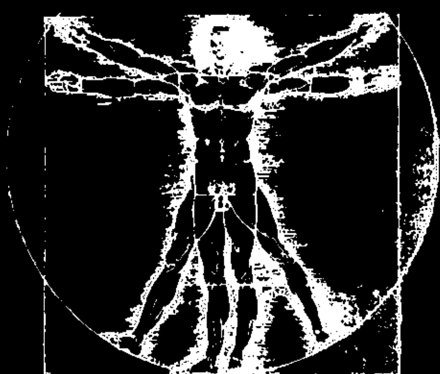
VOLUME

II

# DOCUMENTATION

OF

IL GRANDE GRIDO



Ruggero Maria Santilli

# DOCUMENTATION

# OF

# *IL GRANDE GRIDO*

*Volume II*

**Ruggero Maria Santilli**

**— 1984 —**

**Alpha Associates**

**Rome, Italy**

U.S. Address: 96 Prescott Street,
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DOCUMENTATION

OF

IL GRANDE GRIDO

VOLUME II

by

Ruggero Maria Santilli

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# PART XII:

# EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH, GENEVA, SWITZERLAND, AND DEUTSCHES ELEKTRONEN–SYNCHROTRON, HAMNURG, WEST GERMANY

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Geneva, January 31, 1978

Dr. R. SANTILLI
Harvard University
Lyman Laboratory of Physics

CAMBRIDGE - MASS.

02138 USA

Votre/Your ref.

Notre/Our ref.
PE/ED/FA/186

Dear Dr. Santilli,

We acknowledge receipt of your application for a Scientific Associate appointment.

This will be considered at the next meeting of the Selection Committee on April 11, 1978.

Candidates will be informed of the results of their applications during the ten days following the meeting.

Yours sincerely,

W. Blair

Head, Fellows and
Associates Service

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 14, 1978

Professor W. BLAIR,
Head, Fellows and Associates Services
CERN
CH-1211 GENEVA 23 Switzerland

Dear Professor Blair,

I would like to express my appreciation for the courtesy of your letter of January 31, 1978 indicating that my application for a Scientific Associate Appointment will be considered at the meeting of April 11, 1978.

In this respect I would like to indicate that a recent grant application with Professor Shlomo Ste rnberg, Chairman of the Department of Mathematics here at Harvard to the U.S. Department of Energy ( formerly ERDA) has been recently funded. As a result, I will have financial support for the next two academic years.

Owing to this new occurrence, I would like to confirm my application for a scientific associateship appointment, but modify my application for an appointment without salary. Whether possible some travel assistance would be welcome.

The reason for my interest in such an appointment is the following. I have been involved since some time in the study of the old idea that the strong interactions in general and the strong hadronic forces in particular are not derivable from a potential. The transition from the conventionally used forces derivable from a potential to the indicated broader form has a number of implications, particularly on mathematodological grounds.

The ultimate objective of these studies is to stress the need of subjecting to an experimental verification the validity within a hadron of those relativity and quantum mechanical laws (Pauli principle in particular) which have proved to be so effective for the atomic (as well as nuclear) structure. After all, the historical occurrence of the invalidity of previously established methods for the structure of the atoms or the more recent, equally historical discovery of parity violation, should not be ignored.

In essence, it appears that at a theoretical level the issue cannot be resolved beyond the level of personal opinions and conjectures which in any case remain far from a scientific truth. The only physically effective resolution of the issue is, in due time, via experiments.

The HADRONIC JOURNAL, of which you are eventually aware, has been organized in this spirit: to promote scientific debates on fundamental issues, in the traditional spirit of unsolved physical problems.

Clearly, the issue I am referring to goes considerably beyond my capabilities as an isolated researcher. My interest in a scientific associateship at CERN is therefore twofold: I would like first attempt to stimulate the awareness of CERN collegues on the need to conduct the indicated experimental verification, in due time. Secondly, I would like to collect the personal viewpoins of experimentalists (on the technical difficulties for a possible verification) as well as theoreticians (on the reasons for or against such an experimental verification).

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

RMS|is

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE

EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

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Geneva, April 18, 1978

Professor Ruggero SANTILLI
Lyman Laboratory of Physics
Harvard University
Cambridge, Mass. 023138
Etats-Unis

Votre/Your ref.

Notre/Our ref. PE/PM/FA/613

Dear Professor Santilli,

Your application for an appointment as Scientific Associate at CERN was considered at a meeting of the Selection Committee held on April 11, 1978. Your letter of March 14, 1978 was brought to the attention of the Committee.

The members of the Committee asked me to give you the following information. The budget and space available were very limited, and the number of applications received was exceedingly high. In these circumstances the Committee unfortunately was unable to offer you an appointment.

Yours sincerely,

W. Blair
Head, Fellows and Associates
Service

HARVARD UNIVERSITY

Area Code 617
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Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

November 15, 1978

Professor GEORGES CHARPAK
Experimental High Energy Physics
CERN
CH-1211 GENEVA 23, Switzerland

Dear Professor Charpak,

I am inviting you to take an active partecipation in the efforts recently initiated at the HADRONIC JOURNAL in relation of the experimental verification of the validity or invalidity for the strong interactions of established physical laws (Pauli's principle and Einstein's special relativity, in particular).

You are familiar with the current line of studies based on(the tacit assumption of) the validity of these basic laws for the strong interactions. I am here referring to quark oriented studies, including QCD. You are perhaps also familiar with the increasing concern by an increasing segment of our community in relation to the fact that, despite truly large investments over a rather long period of time, the fundamental problematic aspects of these studies have not been resolved and, according to the view of a group of physicists, are actually increasing in time.

I do not know whether your are aware of the fact that there exist a number of physicists in USA, Europe, Japan and other Countries who are actively working on the violation of basic physical laws for the strong interactions and the search for conceivable generalizations. This is, first of all, a clear expression of the fact that the laws considered simply do not have at this time an experimental backing of any relevance for the case of the strong interactions. Secondly, this occurrence, appears to be an expression of a rather profound dissatisfaction with respect to the actual physical effectiveness of these laws for the interactions considered, as compared to the fascinating physical effectiveness of the same laws when applied to the electromagnetic interactions. As editor of the HADRONIC JOURNAL, I have been particularly exposed to this scientific current and I believe you might be interested in its existence.

In essence, I have no words to express my personal concern on the current status of hadron physics. It appears that the situation is not only at the stage of mere opinions, but actually in limbo and will likely remain in limbo until the problem of the basic physical laws is seriously confronted by the experimentalists and, in due time, resolved.

I enclose copy of a paper by Professor D.Y.KIM (now at Cambridge, England) on a review-comment of the problem. This paper also contains the most relevant references which are apparently available at this time. In case you need additional copies and/or other material, please let me know.

I would appreciate your inspection of Kim's review and your assessement of the current state of the art by theoreticians on the identification of currently feasable proposals of specific experiments.

page 2.

Since I am not an experimentalist, I am unable to achieve such an assessement. I am, however, fully aware that we are at the very first steps of an expected long and laborious scientific process. I am also aware that the current state of the art is indeed rudimentary. But for the problem considered I believe in the traditional scientific process of trial and error, presentation of ideas and critical inspection by independent researchers.

In case you are interested, I would be happy to provide a more detailed presentation of my view, with a differentiation with respect to hadron and nuclear physics and with respect to relativity and quantum mechanical laws. In the final analysis, all these aspects appear to be related.

I did enjoy reading your article in the recent issue of the PHYSICS TODAY and I sincerely hope that "multiwire and drift proportional chambers" can some day also be used for truly fundamental experimental verifications, in addition to the valuable applications currently under way.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/cgg
encls.

HARVARD UNIVERSITY

Area Code 617
495-3352

Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

November 15, 1978

Professor WILLIAM J. WILLIS, Head
Isabelle Detector Division
Brookhaven National Laboratory
UPTON, Long Island, New York 11973

Dear Professor Willis,

I am inviting you to take an active partecipation in the efforts recently initiated at the HADRONIC JOURNAL for the promotion of the experimental verification of the validity or invalidity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions, with particular refrence to Einstein's special relativity and Pauli's exclusion principle.

You are aware of the current line of theoretical studies based on the (tacit) assumption of the validity of these laws for the strong interactions. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community of the fact that, despite truly large investments over a rather long period of time, the fundamental problematic aspects of these studies have not been resolved and, according to a group of physicists, are actually increasing in time.

I do not know whether you are aware of the existence of a significant number of qualified physicists in the USA, Europe, Japan and other Countries who are actively working on the violation of the laws considered in the arena considered, and on the search for possible generalized laws.

As editor of the HADRONIC JOURNAL I have been particularly exposed to this new scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists genuinely interested in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere opinions by individual or group of researchers either in favor or against basic physical aspects, but actually the entire theoretical efforts of this sector are IN LIMBO and WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONSIDERED BY EXPERIMENTALISTS AND, IN DUE TIME, RESOLVED IN UNEQUIVOCAL TERMS.

I enclose copy of a paper by Professor D.Y.KIM (now at Cambridge-England) on a review-comment of the subject. This paper also contains the pertinent references generally known at this time. In case you need additional copies and/or other material, please let me know.

page 2.

I would appreciate the inspection and assessement of KIM's analysis by you and/or some of your associates. I am particularly interested in knowing whether the specific experiments which have already been proposed (via measurement of the mean life of unstable particles) are actually feasable at this time and, if so, whether they are actually valuable for the problem of Einstein's special relativity under strong interactions. Since I am not an experimentalist, I am unable to reach this assessement.

The problem of Pauli's principle (and other quantum mechanical laws) under the same interactions appears to be complementary and, in the final analysis, deeply related to that of the relativity profile.

In its simplest possible form, the intriguing scientific controversy under way is the following. If the hadronic constituents are assumed as point-like, established laws are expected to apply in full. Quark-oriented studies are then consequential to a considerable extent. On the contrary, if the hadronic constituents are interpreted as charged, massive and NON-point-like particles, they result in a state of penetration of their charge volumes while within a hadron. Studies of this rather peculiar occurrence (absent in the atomic and most of the nuclear setting) indicate the necessary presence of strong forces more general than those derivable from a potential (variationally nonselfadjoint strong hadronic forces). In turn, these broader forces appear such to produce a breaking of the SU(2)-SPIN. Still in turn, such a breaking has such a fundamental character, to JOINTLY render inapplicable established quantum mechanical and relativity laws. In conclusion, experiments on the relativity profile are expected to have an "image" or counterpart of dynamical character as far as the quantum mechanical laws are concerned.

I did read with sincere pleasure and interest your excellent article in the recent issue of PHYSICS TODAY. Permit me the liberty of expressing the hope that "the large spectrometers" may some day be used also for the experimental verification of fundamental physical laws.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor JACK SANDWEISS, Chairman
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Sandweiss,

I am inviting you to take an active partecipation in the promotional efforts recently initiated at the HADRONIC JOURNAL in relation to the experimental verification of the validity or invalidity for the strong interactions of the fundamental physical laws experimentally established for the electromagnetic interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

You are aware of the current line of theoretical studies on strong interactions and hadron structure which are based on the often TACIT ASSUMPTION of the validity of the laws considered in the arena considered. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community on the fact that, despite truly large financial investments over a rather long period of time, the fundamental problematic aspects of the quark models have not been resolved and, as a matter of fact, are increasing in time according to the view of a group of physicists.

I do not know whether you are aware of the fact that there exist nowadays a significant group of qualified physicists in the USA, Europe, Japan and other Countries who are actively working on the VIOLATION of the laws considered in the arena considered, and are searching for conceivable covering laws.

As Editor of the HADRONIC JOURNAL I have been particularly exposed to this scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists with a genuine interest in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere OPINIONS by individual or group of physicists, but actually, in my view, THE ENTIRE THEORETICAL EFFORTS ON STRONG INTERACTIONS AND HADRON STRUCTURE ARE CURRENTLY IN LIMBO AND WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONFRONTED BY EXPERIMENTALISTS AND, IN DUE TIME, RESOLVED IN AN INCONTROVERTIBLE FORM.

Almost needless to say, I have encountered numerous oppositions (even in my own campus) against the very consideration of the issue. You can however rest assured that I intend to pursuit it until the experimental verifications under considerations become unavoidable.

page 2.

I enclose copy of a recent paper by Professor D.Y.KIM (now in Cambridge-England) recently appeared in the October issue of the HADRONIC JOURNAL on a review-comment of the issue. This paper also contains the known references on the subject. In case you need additional information and or material, please do not hesitate to call me.

Trusting in your scientific vision and interest, I would be grateful for your inspection and assessement of KIM's analysis. I am particularly interested in knowing
- whether the proposed experiments (via measurements of mean lifes) are actually feasible with currently available technology; and, if so,
- whether they can actually contribute to the problem of the validity or invalidity of Einstein s special relativity at small distances; and, if not
-whether alternative experiments are conceivable.

Since I am not an experimentalist, I am unable to reach such an assessment.

The problem of Pauli's principle and other quantum mechanical physics laws is expected to be complementary to that of Einstein's relativity, and viceversa (see Hadronic J. 1, 223 (1978), 1, 574 (1978) and 1, 1279 (1978)).

In its simplest possible form, the following intriguing scientific debate is under way. If the hadronic constituents (and all hadrons in general) are assumed as being point-like, the established relativity and quantum mechanical laws are expected to apply in full to the hadronic structure (and strong interactions in general). However, if the hadronic constituents are interpreted as being charged, massive and physical particles, i.e., non-point-like, they result to be in a state of penetration of their charge volumes while within a hadron. Studies of this rather peculiar situation (absent in the atomic and most of the nuclear settings) indicate the need of realizations of the strong interactions in terms of forces more general than those derivable from a potential (as in QCD), called variationally nonselfadjoint strong forces. In turn, these broader forces result to have such dynamical effects to imply the breaking of the SU(2)-SPIN (in the sense that the conventional notion of spin would be inapplicable, say, for a particle producted in the core of a neutron star). Still in turn, the breaking of the SU(2)-spin has such fundamental character to imply the JOINT INAPPLICABILITY of Einstein's special relativity and Pauli's principle . In conclusion, experiments on the relativity profile are expected to have a "dynamical image" as far as basic quantum mechanical laws are concerned.

I did read with sincere interest your excellent article on the recent issue of PHYSICS TODAY. In closing, permit me the liberty of expressing my personal hope that "the high-resolution streamer chamber" will some day be used for truly fundamental experimental applications.

Very Truly Yours

Ruggero Maria Santilli

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor DAVID R. NYGREN
Lawrence Berkeley Laboratory
Berkeley, California 94720

Dear Professor Nygren,

I am inviting you to take an active partecipation in the recently initiated efforts at the HADRONIC JOURNAL for the experimental verification of the validity or invalidity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle,

You are aware of the current line of theoretical studies on hadron structure which are based on the often tacit ASSUMPTION of the validity of the laws considered in the arena considered. I am here referring to quark-oriented studies, including QCD.

Perhaps, you are also aware of the increasing concern by an increasing segment of our community in relation to the fact that, despite truly large financial investments over a rather long period of time, the fundamental problematic aspects of the quark models have not been resolved and, according to some, are actually increasing in time.

I do not know whether you are aware of the existence of a significant number of qualified physicists in the USA, Europe, Japan and other Countries who are nowadays devoted to the study of the INVALIDITY of the laws considered for the strong interactions and to the search for possible covering laws.

As Editor of the HADRONIC JOURNAL I have been particularly exposed to this new scientific current and I believe you might be interested in knowing its existence.

The overall picture of theoretical hadron physics which emerges from this situation is rather distressing and such to call for genuine concern by physicists with genuine interest in the pursuit of fundamental human knowledge. In candid language, we are not only at the level of mere OPINIONS by individual or groups of researchers of this or that other inspiration, but actually, in my view, THE ENTIRE THEORETICAL EFFORTS ON HADRON STRUCTURE AND STRONG INTERACTIONS IN GENERAL ARE CURRENTLY IN LIMBO AND WILL REMAIN IN LIMBO UNTIL THE PROBLEM OF THE BASIC PHYSICAL LAWS IS SERIOUSLY CONSIDERED BY EXPERIMENTALISTS AND , IN DUE TIME, RESOLVED IN THE NEEDED INCONTROVERTIBLE FORM.

I enclose copy of a recent paper by Professor D.Y.KIM (now at Cambridge-England) appeared in the October issue of the HADRONIC JOURNAL on a review-comment of the issue, with a valuable reference list. In case you need additional copies and/or other information, please let me know.

Trusting in your scientific vision and interest, I would appreciate your assessement of this paper. I am particularly interested in knowing
- whether the proposed experiments (via measurements of mean life) are actually feasable with current technology or not (see the original proposals, refs. 14, 15 and 16); and, if yes,
- whether they are actually valuable for the resolution of the problem of Einstein's special relativity; and, in any case;
- whether alternative experiments are also conceivable at this time.

Since I am not an experimentalist, I am unable to reach this assessement.

The problem of the experimental verification of Pauli's principle and other quantum mechanical laws is expected to be complementary to that of Einstein's relativity, and viceversa.

I did read with sincere pleasure and interest your recent article in PHYSICS TODAY. Permit me to express my hope that, some day, "the time projection chamber" can be used for truly fundamental experiments.

Very Truly Yours

Ruggero Maria Santilli

RMS/cgg
encls.

HARVARD UNIVERSITY

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 7, 1979

Dear Drs. GEORGE CHARPAK, WILLIAM J. WILLIS, JACK SANDWEISS and DAVID R. NYGREN,

As you will recall, on November 15, 1978 I wrote an individual letter to each of you asking for advice and council on a rather crucial problem, the identification of the state of the art on the currently available proposals for the experimental verification of the expected invalidity (according to some) or possible validity (according to others) of the basic physical laws used in current trends in strong interactions. I was referring in particular to Einstein's special relativity and Pauli's exclusion principle.

I stressed in my letter to you that I was in need for such an assessement not only as an individual researcher, but also in my function as Editor in Chief of the HADRONIC JOURNAL. I also stressed that I am not an experimentalist. As such, I am not in a position to reach such an assessement, apart the selfevident expectation of a long way to reach maturity. The question was,however, how long? Is the proposal by Kim (Lett. Nuovo Cimento 12, 591 (1975))to test Einstein's special relativity via a measurement of the time-life of unstable hadrons truly lacking a germ of promise? is the proposal by Santilli ( Hadronic J. 1, 574 (1978)) to test expected small deviations from Pauli's principle in nuclear physics (via low energy nuclear experiments for nuclei obeying certain criteria of selection) truly unrealizable via available technology and without a germ of promise?

I also stressed in my letter that the validity of the laws considered in the arena considered is a mere belief at this time, irrespective of the autority of its source. This creates a condition of question on the effectiveness of theoretical studies in the sector. At the extreme, it may even invite a process to our scientific accountability. After all, we are spending truly large amounts of money in strong interactions, all based on the assumption of the validity of the basic laws. How long can we continue this situation? How long can we wait before hadron physics is brought back to the traditional approach of physics in fundamental issues, that via experiments rather than beliefs?

I feel obliged to express my disappointment that none of you has even acknowledged reception of my letter.

In the meantime, the situation in theoretical hadron physics has predictably deteriorated. The enclosed paper is a manifestation of this situation.

page 2.

It has been released for wide distribution (15,000 copies via the Hadronic Press)*to indicate to quark-committed colleagues that a critical inspection of quark conjectures is in motion (jointly with the study of fundamentally different conjectures for hadron structure). If they have technical arguments to disprove these criticisms, they must express them via scientific papers. The corridor-type of talks sometimes used by quark-committed physicists on quark-non-oriented studies is no longer effective or scientifically valuable. Nowadays, there are outstanding physicists in various Nations who not only question the quark models, but question the basic physical laws used in these models and are working at conceivable covering laws.

It is an easy prediction that the situation at the theoretical level will further deteriorate until the experimentalists assume their responsibilities, in this case, to initiate a predictably laborious, but essential study of the resolution of the basic controversies at the experimental level.

I sincerely hope you will reconsider your apparent negative attitude on these fundamental physical problems, despite potential, conceivable conflicts of the study considered with your current academic committments.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encl.

* this paper will be soon distributed to your institutions.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
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SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 14, 1980

Dr. L. VAN HOVE
Director
CERN
CH-1211 GENEVA 23, SWITZERLAND

Dear Dr. Van Hove,

As a gesture of courtesy, I am enclosing copy of a draft of my paper "Remarks on the theorems of inconsistency of Heisenberg/Lie/symplectic formulations" quoting your contribution on the topic of 1951.

Any critical advice would be gratefully appreciated.

As director of CERN you should be informed that at the HADRONIC JOURNAL and, to my understanding, also at other Journals, a moratorium on the publication of papers on nonrelativistic quark models has been recently implemented.

My personal editorial experience is rather significative. I submitted for referee a paper on the topic in 1979 to a physicist expert in quarks, and a mathematician expert in quantization. The quark expert recommended the paper for publication. The pure mathematician, expert in quantization, rejected the paper as fundamentally inconsistent, because of the activation of the no-go theorem on ( full)quantization, inconsistencies in the(pre-) quantization, intrinsic inconsistencies in the activation of the breakdown of the equivalence of Heisenberg's and Lagrange's equations, etc.

Regrettably, we had to dismiss the judgment by the quark expert, and rely on that by the independent mathematician.I should add that we have implemented a "moratorium", that is, a temporary suspension of judgment either in favor or against, until the issue is resolved. Also, QCD and other field theoretical settings are not included (at least at this moment, pending studies by mathematicians in the subject, to my knowledge).

It is a question for us of scientific ethics to avoid any preconceived restriction in the conduction of research, and actually solicite the view of colleagues of different orientation. We hope in this way to achieve a more mature judgment.

I do not know your personal view on the problem of the consistency or inconsistency of nonrelativistic quark conjectures. Nevertheless, you can rest assured that the expression of your view would be appreciated and welcomed irrespective of its orientation. Also, you can count on our best possible confidentiality.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls

P.S. I will be in Europe for a tour of invited lectures from February 24 to approximately March 12. I will be occasionally in phone touch with my parents in Rome, Italy (Dr. Ermanno Santilli, Via Virgilio Ramperti 19, 00159 ROME, Italy- Tel 06 43 81 507), and you can reach me there in case you so desire.

HADRONIC JOURNAL 3, 854-914 (1980)



**Remarks on the problematic aspects of Heisenberg/Lie/symplectic formulations**

**Ruggero Maria Santilli***
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Received February 14, 1980

Abstract

A number of problematic aspects of conventional quantum mechanical formulations have been recently focused. A few rudimentary remarks are presented in the hope of contributing toward a more adequate identification of the open problems, as a prerequisite for their future resolution.

*Supported by the U.S. Department of Energy under grant number AS02-78ER04742.

**THE INSTITUTE FOR BASIC RESEARCH**

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

July 14, 1981

Professor F. JAMES,
Data Handling Division
C.E.R.N.
Geneva, Switzerland

Dear Professor James,

Thank you for the courtesy of sending me copy of your paper "Determining the statistical significance of experimental results", which I have found one of the most brilliant articles in the field.

Here at our Institute we are currently attempting the setting up of a committee of experimentalists, theoreticians, and mathematicians to initiate experimental studies on the exact or only approximate validity for the strong interactions of the conventional physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity (the Poincare symmetry), Pauli's exclusion principle and other basic laws. The committee is expected to conduct a feasibility study for possible new experiments on this fundamental open problem. Jpintly, the committee is expected to assess the possible re-elaboration of old data, as well as to evaluate existing experimental information. Additional information is available on request. Your participation would be, in my view, invaluable. Perhaps, it would be the most challenging and physically significant applications of your studies in statistical significance.

For your information, we have made some progress in the topic in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified the following experimental information:
- the apparent, quite large, deviation from conventional values of the magnetic moments of hadrons under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the prediction of conventional theories in the angle of precession of polarized neutron beams within matter, according to the experiment by Forte et al;
- the apparent, also substantial, violation of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al;
- the apparent, also substantial, deviations from the predictions of the exact SU(2)-spin symmetry via 4 spinor symmetry experiments by Rauch et al (which DO NOT recover 720$^{\circ}$); and other data.

Admittedly, the experimental information is still preliminary; all data can be suitably manipulated (theoretically) to force compatibility with orthodox doctrines (and interests...); and all experiments could be, in principle disproved by future, more accurate measures. However, the information is such to establish the fact that the validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information, when taken together, points toward an alteration of the intrinsic, space-time characteristics of particles under strong interactions which is quite plausible theoretically (see below), and which, if confirmed by future tests, would imply the irreconciliable invalidation of the entire Poincare symmetry, as well as the trust toward the pursue of fundamental advancement.

Apart isolated attempts, no coordinate effort is currently under way in the U.S.A. in experimental high energy physics, to my knowledge. As you know, experimentalists in the field simply assume conventional electromagnetic laws as valid, and use them in the data elaboration for experiments in strong interactions. For instance, the Poincaré symmetry is currently used as a central tool for the data elaboration of deep inalastic scatterings, to mention only one case, but without clear experimental information on the validity of the symmetry considered in the arena considered. The experimental results then have more the character of physically valuable indications, rather than that of terminal measures, and this situation will persists until the laws used in the data elaborations are established experimentally in a direct and independent way. You may consult Sections 4.2 and 4.3 of my enclosed invited paper at the 1980 Clausthal Conference (HJ 4, 1166 (1981)) to have an idea of the difference in the experimental results depending on whether the basic laws are valid or in need of suitable generalization.

I presume you are familiar with the basic theoretical alternatives. If the familiar point-like abstractions of hadrons are truly effective for the strong interactions, there is no ground to expect deviation from conventional laws. In fact, points can only interact at a distance; the forces are then necessarily of potential type; and the familiar, local, Poincaré covariant, Lagrangian theories are consequential. BUT, all hadrons have a dimension of the order of the range of the strong interactions, and they are constituted by wave packets (rather than points). As a result, strong interactions demand the mutual penetration of wave packets for their activation. This, in turn, is a typical contact interaction in an extended region of space for which local/differential models are excessively approximative, and the notion of potential has no physical basis. Still in turn, nonlocal nonpotential interactions demand a nonunitary time evolution under which the electromagnetic characteristics of particles are not conserved, with consequential, irreconciliable invalidation of the entire (connected and discrete) Poincaré symmetry, and the need for broader physical laws.

A possibility of accomodating nonlocal nonpotential forces has been identified via the replacement of the conventional associative envelope of quantum mechanics via a suitable nonassociative, Lie-admissible, form, along much of the open legacy by Jordan, von Neumann, and Wigner. In turn, this appears to offer a genuine hope of generalizing atomic mechanics for point particles into a form for extended particles under mutual wave overlappings which remains invariant under unrestricted transformations of integrodifferential type. A feverish activity is now under way in the studies along these theoretical lines, under the name of Lie-admissible formulations. What is important for this letter is that these studies are producing alternative theoretical tools for the data elaboration of experiments in strong interactions, as well as the technical identification of the conditions under which a test of a basic laws is credible.

You should recall also that these possible deviations from orthodox views in physics are strictly internal effects for systems under strong internal forces, and that they are not detectable from the outside via long range electromagnetic interactions. In fact, the clear unitarity of the time evolution of a hadron under long range electronagnetic interactions (e.g., for a proton in an accelerator) by no mean implies the unitarity of the time evolution of each constituent. You can have a schematic view of this situation by considering the Earth as isolated from the rest of the universe.

When seen from the outside, the time evolution is canonical, and the total energy is conserved. However, the motion of internal systems (such as a satellite during re-entry in atmosphere) occurs according to a noncanonical law, as a necessary condition to prevent perpetual-motion-type of approximations (in fact,nonconservative forces are non-Hamiltonian by conception). In the final analysis, our Earth has resulted to be a truly complex system beyond simplistic, Lagrangian/Hamiltonian models, and can be conceived as a Newtonian image of the structure of hadrons and nuclei in exactly the same measure as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to emphasize the complexity of the problem I am inviting you to participate. In fact, the acquisition of true scientific knowledge in the problem calls for direct measures under strong interactions, which is not an easy task. The problem also calls for an assessement of the impact of unverified theoretical assumptions in the data elaboration. A most important question is exactly in your field, and consists of the identification of the "scientific credibility" of existing experimental information in high energy physics in regard to the validity of basic laws under strong interactions.

However, permit me to confess candidly that we do not see the complexity of the problem as a reason to justify inaction, nor we accept supinely predictable attempts to prevent the acquisition of fundamental new knowledge. After all, the open character of the basic laws under strong interactions is too well known (after several conferences and countless articles) to justify the continued ignorance of the problem without risking questions of scientific ethics; the human and financial resources we spent in the development of the theory of strong interactions are too huge to justify ignorance of the basic aspects without risking dangerous administrative unbalances; and the implications (e.g., for controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and unpredictable consequences.

Please do not feel obliged to reach a final decision in any direction following this letter. Perhaps, you can follow our efforts, and decide the initiation of active involvement at some later time. On our part, we would simply need the indication of a sincere interest, for us to keep you informed. Our group will gather at the forthcoming

-FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS to be hald here in Cambridge from August 3 to 7 under partial support by the U.S. Government via DOE;and
-FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, to be held in France from Juanuary 5 to 7, 1982 under partial support by the French Government via local Institutions.

In case you can attend these meetings either as an observers or as an active participant, you would be sincerely welcome.

Very Truly Yours

Ruggero Maria Santilli
Chairman of the Board of Trustee and Director
THE INSTITUTE FOR BASIC RESEARCH
RMS-ml
encls.

PR - Öffentlichkeitsarbeit

**DEUTSCHES ELEKTRONEN—SYNCHROTRON DESY**
NOTKESTR. 85 · 2000 HAMBURG 52 · TEL. 040—89 980 · TELEX 2 15 124 desy d · TELEGR.-ADR. DESY HAMBURG

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
attn. M. Mary Lou Wright
Cambridge, Massachusetts 02138
U.S.A.

August 17, 1982

Dear Madam:

The name of our Director is

Volker Soergel.

In German we call him Prof. Dr.

DESY is lead by a Directorate of five members of which Prof. Soergel is the head.

The other members are:

Richard Laude (Administration)
Prof. Paul Söding (Research)
Dr. Wolfram Schött (Services)
Prof. Gustav-Adolf Voss (Accelerators).

Yours sincerely,

P. Waloschek

( P. Waloschek )
DESY-PR

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859

September 7, 1982

Ms. BETTINA KLOPRIES, Librarian
DESY
Notkestr. 85
2000 HAMBURG 52 W. Germany

Dear Ms. Klopries,

I am writing you in regard to my letter of August 27, 1982, requesting information containing names of the Director General or DESY and its primary officers.

Please be informed that I received this information per a letter dated August 17, 1982, from DESY–PR, P. Waloschek, several days ago.

Thank you again for your cooperation.

Sincerely,

Mary Lou Wright

(Mrs.) Mary Lou Wright
Secretary

mlw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

December 22, 1982

Professor H. SCHOPPER
Director General
CERN
1211 GENEVA 23, Switzerland

Dear Professor Schopper,

Our Board of Governors is preparing a report on the current status of High Energy Physics for submission to President Ronald Reagan, and to appropriate U. S. Governmental Agencies.

The outcome of the experimental search for the $W^{\pm}$ and $Z^{o}$ bosons currently going on at your Laboratories, whether positive or negative, is important for the finalization of our presentation.

We would therefore appreciate the courtesy of forwarding to us an indication of the current status of the search for the heavy bosons,even a preliminary and tentative one, for our own information, as well as for inclusion in our report.

We believe that our report may be of value also for your Laboratories, inasmuch as it touches on certain fundamental aspects of contemporary trends in strong interactions. It would be therefore a pleasure for us to send you a copy of the report.

I would like to take this opportunity to wish you and all at CERN our best for a happy and prosperous 1983.

Best Personal Regards,

Ruggero Maria Santilli
President and
Professor of Theoretical Physics

RMS/mlw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

January 20, 1983 *Ruggero Maria Santilli, Professor of Theoretical Physics and President*

Professor H. SCHOPPER, Director
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH
1211 GENEVA 23, SWITZERLAND

Dear Professor Schopper,

I am taking the liberty of recommending, most respectfully, to you and to your associates:

**The consideration of the experimental resolution at CERN of the exact or only approximate validity of Einstein's special relativity for the interior of systems with strong interactions.**

I enclose a general description of the studies conducted until now which, even though non—technical, contains sufficiently diversified information indicating that quantitative studies of the problem are within experimental, theoretical, and mathematical reach.

A collegial way to proceed would be the setting up of a Committee of Study for the purpose of:

(a) Evaluating the pitfalls of the arguments conceived in the hope to nullify the need of the tests (see pages 78-81 of the enclosed report for a review);

(b) Identifying and assessing all existing proposals (such as Kim's proposal to measure the mean life of unstable hadrons in flight at different speeds);

(c) Pointing out theoretical topics deserving further study as a necessary pre-requisite for effective tests (such as Mignani's nonpotential generalization of the potential scattering theory currently used at your Laboratories for the data elaboration of experiments in strong interactions);

(d) Identifying the equipments at your Laboratories which appear most promising for the tests (by keeping in mind that we are referring here to the new challenge of actual measures under strong external interactions); and

(e) Identifying new equipments that appear needed for low-energy, highsentitivity and moderate costs, (such as the neutron interferometers used in the main available test described in Section 3.2).

In case you are interested in additional information, you can count in my best possible assistance, including my availability to visit your Laboratories at some mutually convenient time. The same holds for all other members of our team.

But, most importantly, please keep in mind the ultimate motivation underlying our research efforts and this recommendation: the need for scientific accountability vis-a-vis our societies. In fact, we are all spending large public sums in strong interactions. Most of these public sums are spent on the basis of a mere belief of the validity for the strong interactions of physical laws clearly established only for the electromagnetic interactions. Scientific accountability then suggests that we de—emphasize all perso—nal theoretical views, whether in favor of old basic laws or in favor of suitable more general laws, and establish the physical foundations of the current theories of strong interactions in the only scientifically possible way: via direct experiments.

Very Truly Yours

Ruggero Maria Santilli
President

cc: Drs.E.GABATHULER,R.KLAPISH,E.PICASSO, J. PRENTKI, A.M.WETHERELL, P.ZANELLA, et al.
CERN

RMS-mlw
encl.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

January 20, 1983

Professor VOLKER SOERGEL
Director
Deutsches Elektronen–Synchrotron
Notkestrasse 85 2000 Hamburg 52, West Germany

Dear Professor Soergel,

I am taking the liberty of recommending most respectfully to you and to your associates:

The consideration of the test at DESY of the exact or only approximate validity of Einstein's special relativity for the interior of system with strong interactions.

I enclose a general description of the studies conducted until now which, even though non–technical, should contain a diversification of elements and ideas confirming that quantitative studies of the problem are within reach.

A collegial way to proceed wouldbe the setting up of a Committee of Study for the purpose of:

(a) evaluating the pitfalls of the arguments conceived in the hope to nullify the need of the tests (see the last section of the enclosed report—pp. 78-81—for an informal review);

(b) identifying all existing proposals(such as Kim's proposal on the measure of the mean life of unstable hadrons in flight);

(c) pointing out theoretical topics deserving further study as an essential pre-requisite for tests (such as Mignani's studies on the nonpotential generalization of the conventional potential scattering theory currently used at your Laboratories for the data elaboration);

(d) identifying the equipments at your laboratories which appear most promising for the tests (by keeping in mind that we are referring to actual measures under external strong interactions);

(e) identifying new equipments that appear recommendable at some future time (e.g., of the type of the neutron interferometry used in the main test described in Section 3.2 of the enclosed presentation).

In case you are interested in additional information, you can count on my best possible assistance, including my availability to visit DESY at some mutually agreable time. The same holds for all other members of our group.

But, most importantly, please keep in mind the ultimate motivation underlying our research efforts and this recommendation: the need for scientific accountability vis-a-vis our societies. In fact, we are all spending large public sums in strong interactions. Most of these public funds are spent on the basis of the mere belief of the validity for the strong interactions of basic physical laws established only for the electromagnetic interactions. Scientific accountability clearly suggests that we de-emphasize all personal theoretical views, whether in favor of established laws or in favor of more general laws, and establish the physical foundations of the current theories of strong interactions in the only scientifically possible way: via direct experiments.

Very Truly Yours

Ruggero Maria Santilli
President
cc:ProfessorsP.SODING, andG-A. VOSS, DESY
RMS-mlw
encl.

EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

European Laboratory for Particle Physics

DIRECTOR-GENERAL

CERN
CH-1211 GENEVA 23
SWITZERLAND

DG/1024-83

Professor Ruggero Maria SANTILLI
President
The Institute for Basic Research
96 Prescott Street

CAMBRIDGE, Massachusetts 02138
USA

Geneva, 1st February 1983

Dear Professor Santilli,

Thank you very much for your letter in which you inform me that your Board of Governors is preparing a report on the current status of high energy physics.

With great pleasure I am prepared to give you the information on our boson search, in particular as you certainly have heard the W has been discovered here recently. Enclosed you will find a copy of the paper of the UA1 experiment which describes this discovery. The second experiment, UA2, has similar results and a paper will be available very soon. I shall send you a copy as soon as I receive it.

Since the production of the $Z^0$ is about a factor of 10 lower than the production of the W the chances to have seen Z particles so far were very small. However, we are starting a new proton-antiproton run in our SPS in April, which will last until July. We hope very much that during that run sufficient luminosity can be accumulated in order to see also the $Z^0$.

If you need any more detailed information please let me know.

I certainly would be very much interested to receive a copy of your report.

With best personal regards,

Sincerely Yours,

H. Schopper

Herwig Schopper

Encl.

Telephone: GENEVA (022) 83 23 00 — Telex: 2 36 98 CH — Telegram: CERNLAB-GENEVA

# EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

European Laboratory for Particle Physics

DIRECTOR-GENERAL

CERN
CH-1211 GENEVA 23
SWITZERLAND

DG/1092-83

Professor Ruggero Maria SANTILLI
President
The Institute for Basic Research
96 Prescott Street

CAMBRIDGE, Massachusetts 02138
USA

Geneva, 22 February 1983

Dear Professor Santilli,

Thank you for your letter of 20 January 1983 and for the copy of your report outlining the work carried out at the Institute for Basic Research.

It is clear that tests of both restricted and general relativity are of fundamental importance. It is equally true that experiments to test these theories need to be of considerable sophistication and carried out with a very high degree of accuracy. It is not possible to judge whether or not the CERN Laboratory is a suitable place to carry out such experiments until detailed proposals have been put forward as for other experiments in high energy physics. Presumably such proposals would only be elaborated after your suggested Committee of Study has reached some conclusions on the five topics (a) to (e) listed in your letter.

Yours sincerely,

Herwig Schopper

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

March 2, 1983

Professor HERWIG SCHOPPER
Director General
CERN
CH–1211 GENEVA 23, Switzerland

Dear Professor Schopper,

On behalf of all I.B.R. members, I would like to express our sincere appreciation for your kind letter of February 1 (arrived during my absence) as well as our congratulations for the outstanding discovery of the W's at CERN. We shall treasure the paper you kindly mailed to us among the memorabilia of our Institute.

You will be pleased to know that, as you can see in the enclosed personal correspondence with The New York Time, we are considering to join others in the recommendation of Dr. C. Rubbia to the Nobel Committee.

We are working on our report on experimental high energy in the U.S.A., and it will be a pleasure to mail you a copy whenever completed. In the meantime, you might be interested to know the main ideas of the report in case of any value to CERN.

**Scientific scene created by the discovery of the W's.** In our view, the discovery of the W's signals the beginning of the end of an era in particle physics. In fact, we have now a new scientific scene in the sense that, besides the predictable discovery of the $Z^0$ (and the confirmation of the W's), *there are no more truly fundamental new particles to discover.* The issue created by this novel situation is therefore the following: which is a truly fundamental experimental program for future pursuit? The answer we submit is: to achieve the experimental resolution of the exact or only approximate validity of Einstein's special relativity under strong interactions, including the discrete and continuous components of the Lorentz symmetry. I mailed to you on January 20, a report on this proposal (ref. [1]). Besides being nontechnical and preliminary, this report is highly insufficient on numerous aspects. Permit me to add here a few comments for whatever their value.

**The need for the test of the Lorentz symmetry under strong interactions.** Stated as simply as possible, the need is created by the fact that all available direct tests, even though highly tentative and inconclusive, point rather clearly toward deviations from the Lorentz symmetry. The need for the experimental resolution of this truly fundamental problem one way or another is then consequential. The arguments often heard in academic corridors in the hope to nullify the need of the tests via the citation of indirect experiments (where the Lorentz symmetry is assumed) should be treated with care owing to their possible manipulative intent (see Section 5.3 of ref. [1]).

Regrettably, the topic is plagued with prejudices, misconceptions, and even religious beliefs. For instance, it is often heard that isolated systems of particles "must" obey the Lorentz symmetry. The violation of the Galilei symmetry for classical, nonrelativistic, isolated systems is unequivocably established in nature by closed non–Hamiltonian systems (think of our Earth when seen from the outside as isolated: the total conservation laws hold, but the internal forces are strictly non–Hamiltonian, by therefore preventing the applicability of the analytic–algebraic–geometric foundations of Galilei's relativity).

It is evident that the classical physical reality does not constitute grounds for the necessary existence of a counterpart at the particle level. Nevertheless, available indications are sufficiently serious to warrant the experimental resolution considered.

In essence, when particles can be effectively approximated as being point–like, the Lorentz symmetry CANNOT be broken, no matter what the interactions are. This includes the virtual totality of the electromagnetic interactions, as well as several aspects of weak interactions (e.g., semileptonic decays).

However, when particles cannot be effectively approximated as being point–like, we have the opposite situation, that is, we have difficulties in preserving the Lorentz symmetry as exact. In fact, once we acknowledge that perfectly rigid objects do not exist in nature, we see the possibility of deformations of the extended charge distributions of hadrons under strong interactions, in which case the rotational symmetry CANNOT be preserved as exact. Even ignoring all other arguments, the breaking of the remaining components of the Lorentz transformations follows.

To put it differently, the exact validity of the Lorentz symmetry for a proton in a particle accelerator constitutes no final indication on the problem of the validity or invalidity of the same symmetry in the interior of the proton.

In fact, the trajectory of the center–of–mass of Earth in the solar system strictly obeys Galilei's relativity as well known, while, as equally well known, the same relativity is broken in interior open problems.

A prejudice lingering in current academic circles is therefore the dream that available experimental information on high energy particle scatterings constitutes sufficient ground to claim the validity of the special relativity under strong interactions. Equally prejudicial in our view is the hope of reaching deviations from the Lorentz symmetry via such experiments. Indeed, all these experiments are conceived for exterior, closed, conservative, center–of–mass scatterings. To look at deviations under these conditions would be the same as looking at deviations from the Galilean character of Earth's center–of–mass trajectory in the Newtonian treatment of the solar system!

For these reasons we consider fundamental that, to be meaningful, the tests of the Lorentz symmetry must be conducted under actual OPEN NONCONSERVATIVE CONDITIONS DUE TO EXTERNAL STRONG INTERACTIONS. Once this crucial aspect has been resolved, then the formulation of the complementary problem for the exterior closed treatment can be consistently achieved.

To put it differently, validity of the Lorentz symmetry under electromagnetic interactions is established not only for exterior closed systems, but also for the open interior ones. In fact, Dirac conceived his equation for an electron under the exterior electromagnetic field of a proton. What we are advocating is essentially the test of the equivalent of Dirac's conception for external strong interactions.

Numerous additional prejudices exist in the current view of the problem. Regrettably, their treatment here would render the length of this letter prohibitive.

**Status of currently available direct tests.** To my best knowledge, the most salient, direct tests currently available, are the following [with the understanding that this letter is being written because of their lack of conclusive character].

(A) *Test of the rotational symmetry of nucleons under low energy nuclear interactions.* It has been conducted for a number of years by Rauch et al [2] via neutron interfermetry. The most recent results indicated about 1% deformation of the spherical symmetry of nucleons within the fields of Mu–metal nuclei, exactly as predicted by Eder [3] jointly with other predictions (also apparently verified, such as the joint anomalous behaviour of the magnetic moment, and the slow–down of the angle of spin precession). There is nothing mysterious here. We merely have the deformation of the sphere xx + yy + zz = 1 into the ellipsoids xax + yby + zcz = 1 (a,b,c > 0) caused by intense fields, with the consequential manifest loss of the rotational symmetry.

(B) *Test of the Lorentz boosts.* The best ones available are those reviewed in ref. [4] regarding the mean life of unstable hadrons in flight (mesons and kaons). I hope you can see in this independent work by H. B. Nielson of NORDITA the plausibility of deviaitons from the Lorentz symmetry. Again, we have nothing mysterious here. In fact, you certainly remember the old idea of nonlocal (integral) dynamics in the interior of hadrons (e.g., E. Fermi), in which case you cannot apply the analytic–algebraic–geometric foundations of Lorentz transformations, let alone the transformations themselves. Once you have internal departures, they manifest themselves via departures from the mean life [4].

My personal view on the problem is the following. I believe in the invariant xx + yy + zz – tct within the physical conditions originally proposed by Lorentz, Poincaré, and Einstein, that is, for motion in vacuum. It is well known that, for motion within physical media, the speed of light ceases to be constant, to acquire a dependence on local physical quantities (time, coordinates, density, wavelength, temperature, etc.). Also, physical media are manifestly inhomogeneous and unisotropic. The preservation of the old invariant xx + yy + zz – tct in classical material media is therefore deprived of scientific value. The minimum we can do is to represent the speed of light as it is, i.e., as a function c = d(t,r, . . .), and admit the inhomogenuity and unisotropy of the media, by therefore resulting in the generalized local invariant xax + yby + zcz – tdt. The local loss of the Lorentz symmetry as conventionally known is then unavoidable, in my view.

The plausibility of a generalized invariant in particle physics is self–evident. In fact, the moment you accept the extended character of hadrons under strong interactions, you have motion of particles within a medium of other particles. Alternatively, the belief that the invariant xx + yy + zz – tct is exact in the interior of a proton may well result to be of mere religious–non-scientific character. At any rate, the issue is too fundamental to be left at the level of personal views, and must be resolved via experiments in due time.

The distinction we are alluding here is the following. The homogenuity and isotropy of the empty space is so evident to prevent sufficient motivation for their additional experimental verification at this time.

However, when extended particles (such as hadrons) move within a sea of other particles (called the "hadronic medium"), the idea that they keep moving in vacuum does not seem to have scientific value. The most logical approach is therefore that of admitting the existence of new media composed of space filled up with wave packets of particles and radiation. The inhomogenuity and unisotropy of such medium is then as evident as the deformation of a perfectly spherical object. Thos loss of the Lorentz invariant and symmetry under these conditions is then as evident as the loss of the rotational symmetry under the deformation of a sphere into an ellipsoid.

Experiments on the Lorentz symmetry under exterior strong interactions should therefore test the nature of the actual medium in which motion occur, and NOT the homogenuity and isotropy of empty space, which is out of the question for us.

(C) *Test of the time–reversal symmetry in open nuclear reactions.* In its best available form, it has been measured numerous times by H. E. Conzett (Berkeley Labs.), R. J. Slobodrian (Quebec) and others [5]. The deviations from the exact symmetry are quite impressive. Again, there is absolutely nothing mysterious here. In fact, all dissipative (open) nuclear reactions are time–asymmetric. You can trivially see it as follows. Consider the Hamiltonians H of these reactions which, as you know, are non–Hermitean. Decompose them into the product of a Heritean term E = energy, and a "dissipative" term C. Then, $H = EC$ and $H^+ = C^+E$, and the time evolution assumes the manifestly time–asymmetric (Lie–admissible) form $i\dot{A} = A \triangleleft E - E \triangleright A$, $\triangleleft = C$ $\triangleright = C^+$.

Note that, by construction, the time–asymmetry ceases to exist when you implement the system into a closed form, i.e., a form for which the total Hamiltonian is Hermitean and conserved.

Note that $i\dot{E} = E \triangleleft E - E \triangleright E = ECE - EC^+E \neq 0$ as a necessary condition of consistency (the reaction being open by assumption). Thus, if you impose conservation, you recover automatically the antisymmetry of the product, i.e., $i\dot{H} = HCH - HCH$, $C = C^+$. Mathematically, you pass from the Lie–admissible algebras, to the simpler Lie–isotopic algebra with product ACB – BCA. The trivial, simplest possible Lie product of current use, AB – BA, is ignored here because excessively dependent on the point–like approximation of particles.

Regrettably, the measures by Conzett, Slobodrian, et al [5] appear to be disproved by re–runs at Los Alamos; the situation is now in somewhat scientific disarray; and the need for a resolution by a third, independent party is essential.

**The lack of scientific disaster in case of confirmation of departures from the Lorentz symmetry.** A number of colleagues have the impression that the experimental confirmation of departures from the exact character of the Lorentz symmetry would constitute a sort of scientific vacuum. Nothing is more removed from the truth. In fact, the mere possibility of departures is stimulating an enthusiastical thrust toward the generalization of old ideas. For instance:

(A') Theories leaving invariant the "deformed charge distribution" $xax + yby + zaz = 1$ have been constructed via a step–by–step Lie–isotopic generalization of the conventional theory of rotations;

(B') Theories capable of leaving invariant the "deformed charge distribution in space–time" $xax + yby + zaz - tdt$ are well under way. Their construction is made possible by the Lie–isotopic lifting of the Lorentz group in which the original group is deformed into a form admitting the inverse of the new metric as the identity, that is, as the Casimir element of order zero. Its invariance is then trivial for all functional dependences of the speed of light.

(C') The possibility of a time–asymmetry is promoting a virtual explosion of novel studies in fields even outside particle physics, such as statistical mechanics or biophysics. The mathematical theory is, this time, the Lie–admissible generalization of the Lie–isotopic theory as indicated early.

**The apparent beautiful compatibility with quark theories and the W's.** Another rather frequent misconception is that a departure from the Lorentz symmetry is in conflict with quarks. Again, nothing can be more removed from the truth. In fact, the lack of exact character of the Lorentz symmetry would merely imply that quarks cannot be considered, strictly speaking, as elementary. In such a case, quarks would merely be COMPOSITES OF MORE ELEMENTARY ENTITIES. As a matter of fact, the approach appears to offer genuine possibilities of achieving a strict form of quark confinement (identically null probability of tunnel effects for free quarks), trivially, because of the profound technical differences between the mechanics for the outside (conventional QM) and the generalized one for the interior dynamics.

Numerous other possibilities for advances in quarks, which are now prevented by the current assumption of a rigidly exact Lorentz symmetry, would be permitted by deviations. In fact, we are organizing a summer workshop on these problems where there is a specific session devoted to "applications to quarks, QCD and gauge theories" (see enclosures).

**The regrettable politics at U. S. National Laboratories and the opportunity at CERN.** Very unfortunately, U. S. National Laboratories are currently controlled by vested academic interests opposing most vigorously the tests of Einstein's special relativity under strong interactions. This is well known in the States and, by no means, it is a confidential disclosure. This momentary weakness of the U. S. can be the advantage of a laboratory such as CERN. In fact, it seems to me that the minds of CERN physicists are more independent, when compared to the monolitically controlled minds of their colleagues in U. S. National Laboratories, thus exhibiting the elements for independence of scientific thought.

In the final analysis, I am contacting you precisely because I have faith in CERN, particularly after your taking over the Directorship.

**I.B.R. possible assistance.** In case you are interested in considering the possibilities in more details, and without any unnecessary formal commitment, our Institute can provide all possible support.

Note that I have studiously abstained from recommending any specific experiment, and I shall continue to do so, even though I have several in mind. In fact, the selection of experiments should be a collegial effort taking into consideration numerous factors, as well known.

Our Institute can assist you toward such a collegial study in a number of ways, e.g.,

(1) *By preparing a collection of papers in the field which are essential for the acquisition of mathematical, theoretical, and experimental knowledge needed for judgment.* I am referring to:

- a few mathematical papers on the Lie–isotopic and the Lie–admissible generalization of Lie's theory;
- a few theoretical papers on the current efforts to achieve the generalization of the Lorentz transformations along lines A', B', and C'; and
- copies of all important experimental papers along lines A, B, C.

(2) *By coordinating a presentation at CERN of members of our team.* The I.B.R. is coordinating all scientists interested in the problem on a world–wide basis. It would be a pleasure to identify a team composed, say, of

- one or two mathematicians in Lie–isotopy or Lie–admissible genotopy; such as M. L. Tomber (Michigan); H. C. Myung (Iowa); et al.
- two or three theoreticians working at the generalizations such as: G. Eder (Atominstitut, Wien), working at the generalization of spin; R. Mignani (Univ. of

Rome, Italy), working on the generalization of the potential scattering theory for data elaboration; and myself, currently working on the generalization of rotations and Lorentz transformations;

- three or four experimentalists who have worked at the problem, such as: H. Rauch (Wien); H. Conzett (Berkeley); R. J. Slobodrian (Quebec); G. Matone (Frascati), et al.

(3) *By arranging possible stays of I.B.R. members at CERN to assist the experimentalists.*

Kindly review these various options and feel free to communicate your comments. You can count on my best cooperation. More particularly, please feel free to indicate whether a possible interest should be kept conficential at this moment. I am full aware of the multiple difficulties of your post, and you can count on my honoring your requests in their entirety.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

References:

[1] *Theoretical, experimental, and mathematical studies conducted at the I.B.R. toward a generalization of Galilei's and Einstein's relativities in classical and quantum medhanics,* I.B.R. nontechnical report dated January, 1983, not intended for publication;

[2] H. Rauch, Hadronic J. 5, 729 (1983)

[3] G. Eder, Hadronic J. 4, 2018 (1983)

[4] H. B. Nielsen, Nuclear Physics B211, 269 (1983)

[5] R. J. Slobodrian et al, Phys. Rev. Letters 47, 1803 (1981)

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 6, 1983

Professor H. SCHOPPER, Director
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARH
1211 GENEVA 23, Switzerland

Dear Professor Schopper,

We would gratefully appreciate the courtesy of indicating to us the procedure for the submission of experiments to CERN.

