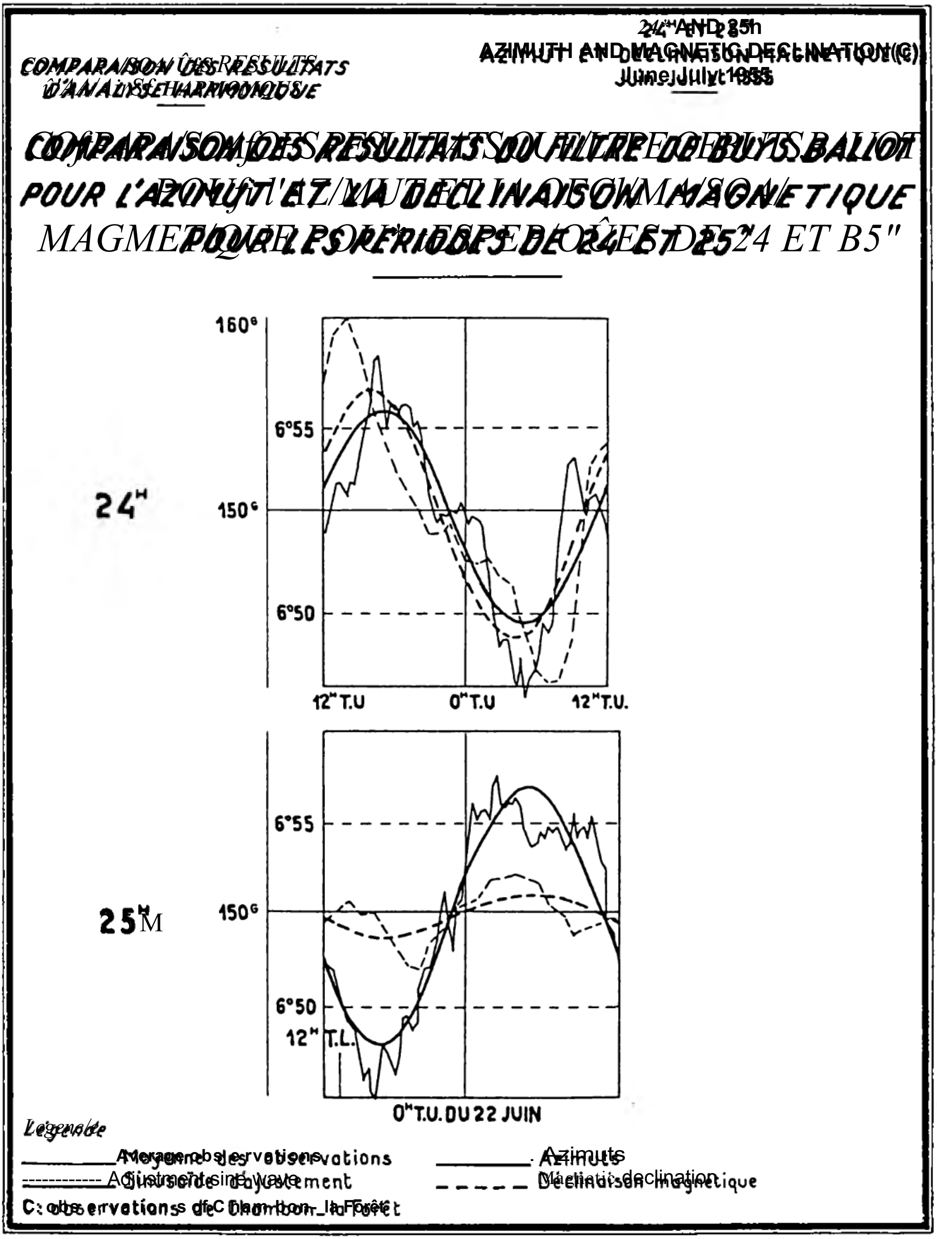
(DVoir ma Note du 16 décembre 1957 à l'Acadëmie des Sciences, "Pfidorie *du pendule* poroconi9ue *ci* in/tuence *lunizoloire"*.
Voir tout partieuli6rement mon mćmoire du 21 avril 19s8, nnomofies *du mouue - menu du pendule* peroconi9ue, p. 23-33.

Graphic XII



Source: Chart VA 3 a from my Conference of February 22, 1958.

Graphiques XIII



Source: Graphique VI 3 de ma Conférence du 22 février 1958.

PARACONIC PENDULUM AZIMUTHS AND MAGNETIC DECLINATION

OBæwafźons deJVouemóre-Zàécemóre J954 eč de ' uin-J'učffeč *19fi5 Pézfiodм::ítéø*

de M et 2'-fi łaeмz-ee

Periods		Azimuths in grades			Sexagesimal minutes declination			2 R'/D' / 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	lh 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	lh 41 mn
	June - July 1955	11,66	0,129	16h 54 mn	6.Œì	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 considered4.
H and H' represent the times of the wave peaks in lunar and solar time. See *Grophiquee ZIII*

Source: My memoir of April 21, 1958 (revised January 28, 1960), *Anomalies in the motion of the poroconic pendulum with* nnisotropic *support*, p . 32.

lunisolar variations of the vertical and the movement of the paraconic pendulum

2- Lunisolar variations in vertical 6 have often been put forward as a possible explanation for the consta- ted 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

$$(1) \qquad \vec{f}_i = \mathrm{grad}_S U_i - \mathrm{grad}_T U_i$$

of the gradients of the potential U_i at points S and at the center of the Earth (relation 6 of *Tobleou* Y above). Assuming the Earth to be inddformable, we demonstrate that the horizontal fj}t and vertical fjp components of fj have the following expressions:

$$(2) \qquad f_{ih} = \frac{3}{2} \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} g \sin 2q$$

$$(3) \qquad f_{ig} = 3 \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} g \left(\cos^2 z_i - \frac{1}{3}\right)$$

M_i and MT are the masses of the celestial body i and the Earth. rT is the radius o f 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

$$(4) \qquad \delta_i = \frac{f_h}{g} = \frac{3}{2}(1 + k - h) \frac{M_i}{M_T} \frac{r_T^3}{d_i^3} \sin 2 z_i \; 1 + k - h - \frac{2}{3}$$

The coefficients k and h are Love numbers corresponding to the deformation of the Earth under t h e action of star i. Thus

$$\frac{\delta_i}{\sin 2q} , \frac{M_i}{MT} r_T^3 , C_i \frac{r_T}{g} \qquad C_i = \frac{M_i}{M_T} \frac{r_T^2}{d_i^3} g$$

The values of 6, / sin 2zj for the Sun and Moon are in rodions and sezogdsimoid se- condes (§ 2.5 and note ld to § 2.6 above).

$$(6) \qquad \frac{\delta_s}{\sin 2q} = C_0 \frac{r_T}{g}' \quad \frac{0{,}862\ 10' \quad 6\ 371\ 10}{wi{,}s} = 2{,}570\ 10'' \text{"d. - s,s0 lo}^{-3}\text{"}$$

$$(\text{'ir}) \qquad \frac{\delta_l}{\sin 2q} = C_l \frac{r_T}{g} = \frac{0{,}862\ 10^{-13}\ 6\ 371\ 10}{981{,}8} = 5.590\ 10\text{" rad.} = 11{,}5d\ 10'\text{ "}$$

See in particular Schureman, 1941, *Mon nel of* îformonie *Arret ycic ond Prediction of Tides,* p. 14, and Stoyko, 1947, *L'ottraction lunisoloire ct lec pendulec,* Bulletin astronomique, Tome XII I, (p. 1-36), p. 3, re lation 2, p. 6, and p. 30.

It is worth noting that for the Sun and Moon, the agreement between the values ob- and calculated deviations § from the vertical is Pres remorquoèïe (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, which 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 'anvier 1959 to the Académie des Sciences "O4termtno£ion *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 secondsa 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 § *(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 [10] 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 changeda
after each experiment, for inclination i = 2064" and angular deviation
§ - J = 50 gr.

Although the individual curves are significantly different, mainly due to the disruptive influence of the beads (see *Graph XTV,1* re- presenting the results corresponding to experiments 5 ä 12), the average curves for the 24 odd and 24 even experiments are not very dif- ferent *(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, 2957,* 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 ayst4matic influence of the **surface** inclination **(1.72** grades for **1032"** for $ - 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 mni-yuin *1957*, showed a syat4matic in- fluence of 2.77 gr for a double inclination of 2064" and $ - J =.
- 33.77 gr.

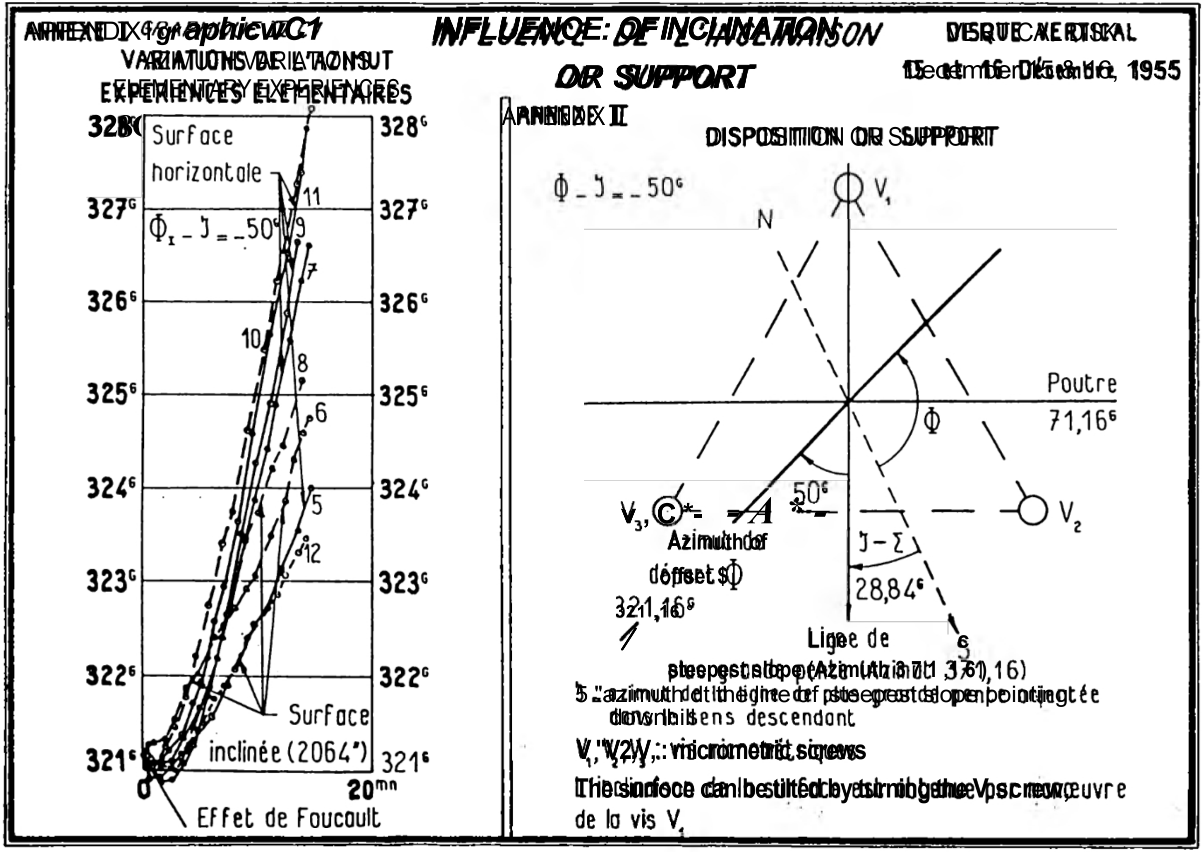
From all these experiments, and taking accidental errors into account, we can conclude that the influence of inclination is approximately propor- tional to i and about 1.5 gr per 1000", for $ - J little different from - 30 gr.

• 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 inclinaiaons never exceeded 8" aexagesimal se- condes 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 ldchera in a given azimuth, or in the course of doubly chained experiments, shows that *the periodic variations in azimuth over time cannot be con- sidtrated as resulting from variations in the inclination of the support with respect to the vertical.*

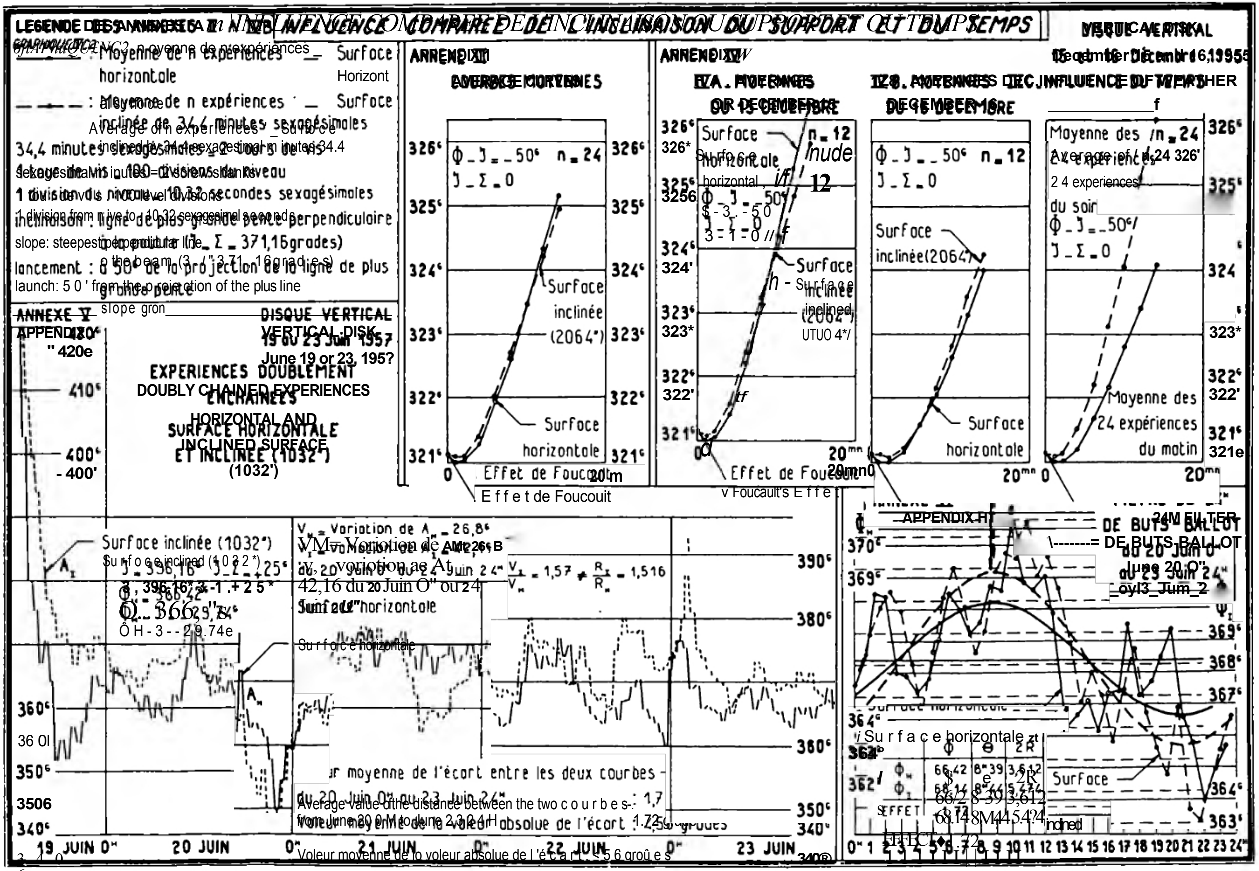
(11) Au cours dea experiments in Bougival and Saint-Germain in July 1958 (see ***Section C*** ci-deacoua, p. 142-161) the eurfacee inclinations have tt4 raised all the **vingt-** minutes by dea levels of precision from the Gëogrnphi9ue *Hotional* institute.

(12) Rnppelone que lee dtwiations thëoriquee de la verticale due à l'action lunieolaire cont de l' ordre du centième de seconde sexagéeimale (§ 3.2 ci-deesus).



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 paraconigue 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 1 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.*

(13) p. 102 above.

C

THE CRUCIFIED EKPERŒNCES OF JUNE-JULY 1958
DE SAINT.GERMAINETBOUGIVAL

1-- £eø cšezzx **faóora¥očres de** źšażnf-Germażn ez ¢fe **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 so1, 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 Minêral"* 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 lscienti fique* (CNRS) and the *Comitt d'Action lScienti{ique de la Dêfenøe 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 [2], 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 [3]

These two *crucial* fout à *doit* experiments showed that the periodic ano- malies previously highlighted *were observed again,* and that, in both laboratories, they presented remarkably corresponding periodic struc- tures*.