The organization of our forthcoming First Workshop on Hadronic Mechanics is proceeding on schedule. A number of participants intend to submit a group proposal to CERN at the conclusion of the meeting.

To provide you with a tentative and preliminary idea, one of the proposals is expected to deal with new measurements of the mean life of unstable hadrons at different energies (pions and kaons, in particular). In fact, available experimental data appear to show a deviation from the exact Lorentz symmetry, as conceivable since several decades because of possible, internal, nonlocal effects.

Thanking you in advance for your courtesy, I remain,

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

June 6, 1983

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Thanking you in advance for your courtesy, I remain,

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

# PART XIII:

# PHYSICAL REVIEW LETTERS

# AND

# PHYSICAL REVIEW D & C

# PART XIII–A:

# CORRESPONDENCE

# WITH

# R. K. ADAIR,

# EDITOR OF

# PHYS. REV. LETTERS,

# IN 1979–1980

Division of Particles and Fields

American Physical Society

TO: Membership of the Division

FROM: L. Pondrom, Secretary-Treasurer

SUBJECT: Election Results and Other News

1.) New Members of the DPF Executive Committee

Vice Chairperson: J. Sandweiss, Yale University

Executive Committee: H. Frisch, University of Chicago
R. Jaffe, Machusetts Institute of Technology
R. Lanou, Brown University

The other officers of the Executive committee for 1979 are: M. Perl, Chairperson, L. Pondrom, Secretary-Treasurer. The other Executive Committee members are: D. Caldwell, S. Gasiorwicz, P. Rosen, and H. Quinn. The next meeting of the committee will probably be during the APS meeting in Washington, D. C., 23 -26 April 1979.

2.) Announcement of Conferences

The International Conference on Electromagnetic and Lepton Interactions will be held at Fermi National Accelerator Laboratory, Batavia, Illinois from 23 August to 29 August 1979. These dates are earlier than those listed in the LBL Pocket Diary. Please note the change.

Los Alamos Scientific Laboratory will host a LAMPF Program Options Workshop which will address critical questions in nuclear and particle physics and how they can best be investigated through the use of intermediate energy accelerators. The meeting will be held in Los Alamos, August 20-31, 1979. Panel membership is by invitation; plenary sessions are open to all interested persons. Further information may be obtained from John C. Allred, Mail Stop 830, Los Alamos NM 87545 USA.

3.) PPF Subscription Drive

A subscription form for SLAC - PPF is included in this mailing for the convenience of those members for the Division who wish to subscribe to this weekly listing of preprints.

4.) Letter from the Editors of Physical Review Letters

A letter to the membership from the editors of PRL is also included in this mailing.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

BROOKHAVEN NATIONAL LABORATORY, UPTON, NEW YORK 11973
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January 26, 1979

To the membership of the Division of Particles and Fields:

The Editors of Physical Review Letters are most anxious to work towards a situation such that we publish the best short papers in theoretical particle physics. At the present time, only about 6% of the Letters are concerned with theoretical particle physics while, for example, about 20% of the pages in the Physical Review (A,B,C and D) are devoted to theoretical particle physics. While such numerology is surely not an absolute guide to an ideal distribution of subject matter in the journal, we do believe that this indicates that we have a serious deficit in the theory of particles and fields--and we can hardly conclude that this deficit follows from a lack of progress in the subject! Aside from the fact that we are publishing very few theoretical particle physics papers, we have a strong feeling that we are missing many of the better papers and that the papers we do publish are not really representative of the best work on particles. We hope that we can find some way to change this: we would like to publish more theoretical particle physics papers, perhaps 10 or 15 a month on the average (which is at least twice what we are publishing now) and we would like to feel that the papers we publish are representative of the most interesting work in the field. We hope that we can achieve a position such that the Phys. Rev. Letters would be the first journal to be considered when an American particle physicist plans to publish a short report on work which he considers outstanding. We recognize that this will only be the case when he is confident that his paper will be considered in a responsible manner. It is clear that this confidence is now wanting.

Our general system of identifying appropriate papers through the counsel of referees who work in the area of inquiry considered by the paper, works well in most fields. It does not seem to work nearly as well, probably not well enough, in theoretical particle physics. There are probably a number of reasons for this state of affairs but we do not think that we really need to understand the difficulties with any precision in order to conclude that there is a problem and to consider remedies for the problem.

Aside from specifics, we believe that we can revive Phys. Rev. Letters as a primary journal for theoretical physics only through some action taken through cooperation of the community and the Editors of the journal. Inevitably, this will require some commitment and increased effort on the part of that community. Equally, the design and implementation of the new procedures which seem to be required will test the ingenuity and flexibility of the Editors and we are prepared to do our best to effect necessary changes.

These changes are constrained by certain practical considerations. At the present time the Editors consider over 2000 papers a year and approve the publication of 1000 papers. We are considering a situation where we hope to handle, perhaps, 300 theoretical particle physics papers a year and to publish about 150. An administratively efficient organization has been developed over the years which, we believe, does a very good job of handling this flow of material to Phys. Rev. Letters and one should consider procedures which make use of this organization. We suggest, then, the following procedures for the handling of theoretical particle physics papers.

The Division of Particles and Fields would recommend to the Editors the appointment of 4 Associate Editors for theoretical particle physics. Papers in theoretical particle physics would be submitted to the journal as they are now. The Editors would select two referees and send a copy of the abstract and title page of the paper and the names of the referees to an Associate Editor. If both of the referees approved of the paper, the paper would be approved for publication with a copy of that approval sent to the Associate Editor. If both referees advised rejection of the paper, the paper would not be accepted but sent back to the author with the referees' comments. A copy of the paper together with the referees' comments would be sent to the Associate Editor. If the two referees disagreed, the comments of the referee who rejected the paper would be sent to the author while the paper and referees' comments would be immediately sent to an Associate Editor for his advice. The authors reply to the referees would be forwarded to the Associate Editor as it is received.

We hope to get 300 papers a year and, with the scenario presented here, we would expect that about 50% of the papers would come before an Associate Editor. This would give each Associate Editor about 40 papers a year which is, we believe, an appreciable but not too onerous a work load. The Associate Editors would be chosen to cover somewhat different areas but there would be no effort to define areas too precisely.

We hope that the changes in procedure listed here will improve the probability that a paper is considered responsibly and then make the journal more attractive to authors. We believe that the journal has a great deal to offer to prospective authors: Phys. Rev. Letters is probably the most widely read journal in physics. We have 6,000 individual (non-library) subscribers and competitive journals have less than one-fifth as many. Our refereeing system will continue to be somewhat more abrasive than the more authoritative system of receiving editors (though we hope that our referees will be a little more tactful in their criticisms of the work of their friends and colleagues) but we hope that our authors will tolerate this abrasion as a part of our democratic procedures. We have a democratic way of handling papers in the American Physical Society Journals and, on some levels, as with so many democratic procedures, we act less efficiently than autocracies. With our journals, the referees which represent the community in a rather representative manner, take over some of the duties which the editor exercises in a more authoritative journal. If the community is responsible, we believe that democratic procedures are, on balance, better. We hope that we can find a way to use the community in a manner such the inherent responsibility of the community can be exercised in a contribution to a better journal.

Sincerely,

Bob Adair George L. Trigg [illegible]

R. K. Adair, G. L. Trigg, G. L .Wells
Editors, Physical Review Letters

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RUGGERO MARIA SANTILLI
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April 16, 1979

Dr. R. K. ADAIR, G.L. TRIGG and G.L.WELLS
Editors, PHYSICAL REVIEW LETTERS
BROOKHAVEN NATIONAL LABORATORY
UPTON, LONG ISLAND, N.Y. 11973

Dear Drs. Adair, Trigg and Wells,

I have read with interest your communication to the members of the Division of Particles and Fields of the AMP of January 26, 1979. I would like to express my support for your action. In particular, I have admired your clear statements of facts related to theoretical papers in your Journal, as well as your clear expression of determination to improve the situation.

I would like to take the liberty here to express my personal view, mostly originating from my indipendent research interests in theoretical physics, as well as my experience as editor in chief of the HADRONIC JOURNAL.

I believe that the conditions indicated in your communication are a reflection of the current, delicate moment of our community of basic research. Permit me to candidly confess that, in my view, the current conduction of research is mainly an expression of personal opinions, or beliefs by individual or group of researchers, and not the manifestation of an experimentally established physical veritas. I am here referring only to the conduction of research in the theory of strong interactions.
I would like to stress that such an occurrence is the necessary condition for advancements in human knowledge. That is, without opinions, beliefs and conjectures, subsequently proved or disproved, there would be no advancement.

Yet, the situation in our community is different, in my view. Permit me to candidly confess that, by and large, the opinions by autoritative groups of researchers are generally considered the physical veritas, and any non-aligned study is generally considered wrong, or without physical value.

This situation is created by the nowaday vexing state of affais of the quark models, quantum chromodynamics and related schools. A series of (rather courageous) articles in the 1978 volume of the Hadronic Journal has stressed the simply unequivocal validity of these studies for the Mendeleev-type, exterior, "chemical", classification of hadrons. Yet, the same articles have expressed doubts on the joint validity of the same models, also for the structure, and have suggested the search of

page 2.

fundamentally different models of structure capable of reaching full compatibility with the established models of classification, while capable of resolving some of the problematic aspects inherent in the quark conjectures. This is much along the conceptual structure which produced the solution of the problem of the atomic phenomenology: one model of classification ( Mendeleev) and a fundamentally different, yet compatible model of structure (Bohr). Almost needless to say, this line of study was advocated as a complement, and not as a substitute for the current studies on quark conjectures. Specifically, the attitude was that studies on quark conjectures for hadron structure should continue, while, jointly, fundamentally different models should be investigated.

On the surface, this appears as a reasonable attitude. In practice, however, it is faced with rather considerable difficulties, most of which, in my view, are of purely emotional character. The issue which is at stake is not whether quarks exists or not. More fundamentally, the issue is whether the basic physical laws used in quark models (Einstein's special relativity, Pauli's principle, the spin-statistics theorem, etc.), which are experimentally established until now only for the electromagnetic interactions, are valid or invalid for the strong interactions in general, and the strong hadronic forces, in particular. See the enclosed leaflet on reprint volumes edited by H. C. MYUNG, S. OKUBO and myself.
It is understood that, if these laws need a generalization for the strong hadronic forces (as suggested by rather numerous arguments, and as nowaday believed by a number of qualified physicists), the quark conjecture is ruled out in the final form. Indeed, there would be the lack of the basic ingredients (e.g., the notion of spinor) to even vaguely define a quark.

Still in my view, this situation has created a clear division of the physics community into "quark-believers" and "quark-non-believers" with divergencies, not of minute technical character, but rather of fundamental nature. In turn, this situation, still in my view, directly appears at the editorial level of specialized journals in the field.

Perhaps, a most representative case is my recent paper joinly with C.N.KTORIDES and H.C.MYUNG, submitted to Phys. Rev. D and entitled "Lie-admissible approach to broken SU(2)-spin under strong non-self-adjoint interactions". The very title tells you the non-aligned nature of the study. The divergences between myself and the Phys. Rev. referee are simply irreconciliable. The inspection of the correspondence would be (amusing, as well as) instructive, in the sense that we might acquire consciousness of the current, deep, disagreements in strong interactions. Please feel free to ask copy of the correspondence to Dr. D. NORDSTROM. On my part, I have no objection for you inspecting it, with the understanding that should not be released outside the circle of the editorial organization of the Phys. Rev. and Phys. Rev. Letters.

page 3.

By returning to your communication, I believe that the organization of your refereeing process is simply impeccable, and so is, of cour e, that of Phys. Rev.

My only suggestion is related to the actual selection of the two referees. Permit me to be candid in this crucial point. If you receive a paper on strong interactions of non-aligned nature with respect to quarks, and you select for the referees two outstanding experts on quark conjectures, this is virtually equivalent, in my view, to the rejection of the paper at the arrival.

I beg you not to consider this as a criticisms of the past. The situation in strong interactions I am referring to has actually materialized in 1978, even though has been lingering for years. I am making these remarks only in the hope that may be of some value for the future.

The way I handle this situation in my Journal is the following. Whenever I receive a paper on quarks, I send it to two referees, carefully selected as being of opposite views, that is, one quark believer and one quark-non-believer. As you can see from the enclosed Table of Contents of Volume 1, our Journal does indeed publish numerous articles on quarks. This means that I accept papers even though one referee states that it is not only wrong, but fundamentally wrong. Exactly the same approach is followed, without any prejudice, for papers by quark-non-believers, that is, I send them to one quark expert and one of fundamentally different orientation. I feel obliged to this type of refereeing because, the problem of the structure (not the classification) of hadrons is still fundamentally unsolved, and any different attitude would create in me questions of scientific ethics.

The implementation of this type of selection of the referees implies, however, a change in the editorial function. Indeed, as an editor, I have to make a judgment of scientific value, despite opposing reports. But, this was, after all, the historical function of editors. It is only brought to light again by the current disagreements in the physics community.

In closing, permit me to express my sincere esteem in all of you. If I can be of any assistance as a referee (of the quark-non-believers type) or for any other function, please do not hesitate to contact me.

Very Truly Yours,

Ruggero Maria Santilli

RMS/ml

c.c.: Dr. D. NORDSTROM, Editor, PHYSICAL REVIEW D.

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May 7, 1979

Drs. R.K.ADAIR, G. L. TRIGG and G. L. WELLS
Editors
Physical Review Letters
Brookhaven National Laboratory
UPTON, N.Y. 11973

Dear Drs. Adair, Trigg and Wells,

Perhaps, the enclosed paper may assist you in clarifying the contents of my letter to you of April 16. Judging from your lack of acknoledgment of this letter, I am under the impression that my letter was not sufficiently exhaustive.

As you can see, the enclosed paper presents a review of the rather numerous and substantial criticisms on quark conjectures which are moved by rather numerous and outstanding physicists all over the world.

I would like to add here that the contents of this paper is only partial, that is, I have absteined from presenting additional technical criticisms on quarks because of the need, in this case, to refer to specific papers by specific authors.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encl

c.c: Dr. D. NORDSTROM

P.S. The enclosed paper is not intended for submission to Phys. Rev.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

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May 25, 1979

Prof. Ruggero Maria Santilli
Science Center, Room 331
Harvard University
One Oxford Street
Cambridge, Mass. 02138

Dear Prof. Santilli:

Thank you for your letters of April 16, and May 7. We apologize for not answering you sooner but I suspect that it is possible to prove that a letter addressed to three people has a much better chance of being overlooked that a letter to one. The human condition is such that each of the three assume that one of the others will answer the letter. I have to assume the most guilt, however, as our division of labor rather clearly assigns to me a major responsibility for communication of our policies with our communicants.

As any responsible editor must be concerned with biases of his advisors, we are concerned over the possibility of the formation of schools where the members of one school reject out-of-hand the work of another school. At Physical Review Letters, we do not consider such problems with schools or sets of views as important as for the broader journals of record such as Physical Review D. We do attempt to avoid sending papers which directly attack a narrowly held position to the authors who have established that position but broader questions, such as the question of the character of quarks and the correct place of quantum chromodynamics in physics, we leave to the general community. We take this position (of largely ignoring the possibility of such biases) for a number of reasons, some of which are peculiar to our journal, a journal of selected short communications.

First, while we recognize that physics and physicists follow trends and styles which are not neccessarily founded impeccably on logically sound foundations, we feel that this is not as damaging as you do because we believe that the bias against views counter to the currents of the time is not so great as you intimate. I know that Chew, Mandelstam, Venieziano and others are deeply interested in a description of the strong interactions which may not, and probably cannot, accommodate the simple (or simplistic?) view of quarks which is prevalent but I am confident that the carefully reasoned papers which come from this group are accepted by the publications of the American Physical Society. We are also less concerned over such possible biases than we might be because we do not consider our journal as a complete journal of record. We reject 55% of the papers submitted to us for reasons which do not relate to the correctness of the paper but to the specific fit of the paper to our journal. If we reject a radical paper, which turns out to be an important and seminal paper in physics, we do not feel that we are suppressing the ideas in the paper; there are other journals which can, and should, publish the paper. In the long run, the market place of ideas should act to select the gold from the dross. We do not feel that our selective journal is the proper market, however.

Sincerely yours,

R. K. Adair/glt

R.K. Adair
Editor

RKA/jw

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RUGGERO MARIA SANTILLI
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May 30, 1979

Dr. R. K. ADAIR, Editor
PHYSICAL REVIEW LETTERS
UPTON, N.Y. 11973

Dear Dr. Adair,

I appreciated your letter of May 25, 1979. I personally agree with most, if not all your comments. Nevertheless, the voice of concern which I candidly communicated to you, expressed at this time by a minority of our community, in my humble view, deserves a serious consideration by all editors, including myself.

Besides the comments of my preceding letter, the issue touches the question: when is a paper on strong interactions well written? A partial answer is:when the assumptions are carefully and specifically identified, the implications of these assumptions worked out to the necessary rigour, and the results confronted with physical veritas.

The concern I am referring to here is that current papers of quarks or QCD orientation are, in general, grossly deficient when inspected from this profile. The point is that simply none of these papers identifies even partially which are the assumptions and which are the established facts. Even though I could not inspect all these papers (there are too many), in all the papers I personally inspected this was indeed the case.

On more specific grounds, the question that Einstein's special relativity is a mere conjecture at this time for the strong interactions, has been indicated by a number of authors, beginning from the very founders of contemporary physics, and lately presented in numerous papers and even monographs (of course, of non-quark inspiration). In 1978 the HADRONIC JOURNAL lanched, via a series of articles, a moment of reflection on the basic physical laws currently used in quark-QCD-type of studies. This effort, in particular, complemented previous aspects with the identification of the fact that Pauli's exclusion principle, the spin-statistics theorem and numerous other quantum mechanical laws are a mere belief, when referred to the hadronic constituents. These papers, not only have received a rather wide distribution, but they have been even reprinted.

The concern is that all (to my knowledge) papers on quarks-QCD simply ignore the totality of these contributions, and assume in a tacit form the validity of the fundamental physical laws. This concern, in my humble view, deserves a serious consideration for a number of reasons. On scientific grounds, we have here all the ingredients for considering the possible existence of a scientific misrepresentation. Responsible physicists are

page 2.

understandably concerned of the potential negative implications for the pursuit of knowledge implied by this situation.

In any case, the recommendation is rather specific: authors of quarks-QCD orientation, irrespective of their scientific authority and their status, should clearly identify in their papers each and every assumption of the study without an experimental backing at this time, and then present their studies. Alternatively and equivalently, the recommendation is that these papers should clearly separate what is experimentally established and what is not, what is a conjecture and what is a physical veritas. If they do not desire to enter into this task, they should at least quote the papers by now specialized in the topic.

If this recommendation unrealistic?

A profile which, quite candidly, we cannot ignore (for our own sake) is the financial aspect of funding research in strong interactions. Of course our Journals do not have a direct connection with this financial aspect . Yet, an indirect connection exists, trivially, because the entire refereeing process, as well as that of presentation of proposals, is based on existing literature. A potential insufficiency at the level of papers then clearly propdgates itself at the funding level. This profile should be seriously considered too because a number of valuable physicists have seen their proposals rejected, their tenure refused and are unemployed with a family to support. The thinking by these colleagues is different than ours.

In closing, permit me to stress that I am in the same situation as yours. Indeed, the articles published by my Journal have been written until now in the traditional style (called in the marked the "Phys. Rev. style"), as far as papers of quark-QCD orientation are concerned. Rather than a form of criticism to you, you should interpret this letter as a call to join forces, reach a mature assessement of the situation, subsequently take the necessary steps for an improvement, and help each other.

Sincerely

Ruggero Maria Santilli

RMS/ml
c.c.: Dr. D. NORDSTROM.

THE PHYSICAL REVIEW
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August 17, 1979

Prof. Ruggero Maria Santilli
Science Center, Room 331
Harvard University
One Oxford Street
Cambridge, Mass. 02138

Dear Prof. Santilli:

I apologize for responding to your letter of May 30 as late as this. I could not answer, responsibly, quickly because I wished to discuss your ideas with others. We editors of Physical Review Letters must not presume to act as arbiters ourselves on scientific matters but to act as arbiters on the communities perception of these matters. I have now discussed the problems which you bring up (as I understand you) and I believe that my correspondents (and I) are not in complete agreement with you. In particular, for almost every scientific paper, the work is based on certain assumptions of the period and I do not believe that it is either practical or desirable that all of these assumptions should be reviewed for each paper. I certainly agree that present QCD theories assume the validity of many concepts which have not really been tested on that scale. I also believe that this is the correct way to procede in physics. But I also believe that it is wise, necessary and altogether a good thing that, occasionally, able people question the bases of present ideas. I believe in quantum mechanics very much as Bohr did, and with comparitively minor caveats, almost every physicist today accepts quantum mechanics in that form. Nevertheless, I have always been pleased that such able people as David Bohm (for example) have continued to question quantum mechanics. But, if I were to act as referee to a paper which used conventional quantum mechanics, I would object to a reference to Bohm as one who questioned QM unless some very special point of Bohm had beem addressed by the theory of experiment described in the paper. I cannot, then, agree with you that QCD papers should refer to others who have questioned the applicability of special relativity, etc.. I do not see that such a set of references would be useful.

As for those who swim against the stream, I am pleased that some do. Perhaps that is really the right direction. But to swim against the stream is not, in itself, enough; you must get somewhere. Some, like Geoff Chew, are getting interesting results of course and, no doubt there are others, going in different directions, who are finding positive results of value. But this is outside of my competance.

Sincerely yours,

Bob /jw

R.K. Adair
Editor

RKA/jw

September 10, 1979

Dr. R. K. ADAIR, Editor
The Physical Review Letters
Brookhaven National Laboratory
UPTON, N.Y. 11973

Dear Dr. Adair,

I would like to express my appreciation for your letter of August 17, 1979 and for your consideration of my comments.

Nevertheless, I feel obliged to express my disagreement with your views.

The aspect under consideration was, what is called in the trade, the "Phys. Rev. style" of presentation of papers on quark conjectures, QCD, and related topics. In particular, the profile under consideration was the total silence on all papers published by the APS Journals, to my best knowledge, of the fact that the validity of conventional physical laws for the strong interactions (Einstein's special relativity, Pauli's exclusion principle, the spin-statistics theorem, etc.) is a mere belief at this time, deprived of any clear, direct, or otherwise final experimental backing.

Upon consultation with your scientific advisers, you have reached the decision of leaving the editorial status quo unchanged, that is, of continuing the current practice of complete silence on this truly fundamental issue.

I believe that this editorial practice can serve the academic (as well as financial) interests of your advisers but, under no circumstance, this practice can serve the pursue of physical knowledge. If you have convincing counterarguments, I would be glad to reconsider my view.

Also, I believe that this practice is one of the most effective ways of opposing or otherwise delaying the experimental verification of the validity of invalidity of the basic laws considered for the strong interactions, trivially, by avoiding the creation of the awareness in the scientific community of the existence of the problem. Again, if you have convincing counterarguments, I will be glad to reconsider my view.

Whether your scientific advisers agree or not, the conjectural character of the basic physical laws used in quark conjectures on hadronic structure and related studies is a scientific reality. It was lingering in our communities for decades. In 1978 it become technically identified and explicitly stated in a number of articles of the Hadronic Journal. Lately, this situation has been verified in all details in the recent Second Workshop on Lie-admissible Formulations, held at Harvard University from August 1 to 7, by a number of mathematicians and physicists from the USA, France, Israel, Switzerland (plus corresponding participants from the USSR and Australia who could not physically attend the meeting because of lack of travel funds).

page 2.

It is inappropriate here to quote technical arguments. I would like simply to report historical facts identified by some participants of this workshop. For instance, Wolfgang Pauli made it quite clear in his historical papers and lectures that his exclusion principle was conceived and must be considered as applicable only under conditions of lack of overlapping of the wave packets. The validity of the same conditions for the spin-statistics theorem is then consequential. Simply calculations show that, whether quarks, partons, eletons, or other, the hadronic constituents must be in a state of overlapping of their wave packets. The current, easy, application of Pauli's principle and the spin-statistics theorem in hadron physics is therefore in strict violation of Pauli's teaching.

Similarly, Einstein made it quite clear in his papers, correspondence and teaching that his special relativity was conceived for point-like particles under action-at-a-distance interactions (electromagnetic). It cannot be otherwise because this relativity is a relativistic generalization of Galilei's relativity which, in turn, is fundamentally dependend on the Newtonian concept of point-like particle and action-at-a-distance forces only (variationally selfadjoint forces). Par contre, the point-like approximation of particles under strong interactions (whether hadrons or their constituents) is strictly against the experimental evidence (all strongly interacting particles have a charge radius which coincides with the range of the strong interactions). The current, easy, application of Einstein's special relativity is, therefore, in direct conflict with Einstein's teaching as well as experimental data.*

Enrico Fermi expressed explicitly and quite clearly his doubts on the validity of conventional geometries, relativities and laws for the region of space within strongly interacting particles (you may consult his lectures in Nuclear Physics).

The list of historical reasons of doubts could continue.

What we have done in the literature on the Lie-admissible coverings of the Lie algebras and related formulations is the identification of a number of technical reasons indicating the expected invalidity of conventional laws for the strong interactions under the conditions of overlapping of the wave packets, because of the necessary emergence of forces more general than $f = -\partial V/\partial r$ (variationally nonselfadjoint forces, consequentlial lack of existence of a Hamiltonian, consequential inability to introduce <u>all</u> Lie

* Please, do not quote in this respect the so-called "experimental result" in certain, recent, deep inelastic scatterings of leptons on hadrons indicating a point-like structure of the costituents of the proton. These "experimental results" are nothing more than a <u>theoretical</u> elaboration of experimental data fundamentally dependent on the (primary) <u>assumption</u> of the validity of the special relativity in the conditions considered. Quoting these "experiments" would therefore only serve the purpose of propagating the current controversies from the theoretical setting to the experimental profile.

page 3.

algebras-let alone those for the SU(2)-spin and of the Poincaré group-, consequential applicability of the covering Lie-admissible algebras for the time evolution law under these broader forces, consequential, possible existence of Lie-admissible coverings for the strong interactions of conventional laws of the elm interactions, etc.).

There is no doubt that studies on hadron structure based on the validity of conventional laws must continue, and I have explicitly stated it in my own papers, but, under the condition that the conjectural character of these laws is clearly stated or otherwise formally acknowledged by the "orthodoxy" that is, by your advisers.

The current policy of complete ignorance of this situation by this orthodoxy can at best be identified as a scientific misrepresentation. I would like to be on record by indicating that the potential implications of this situation , not only for the pursuit of physical knowledge, but for the supporters themselves, could be conspicuous if excessively protracted.

One of the primary duties of our profession is to separate beliefs from facts, and to promote the experimental resolution of divergencies. When treating truly fundamental issues, such as that of the basic physical laws for the strong interactions, the fulfillment of this duty becomes mandatory.

I disagree with virtually all passages of your letter. For instance, you indicate your view that "for almost every scientific paper, the work is based on certain assumptions of the period and I do not beleive that it is either practical or desirable that all of these assumptions should be reviewed for each paper".

My comments are the following. Suppose that AT LEAST ONE PAPER ON QUARKS OR RELATED TOPICS BY AN AUTHORITATIVE SUPPORTER (a list of names could be easily formulated at this point) would clearly state and identify the conjectural character of the basic physical laws in his studies on the hadronic structure. Then, I would have accepted your view in its entirety. Indeed, once this first paper of this character appears in the literature, there is no need to repeat the passage in each and every paper along the same lines. The point remains that I do not know even one single paper, by even a less authoritative quark supporter providing this crucial function. How can I then accept your statement without questioning it?

Similarly, you state that "I cannot agree with you that QCD papers should refer to others who have questioned the applicability of special relativity, etc.. I do not see that such a set of references would be useful."

As indicated in my preceding correspondence, there is indeed no need to quote papers questioning the validity of the fundamental physical tool of QCD, the special relativity. This however, under the assumption that the literature in QCD has at least once and in one single paper clearly performed the duty indicated early: the separation of beliefs from facts. When

page 4.

the totality of the literature in the topic is complétely silent on this truly crucial aspect, the perspective of a possible scientific misrepresentation is unavoidable. Again, if you have counterarguments of even a minimum of convincing character, I would be glad to reconsider my view.

I am also under the impression that your advisers are substantially non-informed of the "positive results of value" (in your language) achieved by researchers currently involved in the formulation of experiments for the future resolution of the issue considered (in the series of reprint volumes "Applications of Lie-admissible algebras in physics" we have already published two volumes and are working on two additional volumes). These studies, however, are written for colleagues with scientific humility and vision and they will be likely dismissed by your advisers as exercises of curiosity (the balance is then restored because of a growing number of qualified physicists considering quark oriented studies as exercises of curiosity).

I am also sincerely concerned of your personal condition. I am fully aware that the Editors at Physical Review Letters must act as arbiters of the scientific community. However, you have selected to act as arbiter of only part of the scientific community, by and large, that committed to quark conjectures. But the moment of reflection on the validity of the basic laws for these conjectures has been launched on a world wide basis (e.g., my recent draft "An intriguing legacy by Albert Einstein: the expected invalidation of quark conjectures" has been mailed world wide in 15,000 samples; the announcement of the Second Workshop on Lie-admissibility- centered on the study of the problems considered- has been mailed to all institutions of basic research). This has activated the brains of valuable mathematicians and physicists. I doubt that this scientific drive to resolve experimentally basic issues will be stopped by quark committed physicists. Their opposition, either direct or in the form of ignorance we are referring here, can only promote a process to our scientific accountability. If this moment will indeed arrive, I have no doubt that your current advisers will turn their back to you, in the sense that they will release the totality of the responsibility on your current decision to you.

At the risk of being pedantic, I am recommending here that you and your associates in the Editorial conduction of Physical Review and Physical Review Letters reconsider the situation and your decisions. In particular, I am recommending that you

(1) consider the suggestions by your current advisers for what they are: personal viewpoints of one part of the scientific community completely unsubstantiated at this moment by experiments;
(2) consider my suggestion as a representation of the opposite viewpoint by a minority (at this time) of the scientific community; and
(3) have the literature on Lie-admissibility inspected by scientists with a genuine scientific vision and humility (for your information, the Proceedings of the Second Workshop on Lie-admissibility are scheduled for publication in the December issue of the Hadronic Journal, Volume

2, number 6, 1979).

I discourage the attempt of having the literature in Lie-admissibility seriously inspected by your current advisers. They represent the orthodoxy and, as by now historically established, they will likely die in the belief of being the recipients of the final physical veritas. You will recall, for instance, the opposition by the Academy of France against the idea that meteorites are bodies from our galaxy..... You will recall the opposition by Boltzmann against this strange idea by Planck, so contrary to established classical knowledge..... The list of episodes qualifying the behaviour of the orthodoxy in the pursuit of physical knowledge could be endeless.

What you are facing, however, is not a minute aspect. Instead, it is related to truly fundamental topics, with either a direct or an indirect primary function for energy related issues (think at the controlled fusion as a laboratory construction of bound states of hadrons). We simply cannot afford the luxury of following beliefs by individual physicists on issues of this type. Of course, I expect that your advisers will dismiss as nonsense this energy-related connection. But, such a possible dismissal may later on result to be a further reason to invite a process to our scientific accountability....

A final point which your should bring to the attention of your advisers is the damage, in my view, that they are producing to Physical Review and Physical Review Letters. I am referring here to the fact that your Journals are completely out of the following efforts (at least at this time)
- to achieve a critical inspection of the validity of conventional laws for the strong interactions;
- to achieve covering laws specifically conceived for the strong, under the rejection of point-like abstractions and conditions of overlapping of the wave packets; and, last but not least,
- to achieve maturity of formulation on the only way to effectively conduct physics: the experimental resolution of these issues.

For instance, a number of months ago I submitted to Phys. Rev. D a joint paper with a mathematician and a physicist entitled "Lie-admissible approach to broken SU(2) spin symmetry under strong nonselfadjoint interactions". The paper was specifically intended to promote the experimental verification of Pauli's principle under strong interactions, beginning at the level of nuclear physics where very small deviations might have escaped currently available studies. This paper has been strongly rejected by your advisers or your entourage because "much out of the mainstream of physics". The understanding is that this paper is out of the mainstream of THEIR physics: that made up of personal beliefs for which experimental verifications are strictly excluded.

Similarly, I have tried to recommend to other colleagues the submission of papers along these lines to your Journals, but with complete failure until now. As one colleague put it to me, he does not intend to submit page 6

any paper to your Journals other than of minute incremental character on established trends, if nothing else, in order "not to be offended by the language of the referees".

Judging from your letter, I have serious doubts whether you are truly aware of the gravity of these occurrences and their implications.

Very Truly Yours

Ruggero Maria Santilli

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RMS/ml

c.c. Dr. D. Nordstrom
96 North Country Rd
Shoreham, N.Y. 11786

Drs. Adair and Nordstrom,
perhaps, one of you should call me at home (or we should see each other). I have serious reasons of concern. Until I can help you, I am sincerely glad to do so.

Best Personal Regards
RMS

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

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September 24, 1979

Dr. Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Massachusetts 02160

Dear Dr. Santilli;

Thank you for your letter of September 10. I will answer you with the special hope that I can clarify my position. On page 4 of your letter, you write; "I am fully aware that the Editors ··· must act as arbiters of the scientific community ····." But we are not arbiters of science; we certainly do not have, nor do we foolishly claim, that competence. I suppose that we are arbiters of certain minor questions of style but even here we serve as best we can as representatives of the community and we are constantly (and correctly) reviewed even in such matters by the community through the Publications Committee of the American Physical Society. The community acts as arbiters through the referee systems and while I recognize that the community, acting as a kind of committee of the whole, is subject to enthusiasms which are not always well founded, I have great confidence that the general open-mindedness and common sense of the community defines a consensus which is wiser and more fair than any substitute which I can imagine. Of course, I can only sample the community through some choice of advisors and you may well consider that my sampling is deficient but I believe that is is most unlikely that the position I have taken is not approved by a considerable majority of physicists (and I would be disingenuous not to state that that position is in accord with my own beliefs also).

I should not present the technical side of my conclusions with the view of opening a discussion with you — you can certainly find much wiser men than I for such discussions — but only as a point of information. My advisors (and I, myself) do not believe that there is any particular blindness in the community towards the fact that the basic laws which you discuss have not been firmly established in the regions of space-time and momentum transfer which are important in elementary particle physics. Though I have been a reasonably active physicist for more than 30 years, I do not know when the applicability of the spin-statistics theorem in particle physics was not questioned! While I have not the time, nor the competence, to penetrate your detailed (and, my advisors say, elegant) discussions, your broader, general statements contain little that I did

Dr. Ruggero Maria Santilli
page 2
September 24, 1979

not believe that I knew. Both my advisors and myself believe that the present direction of the main flow of particle theory which, tentatively and conservatively, assumes the validity of basic concepts, unproven as they may be, is in the best tradition of physics. As you know well, most theoriests do not believe that it is yet necessary to give up on the basic assumptions which you question and, I believe, that most theoriests consider that these assumptions should not be given up until it is necessary. It will be a long time before we will know who was right and how we should have proceeded. In the mean time, I believe that the journals are appropriately open to substantial contributions which assume the validity of these assumptions or question the assumptions.

All of us, theorists and experimentalists, are quite interested in the possibility of proving -- or disproving -- the fundamental theoretical concepts, such as the spin-statistics theorem, in particle physics. I would be very interested in making such measurements myself if I could be convinced that the measurements would bear strongly on the relevant questions. Needless to say, I must be very careful about committing many man-years of effort and very large sums of money to measurements (and that is what is involved for even simpler particle physics experiments) unless I am strongly convinced that the efforts will be very useful. At the present time, I know of no such possibilities and I do not promise that it will be easy to convince me to attempt such measurements.

Sincerely,

Bob Adair

R. K. Adair

RKA/ja

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RUGGERO MARIA SANTILLI
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October 23, 1979

Dr. ROBERT K. ADAIR
50 Deepwood Dr.
HAMDEN, Connecticut 06517

Dear Dr. Adair,

I would like to express my appreciation not only for your letter of September 24, 1979 and for your time, but also for its contents and for its style of presentation.

I believe that we are having a valuable scientific interaction, which may be mutually beneficial. Permit me the liberty of expressing candidly my comments The candor of my language is solely intended to communicate with you in the sole language that may be effective for expressing physical issues.

I am in COMPLETE AGREEMENT when you state that

"Both my advisors and myself believe that the present direction of the main flow of particle theory which, tentatively and conservatively, assumes the validity of basic concepts, unproven as they may be, is in the best tradition of physics."

Actually I have rarely seen (these days) a deeper maturity of presentation of the essence of physics : a sequential chain of approximations, which therefore calls for doubts and critical examination of each and every step.

I am in SUBSTANTIAL DISAGREEMENT with the way this style is implemented via the current editorial practices at Physical Review D and Physical Review Letters. These well worded doubts are simply absent in the style of presentation of quark-oriented papers. All I was indicating in my preceding letters is that the style of presentation of quark-oriented papers has received, lately, a negative reaction by an apparently increasing numbers of physicists. Most of them are silent with you. I have selected to express this point to you in the sole intent that it may be of some value to you.

I am in IRRECONCILIABLE DISAGREEMENT when, in regards to the experimental verification of basic laws for the strong interactions, you express the view that

"I would be very interested in making such measurements myself if I could be convinced that the measurements would bear strongly on the relevant questions. "..."I must be very careful about committing many man-years of efforts and very large sums of money ...unless I am strongly convinced that the efforts will be very useful.At the present time, I know of no such possibility."

This view simply establish that YOU HAVE ZERO TECHNICAL KNOWLEDGE OF THE STUDIES OF LIE-ADMISSIBILITY, ZERO KNOWLEDGE OF THE STATUS OF FORMULATION OF EXPERIMENTS, AND ZERO KNOWLEDGE ON THEIR TECHNICAL AND HISTORICAL IMPLICATIONS.

There is little I can do to improve this situation. What it calls for is time, considerable time, to read the literature, which is already quite large, and expanding rapidily. Please feel free to express this views to myself (because you can count on my confidentiality), but I urge you to abstein from expressing views of this type to others, before achieving a necessary knowledge of the literature.

First of all, we have an experiment on the verification of Pauli's principle in nuclear physics that is feasible with current technology, according to the view of a number of experimenters (NOT COMMITTED TO QUARKS), with the understanding that the experiment is predictably delicate and will predictably call for a further joint effort by experimentalists and theoreticians.

Secondly, this experiment is in nuclear physics and, as such, it will cost expectedly less money and time than a corresponding experiment in particle physics. As a matter of fact, this is the reason why we have suggested the initiation of experiments at the nuclear level. Recent studies re-elaborated at the Second Workshop on Lie-admissibility (you may study the Proceedings) have indicated the conceivable existence of very small deviations from the totally antisymmetric character of identical nucleons in nuclei whose charge volume is below that predicted by the proportionality rule with the total number of nucleons. For these nuclei, the nucleons are in an experimentally established, statistically small state of penetration of their wave packets. This is sufficient to activate the Lie-admissible formulations via a very small departure from the conventional Lie's formulations, as representative of small forces nonderivable from a potential. In turn, this implies a small breaking of the SU(2) spin symmetry and, thus, a statistically small departure from the exact fermioni character of the nucleons, under the conditions considered.

Thirdly, your view implies a gross disrespect to the Founding Fathers of contemporary physics. What we are doing IS NOT NEW, as you have stated yourself. We are simply trying to bring the physics community to its senses. The forces we use were suggested by Fermi. The proposed experiment is intended to test FERMI'S LEGACY which you ignore. Furthermore, the forces considered imply a nonunitary time evolution law and, thus, the invalidity of the conventional uncertainty in a small amount. This is exactly Einstein's view on the lack of terminal character of the conventional indeterminacy. The proposed experiment is intended also to test EINSTEIN's LEGACY, which you also ignore when you express doubts on the advisability whether to spend the money. Furthermore, the mechanics of the departures expected from Pauli's principle is necessarily realized at the level of the enveloping algebra (to accomodate broader forces). This is exactly the view ex pressed by Jordan, von Neumann and Wigner (the enlargement of the envelope of Heisenberg's representations, from the associative to a nonassociative form). As a matter of fact this view by these Masters IS AT THE FOUNDATION OF LIE-ADMISSIBILITY. The experiment proposed is intended to test also this legacy by JORDAN, VON NEUMANN, AND WIGNER, which you also disregard with your attitude on the experimental profile. Yet more, the experiment is also intended to test a RATHER INCONTROVERTIBLE, CLEAR, AND WELL STATED LEGACY BY PAULI: he made it clear that his exclusion principle was conceived under the conditions of LACK of overlap of the wave packets (the atomic structure), trivially, because under conditions of overlap he was expecting "stronger" forces (FERMI'S LEGACY) which would prohibit him to even SEPARATE THE WAVE FUNCTION, LET ALONE TO ESTABLISH ITS TOTALLY ANTISYMMETRIC CHARACTER. Our proposed experiment is intended to TEST THIS TEACHING BY PAULI SO GROSSLY IGNORED, NEGLECTED, AND ABANDONED BY HIS FOLLOWERS.

What shall we do to bring the physics community to its senses? What do you need more than that? Which language shall I use?

Fourthly, we are currently spending billions of dollars of taxpayers money in experiments on strong interactions, ALL based on the assumptiom of the validity of the basic laws, and NONE intended to test the basic laws themselves. In particular, most of these experiments, and most of the most expensive experiments, are devoted to aspects, certainly valuable, but of purely minute incremental character which may, on a long term basis, eventually attract only the attention of curious historians. Your view implies that it is better to continue this status quo, rather than entering into the experimental verification of the basic laws, that is, INITIATE ACTIVE EFFORTS OF TRIAL AND ERRORS, RATHER THAN SITTING PASSIVELY IN AN ATTITUDE OF WAIT AND SEE. This is the reason why I have recocomended you to abstein from expressing views of this type to others. Owing to the large amounts of money spent in conventional stuff, and the comparatively minute amount needed to initiate the test of the basic laws, your attitude might trigger, at the extreme, a procees to our scientific accountability.

Fiftly, the most paradoxical aspect, in my view, is the fact that the opponents to these crucial experiments (generally quark committed physicists are simply not aware of the fact that the possible invalidity of basic quantum mechanical laws would leave unaffected the validity of unitary models as well as QCD. This is again due to their total ignorance on the technical treatments of Lie-admissibility. Their minds are simply obfuscated by the unequivocal physical results of these models, in the sense that they are unable to separate what is unequivocally established by these experiments and what is left fundamentally open.

In the Lie-admissible literature we have repeatedly expressed the view that the rather large volume of physical results of unitary models and QCD establish the validity of these models for the Mendeleev-type classification of hadrons only (or, you may say, their "exterior" treatment, or "chemistry"). The essential character of the CLASSIFICATION has been established by the Nobel assignements for the $\Omega^-$ prediction and discovery, and, more lately, by the prediction and discovery of the $J/\psi$ particle and related states. These are results that are, and will remain in the history of physics. No further, potential or actual, advancement of our knowledge can invalidate these results.

Nevertheless, these results DO NOT ESTABLISH THAT QUARKS ARE REAL PARTICLES that is, they do not establish that the same models provide a joint classification of hadrons into unitary multiplets and a structure of each individual member of a given multiplet, all at the same time, all via the same model. This occurrence did not make sense for the atomic phenomenology and there are reason, serious reasons, in our view, that a similar separation classification/structure may eventually result to be necessary at the hadronic level. After all, our efforts on Lie-admissibility are centered in achieving a fundamentally different model of structure, but under the condition that it achieves strict compatibility with the established models of classification. This is exactly along the efforts by Bohr, Thomas, and Fermi to achieve compatibility with Mendeleev. But these Founders of contemporary physics did not search, as the quark physicists do, for one single model capable of representing the totality of the phenomenology considered.

In particular, if you read deeper in the quark literature, YOU DO NOT NEED TO ASSUME THAT QUARKS ARE REAL PARTICLES TO ACHIEVE THE SAME RESULTS. Technically, quarks are representation of a unitary group (apart phenomenological jargon). Thus, the idea of quarks is deeply linked to that of a unitary multiplet. This is, in our view, PURE CLASSIFICATION.

If you read the Lie-admissible literature, you may see that a possible invalidation of conventional laws within a hadron would merely establish this dichotomy classification/structure; leave the physical validity of the unitary models unaffected for the classification profile; and identify their arena of physical relevance: a good, but first-approximation of the hadronic world, under the point-like abstraction of particles (or lack of overlap of the wave packets) as necessary under the validity of the special relativity (in Einstein's own view).
In conclusion, if the legacies by Fermi, Einstein, Jordan, von Neumann, Wigner, Pauli and other will eventually be proved to be true (if physicists stop being passive on the matter and start working on them), this would mean no disaster for the unitary models and QCD, but only the identification of the next logical step: a first, but genuine treatment of particles as extended objects under conditions of overlapping of their wave packets and forces beyond the trivial $f = -\partial V/\partial r$.

The true problem for a possible genuine advancement, along the teaching of the Founding Fathers of contemporary physics, is of HUMAN AND NOT OF MERELY TECHNICAL CHARACTER: the desire by the orthodoxy in physics to remain attached to old views as much as possible.

page 5.

This situation can be best expressed via Heisenberg's words (see his touching memoires "Physics and Beyond",pp.70-71).

> "In science, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hadardous leap forward."

To which, he adds, soon thereafter:

> "However, when it comes to enter new territory, the very structure of scientific thought may have to be changed and that is far more than most men are prepared to do."

Sincerely

Rugg Ma Santilli

Ruggero Maria Santilli

RMS/ml
encls.

P.S. You might be interested to know that my recent paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" has been accepted for publication by a leading Journal other that the Physical Revied D or the Hadronic Journal.

I enclose "Chart 4.9" of my Volume II with Springer-Verlag of "Foundations of Theoretical Mechanics" now in press. This chart (intended in its nautical meaning) may provide you with a quite readable account of the issues here considered, and it is written in a form understandable to graduate students. I would like to stress, however, that the technical treatment is elsewhere, and it is re-elaborated in the Proceedings of our recent Workshop. I would like to bring your attention, in particular, on Part 9, pp. 343-349 of this chart on the historical, authoritative, voices of doubts, so forgotten by our community, so misrepresented, . so mistreated , and, lately, so opposed in their experimental verification, or even treatment (see the case of my paper submitted on January 4 at the Physical Review D).

Oct. 30, 1979

Dear Dr. Santilli:

I have received your insulting letter of Oct. 23 and write this note as a termination of our correspondence.

R. K. Adair RKA

# PART XIII–B: CORRESPONDENCE ON THE MORATORIUM ON NONRELATIVISTIC QUARK THEORIES AT THE HADRONIC JOURNAL OF 1980

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
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February 13, 1980

Dr. R.M. Santilli
Department of Mathematics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

I have read with interest your general letter of 8 January to editorial and advisory boards of journals in theoretical physics. As I trust you recognize, the nature of our journal is such that I can take no explicit action regarding the journal in response. However, I am personally interested in the fundamentals of quantum theory. Accordingly, I would greatly appreciate it if you could send me a reprint of your review paper, Hadronic J. 2, 1460-2018 (1979) (your Ref. 3). I infer that it would be a good place to start to learn more about the problem.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor

GLT/jaw

(PUBLICATIONS OF THE AMERICAN PHYSICAL SOCIETY)

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February 14, 1980

TO: The Editorial and Advisory Boards of Journals in Theoretical Physics

FROM: R.M.Santilli, Editor of the HADRONIC JOURNAL

SUBJECT: Follow up of my letter of January 8, 1980

Dear Colleagues,

I would like to express my appreciation and gratitude for your interest in regard to the topic of my letter to you of January 8, 1980 and for the request of more specific information I received from a number of colleagues (I understand that there are difficulties in locating in research libraries the "Foundations of Mechanics" 1979-edition by Professors Abraham and Marsden, and the Proceedings of the Second Workshop on Lie-admissible Formulations, Hadronic J. Volumes 2, number 6 and 3, number 1, 1979).

I have prepared a preliminary, hand written note on my (limited knowledge) on the so-called "theorems of inconsistency of Heisenberg/Lie/symplectic formulations". A copy is enclosed in the hope that can be useful in reaching a first idea of the technical aspects, problems, and issues. Any critical remark, comment, or advice would be appreciated.

You will be pleased to know that a systematic, coordinated study of the issue has been initiated, with particular reference to the editorial profile of papers activating the inconsistency theorems in the various branches of physics (quantum mechanics, quantum field theory, quatum statistics and plasma physics, and quantum gravity). Particularly gratifying has been the answer to our call for help by a number of mathematicians, experts in the field.

At the HADRONIC JOURNAL we have initiated a special file on references (and copies) of past and expected, future, contributions in this (rather intriguing) issue. This information is at the disposal of all of you, as well as of your Referees.

The study of the problem at the THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (August 4 to 9, 1980) has been confirmed, and, again, you are welcome to join us.

We hope that these efforts will result in a precise identification of the problem, as well as the achievement of a mature editorial decision on all quantum mechanical papers with generalized Hamiltonian structures activating the no-go theorems.

Your participation to this scientific effort is appreciated.

Sincerely

Ruggero Maria Santilli

Ruggero Maria Santilli
Editor
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February 16, 1980

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
Brookhaven National Laboratory
Upton, Long Island, New York 11973

Dear Dr. Trigg,

Your kind letter of February 13, 1980 reached me just while I am leaving for Europe for a few weeks (to deliver invited lectures on the so-called inconsistency theorems). Regrettably, I do not have complimentary copies of my memoir-review (I have zero research funds). I do not have also complimentary copies of the Proceedings (the few available were committed months before their appearance owing to the considerable demand).

Nevertheless, I would like to do my best to assist you. I have therefore instructed Ms. Lyons, my secretary, to mail you my own personal copy of the Proceedings (two volumes). I would be truly grateful whether, after keeping them for, say, one or two weeks, you return them to me. I would need them on my way back from Europe during the second week of March . In case of lack of reception, please feel free to contact Ms. Lyons at this address (tel (617) 495 3352/mornings) during my absence.

Permit me add a few comments, in case of any value to you. The following three different inconsistencies of Heisenberg/Lie/symplectic formulations have come to light.

INCONSISTENCIES IN THE QUANTIZATION OF HAMILTON'S INTO HEISENBERG'S EQUATIONS. These inconsistencies (read, no-go theorems) have been studied in great details by Abraham and Marsden in their recent edition of "Foundations of Mechanics". I would like to encourage you most warmly to look directly at this source because the excellent technical presentation of this volume is reduced in my review to only a few lines (p. 1781). In particular, I recommend the inspection of pages 434-439 (from the definition of quantization in the language of the symplectic geometry to the proof of the lack of its existence).

Incidentally, these initial inconsistencies could be disposed off, from an editorial viepoint, by saying that a new theory should not necessarily admit rules of construction from an old one. In different terms, if these no-go theorems are taken alone, they might not constitute yet reason of concern on editorial grounds .

INTRINSIC INCONSISTENCIES OF HEISENBERG EQUATIONS . Two different types have come to light, and additional ones are forthcoming (judging from possible papers in our Journal). The first is an intrinsic inconsistency of the time evolution law $\dot{A}| \rangle = (AH-HA)| \rangle$ for all polynomial operators A and H in r and p of at least order three. The best prove of this inconsistency is given in Abraham-Marsden book, page 439. A vulgarized proof is in my memoir. In essence, the value of the commutator depends on the selected use of the differential rule. The inconsistency explodes in the face of sceptics when one shows that $[H,H] \neq 0$ or $= 0$, depending on the computational channel.

Independently from that, Lagrange's and Heisenberg's equations become inequivalent for all Hamiltonians of the same type (polynomial order higher than two, e.g. $p^3$, $p^2r$, $rp^2$, etc). Intriguingly, this inconsistency is completely absen for Hamiltonians of electromagnetic type. This means that the inconsistency is absent also for unified gauge theories of weak and electromagnetic interactions, QCD, and all models with exactly the same structure of the electromagnetic interactions ( free term plus an interaction term at most linear in the momentum or derivative coupling). Nevertheless . the inconsistency is activated rather clearly by a number of topics, e.g., nonrelativistic chromodynamics, dissipative nuclear processes, gravitation, plasma physics, etc.

page 2.

This (second) inconsistency has been studied in great detail in Hood's thesis (while I was at Boston University). See also the article by Hellmann and Hood, Phys. Rev. D5, 1552 (1972). A rudimentary summary is presented in a few lines of my memoir (p. 1779).

The relationship between these two inconsistencies is also intriguing, and so is that with others under study (e.g., in Feynman path approach), under the same conditions.

These inconsistencies are an editorial problem, in the view of a number of editors (me included) deserving a serious attention. In essence, we lack at this moment sufficient technical information to reach a mature decision whether to accept, or reject, or hold papers activating these inconsistencies. The reasons are rather clear. Suppose you reject by fiat the use of the differential rule hoping to salvage old stuff, but then you cannot escape from inconsistencies at the Heisnberg-Lagrange level, as well as at the level of the presumed equivalence Heisenberg-Schrödinger equations. Similarly, suppose you assume as "true" Heisenberg equations (to try to salvage QM) and claim as "untrue" Lagrange's equations. But then QCD is at stake because based on "untrue" equations. Similarly, suppose you claim as "true" Lagrange's equations (to salvage QCD) and as"untrue" Heisenberg's equations. But then QM is at stake (these are some of the "suggestions" I received to salvage as much as possible old knowledge).

INCONSISTENCIES IN THE DIRAC'S LIMIT OF HEISENBERG'S INTO HAMILTON'S EQUATIONS. These inconsistencies were studies at our Workshop and are reported in p. 1780 of my memoir. They can be interpreted as an "inverse" formulation of Abraham-Marsden no-go theorem of quantization, but the implications are different, particularly in regard to the presumed equivalence Heisenberg-Schrödinger representations.

The idea of our Third Workshop (scheduled for August 4 to 9, 1980) is to gather mathematicians, physicists and editors in a selected and restricted number (maximum 20-23, to avoid dispersal of energies), and conduct a study of the problem. The hope is to achieve some valuable information for us on how to handle papers activating the inconsistencies (and they are quite numerous, in my view). The understanding is that academic dances of mumbo-jumbo hand wavings (such as "Heisenberg's equations are true and Lagrange's equations are false') are ignored, and the advice by specialists, experts in the field is taken in due account.

As of this moment, it appears that the response is promising for rendering this meeting a reality (despite the predictable existence of questionable opposition).

We would be sincerely pleased to have you with us. In case you can attend, please let me know in advance, so that I can secure for you the best possible accomodation.

Also, It would be a pleasure for me to meet you before the Third Workshop, and have a friendly, relaxed, informal exhange of views in this intriguing situation. Beginning from the third week of March, 1980, you would be most welcome here in Cambridge, or at your discretion, I would be glad to drive to Yale.

As a final comment, you might be interested to know that this situation was triggered by a paper on nonrelativistic chromodynamics. A leading expert on quarks recommended the paper for publication as excellent, but a mathematician expert in quantization indicated that the paper was fundamentally inconsistent. I therefore recognized that my scientific accountability was at stake here. My letter to editors-colleagues of January 8, 1980 was motivated by the desire to share this experience with all interested physicists, even though I was fully aware that the letter is strictly anti-career-oriented, as far as my future is concerned. This is a fact of contemporary academic life.

Sincerely

R. M. Santilli

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL
RMS/ml

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
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SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

March 19, 1980

TO: Mathematicians interested in quantum mechanics
FROM: R.M.Santilli, Editor of the Hadronic Journal
SUBJECT: call for help for an intriguing editorial impasse

You might be interested to have some information about an editorial impasse which occurred recently at the Hadronic Journal. It concerns all physics articles in nonrelativistic quantum mechanics based on Heisenberg's equations (and related physical laws) with generalized Hamiltonians of the type

$$H_{gen}(q,p)=T_{gen}(q,p)+V(q,p)\,;\ \text{Polyn. Order } T_{gen}(q,p)\gtrsim 3;\quad V(q,p) = \text{linear in } p, \qquad (1)$$

e.g., $H_{gen} = \frac{1}{2}pF(q)p + V(q)$ (Nota Bene: the impasse excludes conventional Hamiltonians $H = T(p) + V(q,p)$ with Polyn. Order $T = 2$, as occurring for electromagnetic interactions).

A significant number of papers in different fields are involved in this intriguing case, with particular reference to: nonrelativistic quark dynamics; nuclear physics; quantum statistical mechanics; plasma physics; controlled fusion; and quantum gravity.

The impasse originated with the submission to the Hadronic Journal of a comprehensive paper in nonrelativistic quark dynamics (for which the use of generalized Hamiltonians is necessary to achieve meaningful mass spectra). The paper was recommended for publication by qualified referees. But other, equally qualified, referees recommended the rejection quite firmly. The inability to resolve the technical differences between these equally qualified, opposing views, resulted in the impasse. The fact that the problems originate in the generalized structure of the Hamiltonian, and the joint use of conventional laws, suggested the extension of the impasse to other fields.

To the best of my understanding, the problematic aspects underlying the impasse are the following.

Problematic aspects in the quantization. As known in mathematical circles, a theorem by Abraham and Marsden (following notes by Chernoff, as well as preceding contributions) (ref.1) establishes the lack of existence of the full quantization for the models considered. A first group sees no problem in this, on the basis that two different disciplines should not necessarily admit a map. A second group disagrees on the basis that, to prevent possible intrinsic inconsistencies of quantum mechanical models, the problematic aspects of quantization should equivalently occur for all quantum representations (e.g., those via Heisenberg's equations, via Schrödinger's equation, via Lagrange's equations, etc.). The issue is therefore whether or not the various representations of quantum mechanics are consistent (that is, mutually compatible) from the viewpoint of quantization, e.g., whether or not the Abraham-Chernoff-Marsden theorem admits a form of image for the quantization of the Hamilton Jacobi into Schrödinger's equation. To my knowledge, no contribution by mathematicians exists on this topic at this time.

Intrinsic problematic aspects. Generalized Hamiltonians (1) activate a lemma by Hellman and Hood (ref.2) according to which, for the Hamiltonians considered, Heisenberg's equations are not necessarily equivalent to the (operator) Lagrange's equations (for conventional Hamiltonians this problem does not exist). A first group dismisses this occurrence, e.g., on grounds that there exist transformations $(q,p) \rightarrow (q',p')$ mapping $H_{gen}(q,p)$ into $H'_{conv.}(q',p')$. The equivalence between Heisenberg's and Lagrange's equations is then regained (under boundedness and other conditions inessential here) for the transformed Hamiltonian, as often used, e.g., in path integral approaches. A second group disagrees quite vigorously on a number of counts, e.g.,

(a) Generalized Hamiltonians violate the imprimitivity theorem (ref.3, p.204) for a genuine validity of Galilei's relativity. Thus, the transition from conventional to generalized Hamiltonians may imply the loss of Galilei's relativity, and, thus, of the notion of Galilean quantum particle.

(b) When the equations of motion are computed explicitly, generalized Hamiltonians imply nonconservative, nonlinear, velocity-dependent forces. In this case, the systems are open, that is, they violate the conservation of total physical (rather than canonical) quantities, such as, total angular momentum, energy, etc. (hint: for Hamiltonians (1) the symbol "p" does not represent the physical linear momentum $m\dot{q}$). This appears to confirm problematic aspects (a).

(c) The time evolution of open systems in the vector field form with local variables q and p = physical linear momentum is noncanonical at the classical level, and nonunitary at the quantum level for coherence of the theory under the classical limit. Under a nonunitary time evolution, most of the conventional laws and principles of quantum mechanics (e.g., Pauli's exclusion principle; Heisenberg's indeterminacy principle; etc.) are not preserved, as shown in ref. 4, pp. 1865-1888. Similarly, the transformations mapping $H_{gen}(q,p)$ into $H'_{conv}(q',p')$ are generally noncanonical at the classical level, and nonunitary at the quantum level. The equivalence of Heisenberg's and Lagrange's eqs. would be then regained at the loss of the basic physical laws. This confirms the problematic aspects for the conventional notion of Galilean quantum particle.

The implications of these occurrences are nontrivial. For example, for models of plasma physics with Hamiltonians (1) the validity of Pauli's exclusion principle is open (theoretically and experimentally, to my best knowledge); for models of dissipative nuclear processes with Hamiltonians (1) the validity of Heisenberg's indeterminacy principle is unresolved at this moment (also theoretically and experimentally, to my knowledge); for nonrelativistic quark models, the problematic aspects prevent at this time a consistent, quantitative, formulation of the hypothesis that quarks are physical Galilean particles, without affecting the physical content of these models as far as the Mendeleev-type classification of hadrons is concerned (the classification can be conducted via spectrum generating, Schrödinger-type equations for which no problematic aspect is know at this time).

Problematic aspects in the classical limit. Even though not universally accepted, classical mechanics is expected to be admitted by quantum mechanics under "a" suitable limit, for the logical coherence of the theory. The open problems are here numerous. For instance, we do not apparently know at this time whether the Abraham-Chernoff-Marsden theorem admits a form of "inverse". Also, we do not know whether problematic aspects in the limit of Heisenberg's into Hamilton's equations equivalently exist for the limit of Schrödinger's into Hamilton-Jacobi equations. The background issue is whether the various representations of quantum mechanics are mutually compatible under the classical limit (ref.5).

Any critical comment, remark, or advice would be gratefully appreciated. To assume full responsibility, I enclose copy of my ref.5 providing an outline of the problematic aspects, while I remain at the disposal of interested colleagues for more specific information.

REFERENCES

(1) R.Abraham and J. E. Marsden, Foundations of Mechanics, Benjamin/Cummings (1979 edition)

(2) W.S.Hellman and C.G.Hood, Phys. Rev. D5, 1552 (1972)

(3) G.W.Mackey, Unitary Group Representations, Benjamin/Cummings (1978 edition)

(4) R.M.Santilli, Hadronic J. 2, 1460 (1979)

(5) R.M.Santilli, Hadronic J. 3, 854 (1980)

P.S. Some of these open problems are contemplated to be studied at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS scheduled in Cambridge, Ma, from August 4 to 9, 1980.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
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May 22, 1980

Dr. R.M. Santilli
Department of Mathematics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

Thank you for lending me the material from the workshop on Lie admissibility. I apologize for having kept it longer than the two weeks or so that you had suggested; I hope that this did not cause you any difficulties.

I find, to my regret, that my familiarity with modern abstract algebra is sufficiently sketchy that I was not really able to appreciate much of the argument. I cannot help feeling, however, that your campaign calls for much more drastic action than is really warranted. As you must be aware, this is not the first instance in which physics theory has made progress on the basis of questionable mathematics, nor is it likely to be the last. I do not mean in any sense to disparage the work that you and others are doing to try to provide a sounder basis; but I do not feel that a moratorium of any sort would be useful.

I thank you again for lending me the material, and I offer my wishes for success of the forthcoming workshop. I regret that my schedule does not permit me to attend.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor

GLT/jaw

(PUBLICATIONS OF THE AMERICAN PHYSICAL SOCIETY)

# PART XIII–C:

# REJECTION

# OF A PAPER

# ON THE

# EXPERIMENTAL

# VERIFICATION OF

# PAULI'S EXCLUSION

# PRINCIPLE

# IN STRONG

# INTERACTIONS

January 5, 1981

Dr. D. NORDSTROM, EBitor
The Physical Review D
Brookhaven National Laboratory
Upton, Long Island, N.Y.

Dear Dr. Nordstrom,

I have now concluded a series of conşultations in regard to my paper "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions", which was submitted to your Journal on October 4, 1980. I am now ready to prepare a revised version. In particular, I would like to implement the following changes.
(1) The paper is essentially intended to solicit the experimental measurements of the intrinsic quantities of hadrons under strong interaction (spin, magnetic moments, etc.) This knowldge is clearly useful for energy issues (the controlled fusion). Clearly, if the magnetic moment of nucleons mutates (in our Lie-admissible language) under the conditions of the strong interactions in general, and those of the controlled fusion in particular, the magnetic confinement calls for suitable implementations. As a first point I would to attempt a better identification of this primary objective via a few introductory remarks.
(2) It has been brought to my attention by a number of colleagues that the mutation of the magnetic moment is an old idea in nuclear physics. In fact, conventional theories cannot interpret the magnetic moment of nuclei (see the Schmidt limits). This simple interpretation of an experimental fact was subsequently abandoned because of the predominant theoretical belief that the intrinsic characteristics of hadrons as measured under long range elm interactions remain the same under the additional presence of the strong and the conditions of wave overlappings. Also in the introductory part, I would like to point out this occurrence (e.g., Blatt-Weiskopf, Theor. Nucl. Phys., p. 21 clearly state in p. 31 the expectation that the magnetuc moment of nucleons change under nuclear conditions). The relevance with the paper is selfevident. In particular, it is quite difficult to construct a quantitative model whereby the magmetic monent mutates and the dpin remains the same.
(3) As directly recommended to me by Professor Rauch of the Atominstitut of Vienna, Austria, during a recent viist of mine at his institute, his experiments in neutron interferometry are capable of testing directly the relationship between magnetic moment and spin because the angle measured for the 4      symmetry is directly linked to the mageetic moment. After all, the precession which is measured is due to a megntic field. Thus Rauch's experiments, if properly repeated, for instance, along the alternatives suggested in my paper, could likely produce an experimental resolution of the issue. The understanding is that the achievement of this experimental knowledge is not opposed.
(4) I have several improvements of details, such as the fact that the actual improvement of the fit via Lie-admissible mutation calls for two-sided representations, and cannot be achieved via the linear one-sided mutation considered in the paper.
(5) On editorial grounds, I have also numerous improvements to implement throughout the paper. In particular, and following a kind suggestion by Professor Okubo and other colleagues, I shall remove from the paper any mention of the quark conjectures.

page 2 -

Since the submissioon of the paper on October 4, 1980, I have not received comments or referee report from you.

Please considered the revisions indicated earlier in this letter and, in case appropriate, let me have your comment and or advice. Also, any other contructively critical criticism -would be particularly helpful for the finalization of the paper.

I would like to take this opportunity to wish to you and to your Journal a happy and prosperous 1981.

Sincerely

Ruggero Maria Santilli
Professor of Physics
University of Massachusetts in Boston

RMS-ms

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21 January 1981

Dr. R. M. Santilli
28 Cross Street
West Newton, MA 02165

Dear Dr. Santilli:

We have received your letter of 5 January regarding your proposed revisions in your manuscript entitled "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions". Just before receiving your letter we received the report of one of our referees on your manuscript. A copy of the report is enclosed.

The serious objections in the enclosed report should be considered before any revisions in the paper are undertaken. Of the three objections listed in the report the third one is of particular concern to us from an editorial standpoint. In your submittal letter you stated that "This paper essentially presents one of the primary results of the recent Third Workshop in Lie-admissible Formulations". According to Reference 5 of your paper the proceedings of this workshop were to be published last year. Thus the implication is, as the referee suggests, that much of the paper "appears to be a rewrite of already published ideas." There would then appear to be little new material in the paper that would warrant its publication.

The delay in obtaining a report on your paper resulted from the very severe constraints on referee selection requested in your submittal letter. We sent the paper to one referee who recommended a

Dr. R. M. Santilli

page 2
21 January 1981

second referee, the individual who returned the enclosed report.

We are returning your manuscript for your consideration of our comments.

Yours sincerely,

D. Nordstrom

D. Nordstrom
Editor

DN:cp
enc.

REPORT OF THE REFEREE:

This paper is unacceptable for several reasons:

1. The claim that this theory gives a better fit to the data is invalid. The data agree perfectly with standard theory, since the experimental error limits enclose $720^{\circ}$. Consequently, any suggested improvement is meaningless.

2. None of the proposed experiments are substantive. Anyone can ask for better accuracy or for a thermal beam of neutral kaons. The Physical Review need not publish idle dreams. (We need constructive suggestions.)

3. Aside from the sections commented on above, the rest of the paper appears to be a re-write of already published ideas.

**Ruggero Maria Santilli**

Editor in Chief
Hadronic Journal

[redacted]

February 3, 1981

Dr. D. NORDSTROM, Editor
The Physical Review D
1 Research Road
Box 1000, Ridge, New York 11961

Dear Dr. Nordstrom,

Thank you for your letter of January 21, 1981 in regard to my paper "Experimental indications for the inapplicability of Pauli's exclusion principle for strong interactions".

Permit me to reassure you that the paper was original at the time of the submission on October 4, 1980, and so is still today. The originality and novelty of content relies on the presentation, apparently for the first time, of the fit of experimental data for spinor symmetry via the SU(2)-admissible treatment of the broken SU(2)-spin symmetry. I believe that the sentence you refer to should be extended to read "the rest of the paper appears to be a rewrite of already published ideas", which is indeed correct.

In regard to timing your referee was only partially informed. In fact, the Proceedings of the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS (which will treat the issue in all necessary detail) have not been published in December, have been delayed for several reasons,and they will appear perhaps in late spring.

The issue is therefore reduced to the capability and-or possibility by your office to process the paper as any other paper calling for refinements of existing experiments (which is a considerable percentage of your publications), and which is apparently processes in one-to-two months. Also, please keep in mind that I have funds for paying the publication charges.

On my part, I can provide you with the final revised version in a matter of days. However, quite frankly, my time is very very limited due to the multiplication of invitations to deliver speeches on the topics, as well as research activities. I will be happy to spend the necessary time, but with the understanding that the paper will receive a serious review.

My comments on the clearly political referee report are enclosed. In case you suggest more moderate comments, please let me know, and I shall rewrite them.

Sincerely

Ruggero Maria Santilli

RMS-ml

AUTHOR'S COMMENTS ON THE REFEREE REPORT OF PHYSICAL REVIEW D ON THE PAPER

**Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions**

DATE OF RECEPTION OF REPORT: January 30, 1981; DATE OF SUBMISSION OF PAPER: October 4, 1980

**Objective of paper.** To suggest the refinement of experiments on the so-called spinor symmetry via neutron interferometers and the measure of intrinsic characteristics of particles (spin, magnetic moment, etc.) under strong interactions. These characteristics have been measured countless times under long range electromagnetic interactions, but no direct or final experimental knowledge exists at this time for the same characteristics under strong interactions.

**Relevance of paper.** The achievement of the physical knowledge advocated by the paper is important for a number of selfevident aspects in physics, mathematics, and engineering. To reach a judgement of the referee report it is useful here to recall the importance of the advocated physical knowledge for the controlled fusion. In fact, the magnetic confinement, as an example, is rather crucially dependent on the value of the magnetic moment of nucleons under the conditions of the controlled fusion (strong interactions at very high pressures, densities and temperatures). The reader is encouraged to reflect on the financial implications of the issue.

**Clear objective of referee.** To prevent the achievement of this physical knowledge.

PRELIMINARY OPEN QUESTIONS.

1. The referee was aware of the date of publication of the PROCEEDINGS OF THE THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS (December 1980), judging from available material. The contents of the paper will be treated at length in these proceedings. At the same time, the referee report is the result of a few minute work (because it contains no scientific elaboration whatsoever, but mere statements of personal views). Yet, the report was delayed several months. Is this a mere coincidence, or a planned machination to achieve scientific obsolence of the paper?

2. The statement that "The data agree perfectly well with standard theory" is known to be false. First, the 720° of spin precession needed for the validity of the "standard theory" are missing in a number of experiments. Second, all the times the 720° are admitted by the data, they barely enter within experimental error and are far from the median value needed for the "standard theory". Third, and more importantly, the recovering of the 720° of spin precession is only a part of the requirement to establish the "standard theory" under strong interactions. A number of additional insufficiencies exist, are well known, and some of them are reviewed in the paper. For instance, there are clear clusters of points outside the curve needed for the validity of the "standard theory" which have no explanation at this time other than that via the breaking of the "standard theory" and its Lie-admissible generalization (conventional interpretations are not excluded here; it is simply stressed that they are lacking). Owing to these clear occurrences, the question opened by the referee report is the following: why has the referee selected a sentence which is known to be false? Was this only an unfortunate error due to a genuine selfconfidence? Or the selection was done because of financial-academic-ethnic considerations?

3. The statement "none of the proposed experiments are substantive" is doubtful at best. The paper predicts a breaking of the SU(2)-spin symmetry under strong interactions and recommends specific experiments for its verification. If this prediction will eventually result to be correct, a fundamental part of contemporary theoretical physics must be reinspected. Is there in the current literature a proposal more substantive than that? The referee appears to be fully aware of this aspect. Yet, he states the opposite. WHY?

MORE SUBSTANTIAL OPEN QUESTIONS.

4. The report has all the ingredient of scientific discrimination in the following sense. A considerable number of papers published by Physical Review (and other Journals) refers to improvements of established knowledge of aligned character. It is an easy prediction that this referee would have supported proposed experiments of this nature, say, an improvement of the current value of the magnetic moment of the nucleons under electromagnetic interactions, or a test of QED at very small distances. Yet, this referee opposes the repetition of experiments on the spinor symmetry. WHY?

5. It is assumed that, to qualify as referee for Physical Review D, this referee has received a good physics education, including nuclear physics. At any rate we must expect that the referee has studied Blatt-Weisskopf, Theor. Nuclear Physics, and that he has read the statement by these authors

> *"It is possible that the intrinsic magnetism of a nucleon is different when it is in close proximity to another nucleon." (loc. cit., p.31).*

The paper submitted simply calls for the experimental verification of this possibility. WHY IS THE REFEREE OPPOSED TO THE ACHIEVEMENT OF THIS PHYSICAL KNOWLEDGE?

6. Experiments on the measure of the intrinsic characteristics of particles under strong interactions undermine the very foundations of the contemporary financial-ethnic interests of the academic world. In fact, possible deviations from the magnetic moment and spin, if experimentally established, would imply the invalidation of Einstein's special relativity and the need for more adequate theories. In turn, this is expected to imply the invalidation of quark conjectures (because quarks are crucially dependent on their very definition on the special relativity). Is this referee a bona fide believers of standard views? Or is this referee an exponent of these financial-ethnic-academic interests? To prove his good faith the referee should give TECHNICAL arguments establishing the validity of standard views, and, to achieve credibility by the scientific community

at large, these arguments MUST NOT be based on a plurality of experimentally unverified assumptions (for instance, the arguments must be completely independent of quark conjectures). WHY NO TECHNICAL ARGUMENT IS PROVIDED BY THE REFEREE IN SUPPORT OF HIS SINCERETY? AND, AT ANY RATE, WHERE ARE THOSE TECHNICAL ARGUMENTS? IN NUCLEAR PHYSICS THE EVIDENCE IS MUCH IN FAVOR OF A MUTATION OF THE MAGNETIC MOMENT AS CLEARLY STATED IN A NUMBER OF WELL WRITTEN SOURCES. IN HADRON PHYSICS THE ISSUE IS UNRESOLVABLE AT THIS MOMENT BECAUSE OF THE CUSTOMARY REDUCTION TO QUARK ARGUMENTS, THAT IS, TO A PLURALITY OF PERSONAL VIEWS BY INDIVIDUALS. WHERE ARE THEN THE TECHNICAL ARGUMENTS SUPPORTING THE SCIENTIFIC CREDIBILITY OF THE REPORT?

CONCLUDING COMMENTS.
This author would have accepted with gratitude a critical report by the referee, but only under the uncompromisable condition that he would have FIRST stated clearly his support for the experiments suggested, and then entered into all deficiencies of the paper .for the achievement of the objective. This has not been the case. The referee has quoted as "dreams" the prediction of the paper. This is in flagrant disagreement with the expectation of nuclear physics. Also, this is in serious disagreement with the social needs to achieve the controlled fusion and, thus, on the social need to reach scientifically credible data on the intrinsic characteristics of particles under strong interactions. But, most of all, this is in disagreement with centuries of tradition whereby sound physical knowledge is achieved via direct and clear experiments. Different views can at best qualify as scientific politics, but not as the pursue of human knowledge.

This author recommend the most vigorous possible condamnation of attitudes of the type reported here. Lacking this action the risks are selfevident. For instance, by keeping in mind the size of the financial investments in the controlled fusion, a rather natural question is:

HOW LONG CAN WE DELAY THE MEASURE OF THE INTRINSIC CHARACTERISTICS OF PARTICLES UNDER STRONG INTERACTIONS WITHOUT RISKING A COMPLETELY UN-NECESSARY CRISIS, SUCH AS A SENATORIAL INVESTIGATION ON THE MATTER ?

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14 April 1981

Dr. R. M. Santilli
28 Cross Street
West Newton, Massachusetts 02165

Dear Dr. Santilli:

Your manuscript entitled "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions" was returned to the referee along with a copy of your response to the referee's first report. A copy of this referee's second report is enclosed.

We also contacted a second referee on your manuscript. We enclose a copy of the report excerpted from the comments of the second referee.

In view of the enclosed reports we regret to inform you that we cannot accept your paper in its present form. We are therefore returning your manuscript.

Yours sincerely,
D Nordstrom SGB
D. Nordstrom
Editor

DN:cp
enc.

SECOND REPORT OF THE FIRST REFEREE:

My opinion has not changed. I do not recommend publication.

REPORT OF THE SECOND REFEREE:

This paper is very poor, basically confused on physical issues, and is definitely not publishable. In this I agree fully with the report of your (experimental) reviewer. In my opinion the author's remarks on spin are totally unfounded and seriously flawed.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

July 16, 1981

Dr.D. NORDSTROM, Editor
The Physical Review D
Brookhaven National Laboratory
UPTON, Long Island, New York

Dear Dr. Nordstrom,

As a gesture of courtesy, I enclose copy of my solicitation for the year 1981 to Dr. Vineyard to initiate active studies at Brookhaven on the open problem of the basic physical laws.

The pressing need for these studies has been elaborated in the letter, as far as the physical aspect is concerned. The editorial aspect is transparent. In fact, the lack of initiation of these studies in national laboratories favors academic mumbo-jambo of the type of the referee report of my article submitted to Phys. Rev. D: "Experimental indications for the inapplicability of Pauli's exclusion principle under strong interactions", as per your recent letter (April 14, 1981).

The terms "academic mumbo-jambo" are the gentlest I can found to qualify these referees. The second claims that my work is "totally unfounded and seriously flawed". He may be true, of course. But to prevent the suspicion of mumbo-jambo the referee should have proved rigorously the statement with all due math. Ventitations of statement of the type this referee has, withour any justification, do nothing more than confirm the view by the famed philosopher at Berkeley, Paul Feyerabend, according to which contemporary physics is conducted via "subterfuge, rethoric,and propaganda." (reference is first to Journals .....).

As I indicated earlier in our correspondence, I reject referee report of this type at the HADRONIC JOURNAL, and I recommend you again to do the same at PHYSICAL REVIEW D. It is the only way our Journals can serve the pursue of knowledge, rather than the pursue of scientific politics.

In the past I have absteined from contacting other members of the Editorial Board of the Phys. Rev. D, such as the Editor in Chief, and I shall continue to do so as a gesture of courtesy to you. Please reinspect again the issue. In case I can bring the case to the attention of the high ranks at Phys. Rev. without causing you any inconvenience, please let me know (phone (617) 964 1684).

For your information, the crucial experiment by Rauch et al on the SU(2) spin symmetry to which my paper was addressed, has been recently re-elaborated by the Authors at the Atominstitut of Wien, Austria. The new value is $\alpha = 715.87 \pm 3.8^{o}$ which DOES NOT INCLUDE THE $720^{o}$ OF THE EXACT SU(2)-SPIN SYMMETRY!! The ultra-mumbo-jambo of the referee is now even more clear (the physical foundations and theoretical rigour of the SU(2)-spin/symmetry-breaking has been established beyond doubt in the literature via the experimentally established wave overlapping; consequential contact, nonlocal, nonpotential forces; consequential nonunitary time evolution at the level of each individual particle;

and, finally, consequential alteration of the electromagnetic spin values). You will see soon the new value published in the literature.

But, what is truly disturbing, and I still cannot accept with grace, is the opposition of the referees to experiments. The words "totally unfounded and seriously flawed" are indeed intended to prevent even the consideration of the experiments recommended. If these people are in good faith, WHY DO THEY FEAR EXPERIMENTS WHICH MAY CONFIRM THEIR VIEWS? After all, the exact SU(2)-spin symmetry may indeed be established experimentally under strong interactions. I cannot accept positions of this type to prevent the feeling of being their accomplice, in an apparent machination to prevent the achievement or otherwise the establishing of fundamental physical knowledge.

Sincerely

Ruggero Maria Santilli

RMS-ml

You are here warmly encouraged to mail copy of this letter to the anonimous referees.

DV1317

D15-3

EXPERIMENTAL INDICATIONS FOR THE INAPPLICABILITY OF PAULI'S EXCLUSION PRINCIPLE UNDER STRONG INTERACTIONS

Ruggero Maria Santilli*

Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

(RECEIVED 7 OCTOBER 1980)

Abstract

Recent experimental data on the $4\pi$ symmetry of the wave-function of neutrons, obtained via neutron interferometer experiments, are inspected in detail. It is shown that the Lie-admissible treatment of the broken SU(2)-spin symmetry under strong interactions is not only compatible with available experimental data, but actually produce a fit better than that for the exact symmetry. It is stressed that, despite these results, the available experimental information is still unable to rule out for the strong interactions the familiar notion of spin as established for the electromagnetic interactions. A number of specific experimental tests are proposed for the final resolution of the issue either in favor or against the conventional notion of spin and related physical principles, such as Pauli's exclusion principle.

*/Supported by the DEPARTMENT OF ENERGY under contract number DE-AC02-80ER10651

# PART XIII–D:

# REJECTION OF A THEORETICAL AND AN EXPERIMENTAL PAPER ON TIME–REFLECTION ASYMMETRY IN STRONG INTERACTIONS

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 16, 1982

Dr. GEORGE L. TRIGG
Editor
PHYSICAL REVIEW LETTERS
1 Rsearch Rd
RIDGE, New York 11961

Dear Dr. Trigg,

I hereby submit for publication in the Physical Review Letters my note entitled "Use of the hadronic mechanics for the best fit of the time-asymmetry recently measured by Slobodrian, Conzett, et al"

For this purpose, I enclose:
(a) Three copies of the note;
(b) two copies of a few separate calculations for referee use (particularly for referees who do not know the "hadronic mechanics");
(c) a collection of the most important experimental and theoretical papers quoted in the note (the theoretical ones being mostly unavable in the Journals of the AIP);
(d) a duly signed copyright agreement; and
(e) the PACS categories: 11.30 Er and 24.70 +s.

In submitting this note, permit me to ensure my best possible collaboration for referee comments, suggestions and criticisms based on explicitly presented elaborations and calculations. I would therefore consider it a personal courtesy whether you encourage the referees to avoid the presentation of unsubstantiated personal opinions and views.

In submitting this note, I would like also to express the concern of a segment of our community for the amount of time that resulted to be needed for Physical Review Letters to publish the experimental results of the international collaboration Berkeley-Quebec (and Bonn) treated in the note (compared to the rapidity with which the Los Alamos rebuffal was passed by Phys. Rev. C). I would like therefore to ask, most respectfully, that this note be processed within the period of time internationally considered appropriate for a letter (say, two months), or that you kindly inform me of foreseable delays.

I remain at your disposal for any assistance you may need.

Sincerely,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS; mlw
encls.
PS: Publication charges will be paid by the IBR.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES

1 RESEARCH ROAD BOX 1000 RIDGE NEW YORK 11961
Telephone (516) 924-5533

20 May 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

Referee's report on LR2111, "Use of the Hadronic Mechanics..." by R. M. Santilli

This manuscript presents a great deal of formalism, the physical significance of which escapes me, which is said to be inspired by an experimental study of $^7Li(^3He,\vec{p})$ and $^9Be(^3He,\vec{p})$ and inverse reactions by Slobodrian, Rioux, Roy, Conzett, von Rossen and Hinterberger (ref. 1 of the manuscript). It is my understanding that the general concensus of the nuclear physics community is that the data shown by Slobodrian, et al., indicating a large difference between the polarization of the protons produced in these reactions and the analyzing power of the inverse reactions, are not correct. A repetition of the $^9Be(^3He,\vec{p})$ and inverse reaction measurements by Hardekopf, et al., Phys. Rev. 25, 1090 (1982), yielded data in disagreement with the measurements of Slobodrian, and found agreement between the polarization and analyzing power, as one would expect from time-reversal-invariance.

Even accepting the results of Slobodrian, et al., which I do not, the purposes of the present manuscript remain obscure. After many equations of exceedingly general and elementary aspect, expressed in a bizarre notation which is said to be "hadronic mechanics," the author comes to the conclusion (p. 5) that "the ratio between the analyzing power of the forward reaction and the polarization of the backward reaction is equal to the ratio of the corresponding units of the enveloping algebras of operators." I do not pretend to understand this calculation, or even its result, but the next sentence seems to give the game away: "... The data... give for [the] ratio... a dependence on $\theta_{cm}$ which is nicely in agreement with the assumed commutativity restrictions for the hadronic units. The fit [to] the data is then reduced to a mere selection of the best function of $\theta_{cm}$ that achieves the desired fit." Some grammatical features of these remarks defeat me, but my best guess is that what the author means is that any function whatsoever which one makes up is automatically the prediction of his theory! This is indeed a remarkable theory.

I do not think the present state of the work as reported is in a condition which merits publication in Phys. Rev. Letters. As a stylistic note, the manuscript is written in broken english which adds greatly to the difficulty of understanding what the author is trying to do. Finally, I note that all references in the manuscript are dominated by a publication known as the "Hadronic Journal," which is unknown to me.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 26, 1982

To the Editors of
THE PHYSICAL REVIEW LETTERS
1 Research Road
RIDGE, New York 11961

RE: "Use of the Hadronic mechanics for the ......"
BY: R. M. Santilli
NO: LR2111

Dear Colleagues,

I acknowledge receipt of the rejection of my paper jointly with a copy of one referee report. The desired referee appears to have a rather complete lack of knowledge of the experimental, theoretical, and mathematical studies underlying the paper. I am therefore respectfully asking that you ignore this report, and select two new referees according to the following qualifications:

(a) the referee should have an in depth knowledge of the indicated studies underlying the paper, as quoted in the references, e.g., proceedings of FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE—ADMISSIBLE TREATMENT, held in France on January, 1982 (copies of all references are available on request);

(b) in case of rejection by these experts, the report should identify technical errors, while expressions of personal feelings should be avoided as much as possible; and,

(c) for reasons communicated separately to your Editor in Chief, Dr. David Lazarus, the referee SHOULD NOT be selected from Harvard University, the Massachusetts Institute of Technology, and other local colleges.

The paper is therefore returned to you enclosed. Since the report mailed to me is purely qualitative, I provide below only qualitative comments. I remain, of course, at your disposal, for additional technical comments.

**AN HISTORICAL ASPECT.** P. A. M. Dirac made it quite clear in his limpid writings that he expected the violation of both the space and time reflection symmetries. In fact, in his paper Rev. Mod. Phys. **21**, 392 (1949), p. 393, he states

> "I do not believe there is any need for physical laws to be invariant under these reflections".

Scholars in relatativity can see in this statement one of the best manifestations of Einstein's teaching. In fact, we learn the equivalent role of space and time coordinates beginning from undergraduate courses in special relativity.

Apparently, the referee ignores completely this historical aspect. WHY?

**A STATISTICAL ASPECT.** The irreversibility of the macroscopic physical reality is established by incontrovertible experimental evidence, while the reversibility of particle physics is a mere conjecture at this time. The problem of the reconciliation of these two contrasting situations has remained unresolved since the time of its identification in the early part of this century.

Any researcher or referee who has done a minimal but serious study of this problem, knows that such a reconciliation is virtually impossible on true technical grounds. For instance, to achieve credibility, the supporter of a reversible particle mechanics must prove that the experimentally established noncanonical character of the time evolution of Newtonian systems can be reduced to a large collection of unitary transformations of the particle constituents. I am, of course, not referring to academic systems of perpetual—motion type. Instead, I am referring to the systems of the real world, e.g., those that are of non—Hamiltonian type because of drag and follower forces, as dayly encountered by engineers.

The most natural resolution of this historical problem is the recognition of a small violation of the time—reversal symmetry in particle physics, beginning with short range nuclear interactions. The experiment by Slobodrian, et al, is a clear indication of the possibility of a future final resolution of the problem along its most natural lines.

Apparently, the referee opposes even the continuation of research for the future resolution of this historical problem. WHY?

**AN EXPERIMENTAL ASPECT.** All experimenters I have personally contacted, besides those of ref. 1, have unanimously indicated their expectation that the time—reversal symmetry is violated in strong interactions. In their view, the only open aspect is the AMOUNT of the violation. The continuation of experimental efforts is therefore vital for the resolution of the issue.

Apparently, the referee opposes the conduction of new experiments. WHY?

**A SOCIOLOGICAL ASPECT.** As we all know well, one of the most important sociological aspects of contemporary research in nuclear physics is the expectation of contributions valid for NEW forms of energy, particularly for the hopes to achieve controlled fusion. In this latter respect, the problem of the reversible or irreversible character of nuclear interactions acquires a rather substantial dimension, not only of scientific—technological nature, but also of administrative—financial character.

This is well known to experts in the field. For the sake of this letter, it is sufficient to note that, say, a deviation in the time—symmetry of the order of $10^{-3}$ [which is more than compatible with the measures by Hardekopf, et al] could imply a rather significant effect for sufficient fluxes of nucleons. In turn, this could have sizable implications in the very design of attempts at the controlled fusion.

In short, rather immense human and financial resources are currently spent by several Countries in attempting the controlles fusion. Scientific accountability in the use of public funds demands that fundamental physical issues of the type addressed by Slobodrian, et al, be resolved in the most exhaustive possible way.

Yet, the referee says that this serious experimental study is not needed. WHY?

**A FIRST THEORETICAL ASPECT.** The referee essentially claims something to the effect that the special relativity should imply only one form of interacting Lagrangians. Since this is not the case, he would therefore conclude by saying that the special relativity is a "remarkable theory". In fact, he uses exactly the same reasoning, although applied to the fact that the rudimentary model of the paper does not predict an explicit dependence on $\theta_{cm}$.

We are all aware that to achieve one given interacting Lagrangian we need considerably more ingredients than Lorentz covariance. Yet, the referee desires a different criterium for the theory of the paper. WHY?

**A SECOND THEORETICAL ASPECT.** We all know equally well that reflection operators depend explicitly on the rotational symmetry. In particular, the exact T—symmetry implies the exact spherical symmetry of the charge distribution of protons and neutrons in the conditions of the experiment by Slobodrian, et al.

We are all aware that the possibility of a perfectly spherical symmetry of the charge distribution of nucleons under impact with nuclei is quite remote. Yet, the referee tacitly implies the validity of this absolutely rigid charge distribution. WHY?

**A THIRD THEORETICAL ASPECT.** The current efforts to construct the hadronic mechanics are essentially oriented toward the representation of nucleons whose spherical symmetry admit small deformations. This is technically realized with generalizations of the enveloping associative algebra into isotopic or genotopic forms, that is, with a generalization of Lie's theory at the level of the envelope (and thus, of the Lie algenras and groups). By no means, these efforts are intended to be the only possible way of reaching a dynamics which is intrinsically irreversible, and numerous other ways are conceivable.

The promotion of theoretical studies of different orientation on the problem of particle irreversibility is clearly essential to achieve maturity of experimental finalization, even for the case of the reversibility. Yet, the referee appears to oppose these theoretical studies. WHY?

**A FEW ADDITIONAL REMARKS.** The following aspects of the report deserve a comment.

(1) My English is admittedly broken. In fact, I never had the time to sit in an English class. Yet, my English has been fully sufficient to communicate with colleagues willing to communicate. Besides, your Journal has some of the best staff in the English language.

(2) The following statement in the report is erroneous

"A repetition of the $^9Be(^3He,p)$ and inverse reaction measurement by Hardekopf, et al ....."

In fact, these experimentalists measured only the polarization of the direct reaction, and assumed the measures by Slobodrian, et al, for the inverse reaction, as clearly stated in their paper. In the final analysis, this is only one (out of several) reasons calling for additional experiments. Actually, errors such as this one by the referee constitute one of the motivations whereby the publication of the paper by Hardekopf, et al, was done excessively soon on a comparative basis with the long consideration process of the paper by Slobodrian, et al, as reported in detailed to Dr. Lazarus.

At any rate, the point confirms beyond any reasonable doubt that the referee does not possess sufficient technical knowledge of the topic.

(3) As a referee of your Journal, when I receive a paper listing a Journal unknown to me, It is my ethical duty to study the relevant papers of that Journal BEFORE passing judgment. If, for any reason, I do not have the time to do that, I simply disclose it to you, and ABSTAIN from passing judgment. This referee admits explicitly that he does not know the Hadronic Journal. He also admits explicitly that he does not know the studies underlying the paper (*). YET HE EQUALLY PASSES JUDGMENT. WHY? Most paradoxically, I submitted the paper with a selection of at least some of the most relevant papers in the Hadronic Journal, precisely to prevent this claim. EVEN WITH THE READY AVAILABILITY OF PAPERS, THIS REFEREE HAS CLAIMED LACK OF KNOWLEDGE JOINTLY WITH THE PASSING OF JUDGMENT. WHY?

For these and other reasons indicated separately to Dr. Lazarus, I beg you:

I : to ignore the report of this referee;

II : to avoid the use of this referee in future editorial processings at your journal; and

III: to implement an equitable scientific process via the selection of two experts in the field of the proposal, as specified above.

In particular, please keep in mind that, if my paper is rejected because of technical errors identified by the referees, not only you can count on my graceful acceptance, but you and the referees will have my sincere gratitude.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
cc: Dr. D. Lazarus, APS;

(*) I refer here not to my papers, but instead to papers by distinguished mathematicians, theoreticians and experimentalists we can identify in the references considered.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 28, 1982

RE: "Use of the hadronic mechanics for ...."
BY: R.M.Santilli
RE: LR2111

Dr. G. TRIGG
Editor
The Physical Review Letters
Ridge, New York

Dear Dr. Trigg,

I would appreciate the courtesy of replacing the NOTE ADDED IN PROOF of my paper with the enclosed one, which is the result of rather considerable consultations with colleagues in the USA and abroad. The version in your possession may be misleading because it does not indicate explicitly that the value 0.0 must be referred to the DIFFERENCE P–A (polarization less analyzing power), and not to each individual one of these quantities, for the exact T–symmetry.

Needless to say, the paper may contain additional imperfections of this type. You can therefore count on my best possible collaboration for technical improvements of this type suggested by qualified referees.

Your assistance in this submission is appreciated.

Very Truly Yours,

Ruggero M. Santilli

RMS-mlw

encls.

**NOTE ADDED IN PROOF.** Upon completion of this work, R. MIGNANI (Univ. Rome, Italy) informed me of the appearance of the rapid communication by R. A. HARDEKOPF, P.W.KEATON,P.W.LISOWSKI, and L.R.VEESER, Phys. Rev. Letters C25, 1090 (1982). Contrary to the statement by these authors, their experiment is still inconclusive for several reasons. In fact, their only four measurements can be fit by several curves, including a possible central peak (not considered in the communication). Also, they measured only the polarization of the direct reaction and relied upon the measures by Slobodrian et al. on the analyzing power of the inverse reaction. These data do not appear to give the value 0.0 for the difference :(polarization less analyzing power), as needed for the exact time reversal symmetry. As a result, the only aspect that the measures by Hardekopf et al may leave open is the AMOUNT OF VIOLATION.

REVISED VERSION DATED MAY 28, 1982

of the Note Added in Proof

of the paper

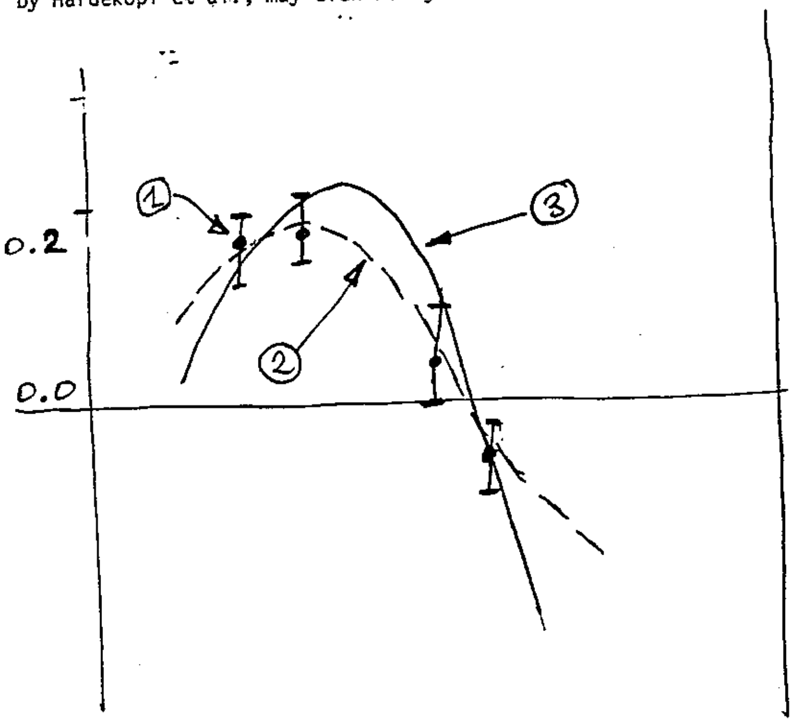
"Use of the Hadronic Mechanics for the ...."

by R.M.Santilli

ref. (Phys. Rev. Letters) LR2111

QUALITATIVE ELABORATION OF THE NOTE ADDED IN PROOF OF THE PAPER
"Use of the hadronic mechanics for ...."
by R.M.Santilli
submitted to Phys. Rev. Letters
PRL ref. no LR2111

ASSUMPTION: That the four measures by Hardekopf et al. do indeed yield a null difference (P- A) at those points.
ARGUMENT: this is not sufficient to establish an exact T-symmetry (i.e., P - A = 0) because the four measures can accomodate a family of curves, all implying a non-null difference P - A.
CALCULATIONS: The statistical probability that the four measures by Hardekopf et al imply exactly the same curve as that of the analyzing power of the inverse reaction is quite small and, depending on the (unknown) error of the four measures by Hardekopf et al., may even be ignorable




NOTES:
(1) indicate the four measures by Hardekopf et al. and their error for the polarization of the reaction $^9Be(^3He,p)^{11}B$.
(2) - - - indicates the curve of the (over fourteen) measures by Slobodrian, Conzett, et al. on the analyzing power of the inverse reaction $^{11}B(p,^3He)^9Be$
(3) ———— indicates one of the infinite number of curves admitted by measures as per note (1) ALL different than the curve as per note (2).

THE PHYSICAL REVIEW

AND

PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES

1 RESEARCH ROAD BOX 1000 RIDGE NEW YORK 11961

Telephone (516) 924-5533

2 July 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg
Editor
Physical Review Letters

enc.

P.S: The referee was chosen from your list of experts.

G.L.T.

NOTE BY RMS: THE REFEREE IS EXPECTED TO BE S. OKUBO.

R2L

MS#023550
Use of the hadronic mechanics for ...."
R. M. Santilli

1) The idea is new but quite unorthodox with many untested hypothesis. The theory contains two arbitrary time-reversal violating interactions associated with two arbitrary operators $^{\blacktriangleleft}T$ and $T^{\blacktriangleright}$ if I understand correctly. Thus it has little quantitative predictive power.

2) A large time-reversal violation in the strong interaction will cause many problems in conjunction with the presence of weak interactions. For example, consider angular correlations between polarization axis and momentum of, say, weak decays of polarized nucleus or polarized $\Lambda$-particle in $\Lambda \to p\pi$. If a large time-reversal really exists, the effect should already have been observed. Usually, this fact is quoted to imply its absence by a ratio of $10^{-3}$ to one. We note that a small effect of similar nature is known to exist in $K_L^o \to \nu\mu\pi^+$ decay. A far more serious problem is the absence of the electric dipole moment of the neutron. Many theories have been simply abandoned because of this fact alone. The author should show that his theory will be consistent with these experimental facts in spite of a large violation of the time-reversal.

3) A large time-reversal violation would, I believe, contradict with the currently accepted cosmology. Although this fact should not be counted against it, it will weaken the philosophy of the paper. Note that the popularity of the grand-unified-theory is partly due to its consistency with cosmology.

4) In conclusion, I cannot say that this paper satisfies the urgency criteria for publication in Phys. Rev. Lett. However, if questions raised here are satisfactorily resolved in a future revised version, then it may be accpetable for publication in Phys. Rev.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

July 21, 1982

Dr. GEORGE L. TRIGG, Editor
The Physical Review Letters
1 Research Rd
RIDGE, New York 11961

RE: "Use of the hadronic mechanics for the best fit of the time—asymmetry recently measured by Slobodrian, Conzett, et al."
BY R.M.Santilli; PRL ref. No. LR2111/L—1

Dear Dr. Trigg,

I would like to express my sincere gratitude for the quite valuable referee's report you mailed me on July 2, 1982. I believe that all the comments by the referee are scientifically sound and critically constructive. I have therefore provided a sincere effort to comply with the referee's suggestions by rewriting the paper entirely [except the calculations and formulae which have been only controlled again].

However, some of the referee's comments call for a critical assessement of current theories (unified gauge theories and cosmology in particular) that does not seems recommendable to conduct in a paper on time—asymmetry. My asnwer is therefore consisting of:

- the enclosed revised version which, as you can see, is as smooth as scientifically possible in the sense that particular care has been provided to avoid possible conflicts with readers of potentially different views, as well as to avoid any comment on existing theories; also, the revised version closes with the indication of possible contributions of the hadronic mechanics, and underlying time—asymmetry, to quark theories which I believe can be potentially relevant; and
- this letter in which I take the liberty of indicating aspects that are not recommendable for consideration in the paper.

EXPERIMENTAL SITUATION. I was in the South when the Los Alamos experiment on time—asymmetry was conceived; I have been in touch a number of times with the Québec—Berkeley and the Los Alamos experimenters; and I have consulted with a number of experimentalists here and abroad. I believe that the Québec—Berkeley experiment is correct as published in PRL. At any rate, the experimentalists have recently repeated the measures via a carbon polarimeter, by confirming the original measures. I hope that their confirmation will soon appear in press. This is the experimental situation in nuclear physics (see below for other fields) independently from any theoretical consideration, e.g., the need to achieve a true compatibility of the particle description with the irreversibility of the classical real world.

WEAK INTERACTIONS. An important point of the referee's report is the sound and predictable need to avoid conflicts with unified gauge theories of weak and electromagnetic interactions. I hope that the revised version has answered this question, by indicating the reasons why the time—asymmetry measured by Slobodrian, Conzett et al. is expected to be fully compatible with gauge theories. In fact, the origin of the time—asymmetry can be identified theoretically and experimentally in the deformation of the charge distribution of hadrons under impact and penetration within those of other hadrons. The time—asymmetry measured by the Québec—Berkeley collaboration occurs for nuclear reactions involving the exhange of two nucleons. In the transition to the leptonic decays of hadrons, such a time—asymmetry is expected to descrease substantially, assuming that a deformation of the charge distribution makes sense for the case of point—like leptons. Also, as stressed by the experimenters, scattering amplitudes do not appear to be sufficiently sensitive to the time—asymmetry. Thus, for any comparison to have sense, the data for the leptonic decays should be re–formulated for the polarization/analyzing power cases, assuming that it is possible for the decays considered.

It appears that a considerable segment of the physics community is under the expectation that the amount of time–asymmetry measured by the Québec–Berkeley group is a sort of new "strong constant", in the sense that should occur for all strong interactions, by therefore re–sulting into a direct conflict with unified theories.

The paper submitted will have achieved one of its primary objectives if it succeeds in indicating the erroneous character of this belief. In fact, a difference in time–asymmetry is al–ready measured in the two different reactions studied by the Québec–Berkeley group.

An aspect which has been omitted from the paper is the indication of the recent proble–matic aspects of gauge theories in regard to their prediction of the heavy bosons. As you know, these predictions have not been confirmed at DESY, and a reshuffling is under way at CERN. The affair has been termed "embarassing" in a recent note in Science here enclosed.

ELECTRIC DIPOLE MOMENT OF NEUTRON. This is another fully sound comment by the referee. However, the null value of the moment has not been touched because the paper is not intended to present a structure theory of nucleons. At any rate, we should not forget that a structure theory of the neutron which is capable of representing the null value of the dipole moment in a form acceptable by the scientific community at large, is still lacking at this moment. In fact, quark theories do not appear to have an explicitly computed, identically null probability of tunnell effect for free quarks [besides other requirements] to provide a conclusive solution of the problem.

COSMOLOGY. Again, the referee is correct in indicating the relationship between the popularity of a theory and its alignment with contemporary views in cosmology. I am also happy to see that the lack of apparent agreement of the Québec–Berkeley time–asymmetry with cosmology is not recommended as a serious drawback by the referee. In fact, no cosmology should be taken seriously unless it is capable of representing in full [actually, it is based on] the irrever–sibility of the real world. This basic requirement does not appear to be satisfied by current theories in cosmology, as one can see from the fact that the PPN approximation is essentially reversible in dynamic al contents, or from other facts. But this is only one of the major problematic aspects of cosmology today. We should not forget that at time zero the universe was the biggest possible black hole. Unless the explosion of a black hole is proved to be possi–ble, contemporary cosmology cannot explain the birth of the universe in any credible way. Also, the basic equations are incompatible with electromagnetism, as one can see in Ann. Phys. 83, 108 (1974). In fact, for a massive body with zero total electromagnetic data, the equations for the exterior problem predict zero source, i.e., are given by $G_{\mu\nu} = 0$. But, matter has a charge structure. Whether in flat or curved space, classical electromagnetism predicts a non–null electromagnetic tensor $T_{\mu\nu}$ for moving charges with null total data of charge, electric and magnetic dipole moments, and radiations , unless all the charges are at rest and at very small mutual distances. The equations should tehrefore be $G_{\mu\nu} = cT_{\mu\nu}$ for the case considered.The situation appears to be clear–cut, in the sense that, either one accepts the basic equations of contemporary theories in gravitations, in which case electromagnetism must be abandoned and reconstructed, or one accepts electromagnetism, in which case the field equations for gravitations must be reviewed from their foundations. Additional serious problems have been raised through the years by Yilmaz [who has an intriguing theory apparently capable of at least reaching compatibility with electro-magnetism]. For these and other reasons, the aspect of cosmology has been completely ignored in the paper.