(1) The installation of the BOU V81 8été laboratory was remarkably carried out by the IRSID workshop with pdfabHquës materials and with the installation of a rigid metal pendulum sup- port identical to that at IRSI D (see § E.3 below). Both pendulums were *identical to* the one described in § A.l 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 IRSI D (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 shifts.
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 oösolutely certain of the existence a t IRSI D $ 2 meters below ground of a diurnal lunar periodicity of 24 h. 50 min. o f an ampli- tude incompatible with the accepted theory o f 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.

- *Nouuelles exptrientes sur le Pend me Peroconid ue b support anisotrope.* CR.CS.,T.247, 1958 p. 1428-1431.
- Sfructure *périodique* nes *mouvements du* Pendule Poroconi9ue d *Bougiuol* et *Saint-Germain* ezt 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 **&ougiuaf 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 har- monic 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 ampli- tudes, and *very remarkable* phase concordances.

In fact, these concordances are on the whole *more marked* for *Graph* AVïï, corresponding to a period of 24 h. 50 min than for *Graph XVI*, corresponding to a period of 25 h.

(l) § A. l above.

(2) TU notation corresponds to universal time. The notation TL corresponds to lu- nary. The passage of the moon over the meridian corresponds to 12 hours TL.

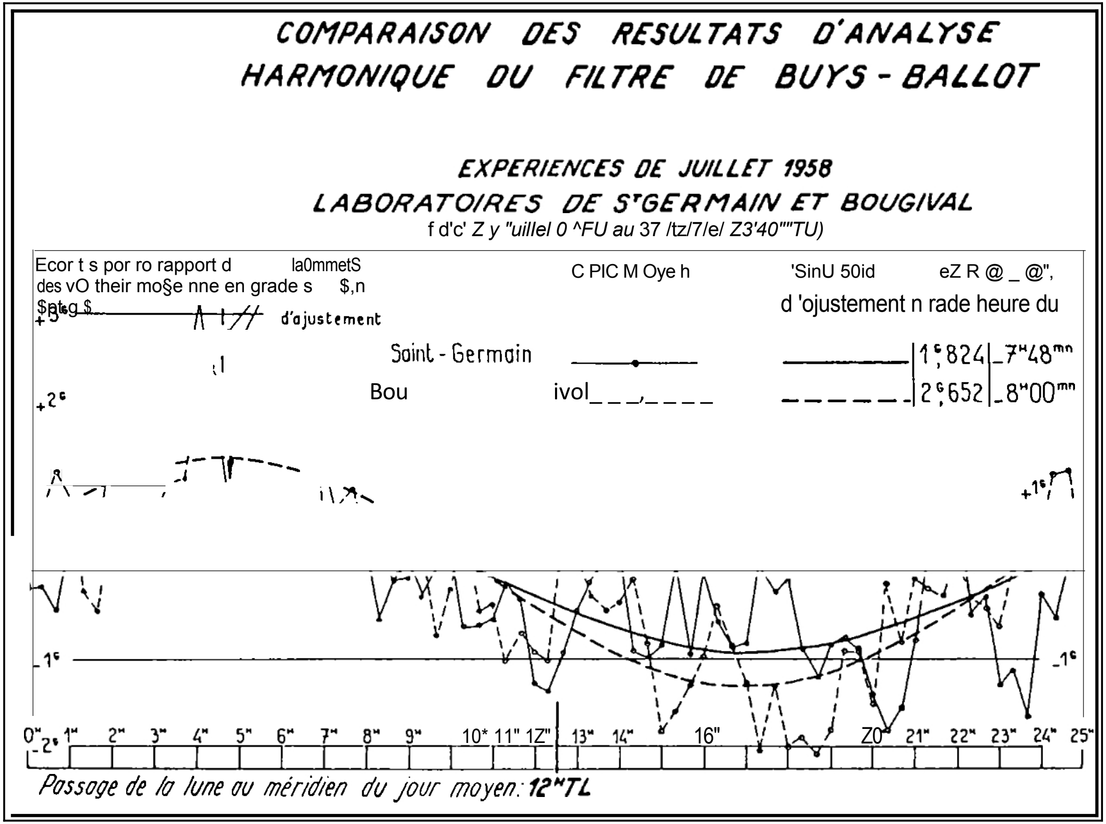
Le *isrophique XXI a* ëtë publ ië dans ma *Note du 3 Novembre 1958 b* l'Académie des Sciences. **Les** résultats correspondant au *Grophique XVII* ont **été** publiés dans ma ***Note*** *du 22 de cembre 1958,* ainsi que dans la *Note Com plém ento ire* de mon Mëmoire de 1958, *"Doit-on Reconsidérer les 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 Enificotiue. The concordance of the results is better when the period of 24h. 50mn instead of the approx imative period o f 25 h.

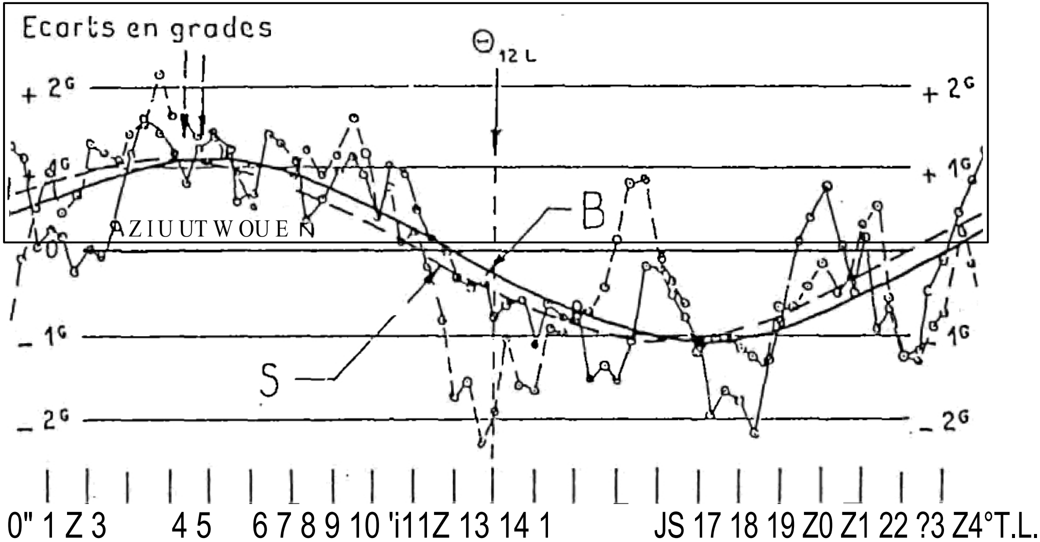


S!ource : Note du 3 novembre 1958 d'l'Atad6mie des Sciences, *Nou uelles exptriences sur le pendule poroconique b support onisoirope, CRAS,* T. 247, p. 1429

Graphique XVII

EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAIN

Boys-BoJIot filter "reeuttat" for id filter - 50 mn-. &



L,égtnde

I30UGI VA L	2,17	— 7 h. 23 mn.
SA IN "F-G EI\À'IA IN	2,10	— 7 h. 55 mn.

———— DOUGI VA L

Fine lines: medium cycles

- - - - - : SA I NT-GE RMA IN

Traits !"l "î8 : 5incis"ï'lcs 'l'a jcisicmcn t

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Sourct: Allais, 1958, *Doit-on recon sidérer* fes *fois de* to grnUifofion, *Nort Complémentaire* annexée.

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 éf'n òfentt

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 Cj / C. = 2.177 (§ B.2.5 above, p. 123).

(4) According to *Table* ïX, 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-diffrence (B - S!) J 2 contains virtually no compounds with a period o f 24 h. 50 min.*
Applying the 25 h Buys-Ballot filter to the half-difference IB - S) / 2 is equivalent to application o 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* Seint-Germoin and d *Bougiual.*

f6) These *average* concordances are all the more remarkable in that the day curves *noÎiéres* azimuths at Bougival and Saint-Germain *differed* quite *significantly.*

Que d'ensembIe

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 2dh 50mn of an amplitude totally inexpli- coòfe *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 dis- cussion, cannot be linked to any known phenomenon.*

(' For an illustration, see Jean Goguel's *fote* à l'Académie des Sciences *"Obseruotionz à propoc du pendule paraconique",* (CRAS, Tome 246, n° 16, April 21, 1958, p. 23d0 to 23d2).
Tette *Note* essentially attributing the effects observed to b$ti - oscillations. ment is typical of the sms ore *grounded* objections I've been4 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 ap- plications 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 billec d'acier p'ir dec billes d'ogale, et nous uerrez lout* ***t changera***

However, when I ***invitation*** to visit my laboratory at IRSID, he replied - dait: "Ypyez-nous .... Quefte *initial* üroit pour *moi* un *dJplocement* d *Saint* -Crermain, cor Je *ne suic pas* un *expérimentaleur".*

EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAIN

overall results

PERIOD FILTER	PENOUtE	PfiRIOO E TEMP3	2R	Θ	Θ_Θ12L	PERIOD FILTER	ASK	PERIO DE TEMP3	2R	Θ	Θ_Θ12L
24^h50^{mn}	B 5	1 Z3+ 4 -	2,174 Z,106		− 7^h23 − 7^h55	t5'	B + S	1 Z 3 4	1,804 Z,880		− 7^h45 8'01
ZS'	B S	1 + 2 + 3 + 4 ″	2,662 1,840		− 8^h01 − 7^h47	24^h	B − S ″	1 + 2 3 + 4	0,502 1,708	1ô 1? 13'59	
24	B S	1 t Z+ 3+ 4 '	1,394 0,77 6	13^h56 11^h40		12^h	B − S ″	1 + 2 3 + 4	1,502 1,986	5^h26 6^h31	
'Z*	B S	1t Z 3 Â '	1,89 0 1,54 30	5'L1 06		25	B S ïB S)	1 2 3 + 4	1,388 1,880		7^h23 7^h54

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: 2 R (24 h 50 mn) / 2 R (2d h) = 2,174 / 1,394 = 1,563

At Saint-Germainwe have: R (24 h 50 mn) / 2 R (24 h) = 2.106 / 0.776 = 2.714

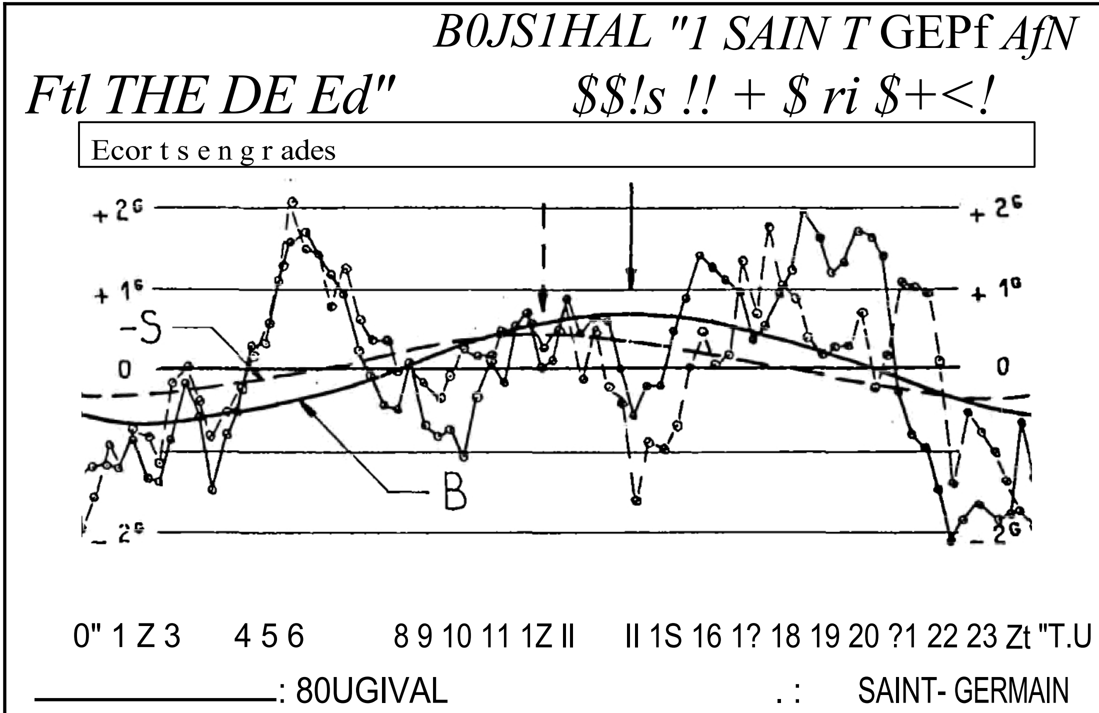
The average amplitude ratio is thus: (1.563 + 2.714) / 2 = 2.138

lsourte : Note b l'Académie des Sciences du 22 décembre 1958, *Strutture périodiq ue des* mouvements *du pendule porotonique* d *Bougiual el ô Sainl-Germain in July 1958, C RAfi,* T. 247, 1958, p. 2285.

Chart CIII

EXPERIENCES OF JULY 1958 IN BOUGIVAL AND SAINT-GERMAŒ

Boys-Ballot Filter Resuyats for id hour filter



Source : *Note h* l'Académie des Sciences, *Slruclure périodique des mouuemenls du pendule paraconique à support anisotrope à Bougiuol et ô Soinl- Germain en juillet 1958,* CRAS, 'I'. 247, December 22, 1958, p. 2285.

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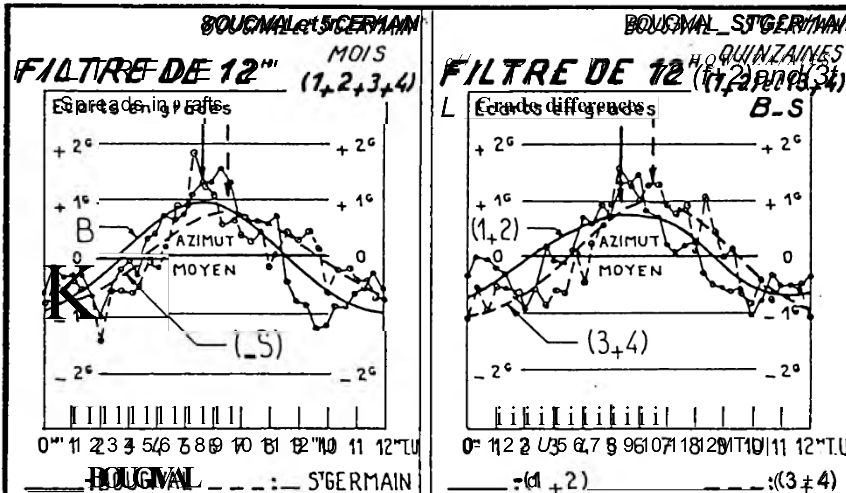
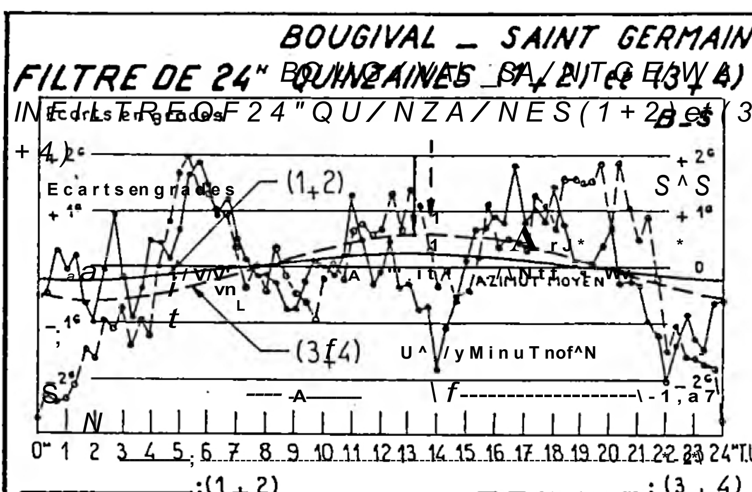
Graphiques XIX, XX, et XXI

EXPÉRIENCES DE JUILLET 1958 À BOUGIVAL ET SAINT-GERMAIN

Résultats du filtre de Buys-Ballot avec des filtres de 12 et 24 heures





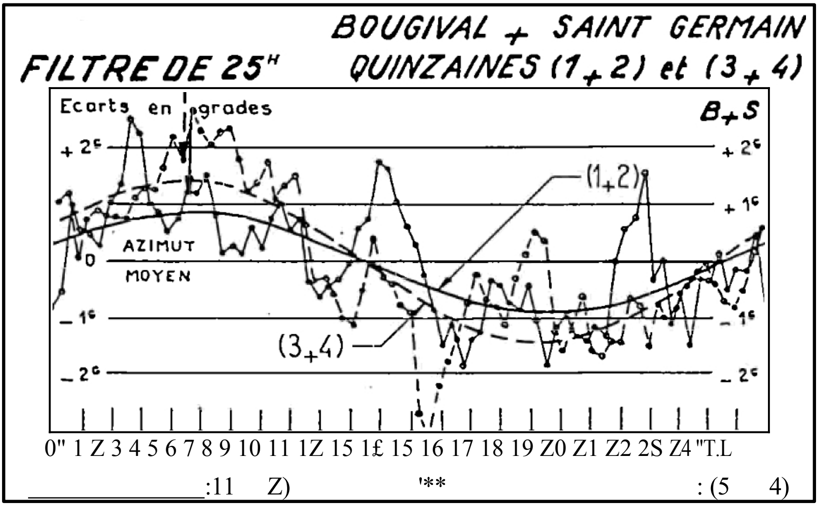
Source : Note à l'Académie des Sciences du 22 décembre 1958, *Structure périodique des mouvements du pendule paraconique à support anisotrope à Bougival et à Saint-Germain en juillet 1958,* CRAS, T. 247, p. 2286.