CONJECTURAL CHARACTER OF HADRONIC MECHANICS. This is a further point of the referee's report which is quite valuable. In the revised version I have therefore taken all the necessary precaution to stress more clearly the conjectural character of the new mechanics. Nevertheless, the agreements of the predictions of the theory with experimental data in nuclear physics should not be ignored. I am referring here to the several contributions by Eder [e.g., in representing nuclear magnetic moments]; the prediction of the deformation of the charge distribution of extended nucleons [of about 1%] and its agreement with the measures by Rauch et al; and, last but not least, the agreement with the Québec–Berkeley measures on time–asymmetry which is simply impossible via the ordinary QM, to our best knowledge at this time.

**PREDICTIVE POWER OF THE THEORY.** There is no doubt that the referee is correct in indicating that the rudimentary model of the paper has limited predictive power. However, we should keep in mind that the time–asymmetry [as well as the space–asymmetry and the rotational–asymmetry] vary from reaction to reaction. Thus, particular precautions have been taken in the structure of the new mechanics to AVOID single, fixed, predictions.

On more explicit terms, QM is based on the operator $H = T + V$ where $T$ is fixed, and $V$ is an "arbitrary" (in the language of the referee) potential needed to represent a sufficiently broad class of potential forces. The hadronic mechanics preserves $H$, and adds generalized forward and backward units $I^* = I + Q$, where $I$ is the unit of QM and the operator $Q$ is "arbitrary" to represent a sufficiently large variety of NON-potential forces. The identification of $V$ calls for experimental informations on the nature of the action-at-a-distance. The identification of $Q$ calls for additional experimental informations on the charge radius, density of hadronic matter, etc. As a result, the time–asymmetry is capable of varying from one reaction to the other, up to the point of being null ($Q = 0$) for point–like structures.

**SUITABLE JOURNAL FOR PUBLICATION.** I believe that the topic presented in the paper is best suited for a letter, and for this reason it has been submitted to you. You can count on my best possible understanding in case you recommend otherwise. However, please keep in mind my considerable uneasiness in turning the paper into a full length version. This is due to the fact that the basic ideas of the hadronic mechanics have by now appeared in print several times, and I see no reason to review them again at this time.

**THE NEED TO PURSUE NOVEL ADVANCES.** I am in full agreement with the general spirit of the referee report that due consideration and respect should be provide for existing theories receiving the majority of consensus. For this reason I have avoided any criticism of current views in the enclosed paper.

However, I believe that, jointly, we must pursue novel advancements via the traditional scientific process of trial and error, as I am confident the referee will agree. Lacking this process, we risk the transformation of physics into a religious preservation of old dogmas over a large financial platform.

In the particular case of the time–asymmetry, I believe that truly relevant advances along established trends are possible, such as a realistic possibility of achieving "strict confinement" of quarks and other possible contributions indicated in the concluding part of the paper. As a result, the acceptance of the experimental results on time–asymmetry, and the theoretical study of its representation, rather than being in conflict with existing trends, constitute the foundations for the possible solution of some of their problems.

Very Truly Yours

Ruggero Maria Santilli

RMS-mlw; encls:

1. Outline of possible applications of the hadronic mechanics to quark theories;
2. Diagram indicating the possible accomodation of curves with $P \neq 0$ in the Los Alamos measures;
3. Note recently appeared in Science in regard to the situation for heavy bosons.

**POSSIBLE APPLICATIONS OF THE HADRONIC MECHANICS TO QUARK MODELS, QCD, AND ALL THAT.**

Nontechnical lines prepared by the staff of
**The Institute for Basic Research**

We assume the reader is familiar with:

(1) The existence, at the mathematical level, of a Lie–isotopic and of a Lie–admissible generalization of Lie's theory;

(2) The existence of a Birkhoffian generalization of (classical) Hamiltonian mechanics as a realization of the generalized Lie theory via functions on $T^*M$; and

(3) The current efforts to build a "hadronic mechanics" as a realization of the generalized Lie theory via operators on (a suitable formulation of) a Hilbert space. The hadronic mechanics is being constructed as a generalization of quantum mechanics for extended hadrons under joint action–at–a–distance/Hamiltonian and contact/non–Hamiltonian interactions, in such a way to admit the Birkhoffian (rather than the Hamiltonian) mechanics as classical image.

The state of the art in the studies by mathematicians, theoreticians, and experimentalists for the construction of the hadronic mechanics is reported in the *Proceedings of the First International Conference on Nonpotential Interactions and their Lie–admissible Treatment*, Hadronic J. Vol. 5, numbers 2, 3, 4, and 5.

There are growing indications that the new mechanics can provide significant contributions in a number of essentially open problems of quark theories and related fields. No active research has been conducted to date in the topic. These few lines are intended to indicate some of these possibilities on a confidential basis.

(1) **Possible alternative to spontaneous symmetry breaking.** A basic idea of the hadronic mechanics is that of representing the extended character of hadrons via an isotopic generalization of the Hilbert space. Under such isotopy, conventional symmetries (those expressed via unitary operators) are generally broken. There are indications that this approach can be a valuable alternative to spontaneous and other treatments of symmetry breakings. A novelty of the approach is the achievement of the breaking without predicting new particles, evidently, because of the realization of the breaking without "action–at–a–distance" forces. This line of study has been proposed by S. K. Yun (IBR and Saginaw Valley State College).

(2) **Possible construction of quarks as clusters of more elementary particles.** The isotopy of the Hilbert space of a conventional QM particle implies the possibility of altering its intrinsic characteristics such as charge, spin, parity, etc. Therefore, it appears that the hadronic mechanics could "build" a quark within hadronic matter, in the sense that a cluster of particles obeying the hadronic mechanics could reach all the desired intrinsic characteristics for quarks. This possibility was formulated by R. M. SANTILLI (IBR) in 1978 and has remained unexplored since that time.

(3) **Possible contribution to the open problem of quark confinement.** The available efforts to reach quark confinement are essentially based on the assumption that the same mechanics holds in the exterior and in the interior of hadrons. The hadronic mechanics recovers the conventional QM for the exterior treatment of a hadron (motion of its center of mass under long range interactions), while it postulates a generalized mechanics for the interior problem. This basic idea appears to be naturally set for a valuable contribution to confinement. In fact, particles obeying the generalized mechanics can occur only under short range, contact, non–Hamiltonian interactions. Whenever these interactions are absent, and the conventional physical conditions of contemporary detection are recovered, particles obeying the hadronic mechanics cannot exist, and must decompose into conventional particles. This idea was suggested by R. M. SANTILLI in 1979, and has also remained unexplored until now, pending the availability of more detailed formulations of the new mechanics.

(4) **Nonrelativistic equations of structure for light quarks.** As is well known, Schrödinger–type equations are currently available for quarks, provided that at least one of the quarks is heavy. For light quarks (e.g., as expected for pions), conventional nonrelativistic Schrödinger–type equations generally yield complex values of the total energy. Apparently, this difficulty can be by–passed by the isotopy of the eigenvalue equations, as it has been rudimentarily illustrated via the use of the Hulten potential by R. M. SANTILLI. As a result, it appears that the hadronic mechanics could provide new possibilities of achieving physically consistent structure equations for light mesons.

(5) **Miscellaneous applications.** If one acknowledges the possibility that the basic physical structure of contemporary quark theories is an excellent, but only approximate characterization of nature, and that a finer physical world exists within a hadron, an array of additional possibilities occur for contributions in numerous (if not all) aspects of quark theories, including possible adjustment of jet theories to experimental data, refinements of the predictions based on gluons, etc.

THE PHYSICAL REVIEW

AND

PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES

1 RESEARCH ROAD BOX 1000 RIDGE, NEW YORK 11961

Telephone (516) 924-5533

3 September 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Re: Use of the hadronic mechanics for the best fit of the time-asymmetry...
By: Ruggero Maria Santilli

LR2111

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

for George Barton

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

Third Referee's Report on

R. M. Santilli: "Use of the hadronic mechanics..."

MS# LR 2111

(1) Without passing judgment on "hadronic mechanics" as developed by numerous authors and papers published primarily in the Hadronic Journal, this paper is not a great contribution: it essentially states that a theory (hadronic mechanics) which ab initio was constructed so as to violate various generally cherished conservation laws including time reversal invariance indeed can account for such a violation. He does derive the appropriate formula but the theory is much too general to allow a quantitative comparison. Nor is it the only class of theories one can construct to account for a time asymmetry. Why should one accept "hadronic mechanics" over other alternatives?

(2) It is not sufficient to argue qualitatively that the violation of time reversal invariance depends on the process. Does this class of theories permit sufficient freedom to allow quantitative (order of magnitude) compatibility between the large violation (if it exists) of reference 1 and the very small violation (if it exists) of the absence of the neutron electric dipole moment? His paper does nothing to support "hadronic mechanics" from the theoretical side. All the support (in the context of time asymmetry) stands and falls with that experiment.

(3) I don't see how the words "for the best fit" in the title are borne out by the paper. The only issue is whether or not there is an effect and the

experiment is not unequivocal.

As I see it, the only point made by this paper i. that "h dron.. mechanics" can indeed account for time asymmetry. Since it .. a very u.. . .ox theory this is hardly much of a recommendation.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 9, 1982

Dr. GEORGE L. TRIGG, Editor
The Physical Review Letters
1 Research Road
RIDGE, New York, 11961

RE: note no. LR2111/L–1
"Use of the hadronic mechanics for ...."
by R.M.Santilli

Dear Dr. Trigg,

I acknowledge receipt of your letter of September 3 returning the paper with the referee's comments. I accept several of them as scientifically valuable. However, I have doubts of various nature on others. I have therefore revised the manuscript accordingly, and I am returning it to you enclosed. In particular, the revisions over the preceding version are the following.

[1] The term "best" in the title has been eliminated because inessential and questionable, as correctly indicated by the referee;

[2] The referee is correct in indicating that other interpretations of the time–asymmetry are possible. I have therefore added a sentence in the first concluding remarks of page 5, to the effect of indicating this expectation as well as soliciting their study. However, I felt obliged to indicate that, particularly the interpretations based on additive terms in the Hamiltonian, must prove their compatibility with the established irreversibility of the macroscopic world. This point is clearly important for physicists interested in a distinction between the pursue of knowledge and that of academic interests.

[3] The referee is also correct in indicating that a quantitative study of the compatibility of the time–asymmetry suggested by experiments[1] and gauge theory is much in order. This study is an important objective of our institute, and it is already under way. I have therefore indicated the appearance of a forthcoming paper in the topic in the fourth paragraph of page 5. I disagree firmly on the need to present the results jointly with the paper submitted. In fact, this paper deals with certain specific nuclear reactions involving the exchange of two nucleons, while the topic under consideration deals with leptonic decays of hadrons. The distinction between these two physical arenas is self–evident, and equally self–evident is the need to treat the two aspects separately.

[4] I have added a sentence to the footnote of page 6 to the effect of indicating that the four measures of the Los Alamos group are insufficient to establish the exact time–symmetry, as indicated in the enclosed diagram previously mailed to you (and not intended for publication because trivial). In the meantime I have visited Slobodrian in Quebec and personally inspected his measures and experimental setting. I have also conducted additional travel and research, all leading to doubts on the [rather fast] Los Alamos work.

[5] I have finally made three linguistic, minor changes (eliminated "back" after Dirac in page 1, and the like.

On the following technical points I disagree with the referee.

[a] The referee appears to be convinced that the variation of the time– and space–asymmetries from reaction to reaction is a mere personal belief. This is not the case. We are all in agreement on the violation of the P–symmetry, as established by experiment[5] and several others. I urge the referee to inspect again these papers and convince himself of the clear experimental evidence indicating the variation of the space–asymmetry from case to case. For the time–asymmetry we only have the two different reactions studied in ref.[1]. The amount of the deviations is clearly open at this moment, as we all agree. However, the fact that the violation changes from reaction to reaction is

incontrovertible, as established by the inversion of the convexity of the polarization curve from one reaction to the other.

[b] The referee appears to be still convinced of the need to reach a formulation of the hadronic mechanics capable of achieving one single prediction of time–asymmetry. I disagree because this would be contrary to experimental evidence. At any rate, it would be equivalent to pretending that the special relativity predicts one single interaction Lagrangian term. This is not the case [if it were, the relativity would have been rapidily abandoned half a century ago]. I still do not understand why the referee then has a double standard of scientific evaluation, that is, he accepts the special relativity even though the theory does not predict one single fixed interaction, while he rejects the hadronic mechanics because its available formulations is too broad.

[c] The referee appears to have genuine doubts on the validity of experiments[1], which are perfectly legitimate. However, he appears to have a much more permissive attitude for other aspects. For instance, has this referee rejected papers on gauge theories because the theory predicts a finite nonnull probability of production of free quarks, along the explicit statement to this effect by Nambu at the Einstein Centennian Celebrations (and as one can verify by himself via explicit calculations when a conventional space–time and a conventional mechanics is used)? —incidentally, I favor the publications of papers on quarks despite these open problems, because the opposite view would imply the halting of the scientific process of trial and error. But then the same standard must be used for the open problem of the time–asymmetry as well as that of the hadronic mechanics.

But most of all, I appeal to the referee for what is at stake here. It is true that my note does not constitute a great contribution, particularly when compared to other contributions by other authors to the construction of the hadronic mechanics [particularly those by H.C.Myung]. However, what is at stake here is whether human knowledge should be maintained at the level of the physical laws discovered long ago, or the scientific pursue of genuine advances should be permitted.

By ignoring all the aspects identified in preceding letters, and ranging from the need to achieve a true compatibility with macroscopic reality, to several others, the sole implications for controlled fusion are such to warrant a differentiated study of the issue, that is, that with hadronic mechanics and that with the atomic mechanics. Rather than preventing the appearance of some of them, the study of all the possibilities should be promoted. The future, rather than any of us, will tell which is the best way to go.

It is therefore hoped that, with the further revisions submitted, the paper will finally meet with the referee approval.

Very Truly Yours

Ruggero Maria Santilli
RMS—mlw
encls.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

September 28, 1982

Dr. GEORGE L. TRIGG
Editor
PHYSICAL REVIEW LETTERS
1 Research Rd
RIDGE, N.Y. 11961

RE: paper # LR2111/L-1, "Use of hadronic mechanics for the fit ...."

Dear Dr. Trigg,

I am rushing copy of a recent paper by the experimental group in Québec which, as you can see, CONFIRMS the existence of the time-asymmetry in the specific nuclear reactions considered. This paper has been submitted for a rapid form of publication. It shows rather clearly that my letter LR2111/1-1 is timely because dealing with a basic, open physical issue.

I would appreciate the courtesy of forwarding one copy of the paper and of this letter to each of the two referees of my paper LR2111/L-1. I hope they will acknowledge that the possibility that time-symmetry is truly violated in the cases considered is quite real, and definitely such to warrant an open scientific debate for the community at large.

A new element has emerged recently at the IBR. It concerns the possibility that the time-reversal symmetry can be restored to an exact form for the case of the experiment by Slobodrian, Conzett, et al, provided that the symmetry is expressed for extended particles. In different terms, the experiments indicate possible violation for the symmetry of contemporary use, THAT FOR POINT-LIKE APPROXIMATIONS OF PARTICLES. If the formulation of the symmetry is done instead by taking into account corrections due to the extended character of nucleons under short range interactions, then the exact character of the symmetry can apparently be restored. The proof for the model of paper LR2111/L-1 is trivial. In fact, the generalized units $I^{>}$ and ${}^{<}I$ are (scalar) multiples of the atomic unit I. The isotopic-antiunitary time-reversal operators $\tau^{>}$ and ${}^{<}\tau$ therefore leave invariant the equations of motion, by therefore being symmetry in the conventional sense. The P-A data are then nothing but a measure of the implications of the extended character of nucleons. To put it differently, the insufficiency does not appear to be in the notion of symmetry, but instead in the simplistic, atomic, definition of symmetries for point-like abstractions, when we have in reality extended charge distributions at distances smaller than their size.

I would appreciate advice whether the current version of paper LR2111/L-1 should be modified to include these latter results.

Very Truly Yours

Ruggero Maria Santilli
RMS-mlw
cc. Dr. Lazarus

CONFIDENTIAL

September 9, 1982

Dear Professor Okubo,

I am taking the liberty of contacting you for advice on a rather delicate situation that, unless kept under control, is risking to degenerate to the detriment of our community of basic research at large.

It concerns a rather strong opposition by referees of the Physical Review Letters to publish my note on the use of the hadronic mechanics for the fit of the time asymmetry by Slobodrian, Conzett, et al.

If considered per se, as an individual case, the rejection of the note is ignorable. In fact, I had in the past several papers rejected and all rejections were accepted with the best possible grace.

The case of my note, however, is fundamentally different this time, and may trigger events that could rapidly become outside the control of all of us. This is due to the fact that this rejection arrives following a chain of too numerous and too questionable [on ethical grounds] occurrences. Thus, it could be the typical drop which overflows the glass.

It would be impossible for me to give you an idea of the episodes I am referring to because it would take a book [perhaps one day I will write my memoirs and I will document them otherwise nobody would believe to the dimension of academic greed]. I therefore simply limit myself to send you a copy of my letter to Dr. Lazarus and of its enclosures. In this way you can see a small part of the facts I am referring to.

Another point you should be aware is that a new mechanics, the Birkhoffian mechanics, has been constructed without one single paper appearing in journals of the APS. This is due to oppositions by referees which can be only interpreted as essentially motivated by personal financial-academic-ethnic interests. Nevertheless the episode is very grave and well known to educated observers.

There is now a great fear that the episode will repeat itself again, owing to the total lack of control on the ethics of the refereeing process. I am referring here to the construction, this time, of the hadronic mechanics again without one single paper appearing in journals of the APS because of referee's problems.

Finally, you should be aware of the number of scholars interested in the field at this time. This may give you an idea of the pressure I have to take into account recommending a public action of containement of the organized academic interests against the pursue of novel physical knowledge.

I am going to Washington on September 14-15-16 for several reasons, but also to discuss this grave situation with qualified observers. One of the topics of the agenda is whether and when to pass to a public disclosure of the situation. This in turn may imply a disclosure of all the past, documented, episodes, and it would be a disaster for all, with international repercussions. A crisis of this nature must be avoided at any costs.

In case you considers it appropriate, I beg you to advice me on the appropriate action to undertake. For instance, should I continue to improve the paper and to resubmit it until approved? or should I simply withdraw the paper and submit it to a journal other than those of the APS?

Each case is very risky. The first is strongly opposed by several observers because of the determination to prevent a repetition of the episodes underlying the publication of the paper by Slobodrian, Conzett, et al [it took 1 ½ years to publish this experimental paper which should have been published immediately, and then criticized in separate papers by other experimentalists, as the SOLE way to have a genuine freedom in the pursue of novel human knowledge].

The second alternative is favored by myself for the simple reason that I do not want to waste my time in academic dances [after all, this is the reason why the Hadronic Journal was founded in the first place]. However, it is a very risky approach because my withdrowal may trigger the crisis I indicated earlier.

Please advice me for the best course of action. You would gain additional reasons for my sincere gratitude. However, if you decide to abstein from any advice, you can equally count on my full understanding.

Sincerely

P.S. I take the opportunity to enclose copy of a general presentation of our institute which I am confident you will like.

THE UNIVERSITY OF ROCHESTER
RIVER CAMPUS STATION
ROCHESTER, NEW YORK 14627

DEPARTMENT OF PHYSICS
AND ASTRONOMY

Sept 14, 1982

Dear Prof. Santilli:

Thank you for your honest and courageous letter. I greatly sympathize with your situation. I have had often, and has still the similar referee problem, ~~and~~ from time to time, needless to say. In general, if my paper were twice rejected by the same journal, I follow one of the following three alternative: I may simply drop the matter and forget it, or rewrite and keep it for suitable later use, or resubmit it to other journals when I feel that the paper is still worthwhile. Although the bad reports of referees is so painful to our ego, and is often down-right incorrect, we have to keep in mind also a impartial perspective on our-own works. Although the formulation you made may be mathematically elegant and appealing to you, others may not think so. Besides, the elegance of mathematics is not enough for physics, since we ~~m~~ have to compare our theory with experiments. We have always to ask the question by ourselves whether any theory can explain the experiments as equally as or better than other theories? You have to concede that the conventional QCD and unified electro-weak gauge theory based upon the quark model can account for impressingly large experimental facts. Also, ~~all~~ many of its predictions have been, since, experimentally confirmed. If you want to contest it, you have to show that your theory can do the same. The point I am making is that your formalism, though it may ultimately turn out to be correct, is still premature for comparison of this kind. This is the same reason why I am a bit discouraged by my works on non-associative Quantum Mechanics. Personally, I believe that the future of non-associative physics is to blend and/or modify the present frame-work of QCD. But it may take many years, if it will ever be successful. Meanwhile, we have to keep a low-profile and to be modest, I am afraid.

Returning to the particular question of the time-reversal violation, the conventional and orthodox approach (although it is not the only possibility) is via the so-called Kobayashi-Maskawa theory within the electro-weak gauge theory of Salam-Weinberg-Glashow. It contains only a few parameters and is compatible with almost all time reversal-violating phenomena (except for the recent work of Slobodrian et al) such as absence of the electric dipole moment of the neutron and $K_L - K_S$ mass differences etc. On the whole,

(No 3)

the theory can account for practically all electro-weak phenomena known at present, and is regarded as a beautiful theory as such. This is the reason why many experts are dubious at any unconventional new approach such as yours on the subject. They feel that it is a waste of time. Also, this is why they doubt the correctness of the experiment by Slobodrian et al. Since the recent history of high energy physics is full of many incorrect experiments, we cannot really believe such an attitude. Indeed, your theory ~~may~~ may explain the experiment of Slobodrian et al (assuming it to be correct) but not the very large bodies of other strong and electro-weak experiments which can be explained satisfactorily by the conventional theory.

May I honestly suggest to you to not make any unnecessary protest on the matter which will be simply ignored at best, or to be worse is interpreted to show how hopeless your position is? The best course seems at least to me that you simply drop the matter or submit it for other journals. The unnecessary protest, I am afraid, will bring nothing but unpleasantness to you, not to mention a fact that it may also damage your reputation.

I hope that the tone of this letter does not sound to be pontifical. If it unfortunately does so, then I beg your forgiveness. It is unintentional and is due to my insufficient command of English.

Sincerely, S. Okubo

CONFIDENTIAL-CONFIDENTIAL-CONFIDENTIAL

September 18, 1982

Dear Professor Okubo,

Please accept the sentiments of my most sincere appreciation and gratitude for your letter of September 14 I have found on my way back from Washington. I am particularly grateful for the open character of the letter which I consider essential for true scientific communication.

Needless to say, I understand and respect your view most sincerely. It is therefore only with considerable regret that I see myself forced by several circumstances to be unable to accept your recommendation to withdraw the paper and submit it to other journals. This is the result not only of a serious consideration of your proposal, but also of consultation with a number of other scientists that would be affected by the decision, as well as with concerned observers. Permit me to stress that your proposal is indeeed fully sound and I have implemented it in the past on several other cases. However, the implications underlying this paper are such to prevent a withdrawal at this time. It is a sort of "Rubicon" created by questionable events over one decade.

Permit me to indicate to you the reasons for my inability to withdraw the paper and then recommend a possible compromise. I shall express the situation as honestly as I can, with the understanding that I can be fully explicit on scientific grounds, but I cannot disclose in full all the political aspects.

THE SCIENTIFIC PROFILE. Permit me to disagree most respectfully but most firmly with your views. I believe that your remarks have no relevance at all for the paper. In fact, all your remarks are related to electroweak interactions, gauge theories and QCD, while the paper treats a fundamentally different field, that of certain nuclear interactions involving the exchange of two physical nucleons. All colleagues I have contacted fail to see how considerations on electroweak interactions can be used to reject a paper in strong nuclear interactions.

Secondly, all the theories you refer to are centrally dependent on the representation of the interactions as closed. In this case, the center-of mass trajectory must necessarily be time-reversal invariant, as stressed clearly in the paper. The model presented in the paper, on the contrary, is centrally dependent on the representation of the nuclear interactions as open (the paper studies nucleons "a" in interactions with the external nuclei "A" of the fixed target). This point alone is sufficient per se, ignoring all the others, to render inapplicable all your remarks. In fact, the violation of the time-reversal invariance CANNOT exist in your setting. This point is stressed in the paper beginning with the example of the center-of-mass trajectory of our Earth, which is strictly time-reversal invariant, and the need to reach an open interior treatment to see the irreversibility of trajectories.

The mention of the Kobayashi-Maskave theory is a confirmation of the complete inapplicability of your remarks. In fact, there is nothing wrong with this theory, even assuming that the time-asymmetry measured by Slobodrian, Conzett, et al is correct in the quantitative amount indicated by these experiments.

This compatibility is total and two-fold. First, you must pass from the open treatment of the paper via Lie-admissible birepresentations, to the corresponding isotopic Hilbert space treatment of the exterior, closed, strong problem. You will see the transition from two units, one per each direction of time (Lie-admissible algebras) to one single unit for both directions of time.

$$\begin{pmatrix} A\,{}^{\triangleleft}T\,B - B\,T^{\triangleright}A \\ \text{OPEN INTERIOR} \end{pmatrix} \Longrightarrow \begin{pmatrix} A\,T\,B - B\,T\,A \\ \text{CLOSED INTERIOR} \end{pmatrix}, \quad T = {}^{\triangleleft}T + T^{\triangleright} \qquad (1)$$

This implies the incontrovertible loss of time-asymmetry and the full regaining of the principle of detailed balancing. In fact, from eq. (9) of my paper you have

$$A^{D}/{}^{d}P = I^{D}/{}^{d}I \Longrightarrow A/P = T^{-1}/T^{-1} \qquad (2)$$

that is, P = A for a closed treatment of the Kobayashi-Maskava theory, as known anyhow.

But this is only a first part. The paper clearly stresses the existence of an intrinsic irreversibility, that is, the compatibility exists even if you turn the Kobayashi-Maskava theory into an open formulation. The reasons are simple. The time-reversal operator depends explicitly on spin, e.g. for $s = \frac{1}{2}$

$$\tau = e^{-i\pi J_2}\Big|_{\mathcal{A}} C \quad , \quad \mathcal{A} = \text{Ass. Alg.} \qquad (3)$$

Now, nucleons are extended objects. The experiments by Slobodrian, Conzett, et al (and more directly, those by Rauch) indicate that these extended charge distributions can experience deformations under sufficient impacts and contact interactions with other nucleons. Without any claim of being unique, these combinations of rotations and small deformations of extended charge distributions are represented in the hadronic mechanics via the isotopy of the associative algebras of operators in which the expansion of the exponential of ( 3 ) can be defined,

$$\tau^{*} = e^{-i\pi J_2}\Big|_{\mathcal{A}^{*}} C \quad , \quad \mathcal{A}^{*} = \text{Isotopic Ass.} \qquad (4)$$

where the new associative product is A*B = ATB, and T is the isotopy operator (T=1 for point-like approximations of the conventional atomic mechanics). Now the departure of T from unit is already different for the two reactions studied by Slobodrian, Conzett, et al. The compatibility with the Kobayashi-Maskava theory is then self-evident. In fact, when you pass to leptons you have experimentally established much smaller charge distributions (for the electron it is less than $10^{-19}$ cm vz the $10^{-13}$ for nucleons). Assuming that charge distributions of such a small size can be meaningfullly deformed, the amount of the deformation must necessarily be much smaller than that of nuclear reactions involving spherical objects one million time bigger. The time-asymmetry, assuming that it can be meaningfully defined for the particles considered, is then ignorable for contemporary knowledge.

These very simple quantitative arguments will be presented in a separate brief note under preparation here which will be submitted for Rapid Communication to Phys. Rev. D (the topic does not deserve a letter for PRL because it is trivial). The fact that this paper on the compatibility is forthcoming has been indicated in the paper submitted to PRL. I hope that these arguments can remove all doubts you may have on the complete lack of relevance of electroweak interactions with the topic of the paper.

A similar situation exists for all the other points you mention. As an example, you indicate the unquestionable successes of QCD. This paper under no way can be considered an alternative to QCD. In fact, the paper <u>does not</u> deal with the problem of the hadronic structure, either directly or indirectly. As a result, problems such as the electric dipole moments of nucleons are basically outside the objective of the paper.

Quite frankly, I am under the perhaps erroneous impression that the viewpoint expressed in your letter (which is much along that of the referee) is suggested by your advisors, and motivated by fears that our studies at large (rather than this paper) might damage the interests of academicians committed to quarks and QCD. In fact, I am confident you see perhaps more clearly than me the technical points indicated above, repeated in the letter submitted to PRL, and stressed in the literature of the hadronic mechanics. At any rate, please rest reassured that your view is not shared by other physicists in quark fields I have contacted. In fact, they see no relevant connection between quarks theory and the problem of time-symmetry in nuclear reactions. Most importantly, permit me to reassure you that these colleques in quark fields see no threat whatsoever to their research by our efforts in nuclear reactions. Finally, you should keep in mind that our institute is actively involved in a number of CONTRIBUTIONS to quark lines via the use of the hadronic mechanics, as well as via conventional mechanics. After all, you should keep in mind that I am the originator of the series "Developments in the quark theory of hadrons" edited by Rosen and Lichtenberg (and actually I have supported this project with personal funds to make it a reality).

But most importantly, even if you ignore the fact that extended charge distributions cannot be rigid, you must consider the experimentally established reality of the macroscopic irreversibility. All theories ofparticle physics which are unable to recover in a quantitative,credible way this experimental reality must be rejected. It is unfortunate that regrettable circumstances had forced you not to be present at our international conference in Orleans. In fact, you would have seen a river of substantial problematic aspects forHamiltonian theories to be truly able to achieve quantitative compatibility. After all, the non-Hamiltonian character of the real macroscopic world is experimentally established beyond any conceivable doubt.

SOME POLITICAL ASPECT. I am afraid that the lack of publication of my note without true, credible, technical criticisms would be ethically wrong. I must stress the ethical profile because the scientific profile leaves no room for academic dances. In fact, as you know well, (I) nucleons are not points, but extended objects; (II) extended objects simply cannot be rigid; and (III) deformations of the charge distributions necessarily imply time-irreversibility because of the structure of operator (3) above. Thus, the only scientific argument open at this time is the amount of the time-asymmetry. This is the primary reason why I have contacted the Editor in Chief of PR, Dr. Lazarus. In fact, I inteded to provide all the necessary information to prevent the creation of a record of unethical refereeing at Journals of the APS.

What is at stake here is not a single paper. First of all, the paper is the culmination and the representative of the virtual entirety of the First International Conference on Nonpotential Interactions and their Lie-admissible Treatment held under joint support by the French and the U.S. Governments with four volumes of proceedings, and participants from virtually all developed and developing countries. What is at state is therefore whether the voice of all these valuable scientists should be permitted or suppressed.

But there is much more. What is at stake is whether the journals of the APS encourage, or ortherwise permit all valuable papers in the interests at large of this Country, or they permit the publication only of papers compatible with the financial-academic interests of quarks/QCD studies.

Put it differently, what is at stake here is the true ultimate spirit of this Land, that is,whether we do have indeed a free pursue of valuable scientific knowledge, or we do have indeed a totalitarian filtering or scientific thought along established financial-academic interests. In fact, the paper does not claim to be the sole recipient of physical thruth, and actually encourages studies along different lines, as the SOLE genuine way to pursue novel knowledge.

These issues are very serious indeed. As indicated to Dr. Lazarus in a recent personal letter, this is the land where my children will live, and I intend to do everything in my power, at whetever personal costs, to contribute to its good scientific health. The future of my children is at stake here. In fact, if a paper is not published because of political reasons only, and without any credible technical reason, then the same may happen for an unlimited number of papers in different fields.

The downspiral of the Country because of ethical reasons would then be inevitable, unless groups of individuals have the courage to act in disrespect of their personal interests.

But there is much more. On military grounds, you should remember that all military systems are non-Hamiltonian. The promotion (let alone the permission) of theories of non-Hamiltonian character has therefore direct military value. It is only the community of quark physicists, in its general immodesty, that claims to have reached final knowledge via a small Lagrangian.

On civilian grounds, the problem of the amount of the time-asymmetry has profound implications for controlled fusion because of its origin (deformation of the charge distribution) and consequences (e.g., alteration of the magnetif moment, as rather natural in nuclear physics).

But there are additional reasons that I cannot disclose here in the best interests of all.

MY PROPOSAL. After three reviews, PRL has been unable to identify even one, credible, technical error or criticism of the paper submitted. It is therefore unlike that additional referees will be able to provide them.

The compromise I recommend is therefore that of
- publishing the paper in the form available with any additional clarification considered recommendable; and
- publishing soon after or jointly another paper by another author such as you which criticizes my paper,e.g., as a Rapid Communication in Phys. Rev. D.

To put it explicitly, I am inviting here you to collect your negative views on the nuclear time-asymmetry and make them available to the scientific community at large in the form of an official paper for publication.

I would be delieghted to be the referee of such a paper, and ACCEPT IT FOR PUBLICATION.

You must understand that we sincerely welcome criticisms, and actually encourage them, as explicitly done in the paper, provided that they are done in a scientifically productive way. We simply cannot accept with grace unethical suppressions of plausible scientific views via a criptic process of an unknown referee.

At any rate, you should know that I have provided by now a complete disclosure of the case and of its documentation to colleagues and observers. Our final decision will be taken collectively (rather than by myself alone). The case, therefore, is already quite serious and under no circumstance should be under-estimated.

Sincerely

Ruggero Maria Santilli

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 18, 1982

Dr. GEORGE L. TRIGG
Editor
Physical Review Letters
1 Research Rd
RIDGE, N.Y. 11961

RE: "Use of the hadronic mechanics for the fit of the time-asymmetry...."
by R.M.Santilli, ref. PRL no. LR2111/L-1

Dear Dr. Trigg,

I am hereby formally asking that you include as part of the file on this paper the following copies of letters.

1. Letter by myself to Dr. Okubo (Rochester Univ) dated September 9, 1982;

2. Letter by Dr. Okubo to me dated September 14, 1982; and

3. Letter by myself to Dr. Okubo dated September 18, 1982.

Thank you.

Very Truly Yours

Ruggero Maria Santilli

RMS-mlw

cc. Dr. Lazarus, Urbana, Illinois

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES · 1 RESEARCH ROAD
BOX 1000 · RIDGE NEW YORK 11961
Telephone (516) 924-5533



October 29, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your note of Oct. 16 and the enclosed material. I am sorry that I did not have time to return your recent telephone call about the manuscript by the Quebec group. However, it is our policy not to discuss manuscripts with third parties.

Sincerely yours,

Gerard J. Dreiss

Gerard J. Dreiss
Associate Editor
Physical Review C

GJD/lf

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

November 1, 1982

Dr. G.J.DREISS,
Physical Review

Dear Dr. Dreiss,

Just a few words to express my appreciation for the courtesy of your letter of October 29, 1982, as well as my full understanding of and agreement with its contents.

In case I can be of any assistance in the consideration of the paper by the Quebec group, please do not hesitate to call on me, either for possible technical reviews of certain theoretical aspects (the sole area of my expertise), or for consultation on advisability of accepting certain specific reports, of course, under the refereeing confidentiality.

From the courtesy of your letter, I am confident you have understood our objectives. We are only interested in the participation of Phys. Rev. in the scientific process of establishing or disproving the time-asymmetry (experimentally and theoretically) via scientific articles. For this purpose I am much in favor of the publication of papers presenting opposite views. Only the future will resolve the issue one way or another. What is vital for a healthy status of research is that plausible or otherwise valuable views are not suppressed at the level of the refereeing process.

Sincerely

Rugg Santilli

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

November 2, 1982

Professor S. Okubo
Department of Physics
University of Rochester
ROCHESTER, New York 14727

Dear Professor Okubo,

I would like to formally bring to your attention the fact that the Kerkeley-Quebec-Bonn experimental group on the time-asymmetry has repeated the experiments, by confirming the original measures. A paper has been recently submitted to Phys. Rev. C as Rapid Communication. A courtesy copy of the paper is enclosed for your convenience.

Following specific agreements reached with Professor Lazarus,Editor in Chief of the Physical Review and Physical Review Letters, my letter providing a possible theoretical interpretation of the time-asymmetry for open nuclear reactions is under major reviews, for resubmission at some future time.

At this moment, permit me the liberty of recommending that you withdraw your comments on time-asymmetry via a formal letter to Dr. Lazarus, on the grounds that at the time of your comments you were not aware of the repetition of the experiments (which is definitely true). We believe that this is a perfectly justified action under the circumstances which will be beneficial to you as a scientists, as well as to the pursuit of novel physical knowledge. Also, your acceptance of our recommendation might halt a rapid deterioration of the case, which has already reached alarming proportions.

The courtesy of your communication as soon as possible of your decision on the matter would be appreciated.

Very Truly Yours,

Ruggero Maria Santilli
President

RMS-mlw

THE UNIVERSITY OF ROCHESTER
RIVER CAMPUS STATION
ROCHESTER, NEW YORK 14627

DEPARTMENT OF PHYSICS
AND ASTRONOMY

November 10, 1982

Prof. R. M. Santilli
The Institute for Basic Research
96 Prescott St.
Cambridge, MA 02138

Dear Prof. Santilli:

I am puzzled by remarks made in your recent letter of Nov. 02. I am embarrassed to confess that I was one of the referees of your paper as you rightly guessed. Although I did not recommend its publication to the Letters, I suggested that it should be published rather in Phys. Rev. Indeed, the urgency criteria for Letters, which the editors demand for referees, did not leave any other choice. However, I did not make any other written statement to Dr. Lazarus which you mentioned in your letter. As a matter of fact, I was obviously in a delicate situation, since you are my friend and since I believe basically the possible relevance of non-associative algebra to physics. Because of this delicacy, I requested of Dr. Lazarus that I would not any more serve as a referee of your paper for the second time, and suggested to him names of some physicists who might judge your paper impartially. That was the extent of my dealings with Dr. Lazarus.

I hope that this letter will clear up any misunderstanding.

Sincerely,

S. Okubo

S. Okubo

SO:jm

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

December 14, 1982

Dr. D. LAZARUS, Editor in Chief
Physical Review and Physical Review Letters
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I hereby respectfully submit for publication in PHYSICAL REVIEW LETTERS the enclosed paper entitled

A POSSIBLE TIME–ASYMMETRIC MODEL FOR OPEN NUCLEAR REACTIONS

As you can see, the paper deals with an intriguing, fundamental, open problem of contemporary physics: the origin of the irreversibility of our macroscopic world. As such, it touches aspects in separate branches of physics. I am therefore taking the liberty of recommending a comprehensive review and, for this task, I enclose some 20 copies of the paper and of this letter.

Confident in your benevolent understanding and cooperation, in this letter I shall identify some of the major technical aspects deserving specific review. The selection of referees in this case does not appear to be an easy task. In the hope of being of some assistance in this respect, I shall also identify the leading experts in each field considered. I shall remain at your disposal for mailing to you on request a copy of all needed references, including monographs and conference proceedings, as well as for any other assistance you may desire.

1. NEWTONIAN MECHANICS. The Newtonian foundations of the paper are evidently the first aspect deserving a specific inspection. This is recommendable also in view of recent advances in the field, with particular reference to the achievement of the Birkhoffian generalization of analytic mechanics for contact/nonpotential forces [see the monographs of ref.s 2,3 ]; which constitute the classical foundation of the analysis.

These advances have not yet reached the physics audience at large, and are known only to experts in the fields. To have meaningful referee reports, it is therefore essential that you select **referees with a record of publication in non–Hamiltonian systems.** The best I can recommend are

– Professor R. BROUCKE
Department of Aerospace
Engineering and Engineering Mechanics
University of Texas at Austin
AUSTIN, Texas 78712–1085

Professor J. KOBUSSEN
Swiss Federal Institute
for Reactor Research
CH–5303 WURENLINGEN, Switzerland

– Professor H. H. E. LEIPHOLZ
Solid Mechanics Division
University of Waterloo
WATERLOO, Ontario Canada

Professor K. HUSEYIN
Department of Systems Design
University of Waterloo
WATERLOO, Ontario N2L 3G1 Canada

2. STATISTICAL MECHANICS. The second branch of physics which should be taken into consideration for any refereeing on irreversibility is, of course, statistical mechanics. As you can see, I have attempted to give proper credit in the paper to Nobel Laureate

– Professor I. PRIGOGINE
Faculte des Sciences
Universite Libre de Bruxelles
1050 BRUXELLES Belgium

and

Center for Statistical Mechanics
The University of Texas
AUSTIN, Texas 78812

In fact, the studies by his group have been fundamental in the identification of the non–Hamiltonian character of irreversibility at the level of statistical ensemble.

Additional experts on the non–Hamiltonian origin of irreversibility that I recommend are

Professor J. FRONTEAU
Departément de Physique
Université d'Orléans
45046 ORLÉANS CEDEX
France

Professor S. GUIASU
Departement de Mathématiques
Université du Québec a
Trois–Rivières
Case Postale 500
TROIS–RIVIÈRES,
Québec G9A 5H7 Canada

Professor A. TELLEZ–ARENAS
Departément de Physique
Université d'Orléans
45046 ORLÉANS CEDEX
France

Admittedly, the non–Hamiltonian origin of irreversibility may still not be accepted by individual physicists. However, since it is incontrovertible at the Newtonian level, it is manifestly plausible in Statistical mechanics, to say the least.

One of the first tasks expected from you, as Editor in Chief of the Journals of the American Physical Society, is that of preventing the possible suppression of plausible fundamental views via the referees process. For this task, permit me to recommend, mot respectfully, that you exercise particular care in the refereeing of the statistical profile. Of course, individual statisticians may not necessarily share Prigogine's view on the origin of irreversibility. The important point is that these personal views by individual statisticians are not used to suppress plausible fundamental advances.

3. EXPERIMENTAL ASPECTS. As stated clearly in the paper, the experimental foundation of the paper is given by the apparent deformation of the charge distribution of hadrons during impact and penetration within nuclear matter, as measured in experiments [ 16 ]. In fact, the time–reversal operator is made up to two terms, a spin term and one for complex conjugation. A possible deformation of the charge distributions of nucleons "must" therefore imply a form of time–asymmetry.

It appears recommendable that, on experimental grounds, you consult above all the originator of experiments [ 16 ],

– Professor H. RAUCH
Atominstitut
Schuttelstrasse 115
A–1020 WIEN, Austria

The additional and more direct experimental basis of the paper is given by the measures of the time—asymmetry in certain nuclear reactions [ref.s 14]. Again, it is advisable to consult the team leaders

— Professor R. J. SLOBODRIAN
Laboratoire de Physique Nucléaire
Université Laval
QUÉBEC G1K 7P4 Canada

Professor H. E. CONZETT
Lawrence Berkeley Laboratory
University of California
BERKELEY, California 94720 USA

who are excellent theoreticians, besides being distinguished experimentalists.

As you are aware, an experimental group at Los Alamos is currently confuting the amount of time—asymmetry of the measures by the Slobodrian—Conzett group, ref. [ 15 ]. Again, I recommend the consultation of at least some member of this additional team, such as

— Professor R. A. HARDEKOPF
Los Alamos Scientific Labs.
Mail Stop 480
LOS ALAMOS New Mexico 87545

Professor L. VEESER
Los Alamos Scientific Labs.
Mail Stop D410
LOS ALAMOS, New Mexico 87545

The understanding is that the disagreement we are referring to here is for the AMOUNT of time—asymmetry. I expect that the Los Alamos group agrees with me that the time—reversal symmetry is indeed violated in OPEN (nonconservative) nuclear reactions. Therefore, and this should be stressed to avoid unnecessary incidents, the Los Alamos rebuffal of the Slobodrian—Conzett measures has NO BEARING on the paper submitted. In fact, the paper presents a possible model of time—asymmetry with the understanding that the amount of violation must be finalized via future experiments.

4. THEORETICAL ASPECTS. The paper submitted is an offspring of my failures to achieve a quantitative interpretation of the EXPERIMENTALLY ESTABLISHED irreversibility of our real world, with the CONJECTURED reversibility of the particle dynamics of the nuclear world.

To understand the problem, you should recall that, on one side,

— the time evolution of open systems of our real world is necessarily NONHAMILTONIAN—NONCANONICAL (different views may tacitly imply the validity of the perpetual motion in our environment....);

while, on the other side,

— the time evolution of currently predominant theoretical views in nuclear physics is of HAMILTONIAN—UNITARY nature.

My failures are due to the inability to reach the former via a large collection of the latters in any scientific way (that is, without politics and related language).

As a result of this situation, the paper proposes an irreversible particle mechanics via a simple generalization of the current (rather old) views, in the hope of contributing toward the future resolution of this magnificent open problem. By no means the paper claims the achievement of a final solution in favor of irreversibility. It merely claims plausibility. By the same token, and this should be stressed to prevent unnecessary incidents, no physicists can claim today final knowledge with his reversible/Hamiltonian/unitary particle dynamics. We must all face this situation and acknowledge that eacn of our personal views is tentative.

In the hope of minimizing some of the (rather numerous) prejudices in the field, the paper stresses (beginning with the title) that the irreversibility is referred to OPEN systems, while a reversible center-of-mass trajectory is recovered in full when the system is implemented into a closed form including the external terms.

Finally, the model tries to stress the expected dependence of the time–asymmetry from the local physical conditions, such as size of hadrons, energy of collisions, ect. This is so in the hope of indicating to possible readers in other fields (such as quarks and QCD) that the model submitted IS NOT in disagreement with their views [in actuality, the model opens up an array of intriguing possibilities of novel contributions to quarks and QCD I hope to have the opportunity to illustrate in some other paper].

It is evident that the best referees of the paper are experts in the field, that is, **physicists with a record of publications, specifically, in NON–Hamiltonian/NON–Lagrangian particle mechanics.** Different views would be equivalent to the submission of papers, say, on quarks to referees who have never written one paper on quarks, which is a manifestly unwarranted editorial practice. It is a fact that physicists "in good standing" at the American Physical Society who are experts in NON–Hamiltonian/NON–Lagrangian mechanics are today very rare. This is not a decifiency of the APS. Instead, it is an indication of novelty of the paper. Nevertheless, this is a fact that should be faced and acknowledged to avoid referees venturing judgments under the illusion of knowledge. It is evident that a non–expert in the field may reach a mature judgment. However, he/she must be willing to reach at least a superficial knowledge of a rather considerable volume of publications which constitute the mathematical and physical foundations of the paper.

The best experts in the field I can recommend to you are

– Professor R. MIGNANI
Istituto di Fisica
dell'Università
Piazzale Aldo Moro, 2
00185 ROMA Italy

Professor A. J. KALNAY
Instituto Venezolano
De Investigaciones Cientificas (IVIC)
Centro De Fisica
Apdo. 1827
CARACAS 1010 A, Venezuela

– Professor G. EDER
Atomic Institute of the Austrian Universities
Schuettelstrasse 115.
1020 VIENNA Austria

Professor Y. NAMBU
Enrico Fermi Institute
University of Chicago
CHICAGO, Illinois 60637

Additional physicists you may consider who are outstanding, but do not have an extablished record of publication in non–Hamiltonian/NON–Lagrangian mechanics, are

– Professor L. C. BIEDENHARN, Jr.
Duke University
Department of Physics
DURHAM, North Carolina 27701

Professor S. OKUBO
Department of Physics and Astronomy
University of Rochester
ROCHESTER, New York 14260

5. MATHEMATICAL ASPECTS. The true foundations of the paper are those of the so–called "hadronic mechanics" [read: isotopic lifting of the Hilbert space]. The novelty of these studies is such that no theoretician, beginning with myself, can be considered an expert of the new mechanics. In fact, the only experts available at this time are mathematicians. This is a reality you should take into consideration to avoid potential basic misjudgments in the referee process, with the consequential creation of unnecessary incidents.

The leading mathematician in isotopic generalization of Hilbert spaces (and algebras) is

Professor H. C. Myung
Department of Mathematics
University of Northern Iowa
CEDAR FALLS, Iowa 50613

I believe you should consider his advice, of course, on the soundness of the mathematician foundations only.

Additional distinguished mathematicians, experts in the mathematical foundations of the paper, are

Professor M. L. TOMBER
Department of Mathematics
Michigan State University
EAST LANSING, Michigan
48824

Professor R. H. OEHMKE
Department of Mathematics
University of Iowa
IOWA CITY, Iowa 52240

Professor A. A. SAGLE
College of Natural Science
University of Hawaii at Hilo
1400 Kapiolani Street
HILO, Hawaii 96720

In summary, I suggest an in depth review by differentiated experts in all the major lines of the inquiry. The task of combining all reviews in a final judgment is, of course, yours.

6. IMPROVEMENTS. Permit me to express my best possible cooperation and gratitude for any suggestion of improvements by the referees. Often, however, one of the most difficult tasks for an author is to understand the improvements desired by the referee. Permit me, therefore, to encourage the referee to be as specific as possible in the desired corrections, not only in their identification by word, or sentence, or formula of the current paper, but also in the desired modification. Also, it is important to prevent that simple modifications suggested by the referee be interpreted as rejection because of lack of sufficiently clear language in the report.

To minimize these rather frequent confusions which end up to be damaging to the Journal, to the referee, and to the author, I have implemented at the HADRONIC JOURNAL the practice of presenting to the authors referee reports favoring a possible future publication, according to the following guidelines:

[a] we first indicate as clearly as possible that the paper may indeed be suitable for publication in case it is improved according to guidelines specified below.

[b] we then identify each and every word, statement, or formula recommended for revision, and for each of them suggest possible improvements; and

[c] often, we also enclose one copy of the paper with editorial markings on the critical passages, to make sure that the authors understands the points in need of revisions.

I do not know whether your Journal can implement a reporting policy of this type. Nevertheless, I passed it to you as one of the possible ways to avoid misrepresentations of the referee report because of their insufficient clarity on the truly essential issue: whether the referee is for or against publication of the paper following his suggested improvements.

7. INSPECTION OF REPORTS. Referee reports constitute a scientific document and, thus, they must be inspected for scientific content, value, and credibility in exactly the same way as the paper itself. It is now common practice at the HADRONIC JOURNAL to reject and return to the referees (rather than to the authors) all reports that are questionable on grounds of language, contents, objective, etc.

The paper submitted is a representative of a growing scientific current interested in exploring possible basic advances in the structure of our contemporary physical knowledge. Your handling of the paper will be important in influencing whether other papers along the same lines of inquiry will be submitted to your Journals, or other scientific conduits should be considered.

For these reasons it is essential, in my view, that referee reports be inspected for scientific contents and value. Comments and/or criticism without sufficient credibility should be returned to the referees, in my view, and they should not be released by your office. In fact, they can be damaging to your Journal.

But, most importantly, you should take into consideration the ultimate hope of the paper, that of promoting an orderly scientific dialogue in a fundamental open problem. Your most important task is therefore that of preventing the suppression of this scientific process at the referee level, and permitting instead the participation of the physics community at large.

The orderly scientific process of trial and error, via the presentations of plausible views in physics journals and their critical examination by other independent, researchers via papers also in physics journals, is, without doubt, the only way to pursue novel physical knowledge.

I have mailed copy of this letter only to Dr. P. W. Anderson (Princeton University) in his quality of Chairman of the Publication Committee of the American Physical Society. I have abstained from mailing copy of this submission to any other member of the editorial organization of your Journals, and left this task to your discretion.

I would like to take this opportunity to express to you and to your family my sincere and best Season Greetings.

Very truly yours,

Rup M Santilli

Ruggero Maria Santilli

cc: Dr. P. W. ANDERSON

RMS/mlw

Encls.:

—two one—sided originals of manuscripts
—calculation of length
—twenty copies of manuscript, of this letter, and of of detailed calculations

Dr. S. —

As you can see, I do <u>not</u> receive papers for our journals, and so have forwarded your material to Dr. Trigg.

Merry Christmas,

J.Z.

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

December 22, 1982

Dr. George L. Trigg, Editor
Physical Review Letters
1 Research Road
Box 1000
Ridge, NY 11961

Dear George:

Attached is a large number of copies of a paper which was just received from Dr. R. M. Santilli, intended for submission to Physical Review Letters. Dr. Santilli is evidently under the misimpression that I, rather than the actual Editors, receive papers for the journal.

There is also a quite long letter in which Dr. Santilli describes what he considers proper criteria for review of the paper, and suggests names of many possible referees. Naturally, all of this information is merely suggestive for you in your selection of referees, since the selection of referees is and has always been the perogative of the Editors.

Dr. Santilli also suggests in his letter certain procedural changes in use of referee reports which are not consistent with usual PRL policies. Naturally, you are expected to follow our established policies for review and acceptance of papers, and ensure that this paper receives the same fair and equitable handling that we give all papers submitted, no more and no less.

A copy of this letter is being sent to Dr. Santilli, and I presume he will receive the usual acknowledgement from you when the paper is actually received at PRL.

Sincerely,

David Lazarus

xc: Dr. P. W. Anderson
Dr. R. M. Santilli

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES · 1 RESEARCH ROAD
BOX 1000 · RIDGE NEW YORK 11961
Telephone (516) 924-5533

February 11, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. LZ2206

Dear Dr. Santilli:

We have received at least one referee report on your manuscript entitled "Possible time-asymmetric model for open nuclear reactions".
There are no criticisms that require your attention now.
Since a decision cannot be reached on the basis of the material at hand, we are seeking further advice.

Sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

GLT/jaw

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533
Telex Number 971599
Cable Address PHYSREV RIDGENY

March 4, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. LZ2206

Dear Dr. Santilli:

We have received at least one referee report on your manuscript entitled "Possible time-asymmetric model for open nuclear reactions." There are no criticisms that require your attention now. Since a decision cannot be reached on the basis of the material at hand, we are seeking further advice.

As for the receipt date, our instructions clearly state that manuscripts are to be sent to this office. If you choose to disregard the rules, you must accept the consequences. The date of receipt of <u>any</u> manuscript is the date it reaches this office.

Sincerely yours,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

GLT/jaw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

April 2, 1983

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
1. Research Rd
RIDGE, N.Y. 11961

RE: Paper LZ2206 entitled
"Possible time-asymmetric model for open nuclear reactions"
Submitted in December 1982

Dear Dr. Trigg,

I would like to express my appreciation for your two notes of February 11, 1983 and March 4, 1983. Please ignore the issue of the date of submission because completely immaterial [at the time of the submission I was unaware of the fact that the "Editor in Chief" of your Journals is not an Editor].

Jointly I would like to lament the unusually long time that is taking for the consi-detation of the letter. In a few days it will be ONE FULL YEAR since the submission of the preceding letter LR111. The new letter LZ2206 has already been at your office for a period of time longer than the average time of publication (let alone review) of papers aligned with vested academic interests in control, not to mention the period of time occurring for the publication of papers signed by members of the editorial organization of your journal.

The reason why this is, for several members of our group, an astonishing occurrence is due to the incontrovertible character of the underlying scientific truth. The case deals with OPEN (NONCONSERVATIVE OR DISSIPATIVE) NUCLEAR reactions, that is, processes that have been hystorically treated via NONHERMITEAN Hamiltonians and NONUNITARY time evolutions

$$A' = \exp(-itH^{\dagger})A\exp(+itH), \quad H^{\dagger} \neq H \tag{1}$$

whose irreversibility is incontrovertible. Our model merely presents an algebraically consistent treatment of an already established fact of nuclear physics. Indeed, the brackets of the infinitesimal version of law (1)

$$idA/dt = AH^{\dagger} - HA \tag{2}$$

do not characterize a consistent algebra, trivially, because trilinear. Our Lie-admissible reformulation

$$\begin{gathered} A' = \exp(-itET^{\triangleright})\ A\ \exp(+it\,{}^{\triangleleft}TE) \\ H^{\dagger} = ET^{\triangleright}\ ; \quad H = {}^{\triangleleft}TE, \quad (T^{\triangleright})^{\dagger} = {}^{\triangleleft}T, \ E^{\dagger} = E \end{gathered} \tag{3}$$

then permits the achievement of a consistent algebra for the infinitesimal behaviour

$$idA/dt = A\,{}^{\triangleleft}TH - ET^{\triangleright}A \tag{4}$$

as well as a number of advances that do not appear to be readily achievable via a time evolution with inconsistent algebraic structure, such as (1). In fact, the time-asymmetry

$$P^{\triangleright}/{}^{\triangleleft}A = {}^{\triangleleft}T \ / \ T^{\triangleright}, \tag{5}$$

is readily achievable via law (3), but not via law (1). Similar occurrences exist for several other aspects of OPEN reactions of EXTENDED CHARGE DISTRIBUTIONS (neutrons), such as the representation of their deformation under sufficiently intense external fields.

The experimental situation is equally distressing and my voice has been continually ignored without any credible counterargument. In fact, the experiments on time-asymmetry by the Berkeley-Quebec group and those by the Los Alamos rebuffal are all of OPEN character, trivially, because they deal with beams on FIXED EXTERNAL TARGETS. Under these conditions, the amount of the irreversibility is certainly open to debate, but the existence of the irreversibility should be out of the question to avoid shadows of scientific manipulations. In fact, to prove that he/she is in good faith, any experimentalist claiming exact time-reflection symmetry under open reactions must prove that law (1) is reversible, which is by far a quite difficult task, assuming that a credible proof can be established.

The letter LZ2206 merely intends to clarify this latter point. Such large delays in its consideration, whether accidental or premeditated, share the same risk: continued doubts of the existence of scientific manipulations at the journals of the APS in the interests of individual groups in academia, and in disrespect of the interests of the Country for scientific advances.

Very Truly Yours

R. M. Santilli

Ruggero M. Santilli

cc.: Drs. D. Lazarus, D. Nordstrom, and G.J.Deiss, PR

P.S. With the passing of time, we have acquired new knowledge that may be useful to improve a few words of paper LZ2206. For instance, it may be appropriate to clarify that the operation of isotopic Hermiticity recalled at the bottom line of page 2 holds for the conditions stated in ref. 9, i.e., for isotopic enveloping algebras acting on a conventional Hilbert space. If the latter is also subjected to the same isotopic lifting of the envelope, than the operation of Hermiticity is the conventional one. If the two isotopies are different, then the operation of Hermiticity is even more general than that of ref. 9 as reviewed in p. 2.

Kindly advice whether this clarification (and a couple of others) should be mailed to you, or I should waitfor possible improvements of the papers suggested by a (true) referee.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE, NEW YORK 11961
Telephone (516) 924-5533

6 April 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Possible time-asymmetric model for open nuclear reactions
By: Ruggero Maria Santilli

LZ2206

Dear Dr. Santilli:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), it is our judgment that the paper is unacceptable for publication in Physical Review Letters. We are therefore returning the manuscript herewith, together with a copy of the criticism that led to our decision.

Yours sincerely,

George L. Trigg

George L. Trigg
Editor
Physical Review Letters

enc.

R2L

April 9, 1983

Dr. GEORGE L. TRIGG, Editor
Physical Review Letters
P.O.Box 1000, RIDGE, New York 11961

RE: final rejection dated April 6,1983,with three referees' reports on the paper LZ2206 entitled "Possible time-asymmetric model for open nuclear reactions", following rejection of paper LR2111 submitted on April 16, 1982.

Dear Dr. Trigg,

The available records point rather strongly toward apparent scientific misconduits occurred at your journals in the handling of theoretical and experimental papers on the problem of irreversibility of open nuclear reactions.

The claim is too diversified to be effectively expressed here. The bottom lines is constituted by the fact that the irreversibility of theoretical models based on nonunitary time evolutions for the description of open (e.g., dissipative) nuclear reactions is an absolutely incontrovertible scientific truth. None of the numerous referees' reports mailed to me over the full year of consideration of my papers has acknowledged, even indirectly, this incontrovertible fact. None of them exhibited even the most minute,true scientific content, or any suggestion whatsoever that could be valuable for the improvement of my papers, or any acknowledgment of the ultimate essence of the paper, as recently reviewed in my letter to you of April 2, 1983 (a mere Lie-admissible reformulation of nonunitary time evolutions, with consequential, manifest, incontrovertible irreversibility). Your suppression of the publication of this plausible view therefore supports quite strongly the allegation of editorial misconduits.

Additional, perhaps graver editorial misconduits are due to a number of apparent discriminatory practices at your journals regarding research conducted under governmental support. I am referring here to the fact that the calls of extreme editorial rigour implemented for my papers do not appear to be equally implemented for all other papers. This situation can be established beyond any reasonable doubt by conducting independent refereeing of papers already published in your journals, such as, to mention only one case, the extension of quark conjectures to the structure of the deuteron without any treatment of its most fundamental and well known property, the lack of exited states. Additional discriminatory practices can be potentially identified in the selection of referees. In fact, referees for papers on quark conjectures are solely and exclusively selected among experts in the field, that is, researchers with a substantial record of publication of papers in the field, as very well known. On the contrary, the referees selected for my papers had no meaningful knowledge whatsoever of the field of the paper (Lie-isotopies and Lie-admissible genotopies of Hilbert spaces), let alone a record of publication in the field. This additional, apparent,discriminatory practice on governmental research can also be established beyond a reasonable doubt by inspecting the referees' report themselves. You have in your file my comments on the preceding reports. Enclosed you will find my additional comments on the last three reports.

But the gravest editorial misconduits have apparently occurred, not in regard to my theoretical papers, but instead in regard to the experimental papers on irreversibility by the Berkeley-Québec collaboration. In fact, the first paper by this group (PRL 47, 1803 (1981)) was kept for over one-and-one-half years, in the apparent intent to give time to an experimental group at Los Alamos to prepare a rebuffal, and have it quoted in the former paper (as it was). More recently, the publication of new measures of polarizations indicating irreversibility by the Québec group (J. Pouliot et al) as rapid communication in Physical Review C,was suppressed at the refereeing level, despite the availability of additional supporting information, as appeared, e.g., in Nuclear Physics A394, 428 (1983). This latter episode followed editorial lines much similar to those of paper LZ2206, that is, by ignoring the fact that the irreversibility for open reactions (such as those considered by the experimenters) is incontrovertible, and only its amount is open to scientific debate.

It is evident that the appropriate editorial conduit in these experimental papers should have been that hystorically followed for the pursuit of novel human knowledge: publication of plausible novel results and, subsequently, their critical examination by independent researchers in separate papers. The rather voluminous file on irreversibility indicates, quite strongly, that you have decided to suppress possible advances at the editorial level, and assumed the astonishing role of arbiters of possible advances whenever they are manifestly or potentially against existing, vested academic interests [the comment does not apply when the papers are aligned with said interests, as one can see from the rapidity with which PRL publishes the papers authored by its editor R.K.ADAIR and his friends].

A most sustantial evidence supporting the allegation of editorial misconduits is provided by a letter of Professor S. OKUBO dated November 10, 1982 [copy enclosed], in which one can read that he recommended the publication of paper LR2111 in Phys. Rev. (rather than PRL). As verbally expressed to Dr. LAZARUS, Editor in Chief, and as confirmed in writing, such type of publication would have been perfectly acceptable to me. Evidence establishes that Professor OKUBO's recommendation WAS NOT followed by your journals, despite the fact that such alternative publication would have resolved the case. Thus, professor OKUBO's letter, not only supports the allegation of editorial misconduits, but, at the extreme, might also be interpreted as indicating a conceivable conspiracy by vested academic-finan cial-ethnic interests to suppress undesired advances in physical knowledge.

For the sake of clarity, I should indicate that, to my knowledge, the apparent scientific misconduits considered here do not violate existing Codes of Laws [with the potential excetion of aspects regarding possible discriminations on papers under governmental support]. The same alleged scientific misconduits also cannot be qualified as being "scientifically unethical" because, as well known, the American Physical Society does not subscribe to a Code of Ethics, by therefore preventing in this way any ground for ethical judgment.

Nevertheless, the scientific, economic, and military damage caused to America, as well as to the human society at large, by your editorial practices has the potential of being much more damaging than ordinary crime.

On my part, I have provided over one full year period all conceivable efforts for an orderly resolution of the case. Since I cannot compromise with my own ethical standards, I feel obliged to undertake all the necessary steps so that the American public, as well as the international public, is fully informed of the entirety of the case regarding the handling by your journals of the theoretical and experimental papers on irreversibility, as well as of other apparent extremes of misconduits occurred in other sectors of the U.S. physics community, the disclosure being expected to be made at some appropriate future time either by myself, or via my estate in Europe, or via interested U.S. attorneys, physicists, and ordinary taxpayers.

Very Truly Yours

Ruggero Maria Santilli
96 Prescott Street
Cambridge, Massachusetts 02138

cc.: Drs. D. LAZARUS, D. NORDSTROM, and G.J.DREISS, Physical Review
Dr. N.D. PEWITT, Office of Science and Technology, The White House

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 1.ON PAPER LZ2206
by R.M.Santilli

This is a scientifically responsible report written in respectful language, but, regrettably, it cannot be used for judgment because the referee, quite honestly, acknowledges his/her lack of expertise in the field of the paper.

Note that this referee recommends SPECIFICALLY, that PRL selects referees who are true experts in

> "...the extensions of Lie algebra to the Lie-isotopic and Lie-admissible constructions described in the manuscript."

Regrettably, it does not appear that this sound, and quite natural suggestion was followed by PRL, as evident from an inspection of the subsequent reports.

In turn, the problems regarding paper LZ2206 are not given by reports made by unqualified referees, but rather by their selection by PRL as well as by the PRL formal acceptance of their report.

T SHOULD BE STRESSED THAT A LIST OF ALL TRUE EXPERTS IN THE FIELD OF THE PAPER IN NORTH AND SOUTH AMERICA AND IN EUROPE, INCLUDING OUTSTANDING SCIENTISTS, WAS MADE AVAILABLE TO ALL EDITORS OF THE PR AND PRL.

Referee's Report No. 1

Title: Possible time-asymmetric model for open...

Author: Ruggero M. Santilli

Ms.No.: LZ2206

This paper describes an attempt to connect the known time-irreversibility of macroscopic physical processes to an assumed time-irreversibility of fundamental nuclear and particle interactions. Although this assumption is contrary to the established theoretical view, it should not be rejected summarily. The established CP-violation in $K^o$ decays implies T-violation via the CPT theorem, and this unique result still has no satisfactory explanation in terms of a T-asymmetric interaction with its manifestations in other particle physics processes.

In my view, the question is whether or not the theoretical development described in this paper has any real merit at the level of nuclear and particle physics, and I am not qualified to make such a judgement.

I recommend that you seek the advice of a nuclear or particle theorist who has some experience or knowledge of the extensions of Lie algebra to the Lie-isotopic and Lie-admissible constructions described in this manuscript.

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 2 ON PAPER LZ2206
by R.M.Santilli

This referee has no meaningful knowledge on the topic of the paper, as defined in report no. 1, let alone a proved record of expertise.

The occurrence can be established beyond reasonable doubts by the claim that there is no clear relation between Prigogine's (statistical) work and the model presented in the paper, despite the recall that the former originates via a "nonunitary transformation".

In fact, anybody with a minimum of knowledge of Lie-isotopy knows that a nonunitary transformation of the conventional Lie product produces exactly the Lie-isotopic time evolution of the paper, Eq. (1), p. 3 of LZ2206 (see, e.g., ref. 2, p. 225). Thus, the fundamental dynamical equations for the CLOSED-EXTERIOR treatment of the model are exactly the particle-version of Prigogine's statistical time evolution, only written in an algebraically understandable way [the clarification of the point was avoided in the note, not because of lack of space in a letter, but because so repetitive to appear verbose and even offensive to experts in the field].

The lack of any qualification whatsoever by this referee is further proved by his/her disclaim of the lack of relationship between Prigogine's nonunitary time evolutions and the non-Hamiltonian origin of irreversibility suggested in paper LZ2206.

Again, anybody with knowledge of the background work leading to the paper knows that the classical image of a nonunitary transformation of Heisenberg's equations CANNOT be Hamilton's equations (they are instead given by the non-Hamiltonian, Birkhoff's equations). Thus, under the conditions of the paper, the non-Hamiltonian character of the model is absolutely incontrovertible and established in all necessary details in the literature quoted in the paper [as an incidental note, Prigogine and his collaborators went to considerable pain in their papers to clarify the care needed before interpreting "H" as the energy under a nonunitary transformation. This is recalled here to established Prigogine's priority of the discovery].

It is therefore evident that this referee, not only is basically unknowleadgeble of the literature on Lie-isotopy and Lie-admissible genotopy, but he/she is basically deficient in the knowledge of Prigogine's work that lead to his Nobel price!

DESPITE THAT, THIS REFEREE PASSES JUDGMENT AND SUGGESTS THE REJECTION OF THE PAPER. IS THEN THIS DECISION MADE IN GOOD FAITH? OR IS THE DECISION THE RESULT OF A CALCULATED MANIPULATION OF SCIENTIFIC TRUTHS AIMED AT NONSCIENTIFIC OBJECTIVES?

SINCE THE MERE SHADOW OF A DOUBT ON ETHICAL ISSUES IN REFEREEING IS SUFFICIENT TO DISQUALIFY A REFEREE, THE AMERICAN PHYSICAL SOCIETY IS HEREBY URGED TO REMOUVE THIS REFEREE FROM THE ACTIVE FILE, AND, MOST IMPORTANTLY, TO ABSTEIN FROM THE SUBMISSION OF PAPERS IN HIS/HER OWN FIELD, LET ALONE OTHER FIELDS.

BUT THE MOST DISTRESSING ISSUE IS NOT THE INCOMPETENCE OF THE REFEREE, BUT INSTEAD THE FACT THAT PHYSICAL REVIEW LETTERS HAS FORMALLY ACCEPTED THE REPORT IN A REFEREEING PROCESS REGARDING RESEARCH CONDUCTED UNDER GOVERNMENTAL SUPPORT. IT IS THIS LATTER ASPECT THAT RAISES A HOST OF SCIENTIFIC, ETHICAL, AND LEGAL PROBLEMS.

Referee's report No. 2

We have read the paper by R.M. Santilli entitled: "A possible Time- Asymetric Model for Open Nuclear Reaction."

Unfortunately, this paper appears to be so obscure that we are unable to judge what is exactly claimed and even less, what is proven.

Certainly, there is no clear relation with the work of Prigogine et al. on the origin of irreversibility in statistical physics, certainly to what seems to be implied by this paper. That work starts with hamiltonian and shows that when well-defined assumptions are made on the nature of the system, the time-symmetry may be broken by a nonunitary transformation. This seems to have little to do with what the author calls the non-hamiltonian origin of irreversibility.

We cannot recommend this work for publication.

COMMENTS ON THE ENCLOSED REFEREE REPORT NO. 3 ON PAPER LZ2206
by R.M.Santilli

This referee too has no significant knowledge of the field of the paper. But, unlike the author of report No. 2, this referee enters into considerably more elaborations totally deprived of any meaning for the topic of the paper, not to mention gross misrepresentations (such as the quote of "non-Birkhoffian mechanics" ?!?!).

The referee recalls from Jacobson that an isotopic lifting ATB of an associative algebra AB is no generalization of the associative algebra itself. This is so trivial that the quote of Jacobson is verbose (my son in junior high school can see it). Paper LZ2206 does not deal with asbtract mathematical structures. It deals instead with the physical implications of different realizations such as ATB and AB. Specifically, it shows that the former permits the recovering of the exact time-reflection symmetry for the center of mass trajectory of extended systems with non-Hamiltonian internal forces, while the irreversibility occurs only for open interior processes.

A most incongrous claim by this referee is that the paper would be "almost completely inaccessible to the general readership of PRL". The pertinent question is then the following: is PRL publishing papers that are accessible to ALL physicists, or PRL publishes papers that are accessible only to experts in the field, or to readers that can become experts upon (and ONPY UPON) studying the literature quoted in the paper? The evidence in support of the latter alternative is to overwhelming to prevent the need of additional comments.

A further, hardly believable posture by this referee is that, since the experimental situation on irreversibility is unsettled, paper LZ2206 should not be published. But NOW (AND NOT YEARS FROM NOW) there is the need for theoretical elaborations, because this is the <u>ONLY</u> way for experimental studies to reach true maturity. The referee's posture under consideration is therefore strictly antiscientific, in my view.

But the most insidious (and for me offensive) suggestion is the last, to the effect that I should prepare a longer paper for submission (apparently) to Physical Review. Apart the fact that a longer paper would imply repetions over repetitions of results published and republished, the claim is rendered insidious because of the years of times that it has taken in the past for my publishing . papers in Physical Reviews, whenever the topic (or even one sentence contained in it) was not fully aligned with existing interests or general views. Thus, the suggestion to write a longer paper is literally equivalent to the suppression of the publication of the model for all the necessary additional time (months) to write the new paper , as well as the continuation of this senseless expression of refereeing deprived of scientific sense, which could likely take a number of years.

AGAIN, THIS REFEREE PASSES JUDGMENT DESPITE ITS MANIFEST AND EXPLICITLY ADMITTED LACK OF EXPERTISE IN THE FIELD OF THE PAPER. IS THIS ACCIDENTAL OR CONSPIRATORIAL?

AGAIN, THE AMERICAN PHYSICAL SOCIETY IS URGED TO REMOUVE THIS REFEREE FROM ITS ACTIVE FILE, AND ABSTEIN FROM SUBMITTING PAPERS TO HIM/HER PARTICULARLY IN HIS/HER FIELD.

ARE THE EDITORS OF PRL USING THEIR NEXT DOOR NEIGHBOORS FOR MEDICAL ASSISTANCE, OR THEY USE TRUE, QUALIFIED PHYSICIANS? BUT THEN, WHY THEY HAVE INSISTED FOR ONE YEAR IN NOT USING QUALIFIED EXPERTS IN THE REVIEW OF PAPERS LR2111 AND LZ2206? WHY? HOW CAN THIS BE ONLY ACCIDENTAL?

REPORT OF REVIEWER **No. 3**

Title: Possible time-asymmetric model for open...

Author: Ruggero M. Santilli

This describes a novel attempt to understand time irreversibility in hadronic processes. This report addresses (a) the validity, (b) importance, (c) the interest of the paper for the readership of PRL.

As to the validity of the paper one cannot be categoric. The difficulty here is that the confines of the Letters' format means that the discussion is necessarily brief, and inevitably somewhat cryptic. This reviewer does not pretend to any special competence in the so-called non Birkhoffian mechanics. However, a possible difficulty arises: according to the algebraist Jacobson, an isotopic generalization of an <u>associative</u> algebra is in fact no generalization at all. In any event this reviewer feels that the algebraic generalization is interesting, but the relevance to time reversibility has not been adequately established--and indeed may not be able to be established within the confines of the Letters journal format.

As to the importance of the results: there is no question that the problem is one of great importance. However, the content of the paper appears to be, at this stage, largely speculative and not definitive.

In the opinion of this reviewer, the paper is almost completely inaccessible to the general readership of the PRL, and would lack interest for them. Part of this is the intrinsic difficulty of the subject and the general format, but part of it is also the fact that the paper is philosophic in tone, descriptive, and even at times verbose.

Since the experimental situation on the validity of the polarization assymetry theorem, ~~it~~ is at this stage controversial, with conflicting experimental evidence, and since the theory proposed here is at best tentative, and not definitive (the author takes pains to point out that there are an unlimited number of possibilities within the framework of his model (none of which is currently singled out)) it seems best to conclude that the publication of this paper is not advisable at this time.

<u>Recommendation</u>: Publication in the PRL is not recommended. It is suggested that the author prepare a longer, much more carefully crafted and explicit paper for a letters* journal.

* Apparently a slip, given the context. — Ed.

# PART XIII–E:

# CORRESPONDENCE

# WITH

# D. LAZARUS,

# EDITOR IN CHIEF,

# AMERICAN

# PHYSICAL

# SOCIETY

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 25, 1982

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 11961

Dear Dr. Lazarus,

I am hereby asking your personal intervention in regard to my paper

"Use of the hadronic mechanics for the best fit of ......"

submitted to Physical Review Letters on April 19, 1982, ref. no. LR2111. A self-explanatory letter to the Editors is enclosed. In particular, I am asking your intervention to assist the Editors in the implementation of the request for two additional referee reports according to specifications (a), (b), and (c).

I would like to bring to your particular attention, the request that no referee from Harvard University, the Massachusetts Institute of Technology, and other local institutions are selected. The reasons are that there exists a rather considerable documentation regarding the opposition by academicians of these local institutions to the experimental, theoretical, and mathematical studies underlying the research presented in the paper. To give you an idea, I enclose copy of the formal prohibition by Harvard (enclosures # 1 and 2) to hold our Third Workshop on Lie—admissible Formulations there. Unfortunately for all of us, the meeting housed a considerable number of truly distinguished scientists (see the Table of Contents of the Proceedings, enclosure # 3). We, therefore, barely managed to avoid a public incident. I also enclose copy of the front page of an application for a federal grant regarding an experimental collaboration Austria—France—USA (enclosure # 4). As you can see, the application was signed in more than one Country, but it was NOT signed by MIT. I abstain from disclosing here the details as well as a number of related episodes. However, permit me to indicate that, again, we barely managed to avoid the appearance of the episode in the Foreign Press with a rather cold assessment of academic politics in the USA. Lately, we have seen the prohibition to list, in the Boston Area Physics Calendar, a seminar for physicists by a truly distinguished mathematician (enclosures # 5 and 6). The topic was the construction of the Lie—admissible groups via the use of changes of tolopogical coordinates. For your information, the Lie—admissible generalization of Lie algebras and groups is at the foundation of the time evolution law suggested as possible in the paper [see equation (2)]. The prohibition implies a rather serious discrimination of research conducted under the federal support. I have, therefore, been advised not to provide you with additional disclosures at this time. Nevertheless, in case you are interested, I can ask the law firm in charge of the case to collaborate with you.

On more general grounds, the scenario of the situation in strong interactions is by far non—reassuring. It is an easy prediction that, unless our community of basic research manages somehow to contain the excesses of academic greed by physicists in position of power, a major crisis of unpredictable proportions will be unavoidable. It is a fact that the current scene is dominated by physicists committed to quark theories, their physical laws, and the underlying river of public funds. It is also a fact that these vested and organized scientific interests have provided systematic efforts to suffocate all possible searches for genuinely novel advances or alternatives. I am referring, here, to jeopardizing actions at the level of jobs, refereeing for grant applications, and submission of papers. To give you an idea, I enclose copy of a referee report (enclosure # 7) for a federal research proposal I submitted for my monographs "Foundations of Theoretical Mechanics," I, II, III. As you can see, the referee report consists of vulgarly offensive language combined with a total lack of scientific content. The point is that my manuscripts were accepted in the meantime for publication in one of the most selective series of research monographs in physics, that by Springer—Verlag. Understandably, vigorous complaints reached the highest possible levels in Washington, and I eventually provided my best efforts to avoid a scandal in the interest of our community.

But, bear in mind, these episodes are and remain "time bombs".

The situation at the Journals of the American Physical Society could predictably be a reflection of the scenario above. In fact, a segment of our community, as well as outside observers, are attempting to convey a growing concern on the conceivable manifestation of the problem at the editorial level. The scenario here is, essentially, an apparent, rather easy acceptance of papers on quarks, QCD, and related fields, joint with rather substantial difficulties experienced by all other papers of nonaligned character. I am myself the Editor in Chief of a Journal in Physics. Thus, I do favor the publication of all valuable papers in hadron physics, whether or not of quark alignment. Nevertheless, a few points should be made clear. On strict scientific grounds, quarks are at this moment a figment of academic imagination, without any experimental evidence comparable to that for the constituents of nuclei and atoms. In fact, all available evidence is in favor of the unitary classification of hadrons of Mendeleev type, but not necessarily of the desired, joint, structure model. Most importantly, you should keep in mind the growing concern for the lack of a rigorously established confinement of quarks. As you certainly know, we do not possess at this time explicit calculations proving that the probability of tunnel effects of quarks are identically (AND NOT APPROXIMATELY) null, while all so—called models of confinement are mainly qualitative. As a distinguished mathematician put it verbally to me,

> "The publication of a paper on quarks without a strict confinent by a journal in physics is equivalent to the publication of a paper on number theory by a journal in mathematics stating that 2 + 2 = 387.245693"

[I have denounced this situation to your Editors a number of times, in writing, apparently without any result whatever or containement of this historically paradoxical editorial case].

What is also distressing is the language in which these papers are generally written. In fact, the language is conveying the idea that quarks are truly real and established. Equally distressing is the feverish remanipulation of models to bring masses, parameters, etc., beyond the existing experimental capabilities. These, and numerous other episodes I prefer not to indicate here, are real reasons of concern for a fast growing segment of our community.

It is imperative that The Physical Review and The Physical Review Letters provide all necessary evidence and reassurance of being independent from conceivable lobbying by physicists of doubtful ethical motivation. The rules for achieving this are quite simple. Permit me the liberty of indicating them here.

SUGGESTED RULE ONE: Theoretical and experimental papers on quark conjectures, QCD, and related topics are plagued by increasing problematic aspects. It is essential that these papers experience exactly the same difficulties in publication as all other papers of nonaligned character.

EXAMPLE: As you can see from the official records of your Journals, the paper by Slobodrian, et al. [ref. 1 of the submitted paper] was submitted on August, 1980, and was published in December, 1981. Jointly, the Los–Alamos rebuffal [see the Note Added in Proof of my paper] was submitted in October, 1981 and was published in February, 1982. It is public knowledge that the former experiment is not aligned with current academic interests, while the latter experiment is. Also, and perhaps more significantly, it is public knowledge that the former experiment is substantially more general, accurate, and diversified than the second. In fact, the former is the result of a considerable and lengthly collaboration of experimentalists in the USA, Canada and West Germany that resulted in numerous measures for two reactions and their inverses. The latter experiment, instead, rushed four measurements only, on one reaction, while relying on the measurement by Slobodrian, et al, for the inverse reaction. Owing to these and other circumstances not disclosed here, I believe that the difference in the processing of these two papers was excessively imprudent. In fact, if the publication of the former experiment required sixteen months, the publication of the latter should have required a similar amount of time. At any rate, you should keep in mind that, in case of crisis, episodes such as this one might be investigated by appropriate senate committees. Your Editors could, therefore, be faced with requests of disclosing the referees' names and all refereeing proceedings to the investigating committee. By keeping this possibility in mind, it is imperative that similar differences be avoided at all costs in the future.

SUGGESTED RULE TWO: Referees should be experts in the field of the paper. This elemental rule does not appear to be necessarily applied in practice. In fact, papers on hadron physics are customarily referred to renowned experts in quark conjectures. The point is that these physicists usually have no knowledge whatsoever of research outside their beliefs. In short, being an established expert in quarks conjectures and related fields IS NOT necessarily a qualification for referring papers in hadron physics.

EXAMPLE: Please inspect the referee report of the paper submitted. You will immediately recognize the referee's total lack of knowledge in the experimental, theoretical, and mathematical studies underlying the efforts to construct the "hadronic mechanics". I am referring to printed research pages now approaching the 10,000 mark, including over 10 volumes of proceedings of conferences, several research monographs, besides a large number of ordinary papers. I believe that, again, the selection of this referee has been excessively imprudent.

SUGGESTED RULE THREE: Referee reports should be examined for acceptance or rejection by using exactly the same criteria as those used for papers. More specifically, referee reports should be rejected when

(1) they contain offensive language

(2) they have manifest, ethically questionable, motivations; and, equally importantly,

(3) recommend acceptance or rejection without a clear technical content.

EXAMPLE: You can see the use of offensive language in the enclosed referee report for a research grant application. It should never have been accepted by the Federal Agency.

In closing, permit me the liberty of indicating, most respectfully but candidly, that I have contacted you for something substantially more important than the submission of a brief paper. In fact, what is ultimately at stake is the genuine lack of discrimination in governmental or private research during the editorial process at your Journals, as well as the genuine implementation of scientific freedom. In addition, there are clear national interests calling for the promotion, support, and pursuit of NOVEL physical knowledge.

Please intervene to prevent that excesses of academic greed create a dark permanent cloud in the beautiful history of the American Physical Society.

In the past, I have given more than sufficient proof of my committment to the orderly resolution of differences, and you can rest assured that the same committment shall persist in the future, of course, within limits set by ethics and human dignity.

If I can be of any assistance with more specific details, or in balancing excessively optimistic statements of quark–committed physicists, or in any other form, please do not hesitate to call me. You can count on my best and most loyal collaboration.

Very Truly Yours

Ruggero Maria Santilli
President of the IBR
and Editor in Chief, Hadronic Journal

RMS-mlw

enclosures:
1– Internal letter at Harvard University from Santilli to Hironaka dated April 25, 1980
2– Answer by Hironaka to Santilli dated May 2, 1980
3– Table of Contents on the Third Workshop on Lie–admissible Formulations under DOE support whose scheduled occurrence at Harvard had been prohibited
4– Front page of a research grant application under IBR administration for a joint AUSTRIA–FRANCE–USA collaboration that was not signed by the MIT representative
5– Letter by Santilli to the editor of the Boston Area Physics Calendar recommending the listing of a seminar by Professor A.A.SAGLE of the Department of Mathematics of the University of Hawaii at Hilo —May 19, 1982 [the listing was rejected]
6– Letter by Santilli to the chairman of the department of physics running the calendar, Dr. Schneps of Tufts University of April 27, 1982 asking for the listing of a seminar reviewing some recent problematic aspects of quark conjectures [this seminar too was not listed]
7– Copy of a referee report accepted by NSF on Santilli's grant application;
8– Copy of paper LR2111 submitted to Physical Review Letters
9– Copy of a paper outlining some of the problematic aspects of quark conjectures (Found. of Phys. vol. 11, p.383 (1981)) whose preprint had been distributed in 15,000 copies [this paper has never been quoted in the aligned quark literature to my knowledge]
10– Copy of the letter by Santilli to Trigg of May 25 suggesting implementation of due scientific process for paper LR2111
11– Copy of PRL referee report on paper LR2111
12– List of experts in the field of the paper for possible sole use of Dr.Lazarus as verification of PRL referees via independent consultations.

HARVARD UNIVERSITY

DEPARTMENT OF MATHEMATICS

(1)

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 25, 1980

Professor H. HIRONAKA
Chairman
Department of Mathematics

UNIVERSITY MAIL

Dear Professor Hironaka,

I acknowledge receipt of your recent note confirming the termination of my appointment on June 1, 1980, and indicating the possibility of my continuing to use the current office for a limited additional period of time (and definitely not beyond August 15, 1980).

For your information, and as a rather important part of my current research under DOE support, the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS was tentatively scheduled in Cambridge (from August 4 to 9, 1980) several months ago.

The organization of this workshop is now close to completion. A list of participants is enclosed. In addition, we contemplate to have a number of distinguished guests (such as editors of physics Journals).

I assume you have no objection for having this scientific event at Harvard, and I am continuing the organization under this assumption.

Very Truly Yours

R. M. Santilli

Ruggero Maria Santilli

RMS/ml
ecls.

c.c. Ass. Dean Leahy.

HARVARD UNIVERSITY

DEPARTMENT OF MATHEMATICS

2

Area Code 617
495-2170

Science Center
One Oxford Street
Cambridge, Massachusetts 02138

May 2, 1980

Professor Ruggero Santilli
Department of Mathematics
Harvard University

Dear Dr. Santilli:

According to my letter of February 12, 1980 which you clearly received and acknowledged in your letter of April 25, 1980, your status at Harvard is to be totally ceased on May 31, 1980.

Therefore you have no right whatsoever to call for a meeting or conference, academic or otherwise, to be held on the premises of Harvard University after the date of the termination of your appointment, unless you were to obtain special permission from the appropriate administrative board of Harvard University. In any event, you have no authorization and no recommendation from our Mathematics Department for the Hadron Workshop to be held at the Science Center during the summer after May 31.

Sincerely yours,

Heisuke Hironaka

Heisuke Hironaka
Chairman

HH/mjm

cc: Dean Richard G. Leahy

Enclosures

3

**PROCEEDINGS OF THE THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS**

**Held at the New Harbor Campus of the University of Massachusetts in Boston from August 4 - 9, 1980**

**PART A : Mathematics**, published in the
Hadronic Journal Volume **4**, Number 2, February 1981

**PART B : Theoretical Physics**, published in the
Hadronic Journal Volume 4, Number 3, April 1981

**PART C : Experimental Physics and Bibliography**, published in the
Hadronic Journal Volume **4**, Number 4, June 1981

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY
under contract number DE-AC02-80ER10651

# HADRONIC JOURNAL

(3)

Volume 4, Number 2, 1981
PROCEEDINGS OF THE THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS
Held at New Harbor Campus of the University of Massachusetts in Boston
from August 4 - 9, 1980
VOLUME A: Mathematics

Contents



Continued over................

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* Corresponding participants

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number DE-AC02-80ER10651

# HADRONIC JOURNAL

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Volume 4, Number 3, 1981

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS
Held at New Harbor Campus of the University of Massachusetts in Boston
from August 4 - 9, 1980

VOLUME B: Theoretical Physics

Contents



Continued over........................

③



* Corresponding participants

The Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number

DE-AC02-80ER10651

# HADRONIC JOURNAL

(3)

Volume 4, Number 4, 1981

PROCEEDINGS OF THE THIRD WORKSHOP ON LIE–ADMISSIBLE FORMULATIONS
Held at the New Harbor Campus of the University of Massachusetts in Boston
from August 4–9, 1980.

VOLUME C: Experimental Physics and Bibliography

Contents

3



* Corresponding participants

he Workshop was supported in part by the U.S. DEPARTMENT OF ENERGY under contract number

E-ACO2-80ER10651

Research Grant Application

Submitted to the

U.S. DEPARTMENT OF ENERGY

by

The Board of Governors of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

Cambridge, Massachusetts 02138

tel. (617) 864-9859

(4)

entitled

**EXPERIMENTAL VERIFICATION OF THE SU(2)–SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA–FRANCE–USA COLLABORATION**

| Proposed Starting Date: | Proposed Duration: | Amount Requested: |
|---|---|---|
| January 1, 1982 | 12 Months | $ 46,500 |

ENDORSEMENTS


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THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 19, 1982

5

Ms. CELIA MEES
Editor
Boston Area Physics Calendar
Tufts University
Physics Department
MEDFORD, Massachusetts 02155

Dear Ms. Mees,

Please list in the Calendar the following seminar

FRIDAY, APRIL 30

The Institute for Basic Research

2.30 - Harvard Grounds, 96 Prescott Street
(next to Fogg and GSD, entrance at the left court)
Algebraic identities, vector fields, and coordinate changes

Prof. [redacted], Univ. of [redacted] Dept. of Mathematics, and IBR, Division of Mathematics.

Thank you.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw

THE LISTING OF THIS SEMINAR WAS REJECTED

RMS.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 27, 1982

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
MEDFORD, Massachusetts 02155

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

6

Dear Dr. Schneps,

I am hereby asking that you list the following seminar in the Boston Area Physics Calendar for the week of May 16–21, 1982

WEDNESDAY, MAY 19

The Institute for Basic Research

2:30 p.m. – Enter at the left court of the Prescott House on Harvard Grounds at 96 Prescott Street, Cambridge (tel. 864 9859)
Experimental and theoretical reasons why I do not believe in quarks
Ruggero Maria Santilli, IBR, Division of Physics

Please note the following:

(1) This letter will reach you with plenty of time prior to the deadline for listings in the Calendar (1:00 p.m. Monday, May 10, 1982).

(2) In case the indication of the logistics of the Prescott House in the grounds of Mr. Harvard, to facilitate colleagues, is unwelcome, simply remove the words "Harvard Grounds".

(3) Following my conversation with Ms. CELIA MEES of April 19, 1982, and subsequent phone conversation with you on the same day, it is our understanding that you have accepted a formal request by the Chairman of the Lyman Laboratory of Physics at Harvard, Dr. KARL STRAUCH, as well as additional faculty there (apparently Drs. S. GLASHOW and S. COLEMAN, as well as others) not to list seminars organized by our Institute, irrespective of (a) the scientific status of the speakers; (b) its specific physical nature and (c) our conciliatory attitude toward the wording of the listings. You are therefore sharing with the indicated persons and institutions the responsibility of the act.

I urge you to withdraw from this apparent, scientifically insane behaviour, and list our seminars in exactly the same way as seminars are listed at your Department, Harvard, MIT and other local institutions, in the genuine spirit of the free pursuit of knowledge, as well as of this Land. I hope you understand the gravity of the gesture, and the reactions that, regrettably our Institute, as well as its numerous members scattered throughout the world, may be forced to implement.

Very truly yours,

Ruggero Maria Santilli
President
RMS/mlw

cc: Law Firm of the IBR
Board of Governors, IBR
All members of the Divisions of Physics and Mathematics, IBR
Ms. Celia Mees, Tufts Univ.

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FORMAL REFEREE REPORT ON SANTILLI'S MONOGRAPHS "FOUNDATIONS OF THEORETICAL MECHANICS", VOLS I AND II, SPRINGER-VERLAG, IN PRESS, ACCEPTED AND RELEASED BY NSF OFFICERS.

I have examined the proposal by Dr. Ruggero M. Santilli PHY7703963 (returned under separate cover). My reaction to it is rather negative. I also thought that Santilli was on the borderline between being a third rate scientist and a crack pot and I do not think that the monumental work can change substantially my opinion. The idea of reading it thoroughly produces in me an incoercible revulsion and if you insist on it I am going to resign as a reviewer. The book is written in a pompous, immodest, self-glorifying style which I detest given also the absolute lack of physical content. In view of this criticism I find the total figure asked for the project quite extraordinary.

OVERALL RATING
- ☐ EXCELLENT
- ☐ VERY GOOD
- ☐ GOOD
- ☐ FAIR
- ☒ POOR

NSF Form 173, Jan

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*PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH*
*NUMBER DE–TP–82–9*

# USE OF THE HADRONIC MECHANICS FOR THE BEST FIT OF THE TIME–ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL

**Ruggero Maria Santilli** *

The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, U.S.A.

IBR reception date: April 14, 1982

**Abstract**

Strong nuclear interactions are assumed to have a non–Hamiltonian component due to contact among extended nucleons, which is represented via the hadronic generalization of the atomic mechanics currently under study by a number of authors. The theory is used for the description of the recent experimental discovery by Slobodrian, Conzett, et al. that the strong nuclear interactions violate the time–reversal symmetry. The fit of the experimental data provided by the hadronic mechanics is remarkable, and does not appear to be realizable via the use of the atomic mechanics.

* *Supported by the U.S. Department of Energy under Contract Number DE–AC02-80ER10651.A001*

Reprinted from FOUNDATIONS OF PHYSICS

Vol. 11, Nos. 5/6, June 1981
Printed in Belgium

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# An Intriguing Legacy of Einstein, Fermi, Jordan, and Others: The Possible Invalidation of Quark Conjectures[1]

**Ruggero Maria Santilli**[2]

*Received September 6, 1979*

*The objective of this paper is to present an outline of a number of criticisms of the quark models of hadron structure which have been present in the community of basic research for some time. The hope is that quark supporters will consider these criticisms and present possible counterarguments for a scientifically effective resolution of the issues. In particular, it is submitted that the problem of whether quarks exist as physical particles necessarily calls for the prior theoretical and experimental resolution of the question of the validity or invalidity, for hadronic structure, of the relativity and quantum mechanical laws established for atomic structure. The current theoretical studies leading to the conclusion that they are invalid are considered, together with the experimental situation. We also recall the doubts by Einstein, Fermi, Jordan, and others on the final character of contemporary physical knowledge. Most of all, this paper is an appeal to young minds of all ages. The possible invalidity for the strong interactions of the physical laws of the electromagnetic interactions, rather than constituting a scientific drawback, represents instead an invaluable impetus toward the search for covering laws specifically conceived for hadronic structure and strong interactions in general, a program which has already been initiated by a number of researchers. In turn, this situation appears to have all the ingredients for a new scientific renaissance, perhaps comparable to that of the early part of this century.*

## 1. THE QUARK MODELS

Truly outstanding achievements have occurred in the study of the strongly interacting particles (hadrons) during the last decades. Beginning with the pioneering proposal by Gell-Mann[1] and Zweigh[2] of using the special

[1] Supported by the U.S. Department of Energy under contract numbers ER-78-S-02-4742.A000 and AS02-78ER04742.

[2] Department of Mathematics, Harvard University, Cambridge, Massachusetts.



0015-9018/81/0600-0383$03.00/0 © 1981 Plenum Publishing Corporation

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April 19, 1979
Revised May 15, 1979

Preliminary draft

Any critical comment by interested colleagues for the finalization of this paper would be gratefully appreciated

# AN INTRIGUING LEGACY BY ALBERT EINSTEIN:

# THE EXPECTED INVALIDATION OF QUARK CONJECTURES

Ruggero Maria Santilli*

Science Center
Harvard University
Cambridge, Massachusetts 02138

* Supported by the U.S. DEPARTMENT OF ENERGY under contract number ER-78-S-02-4742.A000

This preprint has been printed and distributed to the scientific community by the Hadronic Press in 15,000 samples.

To be submitted for publication.- [redacted]

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 6, 1982

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 11061

Dear Dr. Lazarus,

As a gesture of courtesy, I would like to inform you about recent developments concerning discovery of the violation of the time—reflection symmetry in

R. J. Slobodrian, H. E. Conzett, et al, Phys. Rev. Letters, **47**, 1804 (1981).

This information may also have some possible follow—up value in regard to my letter of May 25, 1982, to you.

1. You are aware about the following repetition of the experiment by R. A. Hardekopf, et al, in Phys. Rev., C25, 1090, (1982). Slobodrian and Conzett have found serious reasons to doubt the validity of the four measures conducted at Los Alamos. Copy of letters from Slobodrian to Veeser at Los Alamos are enclosed on a *confidential basis*. Experimentalists contacted by us have indicated that the apparent inconsistencies of the Los Alamos measures are truly sound.

2. The Québec—Berkeley experimental group has repeated again their measures and found values very close to the original ones. It appears that a communication by the experimentalists on these additional measures will be made publicly available in the near future.

3. Even assuming that they are correct, the four measures conducted at Los Alamos are not sufficient to establish the exact time—reflection symmetry. This point is treated in my paper submitted to Physical Review Letters on April 19, 1982, Ref. No. LR2111. Copy of an illustrated diagram is enclosed.

In addition to the direct information, you should also keep in mind the considerable amount of indirect information supporting the violation of the time—reflection symmetry under strong interactions.

I am referring here, for instance, to:

a. The available measure by Rauch's experimental team on the apparent deformation of the charge distribution of neutrons in the field of nuclei. As you know, the underlying rotational—asymmetry, if confirmed, will imply a necessary violation of the time symmetry. Copy of a paper by Rauch is enclosed.

b. An increasing number of theoretical studies indicate the existence of new, rather substantial, problematic aspects in the relationship between the experimentally established macroscopic irreversibility and the conjectural particle reversibility. These problems were studied at our recent International Conference at Orleans [see, for instance, a paper by Tellez—Arenas]. It is clear that the best resolution of this historical problem is that along the experiment by Slobodrian, Conzett, et al.

c. An additional array of problematic aspects is currently surfacing for a joint time—reversal symmetry combined with the established, broken space—reversal symmetry. I am referring to inconsistencies in the structure of the Special Theory of Relativity. After all, Einstein taught us the equivalence of space and time, and Dirac has stressed, since 1949, his expectation of a joint space—asymmetry and time—asymmetry.

Finally, I believe you should be informed that the NOBEL COMMITTEE in Stockholm, has apparently initiated the monitoring of the scientific events that are expected to unfold in the near future in regard to the time—asymmetry. This is the result of a world—wide wave of independent recommendations to the NOBEL COMMITTEE for the appointment of Professors Slobodrian and Conzett as NOBEL CANDIDATES. I enclose copies of letters of recommendations that have reached Stockholm in the past few months.

I hope that this information is of value to you and to your editors.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Editor of Physical Review A,B,C, and D
and Physical Review Letters

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GENIE
CITE UNIVERSITAIRE
QUÉBEC P.Q. CANADA
G1K 7P4

16 February 1982

Professor Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachussetts 02138
USA

Dear Professor Santilli,

Thank you for your letter of February 8. 1982. Please find enclosed a copy of the letter I have sent to Dr. Robert Hardekopf concerning the Los Alamos experiment. It is my belief that they did not have sufficient energy resolution to separate the transition to the ground state in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction.

I am enclosing a list of references which may prove useful and pertinent to the general problem of time asymmetry. However, I would personally be inclined to look closely at spin-dependent effects, i.e., for example polarizations and analyzing powers: The crucial formulae for the observation of a spin 1/2 particle are

$$A_j = \frac{tr(T\sigma_j T^{\dagger})}{tr(TT^{\dagger})}$$

and

$$P_j = \frac{tr(\sigma_j T^i T^{i\dagger})}{tr(T^i T^{i\dagger})} \quad .$$

It is required that $T^i = T^{\dagger}$ for the validity of the polarization analyzing power equality. However, the theorem may breakdown for other reasons. For example, behind the formalism there is the assumption of operator linearity. Hermiticity of operators corresponding to observables is also implied. The SU(2) exact symmetry is also basic to enunciate the formal expressions for polarizations and analyzing powers. Hence a breakdown of this symmetry may entail an essential

.../2

breakdown of the theorem. As you have shown if such were the case one would face also an essential time asymmetry in hadronic processes.

There are other delicate points which I do not feel qualified enough to discuss in depth: the interference of the long range electromagnetic field with the hadronic field and the general implications of Lorentz invariance, space time structure, etc., for nuclear reactions. Causality violations in quantum systems may also introduce irreversibility effects. I enclose a copy of some pages from the book by Davies, in case you have not seen it yet, dealing with time asymmetry.

In closing, I would like to stress once more the point made at the Orléans conference: The sensitivity of spin-dependent effects to time asymmetry is high, hence the observed P-A difference may stem from rather modest causes.

With best regards.

Cordially,

RJS:dcv

R.J. Slobodrian

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC, P.Q. CANADA
G1K 7P4

8 February 1982

Dr. Robert A. Hardekopf
Los Alamos National Laboratory
Mail Stop 480
Los Alamos, New Mexico 87545
USA

Dear Bob:

As I wrote to you in December, we are now running once more on $^{9}Be(^{3}He,\vec{p})^{11}B$, with our new Si-polarimeter system. Our work has been somewhat slowed down by the breakdown of the van de Graaff belt and other (minor) problems. Nevertheless, our values with the new system thus far agree with all of our Si-polarimeter results, hence they continue to disagree with yours.

I have then studied your preprint and your NIM 114 (1974) 17 paper in detail. The latter shows calibrations with a 100μ and 300μ passing detector. However in your recent work on (t,p) and ($^{3}$He,p) you used a 500μ passing detector. Is it right to construe from your fig.2 of NIM that the analyzing power drops dramatically at about $E_p$ = 11 MeV for 500μ?

You have tested target thickness effects with the 17 MeV triton beam, changing the $^{12}C$ target from 1.9 to 4.9 mg cm$^{-2}$, that gives ΔE = 100 keV and ΔE = 300 keV respectively. However, the energy spread of 14.3 MeV $^{3}$He on a 4.7 mg cm$^{-2}$ $^{9}$Be target is ΔE = 1400 keV, about 4.5 times greater. Also r.m.s. multiple scattering effects are considerably higher. I am doubtful that this test could have given adequate information.

Referring now to your figure 5a) the arrows include a peak in your passing Si detector. From the text it is implied that it corresponds to the ground state peak of $^{11}B$. However, I have calculated the ratio of ΔE's from the ground state and first excited states in a 500μ Si detector following the Ta and steel degraders.

.../2

I obtain $(\Delta E)_1/(\Delta E)_o = 1.14$. The ratio for the centroids of your two peaks is 1.30, i.e., percentage wise there is 14% against 30%, a factor of two discrepancy.

Taking into account your 1.4 MeV spread due to the thick $^9$Be target, about 0.2 MeV from finite kinematics, some 0.3 MeV from multiple scattering and 0.3 MeV from energy straggling, it seems impossible to separate cleanly, in a 500μ detector, the ΔE pulses from states 2.1 MeV apart, with 22 MeV incident energy. In fact I would say that it is impossible, we have our on-line accumulation of ΔE vs $E_{Total}$, with 2.7 mg $cm^{-2}$ target and a 1000μ Si detector (without degraders) and there is no way of separating the ΔE peaks. It seems to me that your peak is the sum of the ground state and first excited states transitions. The second peak may be a residue of the doublet near 4.7 MeV excitation.

I would be grateful if you could look into the above points. It turns out that if the polarimeter were analyzing a composite peak of the ground and first excited states, the effective analyzing power should be lower than -0.63, and might change drastically with kinematic effects as a function of angle, particularly because the X-section of the first excited state is at least a factor of two larger than that of the ground state. In your tests with the $^{12}$C target the situation is vastly different. The incident proton energy after the degrader is about 15 MeV and the first excited state of $^{14}$C is at about 6 MeV.

With regards,

RJS:dcv

R.J. Slobodrian
Physics Department

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC P.O. CANADA
G1K 7P4

April 20, 1982

Dr. Lynn Veeser
Los Alamos Scientific Laboratory
Mail Stop D410
Los Alamos, New Mexico 87545
USA

REF: p-14-82-U-163

Dear Dr. Veeser:

I am writing to you again concerning your helium polarimeter experiment. It would be helpful to me to have a detailed large scale drawing of it. In particular, to know the exact position of the 500 μm of Si of your passing detector and the diameter of it, i.e. the diameter of the active surface presented to the protons.

As I have commented before to Bob Hardekopf, the Ta degrader introduces a large r.m.s. multiple scattering angle to the proton beam. The polarimeter calibration, however, was carried out with a polarized beam, quite parallel, without degraders. The analyzing power of the polarimeter depends critically on the range of angles of the scattering off helium. Such range, for 62% of the protons when degraded by 587 mg $cm^{-2}$ of Ta, is increased considerably, and it is no longer defined by the copper vanes to $\pm 7.5^{\circ}$. I obtain $\pm 16^{\circ}$. A quick calculation then gives a much lower analyzing power for your polarimeter.

Finally, it seems to me that the passing detector spectrum shown in your paper is ungated. I would be thankful if you could provide me with a coincidence spectrum of your passing detector with your side detectors. It is this spectrum which is crucial to determine the degree of separation of the ground state and 1st excited state in your experiment.