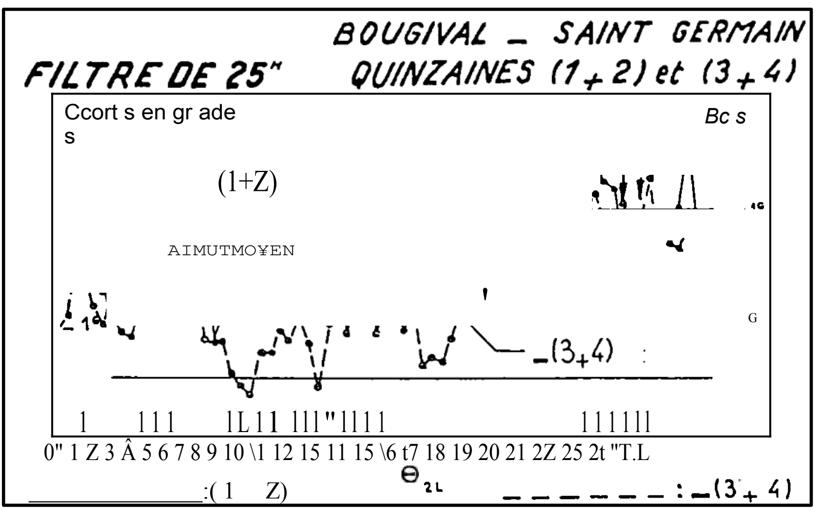
Source: Note **à l'Académie des Sciences, December** 22, **1958,** *Structure périodique des mouvements du pendule paraconique à support anisotrope à Bougival et à Saint-Germain en juillet 1958,* **CRAS, T. 247, p. 2286.**

Graphiques XXII et XXIII

EXPERIENCES OF JULY 1958 AT BOIJGIVAL AND SAINT-GERMAIN

Aésu*fars du /ïffre de *Guys- altot pozzz- urxÇfifz-e de 2fi etzz-es*





isource : Note b l'Académie des Sciences du 22 décembre 1958, *Htrutture périodique des mouvements du pendule paraconique ô support anisotrope ô Bougiual* ef ô J *oirtt-Cermoizt* en /ui//ez *958,* CRAS, 'I'. 247, p. 3286.

P'réquencigramme de la sêrie de Bougival de juillet 1958

4- Consideration of Bougival's monthly sörie fröquencigramme 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 ä 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. ll3).

(8J in neze *II B b* ma Communicatio n of 196 1 at the Institut internotionot *de*

(9) Grop5Îgue *77* of § A.2 aboveu8, p. 89.

(10) *Annex:e III C ä* ma Communication de l9 61 à l'fnstif ut *Internotionol de lslolistigue.*

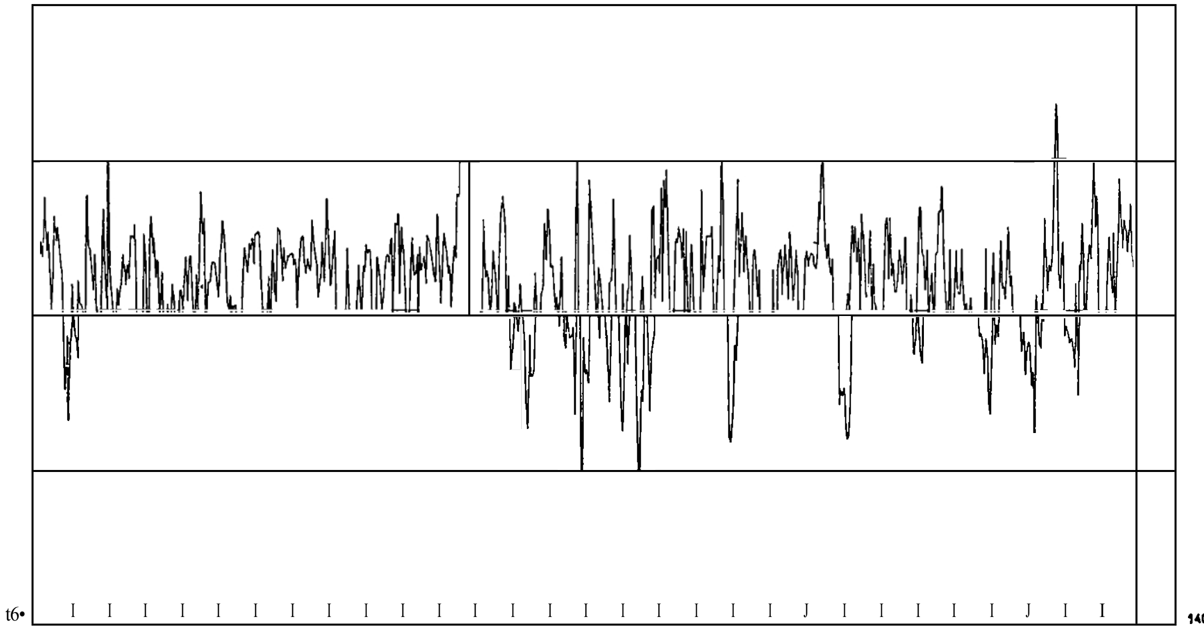
(lD My 1961 communication, § 18. The value of ti corresponding to the period of 2d h. 50 mn. is 7.28 with P = e> = 0.0007 See § B.1.3 above.

(12) For comparison, see the chart of the 721 ho - values series. raires of November-December 1954 *(Crrophuf ueXI* of§ B.1.3 above).

BOUGIVAL'S JULY 1958 OBSERVATIONS

Acimut:e de plan d'oœillatian

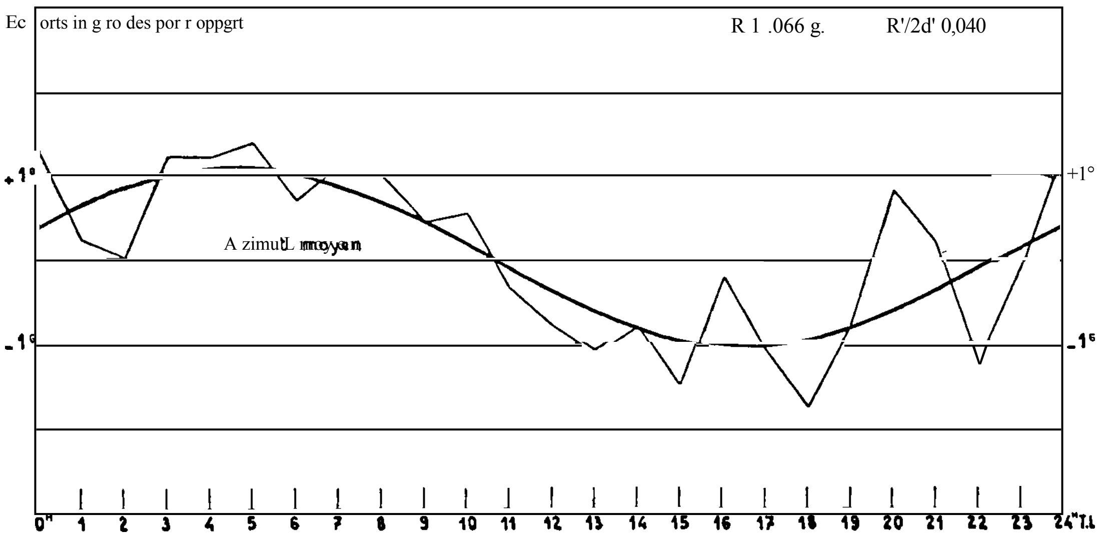
N -- 721 hourly thieves



Source*: Appendix II B* of my 1961 paper f o r *the Institut Interne.tional de* Stntisiiqitr, "'i "esi *de ptriodicitè. Généralisation du test de ischuster ou cas de* sJries *temporelles outocorrelées dons l'hypothèse d'un processus de perturbations aléatoires d'un* ***système stable"*.**
The 721 hourly azimuth values of the Bougivnl paraconic pendulum are shown in Anneze *II A* of my Commu nization of the *International Institute of Isiatistics* in 1961.

OBSERVATIONS FROM JULY 1958 IN BOUGIVAL

cimute du plan d'oacillation



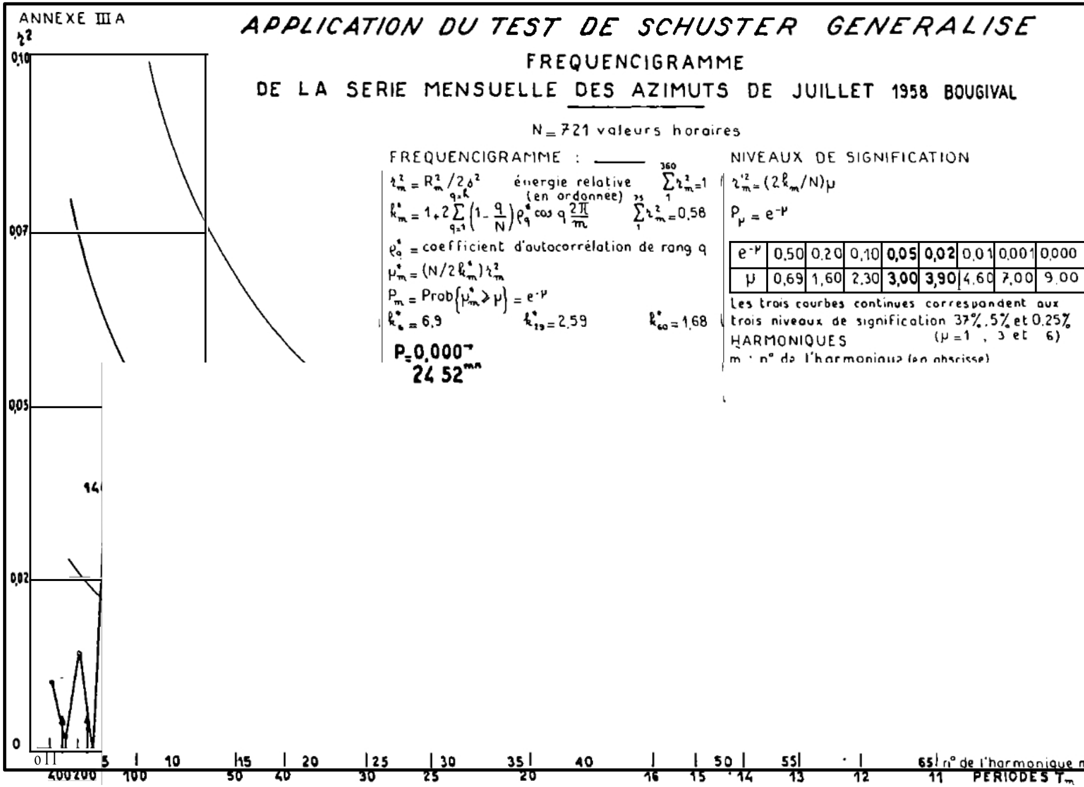
Mgende ——— observed ——— valuessinusoid of adjustment

}t 1,{}f-¡6 g. R '/ 2 o' = 0,040

isource : Annese III C de ma Communication de 1961 è *l'Institut International de islaiistiqut,* "'i "est dr *pèrio-diciit. Oëndrolisotion du item de Schester au cos de séries iemportllez out'* *orH1Je# dan"* l'AypoNse *dt un processus dt perturbations oltatoires d'un système siablt*

Graphique XXVI

OBSERVATIONS FROM JULY 1958 IN BOUGIVAL



Legend: On test formulation, see § B.1.3 above and *Legend* to *Graph XI.*

isourct: Annext III A of my 1961 Communication to *the Institut lnterriotioriol de istatistiqut* (see Source *of Graph XXI1).*

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 Bougivtll dl1 hr All 31 july 1958 [1] taking into account both linear trends [2] and sinusoids with a period of 27.322 days *equal to the sidërole period of the moon !-* *.

We **can see** that the two adjustments in *Graphs **XXVII** and XXVIII 6onï* practically in phase. The (S+B)/2 adjustment has its som- met 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* XXVïïï are virtually identical.

(1) In contrast to *the* coitus *that precedes, the* adjustments *that* follow have been *effective.* tu4s recently in jonuier J996.
On the *ZXVII graph*, azimuths are counted *in degrees from North. direct meaning.*
Let me remind you that we have

(1) f / W = 1 - R

The diifrence 1 - R* r**eprésente** 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 INOt s (see *Ghopttre V,* Section B, below).

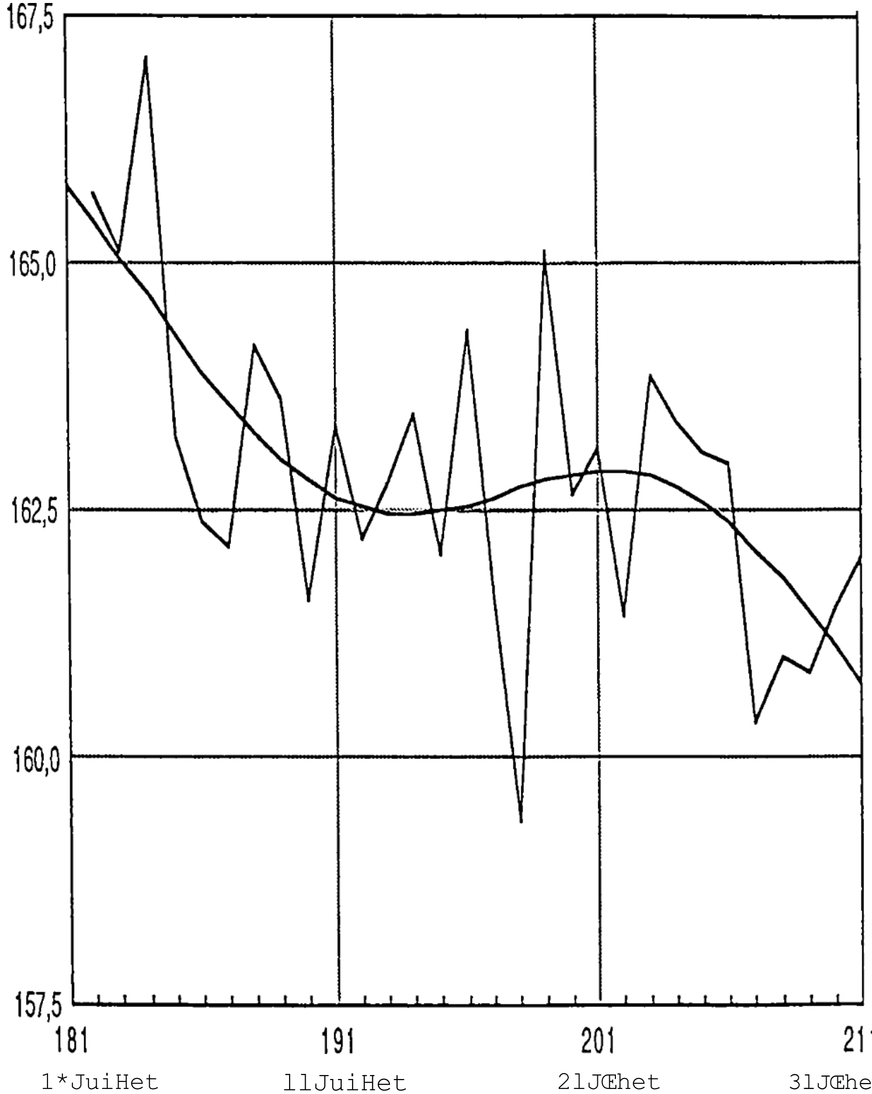
(3) In fact, the moon's sidereal and synodic periods give almost exactly the same correlation coefficients. The reason for this is that for a total duration of 30 days (i.e. 720 hourly values), both sidereal and synodic periods are *indis-* cernoid.
I have considered lo *sidtrale period* to facilitate any comparisons. with Esclangon's and Miller's results corresponding to sidereal time.
In fact, This choice was fully justified by the analyses of paraconic pendulum of obsezvaCions **du** series with isotropic support (see Chapter ïi, §). E.2 and F'.3).

(4) Between the sidereal period T t and the synodic period T2 of the moon, and the annual period T of the Earth, expressed in mean days, we have the relationship

(1) $$\frac{1}{T_1} - \frac{1}{T_2} = \frac{1}{T}$$

with : Tt = 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

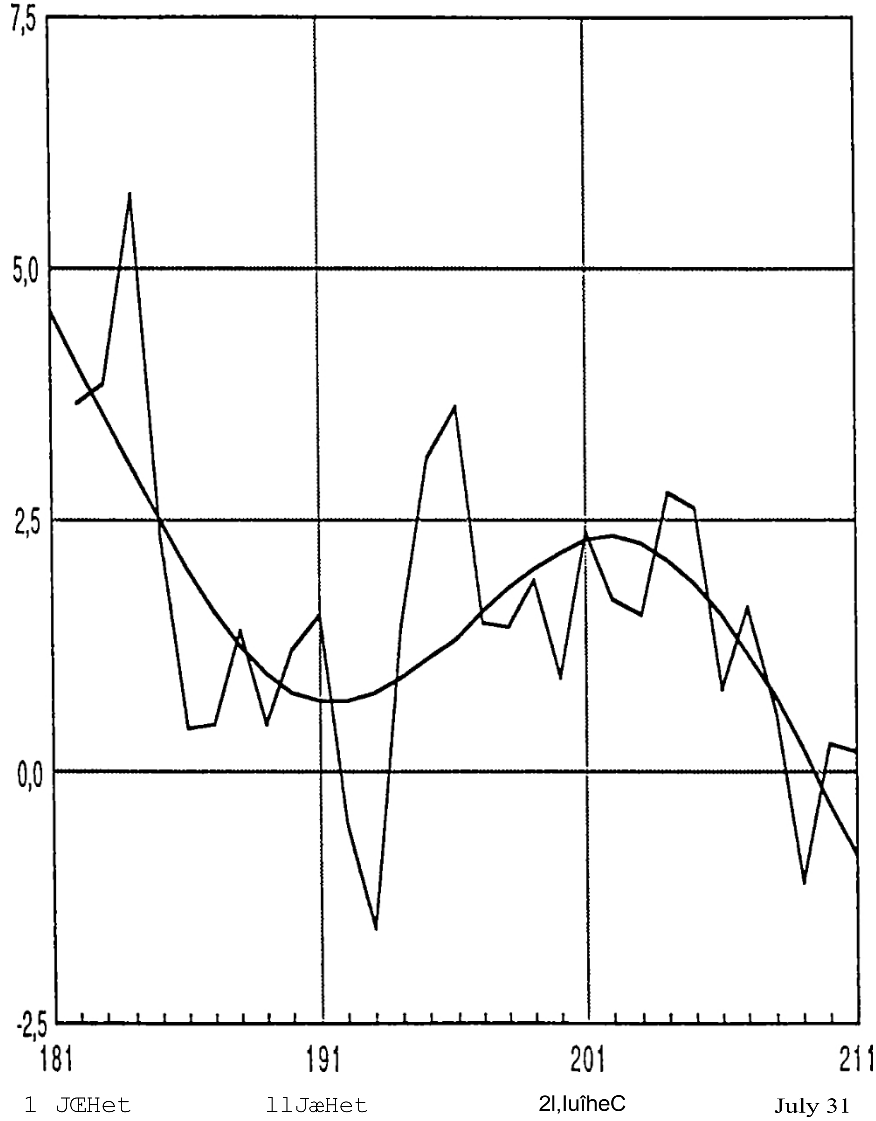


Ligende : Z = 1.61 ; R = 0.629 , l-R = 0.60d ; m = 162.9 grades , trend = - 0.149 grade per day, r = 1.02 grade, o = 1.25 grade ; N = 30 days.
Z = standard deviation of data sinusoid amplitude ; R = multiple correlation coefficient: r = half fit; ri = d'standard deviation of residuals.
Sinusoid maximum and minimum dates: July 24, 8 p.m. (s = 204.85) and July 11 4 h. lsj = 191.19). Dates s are counted in days from l''-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 = l.5l ; R = 0.680 , l-R = 0.537 ; m = 1.55 grade , trend = - 0.147 grade per day , r = 1.70 grade , ri = 1, l1 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. (sj = 190.37). Dates s are counted in days to From January 1, 1958, 0 h.

sources: Calcul 948-, Chart 13665 (June 26, 1996).

4.-Vtred'enxentdfe

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* identttd 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 inva- riable* 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 osti *swept away all the objec- tions previously presented* on my experiments with the pa- pendulum. This is particularly true of the 24-hour diurnal periodicity. 50 min [1] 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 ex:perents* or *meme moment dons two di{ftrents* places, *and if we oblen ier similar rtsultots, it will be decisive. the question will be heard, it will sero'f judged, and it sem it pro ué that we* o 0 *rezison". A very tame staff member* even said to me*: "It's not even necessary that there should be the same rtsul - tels. If your pendulum had onmoques movements, that would be a considerable flow".*

En fort, l'identité des composantes *périodiques de 24 h. 50 mn. o tlé bien ou delà de ce qui, ouanl* les *expériences de juillet 958 ô Bougiual el ô* Soinï-Germain, *élail con - sidtrt tomme denom etre détisi[.*

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 *moinc* de *l'Acodtmit dec* Sciences pensaient *qut* mes *exptritncez* devaient être *pourzuiaits,* meis *opporem mtnf il8 étaient minoritaires (§* G.6 ci-dessous, note *3, p.* 232).

DFVIAJIONSOBRDRVEEELORSDEDFUKECLIPEES TOTALISDEMDUBL

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 toŁal solar eclipse on June 30, 1954, the oscillation plane of the paraconic pendulum abruptly shifted by about 15 grades.

Graphs XXŒ 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.

(l) 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 poratonique 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,thatu 'it came as a complete surprise to the observer, Jacques **Bou eot** (my lab manager), and that i1 called me right away on the t6l6phone. II 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 oacillation 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 perio
dic azimuth 3 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.

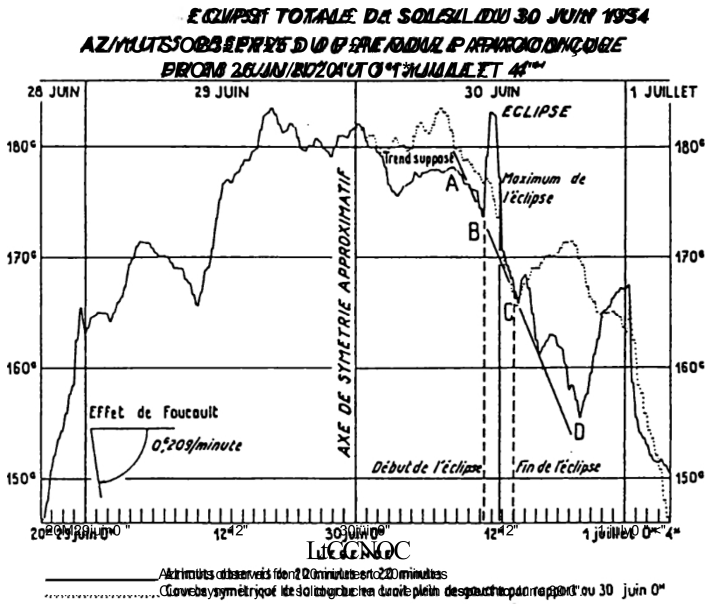
(3) On the almost periodic structure of a series, see § A5.4 above, p. 101.

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*.

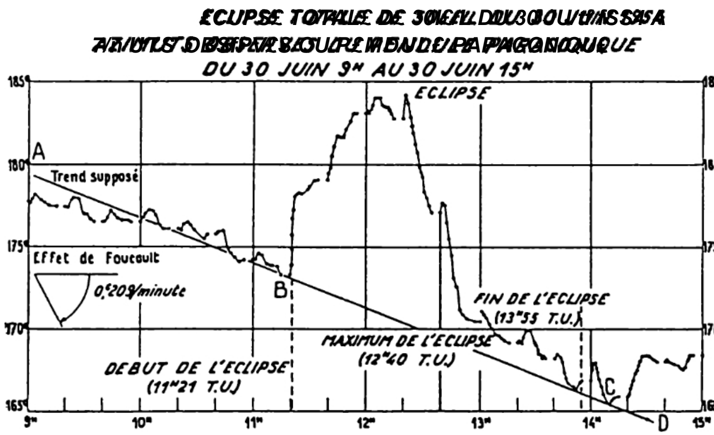
Finally, we can see that the rapid azimuth variations ob- served from 11 h. 20 min. to 12 h. and from 12 h. 20 mn. to 13 h. correspond to angular velocities of the order of 0.62.10 * and 0.79.10-* radian per se- conde, i.e. respectively **1.13** and 1.43 times the Foucault effect (i.e. 0.55.10 * radian 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, *Comptez 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 *rendered,* 239, 19Sd, p. 465).

Graphique XXIX



Graphique XXX



Source : Note du 4 décembre 1957 à l'Académie des Sciences, *Mouvements du pendule paraconique et éclipse totale de Soleil du 30 juin 1954*, CRAS, T. 245, p. 2001-2003 (reproduction photographique)

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* XXXfï.

The experimental conditions (laboratory, support, pendulum, use o f the same ball for each series o f 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 i s 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* *.

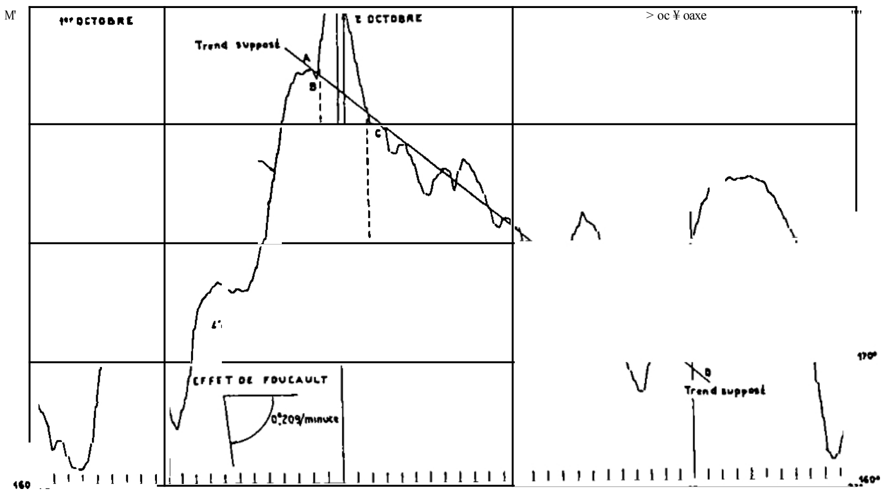
(1) See my unpublished note of November 10, 1959: "Surprises of *the porc - conique pendulum* and Eclipse totob de *coleil of October 2, 1959.*

(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* ïï, Section H.

TOTAL SUN ECUPSE OF OCTOBER 2, 1959

aZIM'ITs PxRAcONioiJE PENDULE OBSERVATIONS

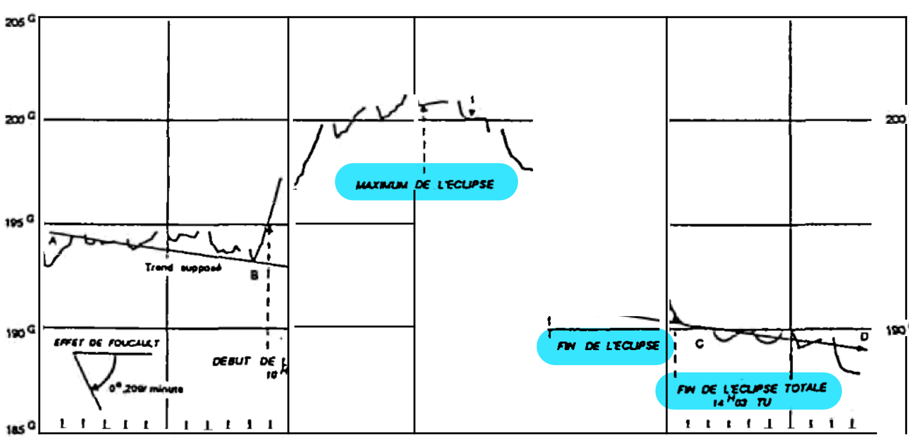
yes ocroaRe is^du s ocroane z:fi



TOTAL SUN ECUPSE OF OCTOBER 2 I9S9 AZiuUTS

oBSERVES DU PENDULE n R CoNIo'IE

DIJ 2 OCTOBRE ghy¡j y g çeuane is h,$j



Sourcc: Allaia, Note, nou publite, du 10 novembre 1959, 3'fouvement du Pendule pnruconi9ur *tt E-cmps* totofr *de* zofr'*d du* 2 '*ctobre J9S9* (reproduction photographique).

g.- Comparaïsozz chesperfzzrôafîozzs oBsemées lors desdezzx *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 durationsa of 3 h. 14 mn. for 1959 and 2 h. 51 mn. for 1954 are represented4ea 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 ten- dances of the pendulum's plane of oscillation before and after the eclipses are not absolutely certain, the perturbations considered in both cases present tt ne *sim il it nde assez remar- quable* .

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 per- turbations 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 mêridian. These deviations are *totally inc:xplicabIes* within the framework of current gravitational theory.

(lJ It should be noted that this similarity only exists if we consider the beginnings and fi ns of the total eclipse for*l all lo* earth 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 dea 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 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.

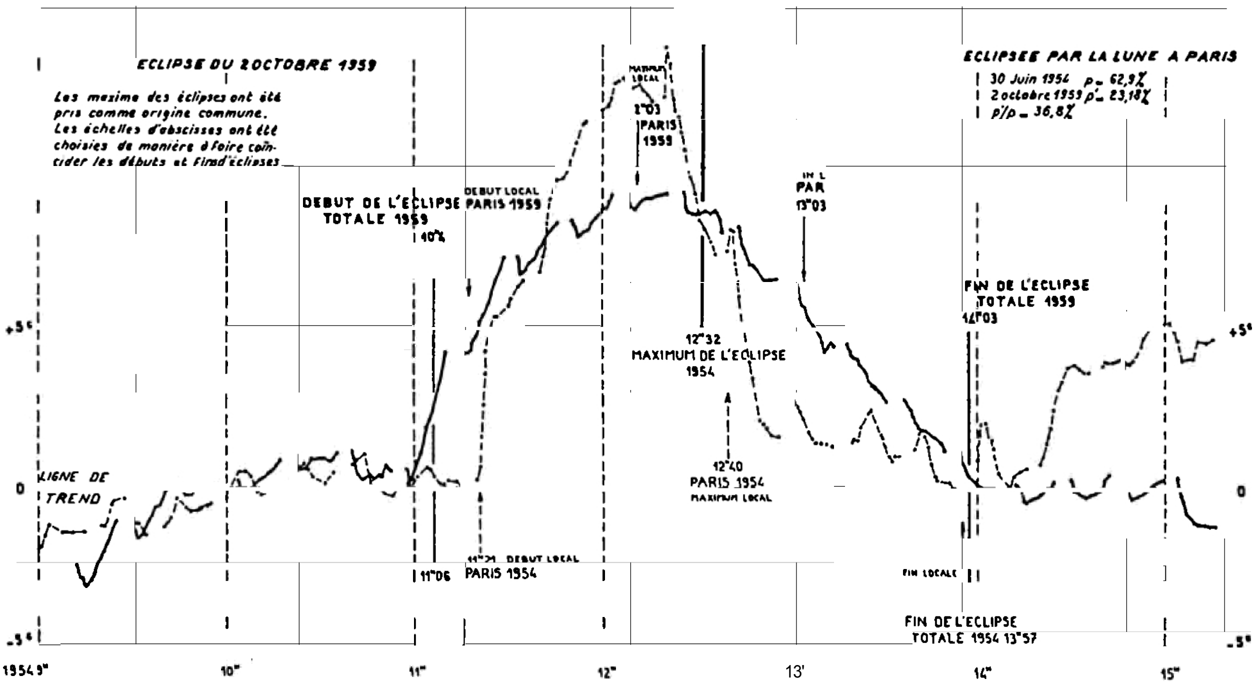
Whatever the intrinsic scientific importance of the onomolies of the paraconic pendulum corresponding to the tclipses considt- rtes, their *relative* scientific significance is in fact quite small compared to that of the periodic lunisolar anomalies observed, the existence of which is *fully established* by the hundreds of thousands of observations that have been4 made and from which they have been4 deduced *.