Sincerely yours,

R.J. SLOBODRIAN
Van de Graaff Laboratory

RJS:dcv

UNIVERSITÉ LAVAL
FACULTÉ DES SCIENCES ET DE GÉNIE
CITÉ UNIVERSITAIRE
QUÉBEC, P.Q., CANADA
G1K 7P4

3 June 1982

Dr. Lynn Veeser
Los Alamos National Laboratory
Mail Stop D410
Los Alamos, New Mexico 87545
USA

Re: P-14-82-U-220

Dear Dr. Veeser,

Many thanks for your letter of May 21 and enclosed information. Peak fitting on your passing detector spectra for the 4.7 mg cm$^{-2}$ Be target indicates to me that you may have 10% of the number of counts assigned to the ground state peak, coming really from the first excited state. You mention also radiation damage, if would be relevant to know your separation at the end, before changing detectors, as such damage results in low energy tails.

In my letter of May 20 (copy enclosed) I had asked the exact position of the 500 μm Si detector (passing detector) and / or a large scale drawing of the polarimeter. Is this information available? It is impossible to ascertain this from the NIM paper.*)

I have looked again at your published L and R detector spectra (I say again because last year I wrote to Bob about them). It seems to me that your procedure to account for backgrounds is not proper. The reason is simply that you have slit scattering and multiple scattering, this means that you have background particles that are real events from the point of view of a TAC as determined by your conditions. One can see this clearly in your figures 5d) and e). I have subtracted backgrounds by looking at the level "far" from the peaks. The asymmetry is $\varepsilon = -0.237$ which together with your value for $A = -0.63$ results in $P = 0.38$, to be compared with $P = 0.275$ obtained using your method, relying on accidental coincidences, which I believe is improper. The polarization value is increased by 50% with the alternative background subtraction.

.../2

*) The NIM paper has a rectangle : where is the 500μ thickness?

Concerning the problem of the effective analyzing power of your polarimeter I have to disagree with your assessment of the effects of angular spread due to multiple scattering. In fact your scattering region is quite short, however, by the same argument you use, the r.m.s. scattering angle privileges the first vanes over the last ones. I have calculated $A_{eff} = -0.50$ for your polarimeter with 587 mg of tantalum. This again would increase P for your publised spectra to $P = 0.48$. Now, Bob explained to me that the peaks in the preprint (and publication) were obtained with the polarimeter at one side of the beam. If we now take your published average at $45^o$, $P = 0.165$, and correct it in the same way the final result is $P = 0.29$ ! This value is certainly consistent with our own.

I would be grateful to receive your comments on the above points and the information requested.

Sincerely yours,

RJS:dcv

R.J. SLOBODRIAN
Van de Graaff Laboratory

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

January 19, 1982

Professor BENGT NAGEL
THE ROYAL SWEDISH ACADEMY OF SCIENCES
NOBEL COMMITTEE IN PHYSICS
P.O.Box 50004
S-STOCKHOLM, SWEDEN

Dear Professor Nagel,

I am taking the liberty of enclosing a copy of my recommendation submitted to the NOBEL COMMITTEE on the same date, suggesting the consideration of Professors R.J.SLOBODRIAN (Canada) and H.E. CONZETT (U.S.A.) as candidates for the Nobel Price in Physics of 1982.

The primary hope of the enclosed recommendation is that the NOBEL COMMITTEE initiates a monitoring of the scientific events that are expected to unfold in the underlying, truly fundamental aspect of contemporary physics, the possible origin of the irreversibility of our macroscopic world in the most elementary structure of matter, that of the strong (nuclear) interactions.

In case of interest by the part of the NOBEL COMMITTEE, I would be glad to cooperate to my best, and in the most confidential form possible, by providing all relevant information that is expected to materialize in the future years in the case.

Thank you for your consideration and time.

Most Respectfully Yours

Ruggero Maria Santilli

RMS-vf

P.S. A number of colleagues from Europe, South America, North America and Australia have recently contacted me indicating their desire to submit a similar recommendation to the NOBEL COMMITTEE. It is my understanding that these independent letters, either have already reached Stockholm, or are in the process of arriving there.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

*January 19, 1982*

*NOBEL COMMITTEE FOR PHYSICS
OF THE ROYAL ACADEMY OF SCIENCE
Sturegatan 14
S–11436 STOCKHOLM, SWEDEN*

*Honorable Committee,*

*I am taking the liberty of recommending*

| | | |
|---|---|---|
| *Professor R.J. SLOBODRIAN*<br>*Laboratoire de Physique Théorique*<br>*Université Laval*<br>*QUÉBEC G1K 7P4, Canada* | *and* | *Professor H.E. CONZETT*<br>*Lawrence Berkeley Laboratory*<br>*The University of California*<br>*BERKELEY, California 94720* |

*as CANDIDATES FOR THE NOBEL PRIZE IN PHYSICS FOR 1982.*

*My recommendation is based on the recent discovery by Professors SLOBODRIAN and CONZETT regarding the violation of the T–symmetry in nuclear physics, as announced in their recently published article*

> *R.J. SLOBODRIAN, H.E. CONZETT, et al, "Evidence of time symmetry violation in the interactions of nuclear particles", Phys. Rev. Letters 47, 1803 (1982).*

*I recently had the privilege of listening to an invited talk by Professor SLOBODRIAN delivered at the*

> *FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE–ADMISSIBLE TREATMENT, held at the Département de Physique de L'Université d'Orléans, France, from January 5 to 9, 1982.*

*Several additional talks by distinguished speakers in related fields were also delivered at this Conference. As a result of these and other circumstances, I believe that the discovery by Professors SLOBODRIAN and CONZETT is of truly fundamental physical relevance, with implications perhaps even greater than those of the discovery of the P–violation. I provide below a brief elaboration of the most salient aspects, while I remain at your disposal for a detailed and technical presentation.*

*HISTORICAL SIGNIFICANCE. The physical reality of our environment provides unequivocal evidence that macroscopic phenomena violate the T–symmetry. The structure of atoms, on the contrary, has resulted in verifying the T–symmetry. It has therefore often been assumed that the symmetry is also valid for elementary particle at large. This has lead researchers to attempt the interpretation of the macroscopic irreversibility via a large collection of elementary particle processes, each of which is reversible. None of these attempts has been able to overcome the numerous inconsistencies inherent in the problem, and to achieve acceptance by the scientific community at large. Jointly, we have seen an increasing number of authoritative studies stressing that the most natural interpretation of the macroscopic irreversibility is that it originates at the level of elementary particles and their interactions. The discovery by Professors SLOBODRIAN and CONZETT provides a resolution of this historical problem which, for a number of technical reasons I cannot review here, is apparently final.*

*PHYSICAL SIGNIFICANCE. As we know well, the violation of the P–symmetry was incorporated in physics without foundamental changes in the mathematical structure of the theoretical formulations. The discovery of the violation of the T–symmetry appears to have much deeper implications. The T–symmetry is at the foundation of dynamics inasmuch it is at the foundation of the time evolution. The discovery of the T-violation may therefore imply a revision of the fundamental dynamical equations of contemporary physics. For instance, according to specialized literature in the field, the forces which appear to be responsible for the breaking of the T–symmetry are the non-potential, non–Hamiltonian forces originating in contact phenomena, such as the mutual penetration of the wave packets of hadrons under the conditions of the strong interactions, the collision of molecules in statistical ensembles, etc. The ordinary Quantum Mechanics, since it is essentially Hamiltonian in character, is potentially unable to represent the type of T–symmetry breaking under consideration. Also, recent advances in the study of symmetry breaking have lead to the understanding that a Hamiltonian (total energy) can be conserved and invariant under a given discrete or connected symmetry, while the underlying equations of motion violate the symmetry. These and other occurrences have suggested the attempt to generalize Quantum Mechanics into a form specifically conceived for the strong interactions, which is now under study by an increasing number of mathematicians and physicists under the name of Hadronic Mechanics. A significant hope of these efforts, beginning with the T–violation, is to achieve knowledge which is relevant to controlled fusion.*

*MATHEMATICAL SIGNIFICANCE. The mathematical implications of the discovery by Professors SLOBODRIAN and CONZETT are equally far reaching. Simply stated, the discovery can provide a crucial impetus to the generalization of Lie's theory, e.g., of the Lie–Admissible type which is already under study by a number of pioneering mathematicians, and which is the mathematical structure of the Hadronic Mechanics. In the simplest possible terms, Heisenberg's time evolution can be seen, from a mathematical viewpoint, as a two-sided Lie module, one module for each direction of time. Quantum mechanics is then structurally T–symmetric in the sense that time reversal essentially map one module into the algebraically equivalent other. When the time evolution is realized according to the covering, Lie–admissible, two-sided modules, one reaches a theory which is intrinsically T–noninvariant irrespective of the invariance properties of the Hamiltonian, inasmuch time reversal maps each module (each direction of time) into an algebraically different module, thus resulting into irreversibility of processes under unrestricted forces. It should be noted here that the two–sided Lie–admissible modules (or other mathematically equivalent structures) demand a generalization of the virtual entirety of Lie's theory, from the enveloping algebras, to the Lie groups, to the representation theory, etc. The implications for the development of mathematics as well as physics, are then self–evident.*

*The historical, physical, and mathematical aspects indicated in this letter have been discussed in detail at the recent Orléans Conference, and are recorded in the Proceedings currently in print. Additional pertinent material is available from the Proceedings of the WORKSHOPS ON LIE–ADMISSIBLE FORMULATIONS held here in Cambridge–U.S.A. from 1978 to 1981, as well as from specialized literature in statistical mechanics and other disciplines.*

*In case this Honorable Committee desires more technical and detailed information regarding my personal recommendation for Professors SLOBODRIAN and CONZETT being CANDIDATES FOR THE NOBEL PRICE IN PHYSICS OF 1982, please let me know. It would be a pleasure to prepare a more detailed technical presentation, possibly with the assistance of other experts.*

*Hoping that I did not abuse of your courtesy and time, and thanking for your consideration, I remain*

*Very Truly Yours*

*Ruggero Maria Santilli*
*Professor of Theoretical Physics*

*RMS–vf*

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

July 21, 1982

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

RE: LR2111: "Use of hadronic mechanics..."

Dear Dr. Santilli:

I am sorry to be so delayed in replying to your letter of May 28, but I wanted to have the time to review the complete file on your paper at the editorial office before attempting to understand the situation. I make only one trip a month to Ridge, in general, so that delays are sometimes inevitable.

As you know from your personal experience as a journal editor, strict criteria for acceptance or rejection of papers have to be established and rigorously maintained, or else the system would have no valid claim to objectivity. By very long tradition, all papers submitted to any of the Physical Review journals, whether from Nobel laureates or complete unknowns, must be referred to independent, expert referees selected by the Editors who must recommend acceptance of papers before they can be published. Our editors, while fine physicists themselves, cannot be expert in all fields of physics and must rely on the advice of outside experts to perfect papers submitted (which are rarely acceptable in precisely the original form) and to reject those papers which are unsuitable for our journals. The referees need not disprove the contentions of a paper to disapprove its acceptance; rather, the burden is on the authors to convince the referees that the paper is acceptable. Clearly, if a paper is not comprehensible to an expert referee, it will not be useful to a less well informed reader. No exceptions are ever made to this procedure, but authors are permitted to exclude certain specific referees, if they so choose.

In the case of your paper, in your initial submission which was received on April 19, 1982, no mention was made about excluding any specific referees, and the paper was routinely submitted to two physicists of considerable eminence for comment. One rejected it out of hand and the second wrote a rather detailed review which was sent to you. Your reply of May 26, together with earlier correspondence was sent to two additional referees, one of whom gave a detailed comment, but did not recommend acceptance of the paper. On the basis of all comments received from referees, the editors had no choice but to reject your paper.

In your letter to me on May 26, you requested that no referees from Boston area institutions be consulted about your paper, a rather large exclusion and one not mentioned earlier. By sheer chance, none of the earlier referees were, in fact, from Boston area institutions, none expressed any familiarity with you personally, and there is not the slightest reason to suspect that there was any personal animus in their appraisals of your paper. Thus, even by the post hoc rules of the game set by your letter of May 26, your paper received an eminently fair hearing and was rejected on objective grounds. No further consideration is merited.

I assure you that, however popular "quark theories" of elementary particles may appear to be, the theorists who expound such models are not an "establishment" which runs the American Physical Society or its journals. I am an experimental solid-state physicist myself and recognize no formal hierarchy in physics which could provide the "right" answers. Physics, by its very nature, is and ought to be contentious. We do not shirk from publishing controversial papers. As for your three "rules" for our journals, they correspond, in fact, to our current procedures: all experimental and theoretical papers whether based on quark models or otherwise, receive precisely the same refereeing procedure, hence the same "difficulties in publication"; your referees were, in fact, experts and physicists of great eminence whose opinions must be respected; referee reports are, in fact, all examined for any signs of personal animus and are rejected for the reasons you mention. I believe that our current editorial procedures, while possibly not perfect, are completely honest and objective and have resulted in our journals maintaining their reputations as the world's best.

Sincerely yours,

David Lazarus
Editor-in-Chief

xc: R. K. Adair
G. L. Trigg

VERY CONFIDENTIAL

September 10, 1982

Dear Dr. Lazarus,

I shall comment on your letter of July 21, 1982 sometimes in the near future in a formal way.

This note is to keep you informed that the continual rejection of my paper has forced us into a first step. In fact, I shall be in Washington on September 14-15-16, among other reasons, to consult with appropriate observers on what we consider needed to bring back the journals of the APS into the genuine fulfillment of national interests via the free pursue of truly novel advancements in physical knowledge. A variety of options will be discussed ranging from graceful acceptance, to the release to the international press of documented views of the situation.

You must understand that, like all other physicists, I had many papers rejected in my life and I have accepted them with grace. This time the situation is different.

An entire new mechanics has been constructed, the Birkhoffian mechanics, without one single paper appearing in journals of the APS. In fact, my monograph reviewing this achievement is just about to be released by the printer. Inspection of the references is then a silent but unequivocal identification of this very grave episode. The reason is simple and it is the usual one: referees have opposed such achievement to the point of disgusting reputable authors.

According to all indications, it appers that established academic interests have decided to repeat the exploit. I am referring to the construction this time of the hadronic mechanics [which is at a rather advanced stage already] again without one single paper appearing in the journals of the APS.

But the the construction of new theories capable of treating non-Hamiltonian systems [such as the Birkhoffian and the hadronic mechanics] is an important part of national interests [you should recall that all military systems are non-Hamiltonian], while the same theories are strictly outside personal interests of contemporary academicians.

We have therefore reached the delineation of all the necessary prerequisites for the typical case of direct conflict between national interests for the pursue of novel physical knowledge, and vested academic interests that are against such a pursue.

Graceful acceptance of such a situation then becomes an unequivocal indication of complicity. To be able to keep looking at our children with clear eyes we need a vigorous opposition, and the undertaking of all the necessary steps to eliminate this totalitarian conduction of research, and the restoration of the genuine freedom in scientific inquiry.

The problem at your journals is incontrovertibly documented by now: valuable research efforts <u>must</u> be published, particularly when dealing with aspects of fundamental character. Criticisms to the same papers should equally appear in print, when valuable. This is the ONLY way to pursue novel knowledge via a free scientific process. When entire new mechanics are built (and this happens only occasionally per each century!] and not a single paper appears in your journals, you have a problem.

This is the land where my children will live. I intend to dedicade my life to its future well being at whatever personal price. You should never doubt about my determination, and not to confuse my preceding gracefulness with weakness.

Sincerely
Ruggero Maria Santilli

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

September 27, 1982

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I am in receipt of your recent correspondence regarding your paper submitted to Physical Review Letters, LR 2111, "Use of the hardonic mechanics......"

You ask that I "intervene in favor of publication." You surely understand, particularly since you are editor of your own journal, that I cannot intervene in this manner for anyone in the world, when referees have not recommended acceptance of your paper. As I wrote to you earlier: no exceptions are ever made to the criterion of acceptance by impartial referees before a paper may be published in any of the archival journals of the American Physical Society (only the Bulletin of the American Physical Society publishes author-submitted abstracts without referral of any sort). Despite your strong statements to the contrary, referees have not been able to see sufficient merit in your paper to recommend its acceptance, even with revision. Accordingly, by our long-established rules for acceptance, your paper cannot be accepted.

You have expressed concern that there may be some sort of "conspiracy" against your work to suppress your opus, organized by "quark-committed physicists." As I wrote to you earlier, I know of no such cabal, nor would I tolerate it. To convince myself, if not you, I sent your paper without comments from prior referees or your rebuttals to yet another physicist, one who is clearly not committed to quark models. The reply was similar to the previous ones: there is not sufficiently original or important contributions to physics in your paper to merit publication- - the mere fact that your Equation (10) relates to a single experiment is not sufficient, without also demonstrating that it is not in disagreement with all other experiments and has specific predictive power for experiments not yet performed. Mathematical elegance is not equatable with important physics.

I am sorry, but the Editor's rejection of your paper, based on several referees' reports, must stand.

Sincerely yours,

David Lazarus
Editor-in-Chief

xc: G. L. Trigg

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 12 1982

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

Dr. DAVID LAZARUS
Editor in Chief
The Physical Review and Physical Review Letters
1 Research Road
RIDGE, New York 19961

RE: Paper LR2111 submitted on April 19, 1982, to PRL entitled "Use of the hadronic mechanics for the fit of the time–asymmetry recently measured by Slobodrian, Conzett, et al" by R. M. Santilli (IBR preprint no. DE–TP–82–9

Dear Dr. Lazarus,

I would like to acknowledge your kind letters of July 21 and September 30,as well as our phone conversation of this past Thursday.

Permit me to stress from the outset that I have nothing but sincere gratitude for you and for Dr. G. L. TRIGG at PRL, not only for the time devoted to the case, but also for the courtesy of keeping me informed.

However, I feel obliged to express my reservations on the referees and on their selection. In the hope of contributing toward the continuation of our communications, I would like to summarize the case as seen from our profile.

STATUS OF PAPER. I understand you have accepted my moderate proposal to the effect of pausing for a couple of months in the consideration of this paper. This would give time to your editors to consider the experimental paper recently submitted to PR–C by Slobodrian et al on the repetition of the measures on the time–asymmetry, while giving me time for improving the paper to my best. Subsequently, I shall submit a revised version for one final review.

I would also like to reinstate that the submission to Phys. Rev. Letters is of mere indicational character, and that the possible consideration/publication of paper LR2111 by Phys. Rev. D, or Phys. Rev. C (say, as Rapid Communication) would be equally acceptable to us.

In fact, our primary objective is to have your Journals participate in the current laborious efforts to generalize quantum mechanics for extended particles. For this task, the selection of Phys. Rev. Letters, or Phys. Rev. D, or Phys. Rev. C, would be equally welcome.

YOUR INTERVENTION. Permit me to stress that I have not asked for your intervention to have my paper published. If I gave you this impression, please accept my apologies, while I assume all responsibilities. I have asked for your intervention to ensure due scientific process, that is, to ensure that the paper is subjected to a serious review by experts in the field, and that *a possible final rejection is motivated by errors, inconsistencies, and/or incompatibilities clearly identified and presented in the due scientific language.* I have insisted for this due scientific process in this case (but not in other cases in the past), because of a number of particular circumstances ranging from certain, unfortunate, preceding occurrences, to the number of observers monitoring the case, and to the negative implications for your Journals, as well as for the American Physical Society in case of unprofessional refereeing.

LACK OF CREDIBILITY OF AVAILABLE REFEREE REPORTS. I have seen reports only by two referees. The first was so unprofessional, to force the raising of ethical issues, as anybody can see from statements to the effect that "I do not know the Hadronic Journal that published the preceding literature, and, therefore, I recommend rejection". Besides all the hardly believable aspects reported elsewhere, this referee did not even understand the most crucial deficiency of the rebuffal to the Slobodrian–Conzett paper by Hardekopf et al. I am referring to their repetition of ONLY HALF of the measures–those of the polarization only–while relying on the measures by Slobodrian, Conzett et al on the remaining measures--on the analyzing power--(see below for additional comments).

The second referee also forced the raising of ethical issues, contrary to our best predisposition. In fact, he insisted in the rejection of the paper via arguments based on quark conjectures on electroweak decays, while the paper deals with certain nuclear reactions involving the exchange of two nucleons.

To understand the case, you must understand the surprise of a number of observers to see that PRL took seriously reports of this type, while they should have been returned to their authors with the request to do better homeworks before implicating Journals of the APS in their personal dances.

Also, the claim that the paper is "mathematical" can do nothing but confirm doubt on the existence of politics in this case. In fact, the paper is entirely devoted to THE INTERPRETATION OF AN EXPERIMENT. Additional shadows of questionable scientific practice are created by claims of lack of originality. In fact, the paper deals with nothing less than a GENERALIZATION OF QUANTUM MECHANICS! How can you expect that physicists nowadays accept such distorsions of reality?

But the statement that creates the highest concern is that the paper must be in agreement with all available experimental information. In fact, when translated in plain language, the statement implies the suppression of all possible attempts at your Journals to pursue truly novel physical knowledge. In fact, to reach one single paper verifying criteria of such extreme exigency one should work for a decade, and write a few thousand pages of research.

Par contre, your Journals publish with considerably easiness a large number of papers based on the assumption that there exist 36 (or so) unidentified quarks, subject to a still doubtful confinement, under the additional hypothesis that .........., etc., etc., etc.

Under the conditions of such extreme disparities, the shadows of partisanship at your Journals with established academic interests is then unavoidable. In turn, this raises a host of rather serious problems I pray you will not overlook.

**THE ERRORS IN THE REFEREE SELECTION.** While the basic rule of ethically sound editorial practices is the scientific credibility of the report, its prerequisite is the selection of referees who are experts in the field. For instance, the papers on quarks published in your Journals have been ALL refereed by experts in quarks. In case you can document ONE exception, please make it public, because it would help considerably this case.

It is evident that the handling of my paper has violated this other fundamental rule. In fact, the lack of any meaningful knowledge by the referees of the topic is manifestly transparent. You must understand that I am referring to a rather voluminous mathematical, theoretical, and experimental literature that constitutes the foundation of the current efforts to generalize quantum mechanics, for over 10,000 pages of published research.

The proof is simple and incontrovertible: HAS ANY OF THE SELECTED REFEREES PUBLISHED EVEN ONE SINGLE PAPER ON CONTACT–NONHAMILTONIAN INTERACTIONS? If not, the only way for your Journals to dissipate allegations of partisanship, is to start sending papers on quarks (including electroweak theories) to reputable quark nonbelievers (there are quite a few!).

It appears that the referees have been selected on the mere basis of their "good standing" at your Journals in complete disregard of their knowledge of the topic. Again, this disparity of editorial practices in the transition from fields aligned with established scientific interests to others creates sizable problems.

**PRECEDING UNFORTUNATE INSTANCES.** As is well known in informed circles, the way PRL handled the experimental paper by Slobodrian, Conzett et al (PRL **47**, 1803 (1981)) has caused considerable concern. One reason is that the paper was kept for an excessively long period of time, and was finally published only after academic groups of vested, opposing interests hadsufficient time to hurry a counter–experiment, and have it quoted in the original paper by Slobodrian, Conzett, et al.

By comparison, the rebuffal was published with such a rapidity, to be truly surprising.

I believe that the difficulties experienced by the first paper, compared to the lack of difficulties experienced by the rebuffal have caused a considerable damage to your Journals, as well as to the American Physical Society. This is a fact, whether you accept it or not. To understand it (as well as to have an idea of the talks on the subjects in academic corridors thoughout the world) you must understand that, while the first paper was the result of a serious experimental work over several years by a number of experimentalists in three Countries (U.S.A., Canada, and West Germany), the rebuffal

(1) was rushed in a period of time too short to constitute final work;
(2) was written in a transparently political language (in fact, it claimed the lack of time—asymmetry, while simple calculations show clearly that the four countermeasures can accommodate an infinite variety of curves of polarization all different than those of the analyzing power).
(3) was based on the repetition of only HALF measures, as indicated earlier.

The Phys. Rev. C has recently received the submission of the new measures by Slobodrian et al. *I pray God that this paper is treated in exactly the same way as the Los—Alamos one, and that your editors will see the implications for a continuation of a disparity in the editorial processing of papers aligned and nonaligned with existing academic interests.*

OBSERVERS MONITORING THE TIME—ASYMMETRY. I brought to your attention the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS we held on January, 1982, at the Université d'Orléans, France, under support of the French Government, with some four volumes of proceedings, and participants from virtually all developed Countries. The conference studied in detail the experimental, theoretical, and mathematical aspects of the time—asymmetry, beginning at the classical Newtonian level, and then passing to the statistical, and to the nuclear—particle profile.

Paper LR2111 constitutes a relevant expression of this conference. Therefore, your final decision will be monitored, not only by the participants to the Conference, but also by all scholars throughout the world who are interested in a credible resolution to the vexing, historical problem of the origin of irreversibility.

*I feel obliged to bring to your attention the additional fact that, following the International Conference, numerous scholars recommended Professor Slobodrian and Conzett to the Nobel Committee.* Contrary to what you may hear from physicists who would be damaged by a confirmation of the time—asymmetry, it appears that some form of monitoring has been implemented by the Nobel Committee in this case.

I pray that your Journals, as well as the American Physical Society, will not come out of this case with the "dark shadow" that suggested my contacting you in the first place.

Finally, we still have additional observers that I prefer to keep confidential at this time in the best interests of all.

CANDID CONSLUSIONS. Permit me to express the essence of the case, most respectfully, but as candidly as possible.

The coordinated mathematical, theoretical, and experimental efforts to generalize the "atomic mechanics" into a form more suitable for extended particles have now been launched, and opposing academic interests cannot stop them. In trying to jeopardize these efforts, they can only lose their face.

The construction of the underlying classical image, the Birkhoffian generalization of Hamiltonian mechanics, has been achieved without one single paper appearing in PR or PRL, as repeatedly noted to you.

You must understand that, if we see a repetition of the case a second time, and the hadronic mechanics is built without one single paper appearing in your Journals, a scandal of international and historical proportions is unavoidable, whether you see it or not.

I could withdraw paper LR2111 from your Journals and publish it (rather easily I believe) in other Journals. However, this would result in nothing else than increased risks for a crisis at some later time and, as such, the withdrawal would be against the interests of the American Physical Society, in my view.

*The primary function of your Journals vis—a—vis national interests is to pursue* NOVEL *physical knowledge. If this task is made unreasonably difficult by established academic interests, the problem of potential conflict between your editorial practices and national interests is unavoidable.*

Very truly yours,

Ruggero M. Santilli

RMS/mlw

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

October 16, 1982

RE: paper LR211 submitted to PRL entl."Use of the hadronic mechanics for the fit of the time-asymmetry recently measured by Slobodrian Conzett, et al."

Dr.D. LAZARUS, Editor in Chief
The American Physical Society

Dear Dr. Lazarus,

I must express my indignation at a letter from Dr. C.M.SOMMERFIELD of Yale University I have just received (copy enclosed).

My entire struggle in this case is to have your Editors producing professional referee reports, with the clear identification of scientifically credible errors, inconsistencies, or incompatibility.I believe this is important for your Journals as well as for the APS in this instance, because of the number and nature of the observers monitoring the case, which include participants to a recent international conference on the origin of irreversibility in nature, as well as scholars interested in this historical problem. In addition, the Nobel Committee has received numerous recommendations from several Countries supporting the candidacy at some future time of Professors Slobodrian and Conzett (who first measured the time-asymmetry in nuclear physics), and a form of monitoring appears to be in place.

The letter by Dr. Sommerfield, under these circumstances, constitutes a clear disservice to your Journals as well as to the APS. In fact, letters of this type could,at the extreme, turn the case into a street fight. To begin, Dr. Sommerfield has no knowledge whatsoever of the field of the paper (NONHAMILTONIAN classical, statistical, and particle mechanics). Thus, his personal opinion has no meaningful scientific value beyond the level of curiosity. Furthermore, he claims that the referees are well known and respected physicists. But by whom? Is this because these referees belong to the group of academic-financial interests of which Dr. Sommerfield is well known to be an active member? At any rate, the lack of credibility and the unprofessional character of the reports (see my last letter to you of October 12, 1982) speak for themselves.

To prevent a completely un-necessary deterioration of this case, with international consequences, caused by Dr. Sommerfield's intervention, I beg you to confirm our rather moderate conclusions we reached by phone on October 6, 1982, to the effect that:

1. We shall pause for a couple of months in the consideration of this paper, to give time to your Editors to consider a paper recently submitted to Phys. Rev.-C by the Québec experimental group confirming the original measures of time-asymmetry (which constitutes a beautiful, if not necessarily final, EXPERIMENTAL confirmation of my paper);
2. I shall subsequently submit a revised version of my letter LR2111 for one, final review. This revised version shall stress in a clealer form the conjectural-speculative character of the paper, as well as its elementary nature, and include any change of style and or of contents deemed recommendable; while
3. You shall let me know the most appropriate Journal for this final re-submission, whether Phys. Rev. Letters, or Phys. Rev. C, or Phys. Rev. D.

Thank you.

Very Truly Yours

Ruggero Maria Santilli

cc.: Professors A.B.GIAMATTI and F.W.K.FIRK, Yale University.

# The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

October 19, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your letters of October 12 and October 16 just arrived in today's mail. The copy of Dr. Sommerfield's letter, referred to in your letter of October 16, was not enclosed, so I have not seen his report, which I presume was requested by the Editor as the standard first step in the formal author appeals process.

This letter will confirm my understanding of our telephone conversation as it affects the status of your paper submitted to PRL:

1. The matter of your earlier paper LR2111, "Use of the hadronic mechanics..." will be placed "on hold" for a couple of months until the editors have had time to consider the new paper by the Quebec group recently submitted to Phys. Rev. C regarding an experimental test of time-assymetry.

2. You plan to submit a revised version of LP2111 for further review.(By our rules, this will probably be considered de nuovo, as a new submission.)

3. Your revised paper may be submitted to any of our journals: PRL, or Phys. Rev. C or D, which you (not I) consider most appropriate/

4. You have the right to submit, along with your paper, a suggested list of (several) possible referees (which the Editors may, or may not wish to use as a basis for referee selection) as well as a list of persons whom you would specifically exclude as possible referees.

I am sending copies of this letter to the Editors of PRL, Phys. Rev. C and Phys. Rev. D.

Sincerely,

David Lazarus

xc: G. L. Trigg
H. H. Barschall
D. Nordstrom

CONFIDENTIAL

December6, 1982

Dr. D. Lazarus
Editor in Chief,
Physical Review Letters and Physical Reviews

Dear Dr. Lazarus,

I have been informed that Physical Review Letters is considering the publication in early 1983 of a paper by Dr. C. Rubbia and his co-workers concerning the alleged identification at CERN of two apparent "candidates" for the heavy bosons they have been looking for.

I am contacting you to recommend the maximal possible prudence in the handling of this case. Also, I am contacting you to express my viewpoint which, whatever its value, is sincerely intended in the interest of the American Physical Society, as I hope you will see.

The need for the utmost possible caution in this case stems from several aspects, such as

[a] the fact that we are gearing up here for a national call intended to promote the formulation, adoption and inforcement by the APS of a code of ethics; even though this action will be as orderly as possible, it will inevitably focus attention on all future developments at your Journals;

[b] Dr. Rubbia's view that he has apparent "candidates" is not sufficiently shared by his own colleagues at CERN and other places, to the best of the information that has reached me; you should therefore take into consideration the possibility that, under action [a], some of Dr. Rubbia's colleagues decide to express publicly his/her own view and the implications of such (not so unrealistic) scenario for our community;

[c] Dr. Rubbia has regrettably made some questionable statements to the press prior to the initiation of these experiments; as an example, the New York Time of mid August 1982 quoted the following statement by Dr. Rubbia: " when the experiment begins running full blast in October, 10 $W^{\pm}$ and one $Z^{\circ}$ particle should be seen dayly." As everybody knows, the reality has been far distant from these salesmen-type statements, and this may have a direct bearing on the implications of a possible publication by (any of) your Journals.

Permit me to express my view, most respectfully, for whatever its value. I believe that Dr. Rubbia paper should be published by Physical Review Letters or, in case of insufficient value, at least as rapid communication in Physical Review D. This is so because of my believe, now familiar to you, that all plausible physical views of fundamental character must be published, and then eventually proved wrong by other papers. The aspects in which utmost caution must be exercised are the following.

[1] the rapidity of publication; it is of the utmost importance, particularly during a forthcoming national call for a code of ethics, that the time of publication of Dr. Rubbia's paper be exactly the same as that of nonaligned papers, in the average of about one year; this rule of thumb would put publication at about end 1983; besides proving lack of partisanship (at least in this case), it will give you time to verify that the team at CERN is indeed aligned, and it will give time to Dr. Rubbia to verify each and every one of his statements;

[2] the language of publication is equally of vital importance for the American Physical Society; I am referring here to the need for a clear identification in the paper of the conjectural character of the claim, and the complete absence of excessive languages favoring the existence of quarks as physical particles, or implying it as established.

page 2

In case you give me the opportunity to review the paper as your personal adviser, or as a formal referee for its theoretical part (only), or in any way you prefer, I can provide you with more specific recommendation. Again, permit me to stress that I favor the publication, and you should not expect an a-priori rejection. Instead, I can advise you on what appears to be the best possible handling, of course not in the interest of Dr. Rubbia and his group, but instead in the best interest of the pursuit of knowledge and of the American Physical Society.

Nevertheless, I beg you not to feel obliged to mail me copy of the paper. I offered this possibility as a sincere manifestation of my desire to collaborate, particularly during the forthcoming call for the code of ethics, in order to minimize or otherwise prevent un-necessary deteriorations.

I have mailed one copy of this letter only to Dr. P.W.Anderson at Princeton University, but I have absteined from mailing any additional copy to members of the Editorial Organization of your Journals.

Best Personal Regards

Ruggero Maria Santilli
96 Prescott Street
Cambridge, Massachusetts 02138
tel. (617) 864 9859

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

December 17, 1982

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your letter of December 6 has reached me at the Editorial Office of the American Physical Society, where I am catching up on various matters this week.

I am completely unsympathetic with your request. Surely, as a journal editor yourself, you must be aware of the fact that all submissions to scientific journals are privileged communications, whose very existence must be presumed to be confidential (except for review pruposes), unless disclosed by the author. Even I have no right to see any submitted paper, unless this is required for review purposes. Accordingly, I have no knowledge of whether Rubbia has, or has not, submitted a paper to Physical Review Letters. In any event, it would be completely improper for me to copy such a paper for you, for any reason, unless you were selected as a referee by one of the Editors of the journal. If you wish a copy of the paper, if it exists, you must write to Rubbia yourself.

I should have thought that someone as concerned about the ethics of publication as yourself would have been more sensitive than to have requested me to do something completely unethical.

Sincerely,

David Lazarus
Editor-in-Chief

DL:pd

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

December 21, 1982

Dr. D. LAZARUS
Editor in Chief
American Physical Society
Department of Physics
University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

Quite regrettably, I must have a record of disagreement with your letter of December 17.

On my own letter of December 6, 1982, as you can see perhaps by reading it again, I submitted a delicate recommendation on a potentially dangerous topic for the APS, (a) in a way "most respectfully", (b) for "whatever its value", and (c) with the explicitly written statement (page 2, line 7)

"I beg you not to feel obliged to mail me a copy of the paper"

As you can see, it is evident that I did not "request" copy of the paper, as your letter tends to imply. After all, I do not even know whether the paper has been truly submitted, owing to the tentative information I am receiving from my contacts at CERN.

Also, I do not see how an editor can do something completely unethical by consulting physicists for additional advice on matters of considerable controversy, such as the alleged "candidates" at CERN are, but this is my personal view, and I am not pretending you to agree.

At any rate, your sentitivity to ethical issues is sincerely appreciated. It may be the focal point in which we can pull all of us together, resolve our differences in an orderly way, and avoid un-necessary public crisis.

Very Truly Yours

Ruggero M. Santilli

cc. Dr. Anderson (only).

P.S. You will be pleased to know that ref.s [2] and [3] of the new paper I recently submitted to you ("A possible time-asymmetric model for open nuclear reactions") have been printed and are now available via ordinary channels (these are the vol. II of my two series of monographs, one with Springer-Verlag and one with Hadronic Press). I thought that the referees might be interested in the information. I confirm the availability on request of temporary copies for the referees convenience.

# The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

January 6, 1983

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I am again at the APS Editorial Office, where your letter of December 21 just reached me after some delay, since I have been away from Urbana for a couple of weeks.

I think that some of the confusion in our letters may be caused by your misunderstanding of my role vis-a-vis our journals. As Editor-in-Chief for the American Physical Society, I have executive responsibility for all of our journals, but I am not an Editor of any of them. Editors receive submitted manuscripts, select referees, conduct correspondence with authors, etc., etc., all directed to selecting (and rejecting) papers for their individual journals. Each journal has one or more Editors: Physical Review A, Physical Review B, Physical Review C, Physical Review D, Physical Review Letters (3 Editors) and Reviews of Modern Physics.Each journal also has one or more Associate and/or Assistant Editors who aid the full Editors in their work.

My role is to worry about the finances of our journals, to establish policy, to interact with the active physics community (of which I am a part), to handle author appeals and other "sticky" situations: in short, to represent the whole of the Society in the operations of all of our publications. Thus I never enter into the matter of selecting referees or soliciting opinions, unless on specific request of an author or an editor. My role is not that of "super-editor," but more that of Chairman of the Users' Group, with financial responsibility.

One small point: our typical time delay between submission and publication is far less than one year, as you suggest. It is closer to 3-4 months, which is still far too long.

Sincerely,

David Lazarus

David Lazarus

xc: P. W. Anderson

The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

February 8, 1983

Professor R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

I have just learned, via a CERN presse release, that Rubbia's paper describing the alleged discovery of the intermediate vector boson will be published in Physics Letters B, 25 February 1983.

Physics Letters is not published by the American Physical Society.

Sincerely,

David Lazarus

# The American Physical Society

DAVID LAZARUS
EDITOR-IN-CHIEF

DEPT. OF PHYSICS
UNIVERSITY OF ILLINOIS
URBANA, ILLINOIS 61801
(217) 333-0492

April 25, 1983

Dr. R. M. Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Paper LZ 2206

Dear Dr. Santilli:

I am sorry to be a bit delayed in replying to your recent note, attached to a copy of your letter of April 9 to George Trigg. All our Editors were away at the Baltimore APS meeting last week, and I wanted a chance to speak with Dr. Trigg before I wrote to you, to be sure that I was aware of all the facts regarding the paper.

First, let me point out that Professor Okubo, by his own request (noted in his letter to you of November 10, 1982), was not a referee on paper LZ 2206; he was a referee, as he stated to you, on your earlier paper LR 2111, and it was that paper which he suggested might be more suitable for Phys. Rev. None of the referees suggested that paper LZ 2206 might be better for Phys. Rev., and no Phys. Rev. editors have ever seen it. Clearly, therefore, there is no way in which it can be summarily accepted for Phys. Rev., since, in fact, it has never been submitted to Phys. Rev., either by you or by referrral of the PRL Editors.

I have read through the comments of the three reviewers of this paper with some care, particularly since I do know their identities. All three are very respectable physicists and leaders in the field, and referee no. 2, who dismissed the paper summarily, is a Nobel laureate. You could go ahead and ask that the paper be submitted to Phys. Rev. D, but my guess is that it would probably elicit similar responses from referees. Instead, I suggest that you look again at all three referees responses and, wearing your editor's hat, ask yourself what advide you might give to an author whose paper, as submitted, elicted these reponses from responsible, even famous, physicist-reviewers. Even more important, ask yourself, as an author, "To whom is this paper really addressed? Who may be expected to read it? What should they learn from reading it?" In this vein, it makes no sense to continue fighting back and forth about finding a referee who is sufficiently well versed in the very estoteric subject addressed by the paper (and, I presume, by your earlier papers which we have had to reject) who can persuade the Editor that the paper should be published. It would still, presumably, be incomprehensible to most of the world's theorists who, apparently, do not even understand your notation and equations, much less their importance. It would be even less comprehensible to less sophisticated general readers whom you would, presumably, like to convince of the importance of your work. Note carefully that referees 1 and 3 do feel that there is probably merit in the work but clearly cannot themselves understand it sufficiently to pass judgement on it. Referee 2 cannot even read the paper, and clearly finds it completely "obscure."

As you well know, authors are often the worst judges of the comprehensibility of their own papers. Facts and statements which are obvious to them (after thinking hard about the subject, possibly for years) are often completely vague to a less well informed reader, even one very expert in other facets of the subject. The purpose of any paper which merits publication, at least in the journals of the American Physical Society, must be to teach a sensible subset of readers something new. We are not running a "Vanity Press" for the benefit of our authors. The two words "teach" and "new" are the operative definitions of acceptance or rejection, and these are always to be judged with reference to their benefit only to readers. We never reject papers simply because their are not "main-line." Controversy in physics is expected,natural, and even healthy. Your papers are not being rejected because they are bad physics (demonstrably bad), or trivial (not "new"), or "anti-establishment." They are being rejected simply because they are not comprehensible to a very large set of your peers. Einstein may have been "anti-establishment" in 1905, but his three famous papers were published in Annalen der Physik, because they were well written and comprehensible.... indeed, they are models of clear,written physics.

Remember that a paper must answer, in advance, all those "little" questions which a responsible reader may ask. Accordingly, it carries a greater burden on the author than is necessary for a speaker on the same subject, who is physically present to answer questions.

If you are writing your papers to be read by readers who are not already expert in Lie-associated, Lie- admissable, and Lie-isotopic constructions, then admit that papers, as you are now writing them, are not comprehensible to such readers.(If, on the other hand, you are writing only for readers who are already experts in this area, the Physical Review journals are not suitable vehicles for your papers.)

I strongly suggest that your consider rewriting your paper completely, very possibly for Physical Review rather than PRL, where you will not be constrained to a very few pages, and try to make it completely comprehensible to a reasonably unsophisticated reader. You might wish to consider a somewhat "neutral" co-author, perhaps someone like Professor Okubo, or possibly Francis Low, or someone else of comparable stature who is experienced in writing comprehensible papers on esoteric subjects. Alternatively, you may wish to write a paper yourself, but bounce it off several such persons before submitting it for publication, and be prepared to revise it massively if the responses indicate that it is unclear. I always ask someone else to read through my own papers before I submit them, and have often gone through several drafts before the paper is actually mailed off to the journal, and my papers are about as non-controversial as you can get! Where papers are controversial and subject to possible misinterpretation, it is even more incumbent on the author to ensure that his submitted paper is absolutely clear and free from errors.

I would like your papers to be acceptable to our journals; I love a good fight, particularly between theorists! I hope you will take my comments as friendly suggestions, in the way they are intended.

Sincerely,

David Lazarus

xc: S. Okubo
G. L. Trigg
D. L. Nordstom
F. E. Low

Dr. D. LAZARUS, Editor in Chief
The American Physical Society

April 29, 1983

Dear Dr. Lazarus,

I appreciated the courtesy of your letter of April 25, 1983. Regrettably, it appears that you have been unable to address the real problems for predictable and understandable reasons. I shall therefore keep the submission of a (revised) version of my note LR2111=LZ 2206 at the European Editor I have contacted. Also, I regret to inform you that I do not contemplate to submit additional papers to APS Journals for the foreseable future (I am writing a considerable number of them for the final stage of my terminal DOE grant).The only exception has been my recent submission of paper DDR231 to Phys. Rev. D (under legal assistance beginning with the submission). This is due to the fact that the indignation of members of our team had reached alarming proportion because of the suppression of the quotation of rather massive references in papers printed in your Journals.As president of the I.B.R. I thought that perhaps I should try to minimize the risks of a direct, open confrontation. But I am still doubtful that my submission was indeed the right thing to do.

It is very regrettable that you could not address the alleged misconduits that have occurred, primarily, in the handling of experimental papers on time-asymmetry, and then on my own theoretical note. There is no point to repeat them here. Perhaps, you should understand why I do not want to waste my time with APS journalsfor the foreseable future. If I put my editorial hat, I would have released the following report on papers LR2111=LZ2206:

"Paper LR2111(orLZ2206) is not suitable for publication in its current form. However, the paper could be considered for possible publication as a Rapid Communication in Phys. Rev. D (or C), provided that Santilli complies with the following suggestions: (1) that he clarifies the connection between his model and Prigogine's statistics; (2) that he identifies more clearly the non-Hamiltonian origin of the irreversibility (plus any other suggested improvement) and, last but not least, (3) that he prepares a longer, more detailed paper on the same topic to be submitted jointly with the revised letter."

My reaction to a constructive refereeing of this type would have been, first, of gratitude, and second, of full and complete cooperation.

Instead, all the numerous referees' reports released by your office stated nothing but REJECT, REJECT, REJECT, and then attempted unbeliavable mumbo-jambo dances in the dream of"smoking out" the rejection. Your seemingly sound suggestion (write a longer and more detailed paper) is therefore shattered by incontrovertible evidence established by over a decade of occurrences of this type. In fact, it would be equivalent to permitting the suppression of the model for a number of additional years. It is evident that the only way to avoid these dark shadows would have been the usual ways followed by papers aligned with vested academic-financial-ethnic interests: publish a short letter (which can be understood, in general, by very very few) and, subsequently, publish a long detailed paper. We should not forget that scientific rigour is at the foundation of any sound advance. However, excesses in the request of scientific rigour are generally a facade for manipulations, particularly when addressing potentially fundamental advances.

You mention that referee no. 2 of paper LZ2206 is a Nobel laureate. This is exactly the same as telling a jewish physicist who survided a concentration camp that the referee of his paper is a famous german scientist.In my letter to you ofNovember 27, 1983 I told you the episode of my visit at Lyman laboratory, where the triplet Glashow-Weinberg-Coleman, two of whom are Nobel laureates, specifically and intentionally created severe hardship on my children and on my family by preventing my drawing my own salary from my own grant. The very mention that referee 2 of paper LZ2206 is a Nobel laureate is a confirmation of the lack of acknowledgment at the journals of the APS of an editorial problem that, according to an increasing number of observers, has now reached the dimension of threat to National interests because of its dimension, diversification and high level of manifestation (see enclosures).In the final analysis, the selection of a (US) Nobel laureate as a referee of my paper may be seen as demonstrably unethical because no (US) Nobel laureate has any meaningful knowledge and record of expertise in the field of the paper (isotopies and genotopies of Hilbert spaces and Lie algebras).

But the apparent scientific crime committed with paper LR2111=LZ2206 is considerably broader then the mere suppression of a theoretical model. As repeatedly indicated to you, the paper was the representative of a new scientific current involving an increasing number of experimentalists, theoreticians, and mathematicians, as well as of a new institute of research, funded and organized via (for us) immense sacrifices.The suppression of paper LR2111=LZ2206 has implied, whether directly or indirectly, the rejection of a considerable number of research grant applications submitted to U.S. Federal Agencies by distinguished U.S. and foreign scholars. In fact, the rejection of the mathematical applications was essentially based on the claim that the Lie-admissible algebras do not have physical relevance because the APS journals do not publish papers on the topic.The rejection of the physical applications was explicitly and repeatedly based on the statement that I do not publish papers in APS journals, and, as one referee put it, the only one I did publish in 1980 "was held back for more than a year before acceptance."

You know well that this Country's God is the "$". Each and every action at your journals has a direct or indirect financial implication. In our case, not only the words, but at times even the typewriters of your referees and those rejecting the I.B.R. grant applications are the same. The password in this latter case is: SUPPRESS, SUPPRESS, SUPPRESS the I.B.R. After all, we have received a truly impressive, massive rejection of applications (totaling over $ 5M over the next five years), in two instances even when the majority of the referees(the 2/3, to be exact) warmly suggested support.It is evident that a few academic barons will be pleased by the on-going assassination of the I.B.R. But, in reality, who will be the real loser? The answer is evident: America is the real loser. Also, where it started? It is evident: at your journals.

As repeatedly stated to you, my letter on the Lie-admissible treatment of open nuclear reactions was a Rubicon. This was the case for several reasons, substantially outside my control. The full year of hysterical reject, reject, reject by your office has forced the crossing of the river. Irreparable damage has now been done. Both you and me are left with nothing else than prepare for the consequences.

In the final part of your letter, you suggest that I should write a longer version of my paper in collaboration with S. Okubo or F.E.Low. Evidently, I would be honored to collaborate with any of them. However, the very mention of their names is a further indication of your lack of knowledge of the gravity of the decay of the U.S. physics community. For your information, in 1980 I wanted to spend a couple of months at Rochester to follow Prof. Okobo's lectures and learn from him (as well as, hopefully, to collaborate with him). My application was REJECTED by the department of physics at Rochester, as Okubo can testify, even though, as explicitly stated in the application, I was interested only in VISITING and the totality of the expenses would have been supported by my DOE grant. The cases occurred at M.I.T. are substantially more grave than this little dance of greed at Rochester. In fact, besides being at the basis of the very birth of the I.B.R., they touch aspects that are too delicate to be treated in this letter[you will hopefully read them one day].

The truth is that the U.S. physics community is slowly dying because of internal suffocation due to extremes of greed.Despite their substantial character, in number and quality, my experiences are nothing but an insignificant corner of putrescence. Multiply my esperiences many many times over. Think at cases such as the recent, public disqualification of Edward Teller in national televisions and newsmedia, and then you have an idea of the dimension of the problem.

Even though I acknowledge your effort (for which I am grateful), your letter contains absolutely no light, by therefore confirming the only alternative left to physicists concerned for the future of our children: GO PUBLIC, GO PUBLIC, GO PUBLIC.

Very Truly Yours

Ruggero M. Santilli

cc. Drs. Trigg , Nordstrom and Dreiss, PRL and PR , and The White House.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

May 25, 1983

Professor D. LAZARUS
Editor in Chief
American Physical Society
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I must express my continued, extreme reservations regarding the editorial-refereeing practices at your Journals. I enclose a self-explanatory letter to Drs. Marchidon, Antippa, and Everett, authors of a paper printed at Phys. Rev. D27, 1740 (1983). The paper essentially claims that "... the slighest extension" of Einstein's special relativity is "... in violent conflict with what is observed in nature."

It is unbelievable how papers of this inspiration can pass your seemingly severe refereeing. The fact is that the severity is applied only for topics non-aligned with vested academic-financial-ethnic interests, while topics that are aligned with said interests are passed with support despite enormous distorsions of the reality.

Everybody can see politics here, but the bad one. In fact, papers of this type, once regrettably printed in your journal, can kill the imagination in young minds at birth. But, is this exactly what desired by the ring of academic barons surrounding your journal? Suppress undesired advances at birth?

How can it be possible that a growing number of international observers see huge editorial problems at your journals (some even talk of "potential crime against humanity")., and you people see nothing?

Very Truly Yours

R.M.Santilli

cc. Phys. Rev. D

P.S. You should be informed that, as expected, my paper LR2111-LZ2206 on the irreversibility of open nuclear reactions has been accepted without modification by a European letter journal after less than three weeks of consideration (while the same paper was rejected for over one year at your journal with the total and absolute lack of any constructive criticism whatsoever by your barons). This is a further element confirming that the problems exist, specifically, here in the U.S. and, specifically, at your journals.

Dr. Lazarus,

In case your editors are willing to honor Professor Okubo recommendation (to publish my paper in Phys. Rev. rather than PRL), you can count on my best possible collaboration, including my excuses for all that has happened on the case.

However, to do so, I now need a formal letter from the editor of the journal considered appropriate. In fact, I have already submitted the paper to a European Editor of a letter journal. I can withdraw it only following a formal letter from your own editor.

I mentioned this possibility as the very last attempt to avoid a truly senseless situation for all of us. The final decision is yours.

Sincerely,

R.M.Santilli

Copy to Drs. Trigg, [illegible] and Dreiss

# PART XIII–F:

# REQUESTS OF

# RESIGNATION

# OF

# C. M. SOMMERFIELD

# AND

# R. K. ADAIR, AS

# EDITORS

# OF

# PHYS. REV. LETTERS

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES - 1 RESEARCH ROAD
BOX 1000 - RIDGE, NEW YORK 11961
Telephone (516) 924-5533

September 30, 1982

Dr. Ruggero Maria Santilli
The Institute for Basic Research
Harvard Ground
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

The dossier on your manuscript LR2111 on time asymmetry has been sent to me in my capacity as Associate Editor of Physical Review Letters. My task is to determine if the referees have properly performed their jobs in evaluating the paper. In the present case the referees, all of whom are well-known and respected physicists, have done just that. Thus I can find no grounds for reversing their unanimous recommendation that the manuscript not be published in the Letters.

Best regards.

Sincerely,

Charles M. Sommerfield

Charles M. Sommerfield
Divisional Associate Editor
Physical Review Letters

CMS/bsk

MAIL RECEIVED

OCT 6 1982

PHYS. REV.-P.R.L.

647 — Ref. Mat. for LR2111

# Time-reversal violation: new polarization measurements in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction

J. Pouliot, P. Bricault, J.G. Dufour[a], L. Potvin
C. Rioux[b], R. Roy, and R.J. Slobodrian

Laboratoire de Physique Nucléaire, Université Laval
Québec G1K 7P4, Canada

PACS numbers: 24.70.+s, 11.30.Er, 25.40.Jt, 25.60.Fb, 29.75.+x

## Abstract

New measurements of the proton polarization in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction at 14 MeV incident energy have been carried out with a setup in three different configurations based on proton polarimeters equipped with Si or C analyzers. Our results corroborate previous measurements which have shown significant differences between polarizations in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction and analyzing powers in the inverse reaction $^{11}B(\vec{p},{}^{3}He)^{9}Be$, implying violation of time-reversal invariance through the failure of the polarization-analyzing power theorem.