(2) Since these anomalies are totally *inexplicable in the context of In théorie currently admired by* i *grouilation.*

(3) In fact, the anomalies observed were confirmed by experiments by Saxl and Allen during the solar eclipse of March 7, 1970 *ï1970, isolate Eclipse ne been by a Torsion Pendulum,* Erwin J. Saxl and Mildred Allen, Physical Review, D, vol. 3, Number d, 15 February 1971). Sam and Allen's article refers expressly to my own experiments.

Graphique XXXIII

COMPARAISON DES AZIMUTS OBSERVES PENDANT LES DEUX ECLIPSES



Source: Allais, *Note* non publiJe du 10 novembre *1959, étonnement du pendule puraconique el Eclipse totale du* Soleil du 2 œtoöre J959 (reproduction photographiquel.

HANGER

I.D.3

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 ⁽²⁾ 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 5 Q at my *Genenrolization of the Test de Slchuster* 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 in- fluences 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.

(41 This theory has enabled me to calculate the exact luniso- lair influence on the motion of the paraconic pendulum (see § B.2 above).

f5) *Note on the interpretation of parotonic 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 lA 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 §' of the azimuth § of the pendulum, or *indirectly*, by creating an ellipse which, through the Airy [2] effect, causes precession.

$' = t3/8) p lx b **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 cau-e 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-), the rotation speed of the plane of oseillation corresponding to the eddy current effect is - ci sin L = - 0.550.10 '* rad./sec. where m represents the Earth's rotation speed (m = 0.729.10 '* rad2sec.).
If no other effect occurs, the pendulum's oacillation plane will make one revolution.
complete in 2n / tu sin L = 11.42 10* seconds, or 31.76 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 o f 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 difhcult 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 scarcity4 of serious work; - the paucity of numerical data on the **results** obtained.

On the Foucault Pendulum experiments, see in particular the *Biblioarophie du Pendult,* très Etendue, published in 1889 by the *ffocift tronçaice de* **Physique** (Collection de mémoires relatifs à la Physique, Tome IV, Gauthier-Villars), and my 1958 me- moire, *"Doit-on reconsidérer les lois de la grouilation î",* p.99-100.

(4) In an interesting article (On the *irregularitiez o(Motion of tht Foucault Ptndulum,* The Phyeicel Review, April 1919, Vol. XIII, n-4, pp. 241-258) A. C. Longden writes:

> *"Mort Thon o* 8core *of uitll-hnouin phy#itiata ond oafronomera ort on rtcord* es**affirmin** *that* the *Foucault Pendulum muet be utry long ond* very /ieovy 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, t h e study of which has certainly been neglected until now [7].

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 porc-* conical (note 3 in § E.1 above1).

In the Second Part of this work *ïChapter* 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 grouifofion?"* p. 101).

3 - Anisotropy of the **cupport**

Experimental process

1- To demonstrate the influence o f support anisotropy on the motion of the paraconic pendulum in an experiment lasting 8 = 14 min, I carried out successive releases at equidistant azimuths of q grades as follows [1].

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 8 = 14 minutes, the plane of oscillation of the paraconic pendulum moves through an angle A$ from the initial azimuth Q considered. Plotting the $ on the x-axis and the mean variations *in grades per minute* on the y-axis

(1) $$\phi' = \Delta\phi / \theta$$

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 ' we obtain a sinusoidal curve with a period equal to 200 grades. The least-squares method can then be used to calculate the si- nusoi'de o f fit

(2) = ap + ausin 2($ - Z)

that best represents all observations.

(1) Voir ma *Noie* du 9 février 1959 è l'Académie des Sciences, *Dëlermination expé - rimenlale de l'influence de l'onisolropie du support sur le mouuemenl du pendule paro- conique.*

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) §' = ap + a¡ sin 2(Q - Z¡) + a2 sin 4($ -)

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 in-ferior part of the pendulum, and the correlation obtained can also be represented by an expression of the type [2]

(4) 2b = 2Q + 2bi sin 2(§ - Z;') + 2b2 sin 2($ -)

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 Pi and P2 and the two virtually identical suspensions @ and Sq that I used in my two laboratories at Saint-Germain and Bougival during the experiments of 1955, 1956 and 1958 [3].

The two *graphs XXXTV* represent for q = 10 grades and p = 5 the re-

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 Zj and Z1 on the one hand and Z2 and Zj on the other correspond remarkably well. The azimuths jj 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 aniaotropic 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 1' = 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 Pohteou 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) g, =- 0.127- 0.677 әп2(g-372.11)

and *in radians per second*

(6) =0.262.10-3 = - 0.8ü3.10* - 1.772.10* Än2(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.lŒ radian per* second, i.e. ä - 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 graàes, cause C is equivalent to around three times the Foucault force [7].

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 . The constant term - 0.127 is of the order of half the Foucault effect equal to $ - 0.21 grade per minute.

(fù Note (1) in § 2.1 above, p. 173.

(6) Assuming, with the notations of Z'oô/eou W (p. 128)

{ï' = Bin 2 (E - iti)

we have (Table X, p. 180f.

tg = (0.174 / 2) / (103 . 840) = 1.01 10 * rad./eec.

(7) 1,772 10 " / 0,550 10 * = 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 *ogëbric* mean of the four series of observations gives in *grodes per minute*

(7) = - 0.047 - 0.897 sin 2 ($ - 374.95)

and *in radians per second*

(8) = - 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 *ZXKV trophics*, the quantity represented is y = yp + a sin 2 ($p- $), while on DIV tJrepAi9ues, the quantity represented is $' = ap + at Sin 2($ -¥t). Corresponding notations are yp = , a = - a .

(9) In the August 1954 experiments, yp = 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-enduIe that I **drew up in 1956,** I counted the angles positiuemenf *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 direet.*

CARACTERISTIQUES DE LA CORRELATION DU MOUVEMENT AVEC L'AZIMUT DE DEPART

5U5FCfi5I0N	7EfIDULC UTILISE	q EN GRADES		NOMBRE TOTAL D'EXPERIENCES	tE RIOSCS D'OBSERVATION	DEPLACEMENT EN AZIMUT EN GRADES/MINUTE $\Phi' = a_0 + a_1 \sin 2(\Phi - \Sigma_1) + a_2 \sin 4(\Phi - \Sigma_2)$ a_0	a_1	Σ_1	a_2	Σ_2	SMALL AXC OF THE C LLIPSE EN FIN 0 5¥PENIEdCE gN EN $2b = 2b_0 + 2b_1 \sin 2(\Phi - \Sigma'_1) + 2b_2 \sin 4(\Phi - \Sigma'_2)$ $2b_0$	$2b_1$	Σ'_1	$2b_2$	Σ'_2
Saint 6trmein	P,	10	5	100	4 ou 10 mars 1955	-0,114	.0,z36	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	O.03o	-0,141	38s,95	.0,0 '7	391,24
"	P	20	2	20	21 mai 1958	-0,157	0,781	371,34	,0,15Z	573,08	+0,016	.0,Z00	372,74	,0 065	373 05
-	P	Z0	2	20	13 août 1958	-0,153	-0,541	365,51	.0,166	390.02	-0,010	.0,17/	371,69	-0.065	40/63
8--ti--l	P,	20	Z.	20	13 août 1958	.0.078	-0,6 16	369, Z40,	087	382,09	+0.0"	-0,188	369,19	,0,0'7	'80,61
PENOUL 55					MOYENNES	-0,127	-0,677	372,11	+0,015	385,38	+0,017	-0,174	374,70	+0,003	384,66

8, : et ngvle utili# t dens le t xpî rie nc es c ntinue s d e t mois en Nov.-Déc. 1954, Juin-Juillet 1955 à St-Germain et Juin-Juillet 1958 à Bougival

P: pendulum utaisi in June - July 1g56 i 5'. 6er moin

EFFET DE FOUCAULT

' . 0.21 grade/oinuLr

t " tI £il5TItU£S 0fi s 5UsP£sSI0HS iiTlL lf £($

AliNut dt lo perpen0itul0ir e ou suppor L l   371.16 grodes

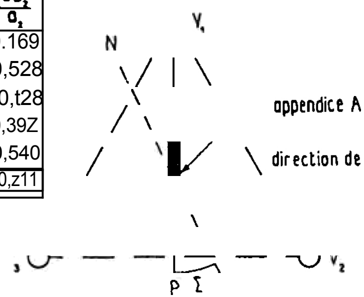
0RRC5P0flUANCE OES AZIHUTS 1

zz o's coErricizxTs a ET b

PERIODS 0'0B5EPVATIOfJ	$\Sigma_1 - \Sigma'_1$	$\Sigma_2 - \Sigma'_2$	$\frac{2b_1}{a_1}$	$\frac{2b_2}{a_2}$
1	.0,23	,8,Z2	,0,198	,0.169
2	.S,3f	,6,z7	.0,232	.0,528
3	1,20	,0,03	+0.tS6	.0,t28
4	.6,18	.12,61	,0,322	,0,39Z
5	,0,05	1,¢8	,0.305	,0,540
L10YENNE5	.Z,59	0,72	,0,263	+0,z11

DISPOSITION 0U SUPPORT

q



7-I   3
V,,V, ,V : tolonLts screws du su porE 5'

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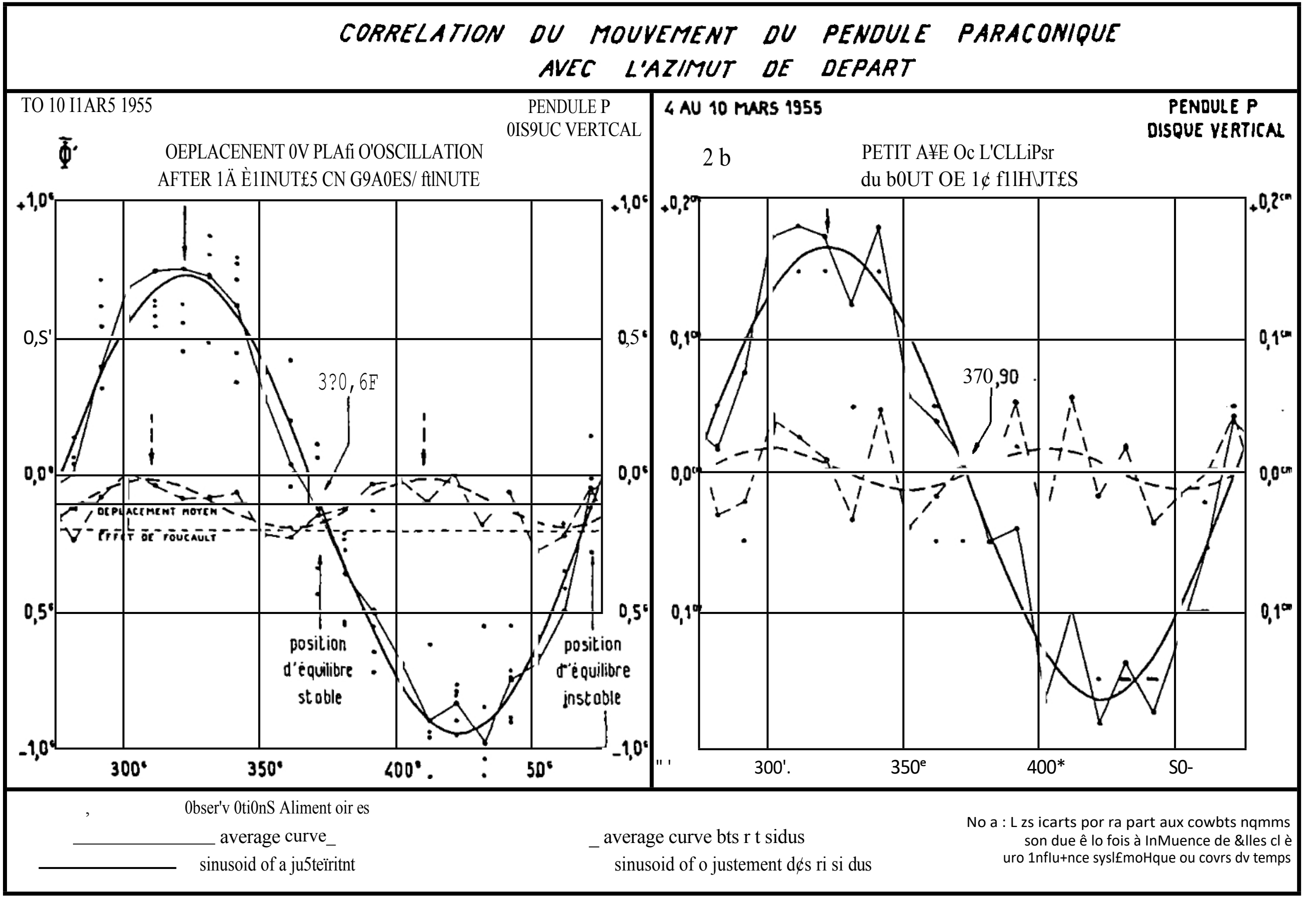
N0sst N" 1Z kgs.   Ptriode T . 1,85 seconat

flofrents principoux d*iner tie par royporL cu center de lo bllle de suspension   Â gZ@9 10' B _ g3,j1 10' _ 0,3M.10'

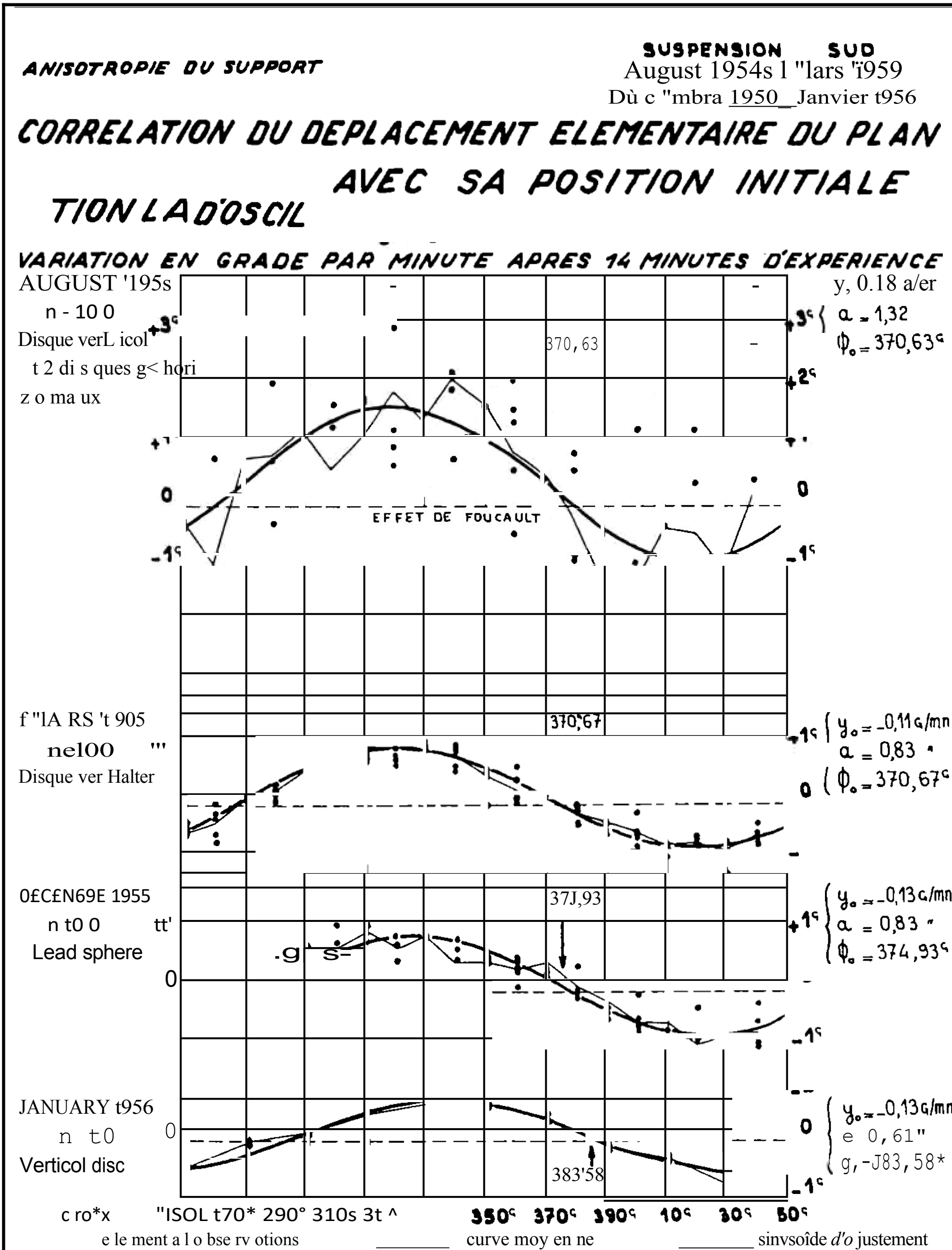
Dist#nct du c0ntrz 0s grovit or tenLre de lo bille   0G t . 83 tm t

R0yon de lo bille   . 0,3Z5 to

Sourct: 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*** onisotropy *of the support* ***on the*** *motion of the* ***paraconic*** *pendulum.*



Source: Trophique IV.B.1 of my Conference of November 7, 1959, and my *Note* of February 9, 1957 to the Académie des Sciences, *Défenmination expJrimenfole* de *l'inftuente de l'onisofropie du support cur le* mouvement *du pendule paraconique.*



Ugende: The eddy current effect corresponds to - 0.21 grades per minute.
Source: €fraphig 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 [1]. 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 [2], **but which e11eS** £1t cannot £1t explain lls v **lriatioZls** co£lsidé-rables observed d'azimut as demonstrated by the triple- mentally chained experiments carried out in May 1957 [3]

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 rt f200 . 840 - 0.47 l0 * rad./sec. whereas the eddy current effect is 0.5G 10'4Mad./see. Both effects are therefore of the same order of magnitude.

f3) § B.1. l 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 o f 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 dece- löed in the seven monthly series of obser-nations of the paraco- nic 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 lu nisolar 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.

n 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 indöceivable, 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 [5], it is essential to compare the orders of magnitude *relo-drawn* from the various periodic lunisolar components corresponding to the paraconic pendulum and to the gravitational forces deduced from the lunisolar po- tentials.

(3) ** The lunar diurnal period of 24 h. 50 min. corresponds to a variation of the order of 0.2.10' radians per second, whereas the Foucault effect is 0.550 10' * radians per second. We thus have 2 10' / 0.550 10' - 0.36 10 $^{-1}$ = 1 / 27.8 (§ A5.3. above1.

(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)l existence of an orbital effect corres 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 onetyses are only *9uefitotiues* and really *little* conc/uorzYes. They are practically unusable (see note 7 ü 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 *ïChopitre* Jï, Section C).

(5) See especially: Paul Schureman, *M'*i nuol o/ Îformonic Anotysis *end Prediction of Tides oY* 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.) [6].

We can see that the *relative* structure of amplitudes is *totally dif-[ërente* 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 {ondomental* 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 Lttne and lsoleil.*

(6) A detailed analysis is presented in the second *volume* of this book. *(Chapter* Yï, 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'o6ïeor 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.	M y 24,84 h.	Ot **25,82** h.	Qt 26,87 h.	Total amplitudes
	An Pa plitude d nde 2R en g ades l'o Pendulum raconique				
June-July 1954	2,34	4,12	2,56	6,76	15,98
Nov.-Zac. 1954 '	11,26	11,54	4,64	6,18	33,62
June-July 1955 3	13,00	10,46	4,78	7,78	36,02
Averages	8,87	8,71	3,99	6,91	**28,54**
Relative values: a	31,08	**30,52**	13,98	24,21	100
	Ts theory of f ces lunisol res				
Coefficients	0,7060	0,0977	0,3771	0,0730	1,2g38
Relative values: b	**56,31**	7,79	30,08	5,82	100
	I pport of their relati es				
a/b ratios	0,552	3,92	**0,465**	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.

isources : 1) *Pendule Poroconique :* Calculations by the Hamburg Hydrographic Institute ma *Note* du 4 décembre 1956, *Analyse harmonique des mozzuements* dzz *pendzzIe* poroconiqzze. *Compléments* (3 p.).

2) *Coefficient s de lo théorie des* /orces *lunisoloires:* Schureman, *Manual o[Harmonic Analysis end Prediction o[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 [2], they have construction defects.

For example, the pendulum's **center of gravity may be** slightly **off-center** pdf rä rapport à l'£fxt priflCfpäl d'fflertie passant p£fr II Cynthia dt la bf1 1e [3], or the metal parts may not be perfectly ho- mogenic. **As for** balls and bearing surfaces**, they** are not perfect **and are** more or less deformable.

But, *whatever these* coiistructton *defects may be, they* can *in no way lead to the appearance of real pertodic effects.*

The effects observed are due to the mngnéttsme *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.*

(l) 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 som maire sur les recherches sur le* peztdute *poroconique,* 13 p., p. 5).

f3) 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 centenial 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 *sta - tistically 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 amplifi cation of an already known geophysical phenomenon. But in fact, all known geophysical phenomena have periodical structures *very different* from those of the paraconic pendulum [4].

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 [5]

In any case, the argument would only be valid if the paraco- nic pendulum oscillated continuously.

(4) *** See § B.3.1 and B.3.3 above.

(5) See *Tobleou II* of § A.S. 2 above and Toöleou 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* 6.

No similar phenomenon has been observed in gravitation experiments.

8- In fact, such an argument is worthless on its own. The

16) Voir mon mémoi re de 1982, *Frtquence, Probobilitt et Hasard,* avec deux Appendices : l.-fréquences *empiriques et fréquences mathématiques - Illustration.* 2 *de* 7'fidorême *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 in fiuences, 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 i1 wrote:

> *"Once the (device) equations have been integrated, or at least properly interpreted, t h e r e will be no more m yst ère".*

In fact, on December 6 1956 I had sent him my geTleral dissertaTion of s£1 s£1pt£1mbre 1956 *"ThêoTie du pendule paraconigue"* [8].

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 tra;iectory of the paraconic pendulum this memoir mdtùodiolisa **utilisait la variation** these constants does Logron,ge and he indicated in his Seventh ***Partie*** of synthesis (p. VI I.1 è VI1.29) all the /orm mes of first *approximation corresponding* - pondotif or movement *of the* poroconic *petidute and to all the* /ncteiirs 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 lŒ! 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 lŒ! radians per second * while the effects observed are of the order of 105 TadionS 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 Hen ri Villat's o *priori* alTirmations within the Aeadëmie des Sciences, I reproduce in the *De* uzième Yolume of this work, in nnneze i.A, 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).

fl0) In a letter dated l ^ June 1960, Jean Leray, member of the Section de Mécanique de l'Académie des Sciences, wrote to me

> Yos *concidérationz Ihtoriquec ... n'oboutiscenf poc à* des *concluszions* numdri9ues; vou4 ne *con{rontez pes les grondeurs que uouc* ouez *nit* mesures d *lturc* 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 "*héorie du pendule* poroconique", which I had sent to him, nor of the orders of magnitude, font ó */oit* essentiefs en *í'ecpèce.*

Visit **fait,** Jean Leray ignored the crucial experiences of July 1958 section **C** abovei.

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 as- tronomic influences, such as the lunisolar action, towards which the pendulum's oscillation plane tends. *This limit* pfnn *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) Q ' = - tu sin L + k sin 2 (X - Q) + K sin 2 (Z - $) + C

with 2

(2) kin2(X-) = in2 -$)

In these relationships - tu sin L represents the Poucault effect, Z the direction of support anisotropy, X; mean azimuth corresponding to astronomical influence i *during the 14-minute period under consideration,* and coefficients k¡ 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 $ ' represents its average varia- tion per unit4 of time over this period, with ' = d$ /dt.

(:l) * § E.3 above, p. 176.

(2) Naturally, we have

(1) k sin 2 X = Z k¡ sin 2 X, k cos 2 X = Z k, cos 2 Xj

(2) k = Z+ 2 Z Ï 'ljcos 2 Ô¢j -)

(3) tg 2 X = (E kj sin 2) / (E k¡ cos 2 Î£¡)

expression of the azimuth of the boundary plane

2- Relationship (l) can also be written as

(3) $§' = - c' \sin L + f \sin 2 (Y - §) + c$

by setting 3

(4) $f \sin 2 (Y - \$) = k \sin 2 (X - 9) + K \sin 2 (Z - §)$

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) $Y = Y(t)$ $X = X(t)$ $k = k(t)$ $f= f(t)$ $c = c (t)$

The Y direction corresponds to the overall effect of the as-tronomics of mean X direction and the influence of Z direction support anisotropy.

f3i Naturally we have

(1) $f\sin 2 V = k \sin 2 X + X \sin 2Z$ $f \cos 2 Y = k \cos 2 X + K \cos 2Z$

(2) f = k2 2kK o 2(X)

$$- \frac{k\sin 2X + H\sin 2Z}{k\cos 2X + K\cos 2Z}$$

We can also write

(4) $$tg\ 2 (Y . E) - \frac{k \sin 2(X - E)}{K + k \cos 2(X - Z)}$$

Limit plan and experience

3- Relationship (3) corresponds exactly to what is suggested by the chained troia aériea experiment carried out from May 6 to 10, 1957.

Naturally, what we observe is the azimuth § of the pendulum at the **cOtlPB of** a chained experiment, and this azimuth is in rela-

tion 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 § oscillation plane to the

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 coef- ficient K [5].

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 8u8pen8ion (see Section *H* of *Chapitrt II).*

8.- Street ¢f'ezzeemôfe

Empirical and theoretical analysis of the obaervationa 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 auspenaion, the random influence of the balls, and periodic influences of astrono- mical origin.

To a first approximation, whether we're talking about lunisolar effects or the effects of substrate anisotropy, the effects observed *result from the* effipfectiveness 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 onisotropy or lunisolar peridic influence, and the motion of the paraconic pendulum is reduced to the F'oucault effect.*

At each instant there is a$_P$ *lan limrte, 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 periodelic influences.

Hi GropAi9ues fïï and 2V of § A4 above, p. 94-95.

THE INERTIA SPACE HYPOTHESIS OPY O F INERTIA SPACE

1-- Z-o fhćorźe ej f'expśrźence

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 10'5 pg pjjs per second in the case of anisotropic suspension *.

La diff6rence des ordres de grandeur entre lee effeta observ6s et calculćs est telle *qu'aucune des personnalit€s* qui viennent viaiter mon laboratoire de I'IRSID n'a contestć *l'impossibilitt totale* d'expliquer lee mouvements obser-vńes du pendule paraconique dans le cadre de la thńo- rie actuelle de la gravitation et des mouvemenŁs relatifs.

fl) See above § I.B.2.5, p. 121-123.
Lee elfets obaervća aont d'au moins de I'ordre de 10'* radiana par seconde dana le caa 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 coef- ficient of proportionalit4 is assumed to be *the* same whatever the maaaes 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) $\vec{F} = m \vec{\Gamma}$

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 $\vec{F} = m \vec{\gamma}$ is assumed to be applicable only with respect to a system of axes O' x' y' z', called *Gtalilëe* axes, animated8 by a uniform translational motion relative to the system of axes, called Copernicus axes, whose origin eats 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) T = 7,* T,* T,

(3) " " " r" Y e" " c

2 , denotes the relative acceleration of point M with respect to the ref-

rential **Oxyz** linked to the **Earth** ; y is the drag acceleration due to the motion of the **reference system** O x y z

with respect to the reference system 0' x' y' z'; and y designates the complementary acceleration, known as the Coriolis acceleration.

Ona

T 7,=2 m v,

where v denotes the velocity of point M relative to the axes O x y z , and

and the instantaneous rotation of the reference frame O x y z with respect to the galilean reference frame O' x' y' z' .

Relation (2) is a pure mathematical identity.

7 - We have

(5) 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 ¡ i s assumed to be proportional to the peaante mass of point M.

In the case of the Foucault pendulum, F2 represents the tension N of the suspension wire. Dana le caa du pendule paraconique F2 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 'rifications of the current theory of grauitation

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, TroitJ *de Mtconique* 2totionnetle, 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 opproximotionz zuccezziuez, les èquationz qui done le tae nit* troíe *corpa scuiement a'ottireni* dorment fes soiutíons *du problhme del* trets *corpz permeltent de tolculer les* ép?idmJridcs pour un *long* interueïe 'fr *temps i* fes *toblez de W Verrier les dorment pour trets z 'tlta fet ouzzi pour* frois si2cíes *en orrihrej. Hi l'on* compore fes posiïion- *toltulèez onet lee pozitionz* oòseruJes, *l'oteord cat une confirmotion* Jcïotonte *del lors de* Newfon : *í'oztre* qui s'éeerte fe pins dr ïe position prdvuc est to Lune qui posse tnnfót en ovonce, tontót *en retord por lo poaition colculèe* en ua que *ce retord 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* ïuniaofoirr et ïec *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 u4rï 'ztïon 'Zen lits defogruu'ttofion

In view of thea *major* anomalies of the paraconic pendulum, we need to examine how accurately thea loia 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 laws8 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 ofa

fl) Allais, 1958, *Do we need to reconsider the feu lois de la* **gravitation,** p. 101-102. See also lement Allais, April 21, 1957, Anomofie- *du* tOUU*mCE1 **! du** *paroconiqua pendulum ä #up-* eitisotropic 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).

eonstet4s deviations for angular displacements between observation and theory is at least of the order of 1 sexagdsimal second of arc [2] cf which gives a relative error of the order of

(1) 1 / 90.60.60 x 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!*

Œ) Amn. deFob.dePæu:Mmoee,TomeIY(taTene);Y(Mecue)Y1Ma8 *tYtnœü,KX1Y Satume)XXYI1t(UzanusandNeptuzne),XXX1(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 ä 126.

The graphs on pp. 36 and 37 of this study show the following times Newtoniana determined using the orbital motions of the Moon, S01ei1, 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			
	CD.T.	A.E.	difference in second deœmpo C.D.T-NE.	gap in second bow
Soleil	18h44m53s, 89	I8h44m54s, 02	- 0,13	- 1,95
3fercury	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
Mara	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,Œ	- 0,75
£fronua	8 33 58,63	8 33 58, 63	0.00	0.ŒI
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 (l) to be the angle covered during the total duration of the observations, which is of the order of a aitcle, 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 the order of 3 10 - *, which is actually quite remarkable *.

It's a conclusion that goes against the grain of conventional wisdom, without any real discussion in f a c t , 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.

Prttision des u4rifications b la surface d e la terre des postu- lats de la mtcanique

2- The most accurate experiments in mechanics at the earth's surface have been those involving the se- conde pendulum. These experiments *show* the well-known relationship :

T=2*ŃUMgl

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.

(41 On the e4cular acctldration of the Moon's motion, see in particular P. Tisserand, 7'roitd *de Mćconique Cćlttie,* Tome III, rposJ dc l'cnsemóle *dez* I *Jones* refifiues eu mouvement dr lx Lune, Gauthiers-Villars, 1894, Ghapitres XIII, XVIII et XII- Voir tgalement W.M. Smart, Celestiof 3fechonirr, Longmans, 1953, Ghapitres 17, 18 et 19.

(5) These laws are so perfect that Hegel felt he had to give a md- demonstration of them. taphysique, fPhifoa'pfiir dr to *Noture,* trans. Vera, I, p. 293, Parag. 270. V0ir lee om- mentaires qu'en donnds Pareto, Unité *de Sociologie,* Y.I., p. 269).

Order o f magnitude of the anomalies observed in the motion of the paraconic pendulum ä anisotropic support

3- These figures are interesting to compare with t h e order of magnitude of the anomalies I've conat4ed. This order of magnitude is that of thirtieth of the Foucault effect [6], and the latter is of the order of three millionths of the pea@jjq- [7],

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. 126J

m" + p m = - 2 m sin L n'

2 _ I = 981 83 = 11,82

n" + p* n = 2 tu cin L m' As we have

Û = M 2

and } has components lm", In", 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 g /1

where ri is the amplitude. Thus we have

Eddy force p 2 tu sin L M p l ri _ 2 ni sin L p l o / PeeanteurM g g

According to my experiments of June-July **1955** (§ A1 and A4 above) m sin L

= 0.55 10' = 83 em = 3.44 = 0.1

Eddy force _ 2 . 0.55 10' . 3.44 . 83 . 0.1 / Peaanteur " %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 aideral day or the lunar day. From the astronomical point of view of planetary motion, we need to match them with forces whose int4grale eats 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 loia will be verified in the course of a planetary revolution ®.