Keywords

[ NUCLEAR REACTIONS $^{9}Be(^{3}He,\vec{p})^{11}B$; $E = 13.6$ MeV; measured $P(\theta)$, $\theta(\text{lab}) = 40^{\circ}, 42^{\circ}, 44^{\circ}, 45^{\circ}, 50^{\circ}$. ]

NOTE OF JUNE 1, 1984: THIS IS THE FRONT PAGE ON THE EXPERIMENTAL ARTICLE ON TIME-ASYMMETRY UNDER CONSIDERATION BY PHYS. REV.C INADVERTENTLY ENCLOSED BY C.M.SOMMERFIELD IN HIS LETTER OF SEPTEMBER 30, 1982.

October 16, 1982

Dr. CHARLES M. SOMMERFIELD
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

CERTIFIED MAIL
RETURN RECEIPT REQUESTED

Dr. Sommerfield,

As a member of the American Physical Society, I am hereby requesting that

**you tender your resignation from your position of divisional associate editor of the Physical Review Letters,**

and terminate all your editorial functions at the Journals of the APS as soon as possible.

This request is the result of your unsolicited letter of September 30, 1982 (which reached me only on October 14, 1982) in which you misused your editorial position, you violated basic codes of our profession, and created doubts on the editorial processing which are damaging to the APS.

In fact, you passed judgement as a physicist on my paper LR2111 submitted to Physical Review Letters dealing with the vast field of non-Lagrangian/non–Hamiltonian, Newtonian, statistical, and particle dynamics in which you have no established record whatsoever of expertise. In addition, the contents of your letter indicates that you did not take the responsibility to become acquainted, even minimally, with this vast new field.

Episodes of this type generally admit the explanation that the editorial action is taken in the sole, intended, specific benefit of particular academic interests, or because of recommendations from members of the same group of academic interests, in disrespect of National interests for the pursuit of novel physical knowledge. In order to prevent even the remote possibility of shadows of this type on the editorial sector of the APS, you are hereby requested to resign.

You must be fully aware that this is a formal request of resignation and that, in case of its lack of due consideration, all necessary action will be implemented as vigorously as possible, as permitted by the codes of laws and of the APS, not to exclude individual and/or group action, in order to protect National interests as well as the image of the APS throughout the World.

Ruggero Maria Santilli

Ruggero Maria Santilli
Member of the American Physical Society
96 Prescott, Street, Cambridge, Massachusetts 02138

cc: Dr. D. LAZARUS, Editor in Chief, APS
Observers

**P.S. You should be made aware that, jointly with your letter of September 30, 1982 rejecting my paper LR2111 on a theoretical treatment of time–asymmetry, I received not one, but two copies (apparently because of a mailing mixup) of the recent paper by the Quebec experimental group submitted to PR–C which confirms the original measures of time–asymmetry, by theorefore providing a beautiful EXPERIMENTAL confirmation of my own paper.**

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES - 1 RESEARCH ROAD
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PHYSICAL REVIEW LETTERS
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Oct. 27, 1982

Prof. R.M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli;

In my capacity as Editor of Phys. Rev. Letters and Chairman of the Divisional Associate Editors, I am responding to your curious letter of Oct. 16 to Charles Sommerfield in his capacity as Divisional Associate Editor.

I am not writing to object to your request (?) that he resign. The first Amendment to the US Constitution gives you the absolute right to ask any one, President, Pope, or Editor, to resign. And President, Pope, or Editor can ignore you.

Instead, I am writing to correct some misapprehension you seem to harbor concerning the duties of an editor and the editorial process. Sommerfield's letter to you was <u>not</u> unsolicited. It was solicited by <u>you</u> in the act you took of submitting your paper for consideration by Phys. Rev. Letters. When you submit a paper to a journal you <u>solicit</u> editorial consideration and Sommerfield's letter to you was a part of that consideration process; a process described in some detail in the center-fold inserted in the first issue of the present volume of PRL. Moreover, you do not seem to understand that Sommerfield acted, as he should, not as a referee but as an editor. I would hope that it is obvious to you that we cannot, and never intend to, have a special editor expert in every conceivable subset of physics. I know that Charles is far from i gnorant of the areas of mechanics which exercise you, but his job is to judge the evidence from referees closer to the subject and not to judge the paper per se.

In your letter to David Lazarus, you speak of the possibility of submitting a revised version of your paper to Phys. Rev.

Letters. I must point out to you that your paper LR211 has been rejected and we will not consider again a paper which is quite similar to LR211.

Sincerely

RK Adair

Robert K. Adair

cc: G.L. Trigg
David Lazarus
Charles Sommerfield

CERTIFIED LETTER-RETURN RECEIPT REQUESTED

November 1, 1982

Dr. ROBERT K. ADAIR
Chairman, Divisional Associate Editors
Physical Review Letters
Department of Physics, Yale University
NEW HAVEN, Connecticut 06520

Dr. Adair,

It was instructively edifying to read in your letter of October 27, 1982 that you associate yourself and Dr. C. Sommerfield with popes and presidents.

I am under the impression that you understood absolutely nothing of the entire issue of my paper LR2111 submitted to Phys. Rev. Letters. However, the position that Yale University continues to give you presupposes you have the full mental capacities to understand the issue. In this latter case a more probable occurrence is that you simply mimic lack of understanding for the pursuance of objectives to be identified at the appropriate time.

As said countless times by now, PRL has the following two alternatives for paper LR211.

ALTERNATIVE I. Paper LR211 is rejected because of the clear identification of scientifically credible errors, inconsistencies, or incompatibilities presented in due scientific language. In this case you should expect nothing more than my respectful and graceful acceptance.

ALTERNATIVE II. PRL continues to reject the paper on the basis that the available referee reports are credible. In this case I shall oppose the decision in any conceivable way permitted by law, beginning with the filing of law suits to you and Dr. Sommerfield, first, as individuals, and second, as associate editors.

All my efforts have been devoted to the implementation of the best possible scientific process in this case, owing to the number of observers, and of international implications, in the best possible interest of the American Physical Society.

Your letter is a total,uncompromisable rejection of this orderly scientific process, on mere grounds that "the professor says so, and therefore it is so".

The action by you and your friend Dr. Sommerfield could be tolerated if it occurred in countries under totalitarial control, whether of political or ethnic color.It appears you forget that we are in the United States of America. If aspects of questionable conduct occurred within public offices are brought to the attention of the public at large, the persons involved are socially dead here, sooner or later. It is only a matter of time.

You associate yourself to presidents, but you forget President Nixon.

Your letter constitutes the second, completely unsolicited intervention in the case. As such it can only prove your personal, uncontrollable desire to prevent the publication of the paper, as well as to support your personal friend Dr. Sommerfield, in complete disrespect of the interests of the American Physical Society, as evidentiated by your presumptuous assumption that PRL will not consider again paper LR2111.

In addition, your letter constitutes the second, unsolicited attempt intended to falsify or otherwise annull specific agreements in regards to paper LR211 reached with Dr. Lazarus as Editor in Chief of Physical Reviews and Physical Review Letters.

In view of these and other circumstances, I am hereby requesting (sic) that you also resign from your editorial post at the Physical Review Letters, and terminate all your associations with the Journals of the American Physical Society.

Finally, I must take all possible precautions, in the interest of the American Physical Society, to truncate this insanity of unsolicited interventions in the orderly scientific process regarding paper LR2111, beginning with formal requests to the appropriate bodies to initiate investigative committees.

Ruggero Maria Santilli, Member of the American Physical Society
cc. Drs.A.B.GIAMATTI and F.W.K.FIRK, Yale University; Drs. D.LAZARUS,G. TRIGG, G.J.DREISS, and D. NORDSTROM, Phys. Rev. and Phys. Rev. Lett.; selected observers.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

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Nov. 12, 1982

Prof. R.M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli;

I am confident that I understand the issues involved in connection with my rejection of your paper LR2111. Clearly, you do not. Physical Review Letters does not select or reject papers according to your Alternatives (I and II). As is well known by physicists of the community, Phys. Rev. Letters operates under a mandate of the American Physical Society as a selective journal. From the set of papers submitted to the journal, a selection (of less than 50%) is accepted by the line-editors and myself for publication on the basis of our judgement that those papers will be of special interest to our general readership. That judgement, which is certainly somewhat subjective, is made after consultative procedures discussed in many PRL editorials and described in a center-fold included in the first issue of the current volume of the journal. The papers we do not accept are not, for the most part, rejected as being incorrect; they are not accepted because we editors do not feel that they fit the needs of the journal. I did not reject your paper because of any judgement by me that the paper was wrong: I rejected your paper because I decided that the objectives of the journal would be better served by other selections.

For better or worse, most scientific journals are selective journals where a portion of submitted papers are selected by the editors of the journal using whatever criteria they choose. Indeed, some journals — Science, for example — publish no more than 10% of submissions. The existence, and policies, of such journals have then a long tradition and the right of journals to publish material of their choice has a firm legal foundation in the First Amendment.

I can only presume from your curious remarks about "unsolicited intervention" by me, that you do not know that I hold the position of Editor of Physical Review Letters under appointment by the American Physical Society and am charged with the responsibility of final decision on journal editorial matters by the Society. Hence, the final responsibility for the acceptance or rejection of papers is mine and you may conclude that what disagreements you have with the Editors -- and Associate Editors -- are disagreements with me. Moreover, inasmuch as your letters to officers of the journal are business letters, those letters are my concern and it is my responsibility to respond to those letters as I choose. I assure you that upon termination of your correspondence with Phys. Rev. Letters, you will receive no more letters from me.

As for your "request" that I resign; after more than four years at this job I have asked to be relieved in the fullness of time but, for the moment, I have more work to do and must reluctantly reject that request.

Sincerely

RK Adair

Robert K. Adair

cc: D. Lazarus
G.L. Trigg
G.L. Wells

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

November 8, 1982

Professor DAVID LAZARUS
Editor in Chief
Physical Reviews and Physical Review Letters
Department of Physics
University of Illinois
URBANA, Illinois 61801

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

Dear Professor Lazarus,

Following a meeting of our Board of Governors, we hereby formally ask that you provide us with all pertinent information regarding the procedure for the initiation of investigations and/or investigative committees by the American Physical Society [or other appropriate institutional body] on possible improprieties by associate editors of your Journals acting either alone or as a possible conspiratory group, and that, in case you are unable to provide the information, you identify the appropriate officer of the APS for the securing of the information. In particular, we would appreciate the courtesy of a copy of the bylaws of the APS [or a reference to their publication] as well as of any other official document treating the procedure for the initiation of formal investigations. Please let us know all the expenses, and they will be promptly reimbursed to you.

We have informed Drs. Giamatti and Firk at Yale University of our best intention to permit a replacement of Drs. Sommerfield and Adair in their respective editorial posts at Physical Review Letters in a way as orderly as possible. Also, we have indicated that the situation at this moment can still be somewhat contained, by therefore permitting the replacement of Drs. Sommerfield and Adair, within reason, in the form preferred by them. The understanding is that formal action should be undertaken as soon as possible, owing to the history of rapid deteriorations of the case. We are referring, for instance, to a possible official announcement by the American Physical Society of the availability of openings of the positions currently held by Drs. Sommerfield and Adair, with a copy forwarded to our office, which would clearly halt all actions aiming at their resignation.

Regreattably, time is running out. You must understand that the action to have Drs. Sommerfield and Adair terminate all their editorial associations with your Journals will be relentless, continuous, and uncompromisable. A chain of actions toward the achievement of this objective are scheduled for implementation in a sequential and progressive way. This letter is only the very first step intended to identify the proper procedures within the context of the APS.

Also, you should be aware that the case of paper LR2111 can be brought at any moment now to the attention of the international press. To maintain the fundamental values of our democracy, it is therefore essential that you provide the information requested in this letter in a way as exhaustive as possible, and, within reason, as promptly as possible.

In regard to the status of paper LR2111, we would like to confirm that we disregard the unsolicited letters by Drs. Sommerfield and Adair, and consider as valid ONLY your letter of October 19, as Editor in Chief. This is clearly essential to contain possible investigations to Drs. Sommerfield and Adair, and to prevent an unnecessary implication of your Journals at large.

Finally, we would like to stress that the scientific processing of paper LR2111 should be considered as completely independent from individual, institutional, or class actions that might be initiated to have Drs. Sommerfield and Adair terminate their editorial functions at your Journals.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

cc: Drs. G. L. TRIGG, H. H. BARSCHALL, D. NORDSTROM, and G. J. DREISS,
Phys. Rev. and Phys. Rev. Letters
Drs. A. B. GIAMATTI and F. W. K. FIRK, Yale University
Selected Observers

P.S. As a gesture of personal courtesy and respect for your person and for your Office, I enclose an outline of my forthcoming Volume II of *Foundations of Theoretical Mechanics* with Springer—Verlag entitled *Birkhoffian Generalization of Hamiltonian Mechanics*. I should be in a position to mail you a complimentary copy within a few weeks.

This monograph reviews and somewhat expands a considerable number of independent contributions in mechanics, algebra and geometry, some of which dating back from the past century, intended for the treatment of closed systems of extended particles with action-at-a-distance, potential forces as well as contact interactions for which the notion of potential (Hamiltonian) has no physical (no mathematical) basis. The name of "Birkhoffian Mechanics" has been selected for the new mechanics for certain historical reasons presented in the text.

Needless to say, this monograph presents not only the classical but also most of the operator foundations of paper LR2111, as evident from even a superficial reading of the paper. As an example, the foundations of the isotopic, left and right, generalizations of Schrodinger's equation (which are at the basis of paper LR2111) are treated in detail beginning from the Birkhoffian generalization of the Hamilton—Jacobi theory, as you can see from the enclosed outline.

The putrescence of our community of basic research has reached such an apparent level, that Drs. Sommerfield and Adair did not even bother to ask for a courtesy preview of the monograph for their own personal curiosity, let alone as a fundamental ethical rule before venturing editorial judgments. In the final analysis, the monograph presents the only genuinely new mechanics built during their life—time. This is, of course, only a minute aspect of their apparent misconduits, which include: disregard of the experimental evidence favoring the time—asymmetry in open nuclear reactions; disrespect of statistical (by now historical) needs for a credible resolution of the problem of the origin of irreversibility; ignorance of the complete lack of any identified error in the paper; etc. All these and other aspects will be duly presented and documented in the applications for the initiation of investigations on the alleged misconduits to be presented to the APS as well as to other independent bodies.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

November 27, 1982

Dr. D. LAZARUS, Editor in Chief
Physical Review and Physical Review Letters
Department of Physics, University of Illinois
URBANA, Illinois 61801

Dear Dr. Lazarus,

I acknowledge receipt of your kind letter of November 24, 1982, as well as of an additional, unsolicited, unfriendly letter of Dr. Adair (Yale University) dated November 12, 1982.

Permit me to confirm, if at all needed, my sincere sentiments of respect and cooperation with your person. Your letter contains an adequate answer to our Institutional request of information of which I am grateful. This information has been passed to our Board of Governors as well as other members of our group for consideration. We are all aware of the difficulties for setting up investigative committees owing to the very regrettable fact that the APS does not subscribe to a code of ethics, as costumary for other societies (an occurrence which, alone, calls for proper reflection). Permit me only to disagree, quite gently, with your impression thatI have an "uncollegial" attitude . If you knew my academic life, you would agree that this is not the case. In fact, I have proved my tolerance in the past, even for excesses that would make you sick [1]. The mere fact that these episodes did not appear in the press is a proof of my collegial attitude. But the case of Drs. Sommerfield and Adair is too grave to be overlooked, or treated lightly. In fact, while preceding questionable experiences dealt with me alone, the case under consideration has clear elements of National interested which simply cannot be ignored.

As seen from our side, the situation of paper LR211 is quite straighforward, and constitutes no problem. In fact, the case was resolved during our friendly phone conversation of late September 1982. You will recall that, on my own initiative, and as a manifestation of my moderate attitude, I suggested a two-months pause in the case, also to give the opportunity to Phys. Rev. C to consider recent experimental studies supporting paper LR2111. We concluded that, depending on the circumstances, I may write a new paper for possible consideration by PRL. These lines were kindly confirmed in your letter of October 19, 1982. The case was therefore resolved along the best possible scientific lines of mutual respect and cooperation.

I subsequently asked for the resignation of Dr. Sommerfield because he wrote an unsolicited letter subsequent to our agreements, and in apparent disrespect of a number of questionable aspects, not to mention the complete lack of usefulness under the circumstances. I subsequently asked for the additional resignation of Dr. Adair because of the nature of his additional, unsolicited intervention in favor of his friend and colleague at Yale, Dr. Sommerfield.

I am under the impression that you underestimate the gravity of the unsolicited statements by Drs. Sommerfield and Adair, as well as the gravity of the continuation of their unsolicited interventions. As a first example, please read again the second unsolicited letter of Dr. Adair of November 12, 1982 (of which you should have a copy). Besides open encouragement for the "termination of your [mine] correspondence with Phys. Rev. Letters" and other aspects, the letter constitutes an implicit confirmation that the rejection of the paper was based on political, rather than scientific reasons. But paper LR2111 deals with possible basic advances [isotopic liftings of the Hilbert space with consequential possible generalization of quantum mechanics for strong interactions] which, as such, should be of PRIMARY interest to PRL. On the other side, the topic of the paper is manifestly nonaligned with existing, vested, academic interests. The most logical explanation suggested by Dr.

Adair's letter (lacking evidence to the contrary) is that rejection was based in the apparent intent of protecting existing, vested academic interests, in disrespect of the pursuit of novel physical knowledge. By keeping in mind that the case of paper LR2111 is not expected to be an isolated one, Drs. Adair's letter confirms that the problem of editorial practices at Phys. Rev. Letters may have reached the dimension of a potential threath to National interests.

The ultimate issue you should address to yourself, as Editor in Chief, is whether shadows of such gravity should be dismissed simply because claimed to be untrue, or they should be dispelled as a result of an extensive, detailed, and comprehensive examination by a number of appropriate, independent, committees. After all, the shadows are not new.

But we are still at the very beginning of the case. In his first unsolicited letter of October 27, 1982, Dr. Adair had the courage of stating (among other things)

> Phys. Rev. Letters..."will not consider a paper which is quite similar to LR2111."

This clearly implies the dishonoring of specific agreements reached with you as Editor in Chief, as well as the exclusion from the future consideration of the totality of efforts currently going on in experimental-theoretical-mathematical circles for the construction of the hadronic mechanics, exactly as I had feared in the first place in my original contacts with you. This is evident from the fact that the time evolution studied in paper LR2111 is at the foundation of ALL these efforts, now summing up to over 10,000 pages of research (virtually none of which published in your Journals), as well as the participation of a number of governments.

AS A RESULT, I HAVE INITIATED, I SHALL CONTINUE, AND, IN DUE TIME, I SHALL MULTIPLY ALL POSSIBLE OR OTHERWISE CONCEIVABLE EFFORTS PERMITTED BY LAW TO HAVE DRS. SOMMERFIELD AND ADAIR TERMINATE ALL THEIR EDITORIAL FUNCTIONS AT YOUR JOURNALS. I hope you understand that, despite my best and most moderate attitude, <u>I HAVE NO OTHER ALTERNATIVE</u>. IN fact, the only alternative permitted by Drs. Sommerfield and Adair is that your Journals should be excluded from the ongoing scientific efforts to generalize quantum mechanics for the strong interactions, and this could likely imply a possible future incident of huge proportions. I beg you to see the situation also from our viewpoint. You will agree that, under the indicated antiscientific-antinational attitudes, it is better to promote a containable crisis now, than a potentially explosive, international scandal tomorrow.

Permit me to reassure you that I do have my own doubts on this admittedly depressive scenario. Nevertheless, a history of complementary episodes accumulated through the years appear to confirm, rather than dispel, the scenario (2). I sincerely wish this was not the case, and I would rejoice in case proved wrong by concrete evidence.

But this is still the beginning of the case. There are additional reasons for the actions I am considering, which are substantially more distressing, because they might imply an escalation of the crisis of unthinkable proportions. I indicated to you other times, and I confirm it here, that it is in the best interest of our community that I am silent on these additional aspects at this time.

What is important here is that you understand the potential damage that may be produced by the continuation of the unsolicited interventions by Drs. Adair and Sommerfield to other quite valuable Editors of Phys. Rev. Letters and Phys. Rev. I am referring to Editors such as Drs. Trigg, Nordstrom, and Dreiss (to mention only a few) whose integrity is beyond any shadow of doubt, as proved by a long history of independence from scientific interests (contrary to the history of association to current scientific interests by Drs. Sommerfield and Adair). I beg you to take all the necessary action so that no damage whatsoever is suffered by Drs. Trigg, Nordstrom, Dreiss, and so many other valuable physicists serving your Journals. Needless to say, you can count on my best possible assistance in this respect.

This is THE LAST LETTER I shall write to you on the matter. The two months paus- of our agreement are about to expire, and a number of decisions must now be taken. I therefore believe that it is important for all that the situation (as seen from our side) is spelled out as clearly as possible. Under the current circumstances, created by the unsolicited letters and the acceptance of the validity of their statement, our only possibilities are the following.

(A) Drs. Sommerfield and Adair resign in writing, with a clear indication of the date of termination of all their editorial functions at your Journals. You can rest assured that, in this case, no action whatsoever will be initiated on my part, other than the continuation of an orderly conduction of research. The understanding is that I shall monitor the election of possible new editors [3] and that I am not a candidate.

(B) Drs. Sommerfield and Adair do not resign, but I receive substantial evidence that they shall be totally severed by all conceivable future considerations at PRL of papers on the hadronic mechanics. In this case you can rest assured that my action shall be as moderate as possible. Jointly, you must understand that certain actions, such as the promotion of a number of investigations on the case, "must" be undertaken because National interests must go beyond personal interests, whether mine or yours.

(C) Drs. Sommerfield and Adair remain in their current editorial posts and continue to participate in the consideration of papers dealing with the hadronic mechanics. Then, you should be certain that a comprehensive effort will be launched aiming at the promotion of all the necessary consideration of the problem of ethics in physics, beginning with a national campaign aimed at the need that the American Physical Society formulates, adopts, and inforces a code of ethics.

In case you see any other possibility, besides those listed above, providing solid evidence of due scientific process at Phys. Rev. Letters, please let me know (even by phone). You can count on my best possible collaboration. The only point I beg you to understand is that time is running out fast.

Sincerely Yours

Ruggero M. Santilli
Member of the American Physical Society

cc.:Drs.TRIGG, DREISS and NORDSTROM, PRL and PR
Drs. GIAMATTI and FIRK, Yale University
Dr. P. W. Anderson, Princeton University
Selected observers

[1] This statement calls for the indication of at least the following episode, from which numerous others followed. In the morning of September 1, 1977 I initiated a visit at Lyman Laboratory of Harvard University as "honorary research fellow". In the afternoon of the same day my supervisor Prof. Giorgi received a phone call from Washington amounting to an invitation for me to apply for a governmental research grant. The application was subsequently filed (with my affiliation at Lyman), and immediately funded. I discovered at that time that I could not draw a salary because of the honorary character of my appointment, according to Harvard statute. I therefore respectfully applied for the removal of the word "honorary" in my title, so that I could draw a salary. Several MONTHS passed without any action on my request. And in fact, a solution was finally reached only the SUBSEQUENT MONTH OF JUNE 1978, via my appointment as research associate at the Department of Mathematics at Harvard. To understand truly the case, you must understand that at that time I had a family of four to support, including two children of tender age, and my wife then a graduate student. The prohibition for me to receive a salary, which was notoriously due to Coleman-Glashow-Weinberg, therefore resulted in severe hardship in my children. In fact, I had no other income; all my savings evaporated after the first months,

and my unemployement benefits (I drew from Newton Corner, Ma) expired in early 1978. Thus, to truly understand the case, you must be in substantial need of money to feed and house your children, while a considerable amount of federal support is sitting in a bank, including your salary, and you are prohibited to draw it by colleagues! I leave it to you to judge your fellows Coleman-Glashow-Weinberg. Here I want only to indicate my "collegial" attitude. In fact, my first volume of "Foundations of Theoretical Mechanics" with Springer-Verlag, written at Lyman under these insane human conditions, carries a gentle and thankful acknowledgement to people at Lyman, as you can see from the enclosed copies. BUT I BEG YOU NOT TO DRAW ERRONEOUS CONCLUSIONS. THIS BEHAVIOUR OF EUROPEAN KINDNESS ON MY PART IS LONG GONE. NOW I ATTACK AT THE FIRST SIGN OF MISCONDUIT.

[2] This statement also calls for an additional example. You should be aware that the Department of Physics of Yale University, to which both Dr. Sommerfield and Dr. Adair belong, has built a considerable reputation of OPPOSING the investigations we are here talking about in between the lines [insufficiency of Einstein's special relativity for strong interactions, as made conceivable by extended charge distributions in conditions of mutual penetration], to the point of apparently suppressing the exposure of young minds at Yale to the Hadronic Journal and other conduits struggling in the search of light in this magnificent problem. I sincerely hope that this information is wrong. Yet, the Administrative Office of the Hadronic Journal confirms that Yale's libraries have received for years all necessary information on the Journal, and no subscription was ever solicited. Also, it is clear that Yale did not passed the subscription to the Hadronic Journal because of financial problems. The most plausible explanation is therefore that rumored around, that is, of political nature, much similar to that surrounding paper LR2111. Again, I sincerely wish that this information is proved to be wrong by clear evidence. Copy of a recent letter of the Administration of the Hadronic Journal to the people at Yale is enclosed for your perusal, because it may give you an idea that I am not alone in my doubts.

[3] Owing to occurrence [2] it is evident that possible editorial replacements should not originate at Yale. In fact, this would likely result in a MULTIPLICATION OF TROUBLES.

# PART XIII–G:

# COPIES OF THE FRONT PAGES OF THE THEORETICAL AND EXPERIMENTAL PAPERS ON TIME–ASYMMETRY REJECTED BY THE A.P.S. JOURNALS AND PUBLISHED ELSEWHERE

PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH NUMBER DE–TP–82–9

# USE OF THE HADRONIC MECHANICS FOR THE BEST FIT OF THE TIME–ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL.

Ruggero Maria Santilli *
The Institute for Basic Research
Harvard Grounds,
96 Prescott Street
Cambridge, Massachusetts 02138

FIRST DRAFT

IBR reception date: April 14, 1982

Abstract

Strong nuclear interactions are assumed to have a non–Hamiltonian component due to contacts among the extended nucleons, which is represented via the hadronic generalization of the atomic mechanics currently under study by a number of authors. The theory is used for the description of the recent experimental discovery by Slobodrian, Conzett, et al that the strong nuclear interactions violate the time–reversal symmetry. The fit of the experimental data provided by the hadronic mechanics is remarkable, and nonrealizable via the use of the atomic mechanics.

* Supported by the U.S.Department of Energy under Contract Number DE–AC02–80ER10651.A001

LR2111/L-1

1.

USE OF THE HADRONIC MECHANICS FOR THE FIT OF THE TIME—ASYMMETRY RECENTLY MEASURED BY SLOBODRIAN, CONZETT, ET AL.

Ruggero Maria Santilli *

*The Institute for Basic Research, 96 Prescott Street, Cambridge, Massachusetts 02138*

new RRL 5-7-82 recd 5-28-82 new 9-13-82

(RECEIVED 19 APRIL 1982 )

*It is shown that the hadronic generalization of the atomic mechanics currently under study by a number of researchers, can produce a fit of the time—asymmetry under strong nuclear interactions by Slobodrian, Conzett, et al., that does not appear to be possible via theories conceived for the electromagnetic interactions.*

SECOND IMPROVED VERSION

A series of experiments conducted over a number of years by Slobodrian, Conzett, et al.[1—3], has produced evidence of the violation of the time—reversal symmetry under strong nuclear interactions (here referred to as "time—asymmetry"). These results were predicted by Dirac in 1949[4], and their roots can be traced back to the birth of the equivalence between space and time, in the sense that the experimentally established space—asymmetry in nuclear physics[5] should occur jointly with a time—asymmetry.

It is evident that results[1—3], if confirmed by future experiments, will provide a resolution of the historical problem of the origin of irreversibility. This aspect was studies in detail at the recent Orléans International Conference[6]. Particular emphasis was put on the existence of rather serious problematic aspects in *quantitative* studies attempting a reconciliation between the *experimentally established* macroscopic irreversibility, and the *currently conjectural* reversibility of particle dynamics, or between the noncanonical character of the time evolution of Newtonian systems [as needed to avoid approximations of the type of the perpetual motion], and the conjectured unitary character of the evolution of the miscroscopic constituents. As shown in detail by Tellez—Arenas[7], these (and other) problematic aspects can be apparently resolved if one assumes the rather natural hypothesis that the macroscopic irreversibility and noncanonicity see their origin in contact/non-Hamiltonian forces among *extended* constituents, whether particles, atoms, or molecules.

These ideas have promoted the construction of two, interrelated, new disciplines that are becoming known under the names of "Birkhoffian mechanics" and "hadronic mechanics". The former is a (classical) generalization of the conventional Hamiltonian mechanics for the local treatment of nonpotential systems, which is the result of a considerable number of contributions in mechanics, algebra, and geometry[8]. The latter is a generalization of the "atomic mechanics" (the ordinary QM) currently under study for the representation of hadrons as extended particles, with consequential contact/non-Hamiltonian (and non-Lagrangian) interactions besides the conventional ones[6—10]. Both new mechanics are made possible by recent studies by mathematicians on generalized formulations of Lie's theory called of Lie—isotopic and of Lie—admissible type [see in ref.[9] the papers by G.M.Benkart, D.J.Britten, Y.Ilamed, M.Kôiv, J. Lôhmus, H.C.Myung, R.H. Oehmke, S.Okubo, J.MOsborn, A.A.Sagle, L.Sorgsepp, M.L.Tomber, G.P.Wene, et al.]. In fact, the Birkhoffian and hadronic mechanics are realizations of the generalized Lie theory via functions on a contangent bundle and operators on a Hilbert space, respectively, with consequential rather remarkable unity of thought.

In this note I shall use the axioms and dynamical equations of the hadronic mechanics as for—

* Supported by the Department of Energy under contract number DE-AC02-80ER10651.A002.

THIRD IMPROVED VERSION

# A POSSIBLE TIME–ASYMMETRIC MODEL FOR OPEN NUCLEAR REACTIONS

Ruggero Maria Santilli*

The Institute for Basic Research, 96 Prescott Street, Cambridge, Massachusetts 02138

Submitted to Physical Review Letters on December 14, 1982

*We show that an isotopic lifting of the Hilbert space implies a time—asymmetry for open nuclear reactions, while recovering time-reversal invariance for center-of-mass trajectories of the implementation of the systems into a closed form. The conceptual, mathematical, and experimental plausibilities of the model are indicated.*

Without doubt, the origin of the time—asymmetry of our macroscopic world constitutes one of the most intriguing (and fundamental) open problems of contemporary physics.

At the *Newtonian level,* the situation is sufficiently (yet incompletely) understood. Consider our Earth as seen from an outside observer. Its center-of-mass trajectory is manifestly time—symmetric. Nevertheless, interior, open (nonconservative) systems are manifestly time—asymmetric. Particularly important for this note is the fact that the time—asymmetry results to be ultimately due to the *non-Hamiltonian* character of the forces, and to the consequential, *non-canonical* nature of the time evolution, as established, say, by a satellite during re-entry. Besides conventional, closed Hamiltonian systems (e.g. the planetary and atomic systems), nature clearly exhibits more general systems of closed non-Hamiltonian type, i.e., systems verifying conventional conservation laws of total quantities, yet the internal forces are outside the capabilities of Hamiltonian mechanics. This novel situation has stimulated the construction of the so-called Birkhoffian[1] generalization of Hamiltonian mechanics[2] for the exterior closed treatment, and of the complementary Birkhoff—admissible mechanics[3] for the interior open case.

At the *statistical level,* fundamental advances in the non-Hamiltonian origin of irreversibility have been made by Prigogine[4] and his group for both classical and quantum mechanical statistical ensembles. Further advances have been made by Fronteau, Tellez-Arenas, Salmon, Guiasu, Grmela, et al, this time for the non-Hamiltonian origin of irreversibility at the level of each individual constituent of a statistical ensemble, as reported at the recent Orléans International Conference[5]. The unity of thought of these statistical studies with the Newtonian profile is remarkable. In fact, the Birkhoffian mechanics is a rather natural analytic counterpart of Prigogine's statistics for closed systems, while the Birkhoff—admissible mechanics is the analytic basis of the statistics advocated by Fronteau et al for open systems, with the understanding that a deeper unity of mathematical structure exists[2,3,6].

At the *particle level,* the situation is fundamentally unresolved to this writing. A primary objective of this note is that of stressing the need for a systematic consideration of all plausible views on the problem, owing to its relevance. In fact, as it has been the case at the Newtonian and at the statistical level, irreversibility may imply a revision of the foundations of particle dynamics, with implications ranging from controlled fusion to solid state physics, as well as to other branches of sciences, such as theoretical biology.

Considerable difficulties have been recently identified for the compatibility between conventional Hamiltonian/unitary time evolutions of particles and the established irreversibility of the physical world[5]. Some of these difficulties are due to the manifest problematic aspects of any quantitative attempt to achieve the established *non-canonical* time evolution of the Newtonian systems of our environment via a large collection of *unitary* time evolutions for its constituents. Other difficulties are of statistical/thermodynamical nature.

LETTERE AL NUOVO CIMENTO VOL. 37, N. 9 2 Luglio 1983

FINAL PUBLICATION

# A Possible, Lie-Admissible, Time-Asymmetric Model for Open Nuclear Reactions.

R. M. SANTILLI (*)

*The Institute for Basic Research* - *96 Prescott Street, Cambridge, Mass.* 02138, *U.S.A.*

(ricevuto il 20 Aprile 1983; manoscritto revisionato ricevuto il 9 Maggio 1983)

PACS. 11.30. – Symmetry and conservation laws.

*Summary.* – We show that an isotopic lifting of the Hilbert space implies a time-asymmetry for open nuclear reactions, while recovering the time-reversal invariance for center-of-mass trajectories of the implementation of the system into a closed form. The conceptual, mathematical, and experimental plausibilities of the model are indicated.

Without doubt, the origin of the time asymmetry of our macroscopic world constitutes one of the most intriguing (and fundamental) open problems of contemporary physics.

At the *Newtonian level*, the situation is sufficiently (yet incompletely) understood. Consider our Earth as seen from an outside observer. Its center-of-mass trajectory is manifestly time symmetric. Nevertheless, interior, open (nonconservative) systems are manifestly time asymmetric. Particularly important for this note is the fact that the time asymmetry results to be ultimately due to the *non-Hamiltonian* character of the forces, and to the consequential, *noncanonical* nature of the time evolution, as established, say, by a satellite during re-entry. Besides conventional, closed Hamiltonian systems (*e.g.* the planetary and atomic systems), Nature clearly exhibits more general systems of closed non-Hamiltonian type, *i.e.* systems verifying conventional conservation laws of total quantities, yet the internal forces are outside the capabilities of Hamiltonian mechanics. This novel situation has stimulated the construction of the so-called Birkhoffian (1) generalization of Hamiltonian mechanics (2) for the exterior closed treatment, and of the complementary Birkhoff-admissible mechanics (3) for the interior open case.

(*) Supported by the U.S. Department of Energy under contract no. DE-AC02-80ER10651.A002.

(1) G. D. BIRKHOFF: *Dynamical Systems*, Amer. Math. Soc. Providence, R.I. (1927).

(2) R. M. SANTILLI: *Foundations of Theoretical Mechanics*, Vol. II: *Birkhoffian Generalization of Hamiltonian Mechanics* (New York, N.Y. and Heidelberg, 1982).

(3) R. M. SANTILLI: *Lie-Admissible Approach to the Hadronic Structure*, Vol. II: *Covering of the Galilei and Einstein Relativities?* (Mass., 1982).

# Time-reversal violation: new polarization measurements in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction

J. Pouliot, P. Bricault, J.G. Dufour[(a)], L. Potvin
C. Rioux[(b)], R. Roy, and R.J. Slobodrian

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PACS numbers: 24.70.+s, 11.30.Er, 25.40.Jt, 25.60.Fb, 29.75.+x

Abstract

New measurements of the proton polarization in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction at 14 MeV incident energy have been carried out with a setup in three different configurations based on proton polarimeters equipped with Si or C analyzers. Our results corroborate previous measurements which have shown significant differences between polarizations in the $^{9}Be(^{3}He,\vec{p})^{11}B$ reaction and analyzing powers in the inverse reaction $^{11}B(\vec{p},{}^{3}He)^{9}Be$, implying violation of time-reversal invariance through the failure of the polarization-analyzing power theorem.

Keywords

[ NUCLEAR REACTIONS $^{9}Be(^{3}He,\vec{p})^{11}B$; $E = 13.6$ MeV; measured $P(\theta)$, $\theta(lab) = 40^{\circ}, 42^{\circ}, 44^{\circ}, 45^{\circ}, 50^{\circ}$. ]

Nuclear Physics A394 (1983) 428–444


# ASYMÉTRIE DU TEMPS: POLARISATION ET POUVOIR D'ANALYSE DANS LES RÉACTIONS NUCLÉAIRES

C. RIOUX[†], R. ROY et R. J. SLOBODRIAN

*Laboratoire de physique nucléaire, Département de physique, Université Laval, Québec G1K 7 P4, Canada*

et

H. E. CONZETT

*Lawrence Berkeley Laboratory, University of California, Berkeley, CA 94720, USA*

Received 30 July 1982

**Abstract:** Measurements of the proton polarization in the reactions $^7$Li($^3$He, $\vec{p}$)$^9$Be and $^9$Be($^3$He, $\vec{p}$)$^{11}$B and of the analyzing powers of the inverse reactions, initiated by polarized protons at the same c.m. energies, show significant differences which imply the failure of the polarization-analyzing-power theorem and, *prima facie*, of time-reversal invariance in these reactions. The reaction $^2$H($^3$He, $\vec{p}$)$^4$He and its inverse have also been investigated and show some smaller differences. A discussion of the instrumental asymmetries is presented.

E | NUCLEAR REACTIONS $^2$H, $^7$Li, $^9$Be($^3$He, p), 14 MeV; measured polarization. $^4$He(polarized p, $^3$He), $E = 28.88, 29.77, 30.40$ MeV; $^9$Be(polarized p, $^3$He), $E = 23.06$ MeV; $^{11}$B(polarized p, $^3$He), $E = 22$–$23$ MeV; measured $A(\theta)$. Natural, enriched targets.

## 1. Introduction

La découverte en 1964 de la violation de la symétrie CP lors de la désintégration du méson-K neutre [1]) a relancé l'intérêt pour la vérification de l'invariance sous renversement du temps (T). Cette violation de CP implique une violation équivalente de T afin de conserver le théorème CPT [2]) dont l'importance et les fortes évidences expérimentales de validité [3]) sont difficilement discutables.

Dans le cadre de la physique nucléaire, deux moyens ont été principalement retenus pour vérifier T; ce sont la balance détaillée et le théorème de polarisation-pouvoir d'analyse [4]). Ces deux voies ont en commun le principe d'invariance sous renversement du temps comme condition nécessaire et suffisante à leur démonstration

[†] Ce travail fait partie des exigences pour l'obtention du Ph.D.; adresse présente: Lawrence Berkeley Laboratory, Bldg. 88, Berkeley, CA 94720, USA.

# PART XIV:

# YALE

# UNIVERSITY

Yale University *New Haven, Connecticut 06520*

PHYSICS DEPARTMENT
*217 Prospect Street*

May 15, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of May 7, 1979 and the copies of the papers by yourself and Ktorides, Myung and yourself. Please accept my apology for not having answered your earlier letter.

The questions you raise are certainly fundamental ones and will undoubtedly be with us for many years. I do have a few comments on your paper which are given below.

1. I have looked at Kim's paper (Lett. Nuovo Cimento 12, 591 (1975)) which incidentally deals with the muon lifetime and hence is probably not very strongly linked with hadronic interactions. The experiment which Kim suggests, however, could, in my opinion, be done well enough (at FNAL or the CERN SPS) to see the effect he calculates for a fundamental length of $\sim 5 \times 10^{-16}$ cm. It would be a major effort comparable to a "standard" high energy physics experiment at these laboratories.

2. I am enclosing a paper which will appear shortly in Physical Review Letters which reports a test of special relativity via a high $\gamma$ g-2 measurement. I understand that the same PRL issue will have an article by the CERN g-2 group on the same subject. In one sense these are very "sensitive" tests in that they go to very large values of $\gamma$ ($\sim 10^4$). However, I believe there is, at this time, no generally accepted calculation linking an hypothesized fundamental length and the size of any violation of the relativistic prediction of spin rotations. Of course the g-2 value of the electron, like the muon lifetime test of Kim, has little direct connection with hadronic interactions.

3. In hadronic interactions, various groups have tested the forward dispersion relations which are traditionally derived from causality, unitarity, and the crossing relations. My own group is completing such an experiment with $\pi^+$, $\pi^-$, $k^+$, $k^-$, p, $\bar{p}$ scattering on protons with energies from 70 to 200 GeV. No violations have as yet been observed but again there seems to be, at present, no theoretical framework to translate the experimental results into limits on the validity of special relativity.

Dr. Ruggero Maria Santilli page 2 May 15, 1979

4. One can contemplate experiments which measure the possible deviation of hadronic particle lifetimes from the relativistic predictions. For those hadrons, e.g. $\pi$'s, or k's, which decay via the weak interaction, one can expect, with some effort, to achieve accuracies in relative lifetimes (at two energies) perhaps as good as 0.1%. Would these be interesting? For the hadrons which decay via strong (or even electromagnetic) interactions one would have to measure the energy width of the state and relative accuracies better than 10% sound difficult, to me at least.

As you know, most of the recent tests of special relativity have been carried out as a kind of "fallout" from experiments which were designed primarily for other purposes. I do not myself know of any plan to do a major experiment, primarily designed to test relativity. I believe the reason for this is twofold. First, relativity has worked so well whenever it has been tested that enthusiasm to test it again is naturally not large. Secondly, there is no alternate theory which is comparable in scope or self consistency which can be used to determine what constitutes an "interesting" level of sensitivity in testing relativity. The ideas of Kin and Redei do set some limits but their theory is necessarily phenomenological and is not really an alternate to relativity. It is more a way of parameterizing an hypothetical breakdown of special relativity.

I hope these few remarks will be interesting to you. Incidentally, if you would like to know more about the high $\gamma$ g-z experiment you might correspond with Professor Peter S. Cooper here at Yale.

Sincerely,

Jack Sandweiss

Jack Sandweiss
Professor of Physics

JS/ja

Enclosure:

HARVARD UNIVERSITY

AREA CODE 617
495-3352

RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 16, 1979

Professor JACK SANDWEISS
Physics Department
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Sandweiss,

I would like to express my appreciation for the courtesy of your letter of May 15, 1979.

Your kind comments will be invaluable, not only for my own research, but also for my conduction of the HADRONIC JOURNAL.

Again, I am not an experimentalist and, as such, I do not have sufficient knowledge to assess the situation. Nevertheless, the following comments might be of some value for the theoretical profile.

I am in full agreement with your general assessement that the questions under consideration will remain with us for some time. In defense of the experimentalists I would like to add that what is still missing is sufficient maturity for the treatment, at the theoretical level, of the possible invalidity of the special relativity. This is, after all, the reason why I have suggested a joint effort by theoretists and experimenters. In this way, experimenters may acquire awareness on the theoretical needs, while jointly providing the theoretists with a better identification of their function.

On more specific grounds, the following comments might be of some value.

1. Kim's original proposal of 1975, as stated, does not appear to be truly relevant to hadron physics because, as you correctly point out, it is related to the muons. I am a firm believer of the special relativity for the electromagnetic interactions and, thus, I do not see much need to test it again in this arena. Nevertheless, the proposal has been subsequently elaborated and extended to the light mesons (see Kim's article in the HJ 1, 1343 (1978)). This profile appears to be different, and constitutes the formulation of the proposal in which a number of theoretists are interested in. More specifically, the issue of experimentally detecting the existence or lack of existence of a fundamental length, oddly, is considered of secondary nature on theoretical grounds. What is considered of primary physical relevance is the possibility of gaining some experimental information on the true fundamental problem, whether

page 2.

the strong interactions are local or nonlocal. In other words, it is the mechanics of the proposal by Kim which has attracted interest. If the proposal could be sufficiently modified and elaborated (of, course, for the case of the light mesons) up to the necessary maturity, it could be one way to resolve the problem of the nature of the strong interactions. I assume you are aware that if the nonlocal nature of the strong interactions can be experimentally established, the invalidation of the special relativity within a hadron is consequential (as outlined in my recent article you received, as well as in the monograph specifically devoted to this subject, ref. 14a). Another aspect of Kim's proposal which has also attracted attention is the possible link of the already experimentally established violations of discrete symmetries with nonlocal strong hadronic forces. The question then raised by Kim's proposal is whether the available experimental data on violation of discrete symmetries could be reinspected to ascertain whether such nonlocal nature is admitted or not. According to Kim's view (presented in the HJ) the violation of discrete symmetries would be nothing else that the "tip of the iceberg", that is, they are a manifestation of the violation of the entire Poincare symmetry at the structure level, and not only its discrete part. I should add that, on theoretical grounds, an unorthodox, "heretical" (so to say) view is implicit in this issue. I am here referring to insights on strong interactions via "weak" processes. The unorthodox view is that the term "weak interactions" will have only a limited life in physics. The weak decays of light mesons are seen as an expression of the structure of these particles (because they are spontaneous). As such, these decays are seen as possessing vital informations on the nature of the strong hadronic forces. To summarize, Kim's proposal has a number of intriguing aspects from theoretical profiles. First, there is the possibility whether the measures on time lifes of light mesons can be experimentally linked to the nonlocal nature of the strong hadronic forces. Even partial results would be invaluable, that is, the experimental finalization that, even though these nonlocal forces cannot be established, at the same time they cannot be ruled out either. Second, there is the issue whether the same objective can be achieved via a simple reinspection of available data of violations of discrete symmetries (without even doing a new experiment at this time).

2. Thank you for sending me copy of the forthcoming article in the PRL on the test of the special relativity via g-2 measurements. I have inspected the article and find it excellent indeed. Nevertheless, I see no connection at all with the issues under consideration. Indeed, the test refers to the typical arena of unequivocal applicability of the special relativity, the electromagnetic interactions.

3. The experiments your group is conducting (via scattering of hadrons on protons) are indeed quite relevant for the issues under consideration. You might be interested to know, however, that these are

page 3.

precisely the experiments under controversy. You might be interested to know in more details the reason of this controversy (only alluded in p. 87 of my recent paper). I beg you not to consider these remarks as offensive. My only desire is to inform you of dissident views. The major criticism is that experiments of this nature do not have a final experimental character (they are called "conjectural experiments" or "quasi-experiments" by extremists). The reason is that experiments of this nature are heavily based on theoretical models. Furthermore, these theoretical models, such as causality, unitarity etc. are all based on conventional local formulations which do not take into account the extended character of the particles. That is, these theoretical models undoubtedly possess physical value, but such a value is only a first, crude, approximation for an expected, subsequent advancement. The criticism then goes by saying that the final data are a mere reflection of these theoretical approximations. In other words, the expectation is that, by using a more adequate representation of the strong interactions one might, in principle, reach fundamentally different data by using exactly the same experimental set up.
I do not know the theoretical methods you use in these experiments. In case you are interested to a more detailed and technical presentation of these criticisms, please let me know in more detail the specific theoretical formulas you use for the elaboration of the data (e.g., which type of cross section and on what theory it is based,etc.). In any case, a job of identifying the incontrovertible aspect of these experiments and the impact of theoretical models in the data computation, appears advisable, also to prevent expansions of current controversy (p. 87 of my paper).
In defense of experimentalists I would like to stress that alternative theoretical models which could be comparatively used jointly with conventional models in data elaborations, are simply lacking at this moment. More specifically, I am not aware of any theoretical study at this moment which computes the cross section under local nonselfadjoint strong interactions (as an approximation of nonlocal settings). This is expected to be a feasible job, e.g., by expanding conventional quantum mechanical techniques for generalized Schrödinger's equations of type (4.6) of my paper. The point is that this job has not been done by theoretists at this very moment, although studies of this type are expected to be done soon.
In conclusion, what may be of some value for your group is the awareness that a number of researchers are working on the generalization of the theoretical models which are expectedly used in your experiments. If these generalizations will actually materialize, they potentially imply a fundamentally different elaboration of data.
My research interest is now precisely in these issues. In essence, after having reached a rudimentary generalization of Galilei's relativity in classical and "quantum" mechanics for local nonselfadjoint strong forces, I am interested in the implications for aspects, such as causality, unitarity, etc. At this moment, I simply see no way to even partially salvage conventional treatments under the condition that

page 4.

the particles are extended in size and under interaction with a necessary state of penetration of the wave packets (to activate the strong interactions). For instance, while microcausality appears to me unequivocal for these particles under long range electromagnetic interactions (for which the point-like approximation is excellent - sec. 3 of my paper), I am unable to even consistently define the same microcausality under the broader conditions considered above. At this moment, a generalization appears to be essential for consistency in the mere formulation. A similar situation occurs for other topics.
After working for a number of years on these issues I have therefore reached the rather distressing (but scientifically stimulating) conclusion that the virtual totality of contemporary theoretical physics is inapplicable to strongly interacting particles when represented as extended objects of dimension equal to the range of the strong interactions. These are Contentions 1 and 3 of my paper.

4. The questions you raise in this point are, in my view, scientifically invaluable. They relate to the extension of Kim's proposal to light mesons (point 1). I believe that a paper on the study of the feasibility of these data with current technonogy would be invaluable. Please consider the possibility that some of your associates conduct a study of this nature. In case the HADRONIC JOURNAL is considered for publication, you can rest assured that studies of this type would have utmost priority.

As concluding comments, I would like to agree with you on your assessement of the fascinating effectiveness of the special relativity until now. Yet, it appears that unequivocal evidence is available only for the electromagnetic interactions. In any case, the use of the same relativity for the strong has not preserved the physical effectiveness, resulting in the by now vexing state of affairs of quarks reported in my paper.

I also agree with you that what is much needed is an alternative (or broader) relativity, specifically conceived for extended particles in a state of penetration of their charge volumes (or wave packets). You might be interested to know that a rather feverish research activity is going on to study the generalization of Galilei's relativity I have recently proposed via the Lie-admissible algebras (HJ 1, 223 and 574). A number of mathematicians and physicists are involved (directly or indirectly) in these studies in the USA and in Europe.

The reason for this interest, as it appears to me, is the possibility of this broader relativity of allowing the interpretation of the constituents of light mesons as being produced free in the spontaneous decays. This possibility is strictly precluded by conventional laws based on point-like particles. It is centered on a more general notion of intrinsic quantities (spin, charge, etc.) which is apparently characterized in a rather direct way by a covering Lie-admissible relativity (e.g., Eqs. (4.34) and (4.37) of my recent paper).

page 5.

In conclusion, the reason why a number of physists are interested in experiments to test the special relativity under strong interactions is that a possible invalidity would allow a resolution of the fundamental problem of the hadronic constituents.

If I can be of any assistance, please do not hesitate to contact me. Again, permit me to express my appreciation for your consideration, interest, and time.

Sincerely

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/ml

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

October 21, 1982

Professor A. B. GIAMATTI
President
Yale University
NEW HAVEN, Connecticut 06520

Dear Professor Giamatti,

A series of regrettable circumstances has forced me to request on October 16, that Dr. CHARLES M. SOMMERFIELD, a member of your department of physics, tenders his resignation from his position of divisional associate editor of the Physical Review Letters, and terminates all his editorial associations with the Journals of the American Physical Society. Copy of my letter requesting the resignation is enclosed, jointly with copies of two recent letters dated October 12, and 16, to Professor D. LAZARUS, Editor in Chief of the Journals of the APS. In case you desire additional information on Dr. Sommerfield's side, please feel free to contact Professor Lazarus at the address of the letters enclosed. In case you desire, for completeness, additional information of the other side, I would be happy to provide you on request with copy of the complete (rather voluminous) file on the case.

This letter is intended for the specific purpose of reassuring you of my best possible predisposition to protect the interests of YALE UNIVERSITY, and to prevent that the personal decision by Dr. Sommerfield is detrimental to your campus. For this purpose, I feel obliged to indicate as candidly and firmly as possible that the action to have Dr. Sommerfield leave the Journals of the APS will be relentless, progressive, and uncompromisable.

At this moment the situation is fully contained. As a result, we are now in a position to permit the replacement of Dr. Sommerfield in a way as smooth as possible, and, within reason, in the way preferred by your faculty member. However, delays and/or resistances, will force an escalation of the situation with the public disclosure of a number of aspects of the current scene in physics which can only be detrimental to all, let alone Yale University. To prevent this unnecessary deterioration, **it is essential that a copy of the letter of resignation by Dr. Sommerfield reaches my desk as soon as possible.** As leader of Yale University, I thought you should have the opportunity to know.

I expect you will agree with me that academic politics has affected the acquisition of novel human knowledge since immemorable times. I do not know whether you are aware of the fact that, recently, the problem has reached such a dimension to constitute a real threat to National interests. This is due to the nature, dimension, and organization of the efforts to suppress the acquisition of novel physical knowledge which is against vested, academic—financial—ethnic interests. The mere birth of our new institute in the hearth of Cambridge's academic community (of which I have been a member for some time) with the participation of so many distinguished scholars is tangible proof of the impossibility to conduct our research at existing institutions in the city, despite the availability of governmental support, because of documented interferences by academicians in administrative control (which have reached at times unbelievable extremes of misconduit). The request of resignation of Dr. Sommerfield is only one case of a rather considerable effort under way by a number of concerned scientists to improve the scientific ethics, as a necessary condition for our survival.