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' times smaller and zero on average during a planet's revolution, they **would** probably be indistinguishable.

R- **L'h32x'f8ê'æ** 'Ie ïzi rompre **de f'eej>zee** 'f'iziez-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 observed4a *. 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-*, *the hypothesis of the anisotropy of inertial space is compatible with all observational data* *.

equations of motion of the paraconic 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.

(l) Allais, August 12 1955, ñfouvemcnts *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) PJanck, /zsi7ïotiôns ô lo Z'hy8igue, 1941, Flammarion, p. 256.

(4) See § E'.1.3 below.

paraconic with aniøotropic support by an anisotropy of the order of 10-* of the space of inertia generated by the influence of the aøtreø >.

• *Table* Xfł 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 Mi of the pendulum relative to its weight Mç is relatively increased by 1 + G in the direction of the celestial body i. Coefficient Gi 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 Om axis is oriented4 towards star i. Ceø rela- tions 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 §' of the azimuth and §' of the minor axis of ellipse 6,

For any orientation of star i's azimuth X, relations (5) and (6) are replaced by relations (7) and (8).

Other i exerts a *direct* influence on §' represented by re- lation (7) and an *indirect* influence on §' via 9' and
Airy's prediction. In fact, direct influence is *relatively negligible* compared to indirect influence 7,

Formally, relations (7) and (8) are Łout ä analogous to the expressions for Ø' and [ì' corresponding to current gravitational theory. *lseuh feeffix:ient-di{fërent.*

(5) The same explanation applies to the paraconic pendulum with isotropic support (see *Chapitrt* if, Section I).

(6) Allais, 1956, *TMorie du Pendule Poroconi9ue,* Fascicule III, n- 1103, Tnòïenu if, p. AI,6.

(D See note (11) in § B.2.5 above, p. 122.

ø' xcl (s' du r "si" xri.

Naturally we can define a **directi** on average **anisotropy of the** inertia **space** in poaant

(1) c sin 2 (X - §) = Z T¡ ain 2 (Xi - $)

• *Table* ZONE gives *the first* a_R *proximation of* the expres- **sions of** $' and Q' taking into account ä foia the Foucault effect, Ai [10]'s **preeeeasion** of the anisotropy of the 8uppp [11]$ 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 §' corresponding to the influence of the Moon (relations 3 and 4).

The order of magnitude of the observed value $§ 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 cl = 0.20.10 [6] of t¡ *(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 Ptndult Paraconique,* Table Tl in *Nett Appendix 28.*

(12) § B.2.6, p. 123.

An oction 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 [1*].

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 [!S-!6].

(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 pro-portional è o°/1 while the effect corresponding to the ac- tual theory of gravitation is proportional è e° f'Pohfenu XfZi, note 2).

This explains why the longer the pendulum, the less the eddy effect is disturbed.

In the caa of the Foucault pendulum in the Panthéon in 1852, the length of the pendulum was 67 meters and the amplitude ri des oscillationa 6tait de 0,06 radiana (§ E.2.3 ci-des- susy. On the assumption *of* the nnisotropy *of the inertia* species, the theoretical effect was about 220 times smaller than in the case of the paraconic pendulum. We have in fact

(o, 2/67)/(0,1*/0,83)1=/224

In the case considered by Dejean de Fonroque ffntrodtiction, § B.2.6, note 7 below, the pendulum 6was suspended by a wire about 1 metre long, and the initial amplitude ii 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 ti = 0.1 radian (about 6-). The o* /1 ratio was thus around 50 times higher than in my own experiments (0.78 * / 100) / (0.1 / 83) = 51.2.

(lö) From relations (1) and (2) of the 'Poéleou *XIII* and by posing k = 3 p ri / 8, tg = p' 'x ri / 4 , pj = p t¡ / 2 we deduce as a first approximation the relation

(1) $" = k [sin 2 (Z - $) + Zsin 2 (X¡ - $)]

Lee very interesting implications of this second-order differential equation will be examined in the Second part of this book, Chapter Z.

Determining onisotropy coefficients z¡

4 - Given the *very limited* data currently available, the precise determination of <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 (l) can be written as

(2) #" = k f sin 2 (Y - $) 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 $ 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) $" + 2 k f$ = 2 k f Y

If Y(tl varies relatively little over an experiment lasting 14 minutes = 840 seconds, the general int4gral of (3) is

(4) # =Y+AcosCt+BsnOt ri = 9 2 k f

For initial conditions

(5) $ = $$ ' = - tu sin L for t = 0

equation (4) is written

(6) $ = Y + (J - Y,) cos f2 t - $\frac{\text{sin L}}{\Omega}$ 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 foeusiZme ro-
I of this work, Chapter f, Section B) that we have approximately: p = jpg = 1.41 10 ,
and therefore

(zrï= 0.611 ' o*e - s " / n = 10284"- 2,86heureB

As a result " -imut $ *oscillates* around the limit plane with a period Ö9 Î'0£dre of 3 houra. For experiments lasting 14 minutes such oscillation does not occur and **l'azimut ϕ du plan d'osc**illation approaches the azimuth of the boundary plane during each ex**périence de 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 experiment8 , the third term of relation (6) corresponding to the eddy current effect gives an average effect of

' - 'f - m sin L cos n t

As the average cos f2 t over a 14-minute experiment is smaller than

At unity we have l #Jl < tu sin L .

(16) On the effects of the nnisotropy 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 f7'oôfeou *II* 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 coeffi - cients Cg and Cj corresponding to the plane of oscillation of the paraconic pendulum. above), we can assume that the

anisotropy coefficients of the Sun and Moon are of the same order of magnitude as the coefficients Cg and Cj corresponding to the current theory of gravitation (§ B.2.5 above, p. 123).

DMFLUENCE DTTN ASTRE i SUR LAZDMUT ET LE PETIT AXE DE LA TRAJECTOIRE ELLŒTIQUE DU PENDULE PARACOIQUE DANS L'THYPOTHESE DE L' SOTROPIE DE L'ESPACE DINERTIE

Notaliona

Om axis = direction of greatest inertia [1]
q = coefficient of the anisotropy induced by the other i [2]

Differential equations in m and ri

(1 (1+j)Œ +p2m=0 $p^2 = g/l$

(2) n + 2n=0 $\varepsilon_i << 1$

As a *first approximation*

(3) œ "+p2 "

(4) n + 2n =0

(5) $\phi_i' = \varepsilon_i p \frac{\alpha\beta}{\alpha^2 - \beta^2}$ coe 2

(6) @ = -q $\frac{p}{2}$ ri ain 2 9

***Any** axes Om , **On** ; Solution*

(7) $\phi_i' = \varepsilon_i \frac{\beta \omega \beta}{\alpha^2 - \beta^2} \cos 2 (X_i - \phi)$ Ofj = azimuth of the *other* f

'* – 2 in2(X-$)

(1) l'4otations in § B.2.3 above and the corresponding 'Foölenu 2V, p. 126.

(2) The anisotropy coefficient £; is a *dimensionless* coe;§'icient.

f3) These equations are to be compared with the equations deduced from the current theory of the universal te€'on and corresponding to the attraction of a star i IG B.2.5, Z'oöfeou W, above, p.128) .

(1) $\phi' = \frac{1}{p} K_i \frac{\alpha\beta}{\alpha^2 - \beta^2}$ cos 2 (X, - $)

(2) Ç= o sn2 -g

EQUATIONS OF MOTION OF THE PARACONIC PENDULUM IN THE LANISOTROPICŒMMPAEHESE OF THE DINERTIE SPACE AND IN THE CASE OF THEANISOTROPIC SUPPORT

Equotion of motion (as a first approximation)

(l) §' = - m sin L + $\frac{3}{8}$ p α β $p = \sqrt{g/l} = 3{,}44$

(2) #' = $\frac{1}{4}$ p' a 'f sin 2 (Z - $) + Z $\frac{p}{2}$ aqso2(-#)

Orders of magnitude of e - Illustration in the context of the L.une - Periodicity of 24h. S0mn.

Theoretical robbery of average lo tomposonia 'j de ' during an **exp-.** rience of duration At = 14 mn = p 1,

(3) $\bar{\phi}'_u = \frac{3}{8} p \bar{\alpha} \bar{\beta}$ a=0,1

3/8 [1 dt p/2 p/2 ᾱ rio 2(/-g)]

= 9.32 zj size 2 (Xj - $)

(4) **Order** of magnitude of #}; = **9.32** e rad./aec.

Order of magnitude of *average thief* $g (§ B.2.6, p. 123)

(5) 9§; = t3.186.10' rad./aec.

Corresponding order of magnitude of q from (4) and (S)

(6) e; = 0,186.10 ' / 9,32 = 0,20.10

According to (l) and (2), the effect of the onizotropy of the inertia space zon ¢ j and

pg;jj 2

(n $(\frac{3}{8} p \bar{\alpha})(\frac{p}{2} \bar{\alpha} \varepsilon_i)$ $\frac{\Delta t}{2}$ sio2(#-g) = $\frac{3}{16}$ $\frac{g}{l}$ ε* $\varepsilon_i \frac{\Delta t}{2}$ sin 2 $(X_i - \phi)$

(1) They indicate qu il s a t des the mean values over the duration At = 14 minutes = 840 seconds of the experiment.

O) **According to the** current theory of gravitation, the effect of universal attraction on ${ is proportional è

$$(\frac{3}{8} p \bar{\alpha})(\frac{1}{2p} \bar{\alpha} K_i) = \frac{3}{16} \bar{\alpha}^2 K_i$$

(2'ooïenu Off, note 31, and according to the relationship f7l

$$(\frac{3}{16} p^2 \bar{\alpha}^2 \varepsilon_i) / (\frac{3}{16} \bar{\alpha}^2 K_i) = \frac{g}{l} \frac{\varepsilon_i}{K_i}$$

ON MY EKPERONCES 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 sta- *tistically significant* amplitude, particularly peridic 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 been4 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 the 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, Esclangon's optical observations, and the interferometric observations by Miller (Chapitres *Uf and TV* below).
See also the p ase concordances highlighted de hase 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 ń l'Acadćmie des Sciences sur l'influence de l'anisotropie du support [2] 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 thńorbme 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 facŁeurs intervenant dans le mouvement du pendule paraconique *.

(1) See in particular § B.2 and D.2 of the Jntroducïion 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 €czire :

> *'Au toura* de *tonueraotiona priućea rttentes. te* mnnque *d'objectiuitt de mea* odversoires *eet deaenu incroyable.* Ainsi, *poroIt, te oil* que *j'oi cc* - ceptd *de* porfer *dona* te codre *du Cercle* Alezondre *Du[our* tendroit d *com - promettre mea* propres 7/s48ea *élont* dozsrt4 *qu'il y o, parmi lea membres du* Cercle Alexandre *Du[our, aertoinea* persozszse8 *ozzti-relotiuiatea et)ugée8 por Iô-m5me onli-acientijfiquea.*
> "It's like reuertu or Go/i/4e time".

(2) See the łntroduction 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 Commiaaion 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 [5], 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'Acodtmie oil publié* quelques *de moe* 2'*fotez; elle ne* peut, sons se *détonzidtrer, poureuiure leur publico-* tion : *dec rtcullote* ezpërimentnus, nïJntoires perce que ïe /rottement y joue un rdfe *prtpondtronl, cons* infdrët *pnrte 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-méme, non justi/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 *lnlrodutlion,* § 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,* ftJoïiiniion *d'un support o u s s i icotrope que* possiöïe (10 p.); the second, *Pendule poroconique à aucpencion ise - Irope. Délermincline dee uarioliona ou* cours *du tempe des toroctèriatiquee de lo corré - 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 Cùopitre ff, Section D).

• *From February 19S4 to June 30* ***1960,*** I continued my experiments on the paraconic pendulum with my two collaborators Jacques Bourgeot and Annie Rolland i n my laboratory at IRSID.

It was thanks to the decision of Pierre Ricard, then *Chairman of Industries mttallurgiques 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 o f two remarkable works on the history of Mechanics [1].

• 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 19SS-{turner 19S9

July 1953: IRSID decides to give me the resources I need for my research.

October 16, 1953: Installation of my IRSID laboratory.

1"fturier 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'Introdut ion* above, § D.1, P. 6d-65.
In July 1953 René Dugas was4 appointed Conseil *acienti ut* by .RS.D ***fiq***

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 p4riodicit4 pour lea **séries** autocorrelated.

2S moi J957 : *Note ä* l'Acadëmie dea Sciences sur la généralisation du test de Schuster au caa dea sëries autz'corrëIëes [2]

November 13 - December 23 1957: Cinq *Noter ö* l'Académie des Sciences sur mea expériences aur le pendule paraconique [2].

February 2, 1958: Lecture at the Ecole Polytechnique *"Font-il reconsidérer Ier lois de lo Gravitation î. Sur une nouvelle ezpë* rience *de Métanique",* organized by Cercle Alexandre Duffy [3]

mort-avril 1958: Installation of a second loborotoire dana la carrière 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.*

2-3O June 1958: Preliminary experiments in Bougival and Saint-Germain.

July 2 - October ¹, 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 [2]

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 [2]

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 enchafnëea 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 nouem6re *1959:* Conference at the Société dea Ingénieurs Civila de France, *"Faut-il reconsidérer leo lois de la* Grouitetion *î Nouveaux réoultaI: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 mirea 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 *mv "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 ph4nomànec considérée néces site* une *connoicconce oppro{ondie dt dizciplineinec trhc diuercec ïmtconique, gJophy-cique, oztronomie, ctaticti9ue, el mothtmotiquesl, elle implique des exptriencec dtlicotec, 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* innccessibtes si on *ne dizposoit pom de mochinez à colcu ler* 4leclroniqizea.
>
> RecAercôes *de ce* genre *ntceccitent benucoup de patience, noire d'ocharnemenl, 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 o f expfrtrntec*, it's ttlui-Iä".*

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 >-7.

• 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 pen- dules, 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 [10]

(6) See section B.3.2, p. 54-55, above.

f7) 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'Aclion Seienti@9ue dt* lo 04/ense JVoïiôno/e and by the *Cenlre National de lo Recherche ScïentiÇsg'ue.*
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.
lSan8 fe *disparition prtmoturét de Pierre Ricord 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 was4 relatively high.

Expenditure in *francs at the time* was approximately 2: 1954-1957: 5 million per year; 1958: 8 million 3; 1959-1960: 6 million. In all, from 1954 to 1960, the experiments cost around 34 million °- *, of which around 50 & were spent on
personnel *.

(1) *A proximotiutmtnt un million froncc de 19s reprtaente cent mille* /rencs *de II 'àe* recalls that the changeover from the old francs8 to the new francs in January 1959 was based on 1 new franc for 100 old francs8).
Dana ce qui auit lec dépener# de foncier 1959 ä juin J960 aout éooluétc tu onciena

(2) The remuneration o f my two collaborators, Jacques Bourgeot and Annie Rolland, amounted to approximately 2 million francs per year. Overtime pay for IRSI D aeents 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** d u* 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 o f 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* Sidërurgie (IRSID) from January 1, **1953** to December 31, 1956 and from January 1, **1959** to June 30, 1960.
The Comiïz *d'Action* Scienti/ique *de la* f1ë/ense Netionele (CASDN) covered expenses from January 1, 1957 to December 31, 1958.
The Centre Notionol *dt la* Aecùerche 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 eoüt of ezp riences**ériences**n 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* ezpdrimentous *et Ihto- riquea* e@ectués *du ltr octobre 1953 ou ltr octobre 1956 dont* le codre de l'Jnrfitut de***Recherche de*** *la Sidérargis* (12 p..), p. 6-7; December 2, 1957, *Recherchers**ur les mou-** utmentz du* pendule *poroconiqut* (6 p.), p. 4; and April 15**, 1959,** Note sur *l'éIoI oct nel dtz* recherches ef fe *financement* 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	"	
			100,

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 Slcienti uf ue de la Dtfen-e Nationale,* chaired by General Bergeron and later by General Guéri n, provided most o f t h e 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 ®.

(7) In my October 1956 memoir (10 p.) to the CNRS, I wrote :

> č/n p/zYnom8ne nouueou o 4t4 mčr en 4uždence e7 i/ es7 indJn/o6/e que /e4 0O/t4dgUg0CčS *du point de uue de* zzo4 conceptiO/t4 7/s4origue4 *du monde* /t/t\4igue /teuuen7 en fitre črg4 cO/t4iddro6/es
>
> *"Z.'dtizde syatčmatique del phdnom8nea* nouueoux o *PIE* dozt4 /e po44d *a txtroordinoire tcondil4*
>
> *'Nouc ne zourionc* d'oiIteur4 *trop couligner que* Io *pourzuile dc cel* rec/serc/ses ne pr4sen7e p/u4 *Oct uellement de* ri4g'Ue. Z-'exi4fence *du phtnomlne 4t udi4 e- I en e{fet 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*** £d4U/f *tts* 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* ***trša*** *firande,* et *il n'y a* actuellement//cmen7, zt *notre* ouc4, *Oucune* compo&t4O/t***possible*** entre /o *rentabilitš 8cientifique dčpenae de* 25 mi//iO/t4 *concatr4t b l'4t ude del mouuemeni- du pendule porotonique* et lo *rentabilil4 scientifiqize d ' une dépense marginale dgale* dont4 le *domaine* ***atomique".***

(8) In my M£moire of April 15, 1959 I wrote to the CNRS :

> *"Dana l'étal acluel* dc *la discussion l'ouia général de* 7ou7e4 les personno-/fit44 C0ztsu/t4e4 e47 §'u- t£4 P4'*A4'W/t4'8 expdFtzzteztto/es doiuen7 eme pour-sucuies …
>
> "3fes rechercfte4 oni čtd /inoncder ju49tf'ter :
>
> *a} par l'I stitut dc* flecfterrAes dc fo *Sid4rurgie du 1-^* nto6re *196S or J january J957,*
>
> *b1 por le Comit4 d'Action* Scienti ique *de la D4 ense Notionale du* ***1 er janvier 1957 au 1er janvier 1959,***
>
> *c1 por le CNRIS b titrt d'appoint en 1958 pour "*zoe *aomme lotale de*

On my request for support, the CNRS indicatedń on June 27, 1960 that it could not take over te financing of mea researcha and referred me6 A another organization *. In view of the closure of I'IRSID's laboratory on June 30, 1960, it was unfortunately only too obvious that aana the scientific backing of a Commission comp6tenŁe ńmanant A la foia de l'Acadńmie des Sciencea et du Centre National de la Recherche Scientifique le recours ńdure de finance-.
ment ńtait totalement irrćaliste 10,

ffollowing note 8)

> *'I-'interaention de I'IREID o tIt une* interuenfion *de dćmorrage* Qui n'*tIt poccible que gr8te ó to lorgesae de une de M. Ritard, oujourd'hui dtććf.* Cette interuention ne ,pouvoit se mnintenir, cor elle sortinit *du* domoine de Ióctiuitd normole de *I'IRISID.*
>
> *Z.'internention du CAfiiDN n' a eu lieu qu'ó titre* temporoire. dn jonuier *1956 Ie CNRS mait re(ua4 tout ce4dit* ef *deuanl* cežże sitzzation *Ie CAfiDN tlait inleruenu* aous to *condition que ann aide ce limiteait on financement de rez retherthec ntcezzairea pour foire to preuue de l'esictence intontea - t*'ible *du* pAJnom2ne.
>
> *Cetle preuue a ćlć apportće par mes* expdriences *de* juf//ef *1958 poursuivui- niem aimultonćment dont deux loborotoirea ó Eoint-Oermoi***ival à** *Bougiua ó t tim de distonce, te* second *tIonI aitut dom noc* corridre souterroine *auec plus de 60 mètres de retour rement de terrain.* Ces expériences ont montrd en eJet *que danc lec deux laboratoirec tež phtnomtnec tonctotta ont unt ztrutlure p4riodique tomporable.*
>
> This *prtuue 'iyani tIt* opporłće te *CASD* I *a eonsidtrć ann oction tomme* terminJe.
>
> "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 Direeteur gćnćral of CNTtS wrote to me:

> *'Lo* Commission *de* m#coni9ue gdndrole d fi9ueIle j'oi soumis votre de - *m'inde o* est *im J que* ce *problème, qui néceczitera de irha groaata dtpenaea, ta d€pocae* et vous suggdre de nous *odreaaer on Comilt InterminiatJriel de to Recherthe Hcienli sue. 68 rue de Bellech swe(*Paris *7tme)".*

(10) There**'s no doubt that** a **favorable scientific opinion would have enabled me to find** dans l'induatrie lea moyens financiera nćceasaires.

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 au- ditions, including my own. Within such a Commission, its various members would have been able to express their respective positions *explicitly and in a reasoned ma- niëre* 1°.

(11) In fact, and in view of the *oppro{ondiee* 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, plua or moina explicit, and the incessant propagation of rumors calling into question the validity of my experiments and mia r4sultäts [2]. [3].

(1) **Dans** In my ñfJmoire *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, i/ y o *incontestablement* un p/tdnoméne nouveau non ezja/i@u4 *ef l'histoire tout entière de lo physique ect lä pour témoigner de l'inttrht* que *peut présenter un lei phénomène.*
> Cet *in térhf* est *d'en font grond* que jusqu'ici l'histoire *de lo Mécanique* Cdteste n'o *comporté que d'tcla Ianti auccèa,* tes *onomoties* conrtotdes ne *consSituent* gJnJrnlement *qu'une fraction* négfigeohïe *des* pùdrzomêzte8
>
> *Un phénomène nouueou o été en évidence et il* est *indéniable que les* conrdquencer *du point de* vue *de* nor conceptions t/idori9ue# *du monde physique peuuenl en Sire mis considérables . ..*
>
> "L'ët *ude* **s***ayzlt moiq***ue des**p/zdztom+ztes *nouueoux o été dont le posté d'une ex:froordinoire ftcondilt".*

(2) See for example § E.6 above, pp. 188-192.

My eontradieteur8 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 grouilation?* (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* ezptiritnient 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/dêre *that the existence of an Iu- niaolaire phenomenon* ert *intonteatoble, mara que* ce *phénomène* est oisJment *ex:plicoble dont* le *tadre* de le *théorie habituelle. Tel outre ou contraire* retonn *lt bien* que lo *structure pézfiodigue* oô8eru4e, si elle dtoif r4e//e, seroif *absolument* inesplicéôie, *mois il* conteste *an rtolité.* Z.c *plus grond aeruice que pour - raient me rendre mes oduercoirea, te* seroit *de* rJ':f **uger** is common lo ré/u - *mmmdes* thtses *que j'ouonce. Je doute [ort q* **u'ils** *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 theory4.

- 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. Hél 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" ń following my September 1996 article, "Lee *exptrience de Doyton***C. Miller** J9£5-J926 et *to* Tfiëorie *de* to re/ntioitJ" shows that these *veryc* Uiuoc rumors continue ô

(4) **Yoir** 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 gćnćralisait 1'ćquation d'Hdly

(1) $$\Delta\varphi - \frac{1}{c^2}\frac{\partial^2\varphi}{\partial t^2} - \frac{2k}{?}\frac{\partial\varphi}{\partial ?} - k@ + 4xk6 = 0$$

to the eas of space anisotropy.

This text ńtait was, to say the least, 6Łonnant, because as I had indicated in my letter of April 24: *"Les r4sultats dont fait ttat ma Note sont in- contestablement nouveaux et leur exactitude mathtmatique est hors de doute"* >.

• In the *Rësumë* 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 dif- ficulties 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). lf 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 Michelaon's experiment for the very foundation of relativistic theory4 > , 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 toutea lee discussions qui ae poursuivies A partir de 1956 au sein de l'Acad6mie des Sciences et du CNRS i1 dtait *tout ó fait comprt- hensible,* et 6 vrai dire *tout ó foit justifit,* que l'on soit sceptique em *pre- mier abord* devant des exp6riences mettant en cause une thńorie de la graviŁation sans cesse v6rifińe depuis des aibcles.

(5) Louia de Bro I' dtait en fait d'outont plus *tlonnonte* qu'elle 6tait en *contradiction flogrante* avec la citation d'un de eee dcrite que j'ai plaeće en tfite de ce volume avant non re(p. 7 above). **Sommaire**
In fact, the H,my work I use is *purely* a tlidortme *of m'*i *IM.*
możiąue8 donż /o uo/idifd dżoif *inmnte8lable.*

(6) See *Cfinpitres* IV et Yłł ci-deeeoua. Voir ćga lement mon article d'aoht-sep- tembre 1996 de la revue polytechnicienne, La Jaune, et la Rouge, Lee Sxpćriences de ***Dayton C. Miller*** *1926-1926* et to 7'IiJorie *de ta Retotiuilć*. Cet artiele et mes rdponsee au *Courrier des Acteurs* sont reproduits dans l'Appendice *H* du Deusidme *Volume* de cet ouvrage (p. 31 ci-dessusua).

(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 reality4 of the anomalies observed, the responsible scientific authorities were reluctant to fund costly experiments.

But it was *totally unacceptable* to deny everything en bloc o *priori,* to refuse to **examine prdsent4eø analyses** on the basis of preconceived judgments, prejudiceø, and *"ttablished uéritts".*

In view of my experimental results, I reduced all discussions to three fundamental questions: - firstly, *did the evidenced funisola ires ptrio- dicitt- diurnes really exist?* The second was *whether* these *periodicities and their amplitudes could be explained within the framework of the accepted theory of gravitation î; and* the third was *whether these periodicities could be explained on the basis of other known phenomena î!*

In fact, n o one has been able to present any valid objections to my three *totally affirmative* answers *to* these three fundamental questions: - *the periodicitts 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 been4 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 pos8ibi- lit4 t o be heard.

(8) Section *B* above, p. 102-141.

(9) The only exception is the note from **Jean Goel** dated 21 April 1958 ä l'Académie des Sciences immédiatement réfut4e par les expériences cruciales de juillet 1958 (voir ct-deasua § 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 enŁendre me and judge the validity or otherwise of my experiments on purely scientific cri- teriaø.

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 [10]. In the end, to my knowledge and for reasons I do not know, this Commission was not constituted by the CNRS Board [1*].

(10) Here is the text of Jean Coulomb's letter of May 13, 1959:

"My Dear Colleague,
"ź-a Commiвeion de ***Mécan****ue o txo mint uoв demondee more* ***n a pas*** *tru deuoir prendre nut décision immédiatote.* ***She*** *demonized ã etre* ***éclairée*** *per a Commission de epJtiolistea.*
'Monsieur PJreв m'o odreeьé to mere dec members of cettt Commission and ***je fais*** ous8ičôt /e nëcessoire #our /es conuoguer.
*"****Je*** *regrettc 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 C o m m i s s i o n , the only ones I have received.

Inc dec more ct *dee motsI dieтussions, ct ä to* suite *du Rapport d'un dec membnee de to Commiseion de ñféconique du* C2'*fRE gut eel ter précent* et 9ue je remercie d'*ouoir bien* voulu вenir *aujourd'hui,* fe Commission *de* ñfčconique o proposé en Øirectoire *du* CNft *S de conctituer une* ***Commission*** *formée de* personnoíitd# *indé pendontec, en* f'esptce *lee* membres *de I Acadéмte dec* Sciences *gut c inléreccenl à* cette question.
'Lo Commiseion de ñfJtonique du C2'*fRE tвt iormée de perconnolitJв* compdtentes. bthe rcnd compte *de to gronde difficulty du sujet ...*
'Je cuts reuenu dte Ernie-Unic quelquec jourc event to rtunion du Direttoire. Je oi rencontrJ *deux personno title dont le poidc ttoit d4tici danc l'affoire. L.'une quí a un* poste *éleué done le C?\IRE m'a dix : "J'oi ton - jourø été port ison de* cette Commis#ion". *She sent me an oimoble letter* in mвi. l'ou7rea *a* 4/eude *situation* in *the Uniuereilé.* '7'*outeø* two m'ont *dix to* veitle *de to rtunion du Directoire: "Nouc minus conctiluer вette Commission et to*

partici erait.
*cet"****Le****question gut o été dtbotlue o ft de вanoir qui y*
be::demain use decision ncgotiue a êté prise. A credit of 2,500,000 *fronts was granted to me on the* understanding *that it would constitute the last* ;finonce- *ment of the CNftS".*

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?*
> "£e *dtni 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 dêlibtrë 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 ä urai say quite inadmissible.

A new phenomenon had been 4 i d e n t i f i e d . To test its validity, it was4 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 peraonnolity does not make i t impossible to explain the amplitude of conatoltea ptriodicittz, particularly that of 24 h. SO mn., therefore the framework of Ihéoriea 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* impossiöilitd. As for the real existence of periodicit4a constat4ea he had been4 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'f. Co9uot, f3 pages).

D'aiíleurs, *à oucun moment, outun* spdcioliste *de lo théorie de* le grouitotion n'avait conte8t4 fe cofcuf *de l'ordre de grandeur* dea amplitudes dea composantes pério- diques lunisolaires que j'avais publié dans ma *Noie* du 16 décembre 1957 ä l'Aeadémie des Sciences, 'Pùdorie *du pendule poroconique et* in;7uenee funisofoire. represented res- pectively for the Moon and Sun, in unit48 **C.G.S.** by the coefficients

$$C_l = g \frac{M_l}{M_t} \frac{r_t}{d_l^3} = 0,B62.10^{-13} \qquad C_s = g \frac{M_s}{M_t} \frac{r_t^2}{d_s^3} = 0,3\%.10\text{-!*}$$

and yet my opponents hadn't lacked the desire to contradict me.

(2) In8 8a May 1959 letter to Werner von Braun, Director of Netioriol Aeronoutics *ond Spoce hdminiatrotion,* General Paul Bergeron, former Chairman of the *Comité d'Aclion lstientifuf ue de* ***lo Œfenae*** *Hotionale,* wrote

> ***"Avant de vous écrire, j'ai*** yugd ndcessoire *to make them both labo-* rotoires *du* f ro;fesseur ***Allais*** *Idoni* one located d 60 mdtres sous terres oc- *tompogné d'Jminent z apétiolictec - dont deux* pro/essence d'***l'école*** *Pol ytechnique. A f t e r a diatucaion 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'oilleurc devoir conc aignoler qu'on courc de ces deux der- nièrec onrites, pluc de dis membrec de l'At mie des* Serrures et plus dc *trente personnolitéa éminentea, cpécialietea b des titres diuera de lo great - tation, aonl uenua uiailer, aoit aon laboratoire de Saint-Germain,* 8oïf son toöorotoire aouferzoin *de Bougiuo* ***ival.***
>
> *"Discuccione oppro(ondiec ont eu* lieu, non 8eu/emen7 Ô ces occo- *cieux,* meta *ègolement à pluaieurz reprises dans diuerc milieux ccienti- (* 2uea, *notamment ä l'Atod mie dec Scientec* et ou *Centre* 2'fotioriol *de la* ReeàercAe *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 been4 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 *Aerospoce Engeenering* the English version of my 1958 dissertation, *lshould the Cawe of Gravitation be Reconsidered?* and that same year the American *Gravit y 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 dêcioion when prominent members of the Académie des lsciences 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'dtot *octuel des re - tGrthez et le* /inoncement de8 *trououx du Profezzeur 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 pourzuiuicz. Il* en ert *de même des* personnoïifJ4 *zcientifiquez trh# nombreuzez 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 domina- tion of "established* truths" *and oppose the progress of science - r).*

This opposition to all progress probably remains as strong today as it was yesterday 7$ and I can have no illusions about how the present work will be received today, so much so that any express-ion of it will have a negative impact.

intolerable, as soon as it clashes with "established truths".

blies" 8y 9

(4) In his aforementioned letter of l * June 1960 (§ E.6.9, note 10, p. 192 above) Jean Leray stated doctorally:

"Les *pend mes de* Foucouït modernes *foncéionntnl d'oillturz donc frottement,* sons tinison oucune: ce sont les satetfiter orti/iciefs".

Such an aasertion certainly deserves to feature prominently in an antho- logy 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* ñféconi9ue 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 *ïstondolt.* **à** *Polytethniqut) :*

Ce qut l'on zubodort tu lui, ct conf des idées révolutionnaires, *remet - tant en qutction dtc poziuloiz concidéréa* comme *dtc dogmec por* certoinr *dtc membres de l'Acadtm ie dtz Scientez".*

Louis Rougier's memoir is reproduced in Appendix i D of Decrême vo-. lume de cet ouvrage.

(6) The burial of my experiments on the aniso- trope-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'iui lotion?* (p. 102, note 38) :

"The *outright* burial of *Miller's memoir* (from 1933) *porolts me un dec ccondolec dt la phycique 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.

(8i 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 uéritt, uouc htez perdu i vous* des *hoctilitéc 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 moniëre* 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 the hoatilit4 I was confronted with from the moment I published my first ?\Notes to the Académie 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* excel *we disagree ....*

"Lee dogmotism and sectorism are not scien- tific 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 *Foezoyeurz du Progrès. L.ce Mondorine tout re lee Pionniers äe* le Science, p. 3d7.