Lack of action would be equivalent to the supine acceptance of the down spiral of this beautiful Country because of excesses in academic greed. This, I cannot accept silently, at whatever personal price: I want to look at my children with clear eyes.

Very truly yours,

R. M. Santilli

Ruggero M. Santilli

RMS/mlw

Enclosures

cc: Dr. Professor F. W. K. FIRK, Chairman, Department of Physics
Yale University

[redacted]

Professor F.K.W.FINK, Chairman
Department of Physics
Yale University
NEW HAVEN, Connecticut 06520

October 25, 1982

Dear Professor Fink,

Yale University is renowned for the completeness of its libraries, with particular reference to its vast subscriptions to technical Journals in physics and mathematics. Yet, your university does not subscribe to the HADRONIC JOURNAL, despite the fact that our Journal has now entered the sixth year of regular and successful publication, and that it is now an established vehicle of research with a fast growing number of subscribers all over the world. It is evident that your physics library IS NOT COMPLETE without the Hadronic Journal.

Every year since 1978 we have mailed to your department, as well as to general libraries at Yale University, information about our Journal. As you know, our Journal is the forerunner in the promotion of experimental, theoretical, and mathematical knowdledge on the rather fundamental physical problem whether the [extended] charge distribution of hadrons is perfectly rigid under strong interactions, or it experiences small deformations. In this latter case, we would have departures from the exact character of the rotational symmetry, with far reaching implications, not only for basic research at large, but also for important aspects of National interests, such as the impact on controlled fusion. In turn, implications of this nature, once matched with the plausibility of the deformations, render the study of the problem simply mandatory, particularly when the use of public funds is involved, with consequential ethical needs for scientific accountability.

It is public knowledge that your physicists are continuing to publish articles with the <u>tacit</u> assumption of the perfectly rigid charge distribution of hadrons [i.e., of the exact rotational symmetry], and are continuing to use public funds along these lines, despite the now established conjectural character of the basic assumptions.

It has been brought to our attention that Yale University has not subscribed to the HADRONIC JOURNAL until now apparently because of the opposition by individual faculty members at your department, rather than because of financial difficulties.

If this is the case, permit me to bring to your attention the fact that such an occurrence:

page 2.

[1] would imply the suppression of valuable scientific information at your campus in the interests of a minoritarian group;

[2] would infringe on the rights of library users at large, with particular reference to graduate students and researchers; and, last but not least,

[3] would raise the possibility of discrimination of research at Yale University under governmental support.

We enclose for your information a list of articles published in all volumes of the HADRONIC JOURNAL until 1978, as well as front pages and table of contents of international workshops and conferences which are part of the Journal's scientific activities. We hope you can see in this way the number of distinguished scientists who have contributed to our Journal, as well as the number of governments who are supporting nowadays the experimental verification of conventional physical laws under strong interactions.

If we can be of any assistance, please do not hesitate to let us know.

Very Truly Yours

[redacted]C.

cc.: [redacted]

Professor A.B.GIAMATTI, President, Yale University.

# PART XV:

# ANNALS

# OF

# PHYSICS

"Santilli has performed a real service in reviving beautiful old ideas and extending them to field theories. Such scholarly virtue is rare these days, and is very important."

REFEREE, Annals of Physics .

for the series of papers on the Inverse Problem in Field Theories published in Volumes 103 and 105 (1977).

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

October 20, 1977

Professor A. M. JAFFE, Editor,
ANNALS OF PHYSICS
Harvard University

Dear Professor Jaffe,

I hereby submit for publication in Annals of Physics my papers entitled

(1) Isotopic breaking of gauge symmetry,
(2) Need of subjecting the validity of Einstein's special relativity within a hadron to an experimental verification,
(3) Need of subjecting the validity of Pauli's exclusion principle within a hadron to an experimental verification,
(4) Possible applicability within a hadron of Lie-admissible coverings of established disciplines,
(5) Possible identification of the hadronic constituents with the electrons under the assumption of Lie-admissible covering disciplines.

Two copies of each paper are enclosed.

I would consider it a personal courtesy if a decision can be reached as soon as posssible. I have worked at these papers several years, as you can see, and I am now in need of a speedy identification of their publisher.

Prior to this submission, I have submitted the material for review to a number of collegues as well as presented it this summer at European departments. I am here taking the liberty of including copy of a review by Professor A. Shimony (now at the University of Geneva) in case could be of some assistance.

I remain at your disposal for any additional material you might need.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Office No. 495 3212
Home No. 969 3465

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

November 8, 1977

Professor HERMAN FESHBACH
Chief Editor
Annals of Physics
MIT, Cambridge, Ma 02139

Dear Herman,

As you eventually know, I have submitted to Annals of Physics, via Professor R. Jaffe, the five enclosed notes. I am here taking the liberty of indicating the background motivations for this submittion.

As you remember from my reports while visiting your Department, since the time of my graduate studies in Torino I have been interested in the old idea (e.g., Enrico Fermi) that strong interactions are local but not derivable from a potential (as an approximation of an expected nonlocal setting) with particular reference with the problem of the hadronic structure.

At my arrival at MIT in January of 1976 I initiated the laborious task of reaching the necessary maturity of presentation of the essential results of my solitary journey which lasted for over a decade.

This resulted in a series of nine papers (I mailed you their abstracts some time ago) on the following five sequential steps.

(1) The delicate, but in my opinion necessary study on a possible nonapplicability of the Galilei and Einstein relativities for the assumed nature of the hadronic forces. The methodology I used for this step is that of the Inverse Problem I had studied from 1973 until recently, with particular reference to my papers in Annals of Physics (although not disclosed in my previous publications, this is a reason why I have spent so much of my time on the Inverse Problem).

(2) The equally delicate but in my opinion also necessary study of the possible existence of coverings of the Galilei and Einstein relativities for the considered type of hadronic force. The methodology I used for this study is that of the Lie-admissible problem I have been involved in since 1965.

(3) Study of the extension of the methodologies of the Inverse Problem and of the Lie-admissible problem to (generally nonintegrable) subsidiary constraints which appearsto be needed to recover the experimentally proved validity of established relativities for the behaviour of the hadrons as a whole under electromagnetic interactions. The methodology I used for this step is essentially a physicist's version of the Problem of Bolza of the CV.

(4) Study of the quantization of the methods of the Inverse Problem and of the Lie-admissible problem with a conceptual emphasis focused on the assumed hadronic structure. For this step the Lie-admissible algebras turned out to be, without any doubt, the most interesting research topic I have been involved in.

(5) construction of one explicit model of hadronic structure and confrontation of the predictions with the available experimental data. As you know from my previous reports to you, my central objective is that of attempting a conceivable but explicit identification of the hadronic constituents with physical particles and of the consequential remouval of the problem of confinement.

Predictably, I went through (truly) many redraftings of these papers. In spring of 1977, even though still far from final maturity, I had reached a stage which allowed me to submit the papers for condidential review to few collegues. In this way I kept improving the presentation. The reaction on the latest versions by collegues with a genuine scientific vision as well as mature capability of selfcritical examination of the currest status of our knowledge has been beyond my best expectation. To give you an indication, I enclose on a confidential basis copy of a review of these nine papers by Professor A. Shimony, now at the University of Geneva (please feel free to contact him if you so desire).

I then spent the subsequent summer to deliver a series of invited talks on these papers in Europe (Instituut voor Theoretische Mechanica of Gent, the Institüt of Theoretische Physik of Zürich, and the departments of Physics of Trieste, Naples and Lecce). The encouragements I received everywhere (please feel free to contact the Heads of the indicated departments) have been also beyond my best expectation. In any case this gave me the opportunity of many hours of direct confrontations with experts on differentiated topics. On my return to the States in August I felt to have reached sufficient maturity for submission.

However, I decided not to submit to Annals of Physics this series of nine papers because of their length (over 900 pages). Publication by other Journals must be excluded because the cost will exceed $ 25,000. A major reduction of the technical arguments had also to be excluded for the simple reason that the methods I use are simply unknown in contemporary theoretical physics. An excessive reduction in their presentation would then inevitably result in misrepresentations.

As a result of this situation, I decided to submit to Annals of Physics five condensed papers (for an anticipated total of less than 30 printed pages) for the, for me, essential need of securing the papernity of the main ideas through journal publication. Joiñtly, I submitted the series of papers for publication as one or more monographs.

I am pleased to report that these monographs have been accepted for publication by Hadronic Press (a new publisher for fast distribution of original monographs in basis research) under the title "Lie-admissible approach to the hadronic structure", Volumes I, II and III. Their appearance will be advertised at the time of appearance of my five notes. The material is now under editorial finalization. I am also pleased to report that my monographs on the Inverse Problem have been formally accepted for publication by Springer-Verlag under the title "Foundations of Theoretical Physics", Volumes I, II and III.

Sincerely

Ruggero

Ruggero Maria Santilli

c.c.: Professors R. Jaffe and
R. Jackiw

Professor H. Feshbach,
Editor in Chief,
Annals of Physics, MIT
Cambridge, Ma 02138

Dec. 9, 1977

Dear Herman,

I would like to confirm our phone conversation of December 2, 1977 following the decision by the Board of Editors of Annals of Physics to hear a second referee on my five brief notes submitted on October 20, 1977.

The submitted notes have been written to be conceptually understandable by an experimentalist. Neverthless, they are technically ununderstandable to the best educated tearetician. In my opinion, this is an indication of their novelty. The methods which I have developed for these studies are simply new. No physicist can technically understand my papers unless he studies in all details: (a) my series of papers on the Inverse Problem in Annals of Physics, (b) my series of papers in several journals as well as books on the Lie-admissible problem, and (c) the rather vast body of literature quoted in these papers. Lacking this knowledge, it would be the same as pretending that a physicist can technically understand, say, the Thomas-Fermi model without any knowledge whatsoever of quantum mechanics. I should stress that all these references are duly quoted in the submitted papers and that my two series of forthcoming monographs (those on the Inverse Problem with Springer-Verlag and those on the Lie-admissible problem with Hadronic Press, Inc.), which are also quoted, are intended to provide a presentation of my techniques understandable by a first year graduate student.

I am at the disposal of the Editorial Board of Annals of Physics to provide any editorial, technical or linguistic improvement which is considered advisable and valuable. The submitted material, in my opinion, should be presented toghether because, if presented in subsequent stages or in different journals, could create misrepresentations. It is my understanding that it is immaterial for Annals of Physics whether the material is presented in one single paper or in five short papers, as you indicated me in our phone conversation of December 2, 1977. Copies of my monographs on the Inverse Problem are filed at MIT and additional copies were given to you in March 1977. A copy of my monographs on the Lie-admissible problem has been mailed to you by Hadronic Press, Inc. Additional copies are at your disposal for the intent of providing all possible evaluational material which is needed by Annals of Physics.

There is one aspect on which we should communicate candidly. The submitted papers are not of the typically minute incremental nature of which all of us are submerged. Instead, they touch some truly fundamental problems of hadronic physics which are unresolved on both theoretical and experimental grounds. More insidiously, the papers can represent a potential danger to the financial interests which have been constructe over the years by the U.S. governmental-academic complex on the idea of quark as the constituent of hadronic matter. I sincerely hope that a decision is taken by Annals of Physics on scientific grounds alone, and that a possible rejection is fully motivatc on unequivocal technical grounds.

Sincerely

Ruggero Santilli

Ruggero Maria Santilli

c.c.: Professor A. Jaffe and R. Jackiw.

# ANNALS OF PHYSICS



May 22, 1978

Ruggero Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Ruggero:

We have now received our referees' reports on your papers. They are negative and we have therefore decided not to publish your work.

Sincerely yours,

Herman

Herman Feshbach
Editor

Arthur, please let me know whether you are interested in a complimentary

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 4, 1978

Professor HERMAN FESHBACH
Editor
Annals of Physics
MIT
Cambridge, Massachusetts 02139

Dear Herman,

I acknowledge receipt of your letter of May 22, 1978 indicating the rejection of my papers submitted on October 20, 1977 calling for an experimental verification of the basic laws within a hadron.

I understand your decision and you can rest assured that I respect it in full. As you know, the submitted papers were truly rudimentary. I am now publishing a series of technical papers on this topic. However, the complete technical presentation is that of my monographs in print with Springer-Verlag and Hadronic Press. You might be interested to know that the reaction by numerous collegues on this call for experiments is truly encouraging.

I would appreciate whether you can release to me technical criticisms on my papers, if any. I am sure you realize that, besides being common practice in editorial matters, this would be a scientific service. You can rest assured that I do not intend to present my countercriticisms, nor I intend to submit another paper to Annals of Physics on this fundamental problem of hadron physics. I am simply eager to know technical criticisms on my studies of the problem so that I can take them in due account.

More specifically, I am interested in critical comments on the central issue: whether an experimental verification of established relativity and quantum mechanical laws for the hadronic constituents is needed or not. Since the papers have been rejected, I assume that your referee has expressed his personal negative opinion. Has he presented a technical argument supporting such personal opinion? or has he quoted papers in which the validity of the laws considered within the arena considered is resolved in the needed unequivocal way (all available papers on unitary structure models of hadrons generally assume in a tacit form the validity)?

Nowadays, besides me, a number of physicists are working on the topic and rather intriguing papers are expected. Please let me know whether you are interested in being informed on a personal basis. On my part, I would be happy to keep you informed of the most relevant steps.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS|ls

c.c.: Professors A. Jaffe and R. Jackiw

# ANNALS OF PHYSICS

MIT Rm. 6-214



June 14, 1978

Ruggero Maria Santilli
Department of Physics
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Ruggero:

We had your paper reviewed by two referees. In regard to possible modifications you might want to introduce in order to publish it elsewhere, I think only the second review would be useful. I therefore enclose that review.

Sincerely yours,
Herman Feshbach / jak
Herman Feshbach
Editor

Enclosure

I have studied the three papers by R. Santilli (as well as the two papers which were originally submitted but withdrawn). These papers deal with topics of interest and one can see the beginning of original ideas in them. But none of the papers are good enough to warrant publication as they stand. They look like author's notes for lectures, rather than scientific papers.

My suggestion is as follows: 1) The author should combine the first three papers into one single paper. 2) He should leave out all hints, allusions and conjectures but instead state the aim of the paper clearly. 3) He should deal with classical discrete systems, quantum discrete systems, classical field themes, etc. in separate sections. 4) Spend more effort in the writing of the paper. 5) If possible get someone to help in proof reading and editing.

I am sorry that the review has been delayed but I dislike making negative decisions. But with the best intentions I cannot recommend publication of the present manuscripts.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

April 15, 1980

Professor HERMAN FESHBACH
Editor
ANNALS OF PHYSICS
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Herman,

As a gesture of courtesy, I am enclosing some material related to an editorial impasse (intended as a temporary suspension of judgment) recently occurred at the HADRONIC JOURNAL.

It consists of the inability to accept for publication at this time a considerable number of papers in several applications of nonrelativistic quantum mechanics with generalized Hamiltonians (conventional Hamiltonians, say, of elm type are excluded).

I believe that this occurrence may interest ANNALS OF PHYSICS, and it would be a pleasure for me to provide any needed additional information. Actually, at the HADRONIC JOURNAL we have opened a special file on this intriguing case which is at the disposal of qualified referees of other Journals.

Needless to say, any contribution by you or by the friends of ANNALS OF PHYSICS which might help in resolving this impasse either for or against publication, would be sincerely welcomed.

It was a pleasure to see you briefly the past week.

Best Personal Regards

Ruggero

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/ml
encls.

c.c.: Professors B.T.FELD, A.M.JAFFE, R.W.JACKIW and R. WILSON,
Assistant Editors of ANNALS OF PHYSICS
Professors J. BARDEEN, J.D.BJORKEN, L.D.FADEEV, P.G.DE GENNES, J.L.GREENSTEIN, S.HANNA, V.HUGHES, P.C.MARTIN, B.MOTTLESON, C.K.N.PATEL J.PEOPLES, J.SWINGER, I.I.SHAPIRO, I.TALMI, G.H.WILKINSON, AND A. ZICHICHI, Members of the Editorial Council of ANNALS OF PHYSICS

# PART XVI:

# NUCLEAR

# PHYSICS

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

July12, 1983

Professor Ruggero Maria Santilli, President

To the Editors of Nuclear Physics B, PARTICLE PHYSICS
NORDITA,Blegdamsvej 17
DK-2100 COPENHAGEN, DENMARK

Dear Sirs/Madams

I here respectfully submit for publication in your Journal, the enclosed manuscripts entitled

LIE-ISOTOPIC LIFTINGS OF LIE SYMMETRIES,
I: GENERAL CONSIDEEATIONS and
II: LIFTING OF ROTATIONS

Two copies of the papers are enclosed (one with editorial markings). The papers have not been submitted to other Journals, nor they will be submitted during your consideration. In case of publication, all copyrights are hereby assigned to North-Holland Publishing Company.

The following additional elements might be of some usefulness in the consideration. This submission regards the first two papers of the series. In case of acceptance, I would like to submit the subsequent papers of this series also to your Journal. Paper III entitled Lifting of the Lorentz Symmetry, is close to completion. A summary of paper III has been published in Lettere Nuovo Cimento. Copy of the letter is enclosed for the convenience of the referee. A possible paper IV currently under preparation, deals with quantization and additional applications.

I have selected your Journal because this series is particularly written for articles already published by you. In fact, Paper III will be particularly devoted to the elaboration of the studies

H.B.Nielsen and I Picek, Nuclear Physics B211, 269 (1982)

In actuality, the entire series might be viewed as an effort to identify the relativity underlying the metric used by Nielsen and Picek for the fit of the current data on the mean life of pions and kaons as well as other aspects. Paper I presents the general background; Paper II treats the space-subcase of the metric, while Paper III treats the complete space-time case.

An additional paper closely related to the series is

C. Rioux, et al Nuclear Physics A394, 428 (1983)

on the measures of violation of time-reversal symmetry in certain nuclear reactions. In fact, Paper IV is specifically intended to provide a fit of the experimental data by Rioux et al published in your Journal, as well as to indicate the apparent relationship between the work by Nielsen and Picek on the Lorentz-asymmetry, and those by Rioux et al on the time-asymmetry.

In case you are interested in scholars familiar with the (rather specialized) work of the papers, I might indicate the following.

Professor W. BEIGLBOCK, Institut für Angewandte Mathematik
Univesität Heidelberg, Im Neuenheimer 5, D-6900 HEIDELBERG 1, West Germany

[Professor Beiglbock is the Editor of Springer-Verlag that was in charge of my Volumes I and II of "Foundations of Theoretical Mechanics"; Vol. II in particular constitutes the foundation of the papers]

Professor R. MIGNANI, Istituto di Fisica, Universita' degli Studi La Sapienza, Piazzale Aldo Moro, I-00185 ROME, Italy

[Professor Mignani is a leading expert in the techniques of the papers called Lie-isotopies, particularly from a physical viewpoint]

Professor G. EDER, Atominstitut, Schuettelstrasse 115, A-1020 WIEN, Austria

[Prof. Eder, Director of the Theor. Phys. Div. of the Atominstitut, is a leading expert in the application of the generalized theory of rotations to nuclear physics, with particular reference to the interpretation of the origin of anomalous magnetic moments and precessions].

A list of additional experts is at your disposal on request, including a list of mathematicians on the Lie-isotopic theory.

Thanking you for your consideration, I remain

Yours Very Truly

Ruggero M. Santilli

RMS-mlw
encls.

P.S. I shall remain here at the I.B.R. until August 8. Thereafter, I shall be traveling in Europe, to be back here in early September.

# NUCLEAR PHYSICS

JOURNAL DEVOTED TO THE EXPERIMENTAL AND THEORETICAL STUDY OF THE FUNDAMENTAL CONSTITUENTS OF MATTER AND THEIR INTERACTIONS

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Professor R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138
USA


01-65-

23 September 1983

*Lie-isotopic liftings ... general considerations (Ref. 7275)*

*Lie isotopic liftings ... lifting of rotations (Ref. 7276)*

Dear Professor Santilli,

The above papers have been reviewed by the referee, whose report is herewith enclosed.

In view of this, we regret that they cannot be accepted for publication in Nuclear Physics B.

Yours sincerely,

K. Jensen

for The Editors

enc.

KJ/kam

REFEREE'S REPORT

Author: R.M.Santilli

Titles: Lie-isotopic liftings of Lie symmetries,
I. General considerations
II. Lifting of rotations .

---

In these papers the author hopes to exploit the mathematical notion of isotopy, i.e. the fact that the product operation in an associative algebra, $a,b \in \mathcal{A} \longrightarrow ab \in \mathcal{A}$ , can be replaced by the operation $a,b \in \mathcal{A} \rightarrow agb \equiv a*b$ , ($g$ fixed $\in \mathcal{A}$) without disturbing the basic axioms of the algebra. This allows one to regard a Lie algebra of matrices as a member of an isotopy class with general Lie bracket defined by the commutator $x*y - y*x$ . In the second paper, the author seeks to apply these ideas to describe deformations of the Euclidean metric: at each point of space the matrix used to define the operation * is nothing but the metric itself.

I do not recommend publication of these papers in Nuclear Physics for the following reasons:

a) The ratio of mathematical formalism to physically interesting results is too high; it is more typical of a journal of applied mathematics .

b) The physical interpretation of the formalism is not satisfactory. The author's concept of metric, given in I I,eq. (2.10), does not coincide with the standard terminology of differential geometry, where the metric defines a bilinear form on the tangent space at each point of a manifold, rather than the very general non-linear function of the coordinates defined by (2.10). Because it is tied to a preferred origin of coordinates, I doubt that this quantity will play any essential role in the physics of deformable bodies, inhomogeneous, anisotropic media, etc. The use of an analogous expression in relativity theory (see "Lie-isotopic lifting of the special relativity....") seems equally unpromising.

Note also: the discussion of the rotation group is not quite correct (but easily corrected). Eq.(2.2a), with (2.4) is not the Euler-angle decomposition of an arbitrary rotation. The quantity (2.2b) is not the inverse of (2.2a) in general (wrong order of factors). Eq. (2.12b) applies, I assume, only to one-dimensional subgroups.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

October 11, 1983

To the Editors of
NUCLEAR PHYSICS
c/o Nordita
Blegdamsvej 17
2100 COPENHAGEN, Denmark

RE: "Lie—isotopic liftings of
Lie symmetries, I and II"
ref.s numbers 7275 and 7276

Dear Colleagues,

Permit me to express, most respectfully but most firmly, my disappointment for the lack of scientific content of your referee report, as well as for his/her apparent lack of expertise in the field of the papers.

The papers were rejected on grounds of the fact that the metric used is "very general" and it is not restricted, in the referee's viewpoint, to the definition of a metric on the tangent space. First, this is untrue. In fact, the dependence of the metric on velocities is explicitly indicated in the papers. Second, the restriction suggested by the referee, if implemented, would prohibit one of the primary objectives of the Lie—isotopy, the incorporation of gravitation. Third, the Lie—isotopic theory must be formulated for the most general possible metric, and definitively not for one of its possible versions.

Admittedly, the paper could be improved with the indication that metric can be referred to its version of the contemporary differential geometry, although the lack of need of specific restrictions on the metric for the general formulation of the Lie—isotopic theory should be jointly indicated. But, as one can see, this is a manifestly secondary point.

Most of all, my disappointment originates from the statement that the Lie—isotopic generalization of Lie theory and of Lie symmetries is "unpromising". The referee and the Editors of NUCLEAR PHYSICS are not apparently aware of the fact that:

— The Lie—isotopic theory has already produced a GENERALIZATION OF CLASSICAL HAMILTONIAN MECHANICS, called Birkhoffian Mechanics for certain historical reasons;
— The Lie—isotopic theory is also at the foundation of the so—called "hadronic mechanics", a possible generalization of quantum mechanics for extended, deformable hadrons;
— Furthermore, the Lie—isotopic theory is at the foundation of a number of additional advances, such as a generalization of GAlilei's relativity in Newtonian mechanics for closed systems of extended particles with internal, non—Hamiltonian, contact forces; a generalization of non—Abenian gauge symmetry; and others.

How can a physicist claim that this is "unpromising"? A sample of informative material is enclosed for the Editors perusal.

Owing to the above (and other) aspects, I am respectfully asking that the refereeing conducted on papers 7275 and 7276 be ignored, and additional, independent referees be identified.

More particular, I am recommending an depth refereeing by EXPERTS in the field, that is, scientists with at least some record of publication in Lie—isotopy (or its more general version of Lie—admissibility). I am also recommending two independent refereeings, one by mathematicians on the mathematical structure of the Lie—isotopic symmetries, and one by physicists on the applications to particles physics, especially to nuclear physics. A list of experts is enclosed in case of any value.

If such new refereeing cannot be done and the rejection is final, please let me know as soon as possible, so that I can submit the papers elsewhere.

Very truly yours,

Ruggero M. Santilli

RMS/mlw

Enclosures

NUCLEAR PHYSICS

JOURNAL DEVOTED TO THE EXPERIMENTAL AND THEORETICAL STUDY OF THE FUNDAMENTAL CONSTITUENTS OF MATTER AND THEIR INTERACTIONS

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Professor R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138
USA

28 November 1983

*Lie isotopic ... I: general considerations (Ref. 7275)*

*Lie isotopic ... II: lifting of rotations (Ref. 7276)*

Dear Professor Santilli,

Thank you for your letter and enclosures of 11 October 1983 concerning the above papers.

The referees are top experts in their field and are chosen by the editors of the journal.

I have also examined the file and agree with the recommendation of the referee, that the material presented is not well-suited for publication in Nuclear Physics B.

I regret having to make this decision final.

The material is being returned to you under separate cover.

Yours sincerely,

K. Jones

for H.R. Rubinstein
Supervisory Editor
Nuclear Physics B

KJ/kam

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

February 9, 1984

Professor H. R. RUBINSTEIN, Supervising Editor
Nuclear Physics B
NORDITA, Copenhagen, Denmark

RE: Rejection of papers # 7275 and 7276
via letter dated November 28, 1983

Dear Professor Rubinstein,

Regrettably, I feel obliged to clarify the following points.

Point 1. The papers, at the time of the submission, were definitely not mature for publication, nor they were ever intended to be. In fact, I knew of a number of errors and imperfections, and several others have been subsequently brought to my attention by colleagues. The papers were submitted with the specific intent of soliciting constructively critical comments by your referees and editors, so that the subsequent, expected, rewritings would have been patterned along the lines recommended by your Journal.

Point 2. Your editorial consideration of the papers consisted of a total, absolute and complete lack of constructive scientific process. What you and your associates have said is simply this: The papers are rejected. Period.

Point 3. You are well familiar with the contents of the papers. It must be reviewed here. The papers dealt with a vexing open problem of nuclear physics, whose lack of proper consideration is creating a considerable lack of scientific accountability for all of us, including you and your associates, vis-a-vis the taxpayer. I am referring to the fact that nucleons, once admitted as extended charge distributions, are expected to experience a deformation of their charge distribution under sufficiently intense external fields, with consequential, manifest breaking of the symmetry under the group of (conventional) rotations, and a number of other consequences, such as the alteration of the magnetic moments. In turn, the resolution of fundamental aspects of this type is expected to be useful if not essential for a number of aspects relevant for society at large, such as the controlled fusion [how people can continue to spend public funds in attempting controlled fusion via magnetic confinement if they do not resolve first the problem whether or not the intrinsic magnetic moments of nucleons change during the physical conditions of the controlled fusion?].

The papers submitted identified the problem of the deformation of the charge distribution of particles, submitted a general theory for the construction of the covering symmetries whenever the conventional ones are broken, and (paper II) constructed explictly the generalization of the rotational symmetry for deformed spheres. The specific applications to nucleons were indicated as forthcoming in the subsequent papers in my correspondence with your editorial office, beginning with my original letter of submission.

Point 4. Because of the above, the papers were conceived for and are manifestly well within the objectives of your journal, at least those officially stated.

Point 5. Whenever rejections of papers dealing with fundamentsl open problems occur via the total absence of constructively critical comments, as you and your associates have done, this inevitably implies the existence of underlying politics.

The issue opened by your letter is therefore the following:

WHICH ARE THE UNDERGROUND POLITICAL REASONS THAT HAVE FORCED YOU AND YOUR ASSOCIATES TO SUPPRESS ANY SUGGESTION FOR THE POSSIBLE IMPROVEMENT OF THE PAPERS AND FORMULATE A TERMINAL, TOTALLY UNMOTIVATED REJECTION?

The asnwer that I consider most probable is the following. The possibility that nucleons experience an alteration of their magnetic moments when under nuclear forces was fully identified in the early stages of the theory and limpidly presented in books in nuclear physics of this early period, such as those by Blatt-Weiskopff and by Segre. Subsequently, the hypothesis remained without consideration and passed to the current stage of silence in most of the contemporary literature [including papers in Nuclear Physics], except a few isolated instances. [such as papers in the Hadronic Journal].

The reasons for the suppression of consideration of the hypothesis, despite its manifest plausibility and known implications, have been identified and are now well known. They are a manifestation of political-ethnic-academic interests due to the fact that, when the hypothesis is studied in any quantitative amount, it implies a violation of Einstein's special relativity, trivially, via the intermediate breaking of the rotational symmetry due to the deformation of shape.

Of course I do not have proof, but I suspect that the reason why you have implemented the suppression of any scientific process regarding the consideration of papers # 7275/7276 is due to an apparent opposition by you, your associates and your referees, against the conduction of quantitative studies on the limitations of Einstein's special relativity and on its generalization.

You should not forget that, as stated in the papers themselves, the subsequent paper III deals exactly with the isotopic lifting of the special relativity for nucleons experiencing alteration of their intrinsic characteristics, that is, deviations from an exact verification of the special relativity because of sufficiently intense, short range, external fields. This is the paper you intended to prevent to appear in your journal, as a prima facie interpretation of your behaviour!

Very Truly Yours

Ruggero Maria Santilli

RMS-mlw
cc. Professor K. JONES, Editor, Nuclear Physics

P.S. Papers # 7275/7276 are no longer available for your Journal. Their essential contents has now appeared in Lettere NC and other journals. The papers themselves have been completely rewritten twice thanks to a true scientific process provided by cooperative editors of another journal.

# PART XVII:

# JOURNAL

# DE

# PHYSIQUE

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

January 30, 1984

COMMISSION DES PUBLICATIONS FRANÇAISES DE PHYSIQUE
LABORATOIRE DE PHYSIQUE DES SOLIDES
UNIVERSITÉ DE PARIS SUD
F-91405 PARIS, FRANCE

Att.: Editors of LE JOURNAL DE PHYSIQUE

Dear Editors,

I here respectfully submit for publication in LE JOURNAL DE PHYSIQUE the enclosed article in three copies entitled

"COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE"

The paper has not been submitted elsewhere nor it will be submitted during your consideration. In case of acceptance, the copyrights are hereby granted to LES ÉDITIONS DE PHYSIQUE.

Please be reassured that I would be sincerely grateful for any constructive, critical comment aimed at the improvement of the paper. In case of any value, I enclose a list of experts in the fieldsof the papers that are not widely known [Lie-isotopies and Lie-admissible genotopies].

Finally, in case of interest by your Journal, I would be glad to submit to you the papers developing in detail some of the arguments [ref.s 24].

Thanking you for your consideration and time, I remain

Yours Very Truly

Ruggero M. Santilli

RMS-mlw
encls.

*Secrétariat de la Commission des Publications Françaises de Physique*
**Bâtiment 510, Université Paris-Sud, F 91405 Orsay Cedex**

Manuscript submitted for publication in *Journal de Physique* our ref. 4-1030

Author (s) R.M. Santilli

Title Comments on Polarization Experiments and the Isotropy of Space

## REFEREE'S REPORT

The paper presented is nothing but a lengthy advertisement for preceding papers of the author and his followers, published in his samizdat "Hadronic Journal".

I have nothing against that kind of literature except reading it myself (Refs. 7 and 8 total more than 500 pages), but I consider that the conceptions of the author derive from a profoundly ill-conceived view of natural sciences, and of physics in particular.

To be specific; the author considers that the most general theory (non-associative, non-hamiltonian, anisotropic, and so on) is the most likely to adjust to reality. It is probably true, or at least it allows to push away indefinitely any conflict between theory and facts. This is another way of saying that such an extensive view of theory has no predictive power whatsoever, since it may be generalized enough to accommodate any fact.

Science proceeds otherwise, or at least, has been creative precisely by posing more and more stringent conditions on theories, instead of relaxing them. This, of course, leads to open conflict with the facts, sometimes, and it is precisely that kind of conflict which stimulates imagination towards better, more constrained, more predictive theories.

The authors misrepresents Ref. 1, which only discusses the possible experimental similarity between space anisotropy and parity violation ; he takes advantage of a 1.1 standard deviation experimental error taken from Ref. 29 ; with these weapons he declares war against hamiltonian Quantum mechanics, ignoring field theory, QCD, and all developments since ten years.

Is that an approach to the problem of nucleon structure ? No.

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

May 24, 1984

Professor J. ZINN–JUSTIN, Editor
Journal de Physique
Commission des Publications Françaises de Physique
Bâtiment 510, Université Paris–Sud
F–91405 ORSAY CEDEX, FRANCE

Dear Professor Zinn–Justin,
I am respectfully re–submitting for publication in your Journal my paper entitled COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE. Three copies of the manuscript are enclosed. As you can see, the paper has been completely rewritten following the report of your referee.

You will note that I have taken in all possible consideration the valuable part of the report (and duly thanked the referee in the Ancknowledgments for that). In fact, I have rewritten the paper in such a way to minimize as much as conceivably possible my own contributions in the field; I have eliminated references to un–essential Proceedings, to avoid the complaint of advertising (!?!); and restricted the presentation to the truly essential part: the deformation of the charge distribution of hadrons under external, sufficiently intense fields, with consequential alteration of the magnetic moments, and the available direct measures by Rauch favoring this setting.

In regard to the offensive part of the report, I beg your personal understanding. I decided long ago NOT to accept gracefully offensive language in scientific proceedings, and I regret being unable to make an exception here. At any rate, the offensive nature of the report goes beyond the contents of the paper submitted to your Journal, and invests all Editors, referees and authors of the HADRONIC JOURNAL. As seen by us, this situation is simply too grave to be accepted lightly. I have therefore enclosed a separate answer to your referee. The courtesy of sending this answer to the referee would be appreciated.

Permit me to recommend that a new referee be selcted for the further review of this paper. In fact, it is unlike that I can have a scientifically meaningful dialogue with the previous referee. Also, permit me to recommend that the review be done in Europe. I shall remain at your disposal for sending you a list of distinguished, senior experts in the field of the paper for your possible use as referees.

As far asI am concerned, I will sincerely appreciate ANY criticism on my paper (s), no matter how harsh they are, provided that they are scientifically constructive and non–offensive. Under these circumstances, you can count on my sincere collaboration and gratitude.

Very ~~Truly~~ Yours

Ruggero Maria Santilli
author
Efitor,
HADRONIC JOURNAL
RMS–mlw, encls.

May 24, 1984

Critical analysis
by
Ruggero Maria Santilli
on the
REFEREE REPORT RELEASED BY THE JOURNAL DE PHYSIQUE
regarding the paper
COMMENTS ON POLARIZATION EXPERIMENTS
AND THE ISOTROPY OF SPACE
(J. de Phys. ref. no. 4.1030 of 8 Feb., 1984)

A well established editorial rule is that the use of offensive language in the refereeing of technical papers is a mascara of scientific corruption, no matter how the papers are wrong. I present below the reasons why I suspect that this referee report is no exception. In case of evidence of the erroneous nature of my arguments, I am ready to present my most humble apologies. However, in case of insufficient evidence, the mere suspicion of dubious ethical standards should be sufficient for the termination of all future associations between the JOURNAL DE PHYSIQUE and this person.

The primary reasons why the paper was written are the recalling of certain manifestly fundamental, theoretical and experimental facts on the conventional rotational symmetry, such as: (A) the hystorical hypothesis of the deformation of the magnetic moments of hadrons under the nuclear conditions; (B) the recent interpretations by Eder et al of this alteration as due to the deformation of the charge distributions of hadrons under sufficiently intense external fields, and, last but not least, (C) the availability of direct interferometric measures by Rauch and his team (totally ignored in ref. 1 with too many others), which, in their current form, DISPROVE orthodox view in favor of the manifestly plausible deformation/rotational asymmetry.

I have reasons to suspect that this referee intends to suppress the appearance of these manifestly plausible physical aspects in the JOURNAL DE PHYSIQUE. In fact, if the referee was seriously interested in the publication of the facts, he/she would have presented a CONSTRUCTIVE report indicating all deficiencies of the paper (which are fully admitted here) and suggesting the suitable improvements. Instead, this referee has selected a totally passive report, which is typical of the referee opposing the publication of the topic considered.

But, WHY THIS REFEREE IS SEEMINGLY OPPOSED TO THE PUBLICATION OF INCONTROVERTIBLE FACTS such as Rauch's experiments, and Eder's studies? A quite conceivable reason is the fact that these experimental and theoretical studies are manifestly against the vested, academic—financial—ethnic interests surrounding Einstein's theories. In fact, the experimental confirmation of the deformation/rotational asymmetry of hadrons would imply the irreconciliable invalidation of Einstein's special relativity for the physical conditions considered. The considerable damage to said vested interest is evident beyond any doubt.

But, above all, the primary reason that leaves this author dubious on the ethical standards of this referee, is the last passage of the report concerning the seemingly "declared war against hamiltonian Quantum mechanics, ignoring field theory, QCD, and all developments since ten years." Since this referee has reached the status of refereeing for the JOURNAL DE PHYSIQUE, I must assume that he/she is fully aware of the following facts (otherwise he/she does not qualify for the review): (1) the "perpetual motion" does not exist in our macroscopic environment; (2) the physical trajectories in Newtonian mechanics are NONHAMILTONIAN—NONCANONICAL as a rule, and hamiltonian—canonical only as rare exceptions, as established by satellites during re—entry, damped giroscopes, all holonomic systems (because of the frictional force of the constraints), and too many additional cases; (3) the reduction of these experimentally established NONHAMILTONIAN—NONCANONICAL systems to a large collection of conjectured hamiltonian-unitary descriptions of particles constituents is manifestly inconsistent in an irreconciliable way. I must insist on the true technical knowledge of this referee (that beyond academic politics), and expect that he/she is capable of proving theorems establishing such an irreconciliable incompatibility between quantum mechanics and our real macroscopic world (that of decaying trajectories and not the preferred world of "perpetual motion" of beautiful hamiltonian-canonical character).

But then, how can this referee dream of being convincing in suppressing this incontrovertible incompatibility of quantum mechanics with the established nonhamiltonian character of the real world? How can this referee dream of suceeding with this author and his known LACK of alignment with vested interests in particle physics? How can this referee dream of succeeding via the mere mention of QCD and the litany of its unspoken problematic aspects and shear inconsistencies (such as the known, but carefully avoided in printed papers, finite, non-null probability of tunnel effects for free quarks in direct contradition with physical evidence, etc. etc.).

The reference to lack of predictive power of the generalization of quantum mechanics under construction (hadronic mechanics) is a rather clear manifestation of the typical ignorance that generally underlies offensive reports. The specific, detailed, quantitative predictions of deformation/rotational-asymmetry by Eder were reported clearly in the paper. Evidently, these detailed predictions are damaging vested interests on Einstein's ideas, but they are there, and they were there in the original version of the paper. There is no point for this author to list the number of additional predictions that are coming from a number of independent sources. The very claim of generality beyond computational capability is studiously erroneous and must be disclaimed here. Hadronic mechanics demands the knowledge of two operators, the Hamiltonian H and the isotopic operator g = I +"QM corrections", the latter one representing the nonhamiltonian forces due to contact among extended charge distributions. The new mechanics DOES NOT restrict the functional dependence of H and g in exactly the same measure as QM does not restrict the functional dependence of H. The strict implementation of this referee view would literally imply the abandonment of quantum mechanics because it implies an infinity of possible models, all those permitted by infinitely many H!!!.

The offensive reference to the "samizdat Hadronic Journal" demands a special comment inasmuch as it appears to be intended, or otherwise invests all editors, all referees, and all authors of the journal. The reason why the Hadronic Journal was founded is known in the trade and must be repeated here. It was due to the known deterioration of ethics in physics which has reached such an alarming level, to suffocate at birth the most vital aspect in the achievement of novel human knowledge, the publication of plausible conjectures. In fact, it is common knowledge that the possibility for Albert Einstein to became a scientist, would he have lived today, would have been so minute to be laughable.

At the HADRONIC JOURNAL we FIRST publish plausible physical conjectures with a sufficient technical maturity, and THEN talk about it.We do not suppress them at birth as done too often elsewhere. But this implies the publication of physical ideas that are manifestly against vested academic-financial-ethnic interests. In this sense the HADRONIC JOURNAL is definitely a "samizdat" journal. But then, this means that the journal pursues physics and not academic politics, thus resulting in a beautiful qualification of our efforts.

PREPRINT OF THE INSTITUTE FOR BASIC RESEARCH NUMBER IBR–DE–84–1

PAC NUMBERS 11.30.–j; 11.30.–Er; and 21. 03.65.–w

Journal de Physique
Manuscrit n° 4.1030
reçu le 8 FEB 1984

# COMMENTS ON POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE

Ruggero Maria Santilli*
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Abstract

*A valuable and courageous note by G. R. Goldstein and M. J. Moravcsik on possible tests of the rotational symmetry under strong interactions has been recently brought to our attention. In these comments we indicate possible additional tests, as well as references on the problem that were apparently unknown to the authors at the time of writing their note.*

Submitted for publication

*Supported by the U. S. Department of Energy under contract number DE–AC02–80ER10651.A002.

# PART XVIII:

# MISCELLANEOUS

# CORRESPONDENCE

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

JAN 16 1984

Office of the President

January 3, 1984

Professor J. A. WHEELER
Department of Physics
University of Texas
AUSTIN, Texas 78712

Dear Professor Wheeler,

I would gratefully appreciate your review of the enclosed paper

GENERAL RELATIVITY AND PLANETARY ORBITS, by Dr. H. YILMAZ

submitted for publication to the Hadronic Journal on December 15, 1983. As you can see, the paper contains an updated and novel presentation of the revision of the general theory of gravitation that Dr. Yilmaz has proposed since 1958.

To the best of our understanding, personally and via referee reports from preceding publications in our Journal, the situation is as follows.

1. Yilmaz generalization is compatible with all available experiments in gravitation, and, therefore, it cannot be ruled out on grounds of available experimental knowledge.
2. Einstein's equations are plagued by a number of problematic aspects, only some of which are presented or reviewed in the enclosed paper; and
3. There seems to be grounds for the initiation of a scientific process of resolution of the issue: whether Yilmaz stress—energy tensor should indeed be added to the gravitational equations for the exterior problem.

Nevertheless, we need advice by qualified experts in the field to verify the veridicity of these aspects, or at least their plausibility. We would therefore gratefully appreciate your advice on the above aspects.

Please keep in mind that the enclosed paper by Dr. Yilmaz does not contain an exhaustive presentation of all the problematic aspects of Einstein's equations identified in the literature, nor is it expected to do that. Nevertheless, a mature scientific judgment should be expressed by taking into account also these additional, at times important facets of this quite intriguing case.

As an example, I would like to bring to your attention an apparently unknown paper I wrote on the subject [Ann Phys. 83, 108 (1974)]. As you know, a fundamental assumption of Einstein's theory is that the gravitational field in the exterior of a body with null total electromagnetic phenomenology [zero total charge, zero electric and magnetic dipoles] has no source, and the equations are $G_{\mu\nu} = 0$. The paper quoted above essentially shows that, as a result of these equations, Einstein's theory is incompatible with electromagnetism in an apparently irreconciliable way. In fact, despite the null character of the total electromagnetic quantities, the total electromagnetic field of the charged constituents of the body is far from being null and cannot be made null unless Maxwell's theory is abandomed. To put it bluntly, Einstein's theory only holds under the assumption that matter has no charge structure. Intriguingly, the electromagnetic fields resulting from the charged structure of matter has precisely the structure of Yilmaz stress–energy tensor. As a result, the paper quoted above, is in rather strong support of Yilmaz's theory.

Permit me to stress that I have no personal claim; that the scientific priority rests on Dr. Yilmaz (I merely presented an argument); and that I see no need to have Dr. Yilmaz quote the paper indicated above in his article. I brought it to your attention to indicate that the issue under consideration here is much more deep, involved, and delicate than that sometimes postured by nonscientific academic circles.

The analysis above is solely referred to the **exterior problem** of gravitation. To complement your judgment, you should also take into consideration the additional, perhaps even bigger problematic aspects of Einstein's theory of gravitation for the **interior problem**. For this purpose, it is sufficient to recall Cartan's point that the equations (actually, the Riemannian geometry itself) do not permit to recover at the Newtonian limit the equations of motion of the interior systems of our Earth, those with contact–nonpotential forces, say, of type of power series expansions in the velocities used by engineers (which have reached powers of the fourth and even fifth order in the velocity). It is evident that, until a theory of gravitation capable of admitting these systems at the Newtonian limit has been built, all current theories are and remain "provisoires".

Also, caution should be exercised in the old idea of by–passing these Newtonian forces via the reduction of the body to point–like constituents. In fact, this idea is plagued by a host of technical inconsistencies, such as the inability to reduce the experimentally established noncanonical time evolutions of interior trajectories of our Earth to a collection of unitary time evolutions of the trajectories of assumed point–like constituents.

Note that Dr. Yilmaz's paper is on the exterior problem only and, in our view, does not need to enter into the interior problem. Nevertheless, the latter should be considered for an overall judgment because the generalization of Einstein's relativity for the interior problem needed to represent the Newtonian systems of our enviroment is also expected to call for the addition of Yilmaz's stress–energy tensor when reduced to the exterior case.

In closing, permit me the liberty to suggest that a scientific process of comparative, constructively critical examination of Einstein's, Yilmaz's, and possibly other viewpoints be initiated via the presentation and examination of all views in the field. To achieve this objective in an effective way, our Institute would like to organize a Workshop and subsequently publish its proceedings in order to leave the necessary scientific record.

Kindly let me know whether your Institution might join the I.B.R. in the organization of this Workshop, and, in case this is not possible, whether you would be interested in contributing to this scientific process via your participation in the Organization Committee.

Thanking you for your time and consideration, and wishing you and your family a happy and prosperous 1984, I remain

Very truly yours,

Ruggero M. Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/mlw

THE SAME LETTER WAS MAILED TO:

- A. PAIS, ROCKEFELLER UNIV.
- S. DESER, BRANDEIS UNIV.
- Y. NE'EMAN, TEL-AVIV UNIV., ISRAEL
- S. WEINBERG, UNIV. OF TEXAS AT AUSTIN

THE UNIVERSITY OF TEXAS AT AUSTIN
AUSTIN, TEXAS 78712

*Center for Theoretical Physics*
*(512) 471-3751*

January 27, 1984

Professor Ruggero M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This to acknowledge the receipt of Yilmaz's "General Relativity and Planetary Orbits", and your thoughtful letter about the same. Responsible colleagueship like yours is the foundation of sound science. I deeply regret that I cannot live up to your fine example because a truly major deadline is staring me in the face, forcing me to return these materials.

Best wishes for 1984.

Sincerely,

John Wheeler

John Archibald Wheeler
Ashbel Smith Professor and
Blumberg Professor of Physics
and Center Director

Enclosures: Abstract
lch

BRANDEIS UNIVERSITY
WALTHAM, MASSACHUSETTS 02254

THE MARTIN FISHER
SCHOOL OF PHYSICS
617-647-2835

January 18, 1984

Dr. Ruggero M. Santilli, Editor in Chief
Hadronic Journal
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your letter and the Yilmaz paper. I have, in the past, had discussions with the author, but always found it difficult to get my questions understood. I therefore feel everyone would be better served with a different referee—I especially suggest Professor C. Will at Washington University, Saint Louis, who is the expert on tests of gravity theories.

I am also afraid I cannot participate in the workshop you propose since my own interests are currently in quite different areas.

Sincerely,

Stanley Deser

Stanley Deser
Professor of Physics

SD/de

THE UNIVERSITY OF TEXAS AT AUSTIN
AUSTIN, TEXAS 78712

*Department of Physics*

January 17, 1984

Dr. Ruggero M. Santilli
Editor in Chief, HADRONIC JOURNAL
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

I am sorry that I will not be able to review the paper by Dr. Yilmaz, or help to organize your Workshop.

Very truly yours,

Steven Weinberg
Josey Regental Professor of Science

שר המדע והפתוח
MINISTER OF SCIENCE AND DEVELOPMENT

24 January 1984
YN/1236

Professor R M Santilli
Editor in Chief
Hadronic Journal
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge
Mass 02138
U S A

Dear Professor Santilli

I received your letter of the 3rd January but regret that due to my present duties, I find it impossible to devote the necessary time and attention required to study Yilmaz's theory.

I am sure you can find advice elsewhere in the GRG community.

With kind regards

Yours sincerely

Yuval Ne'eman

Yuval Ne'eman

YN/bmr

ירושלים, משרדי הממשלה, הקריה המזרחית טל. 02-277046/7

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

March 31, 1983

Dr. D. L. NORDSTROM, Editor
Physical Review D
1 Research Road
RIDGE, N.Y. 11961

Dear Dr. Nordstrom,

I here submit for publication as "Rapid Communication" in PHYSICAL REVIEW D my enclosed paper in two copies entitled

COMMENTS ON THE NOTE "POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE" BY G.R.GOLDSTEIN AND M.J.MORAVCSIK

The PACS numbers are in the front page of the article; the copyright transfer letter is enclosed; and the publication costs will be paid by the I.B.R.

The reasons for submission of the comments as "Rapid Communication" are self-evident in this case. In fact, the note submitted presents a series of mathematical, theoretical, and experimental references on the problem of the isotropy of space (i.e., of the rotational symmetry) that were not quoted in the note by Goldstein and Moravcsik. A rapid correction of the occurrence is therefore recommendable to avoid the appearance of additional papers in the field with major deficiencies in the listed literature. Additional reasons for the "Rapid Communication" are due to the apparent increase of experimental interest in the field. It appears therefore recommendable to provide the community with additional tests that seems to be better understood, more effective, amd readily feasible with available technology (neutron interferometry).

Very regrettably, recent extremes of decay of scientific ethics in the U.S. physics, particularly in refereeing, force me to submit this paper under legal assistance from the very beginning.

Please do not feel offended by this unsual form of submission. In fact, I believe that you, as Editor, are a victim of the decaying ethics of our community much more than the authors. At any rate, I put in writing in the past my unconditional faith in you as a person, and I confirm it here.

Also, I believe that Goldstein and Moravcsik were in good faith when they published their paper in your journal, and, under no circumstance, the legal assistance is due to their persons. In fact, I know them personally; I consider them highly; and a significant scientific exchange of ideas has been lately initiated among us.

Furthermore, please be reassured that I shall be most receptive to any constructively critical suggestion for the improvement of the paper submitted. To put it explicitly, in case an orderly, respectful, and effective scientific process is implemented in the consideration of the paper hereby submitted, it would be a point of honor for me to respond in a way as respectful and cooperative as possible.

The possible activation of the legal assistance is therefore solely restricted to refereeing practices that, lately, have become not unfrequent, such as: use of offensive language in referee reports; use of refereeing authority to delay the consideration process for 6 months to one year (or even more in certain known cases) to favor other groups or for other nonscientific objectives; manifest manipulations and distorsions of scientific truths in the apparent attempt to suppress at birth undesired advances; etc. etc. etc.

In the hope that we can unite forces to contain such ethical decays in the interest of America, I remain

Sincerely and Gratefully Yours

Ruggero Maria Santilli

cc. Mr. [redacted], Esquire
[redacted]

and
Drs. D. LAZARUS and G. TRIGG, Physical Reviews

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH
96 Prescott Street, Cambridge, Massachusetts 02138, tel. (617) 864 9859

*Ruggero Maria Santilli, Professor of Theoretical Physics and President*

March 31, 1983

Professors G. R. GOLDSTEIN, Tufts University and
M.J.MORAVCSIK, University of Oregon

Dear Professors Goldstein and Moravcsik,

I enclose copy of a paper presenting a few comments on your paper published in PR D25, 2934 (1982), which has been submitted to PRD as Rapid Communication.

Any constructively critical remark and/or advice you may have to achieve a better maturity of presentation would be gratefully appreciated.

Also, I would like to take this opportunity to inform you of the forthcoming I.B.R. meetings this summer (see enclosed announcements) from August 2 to 7, 1983 where our common interests on the tests of the rotational symmetry under strong interactions will be studied in all possible mathematical, theoretical, and experimental depth. In case you are interested to attend, you would be sincerely welcome.

Very Truly Yours

R. M. Santilli

Ruggero M. Santilli

cc: Dr. D. NORDSTROM, PRD

MICHAEL J. MORAVCSIK
• Theoretical Physics
• The Science of Science
• Science Policy and Development particularly in the Third World

Professor Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street, Cambridge,
Mass 02138

April 6,1983

Dear Professor Santilli,

I want to thank you for sending me a copy of your paper :"Comments on the Note ..... Moravcsik". I admired your breadth of vision and coverage exhibited in that paper, and I really have nothing to add to it in response that would be worth printing. Perhaps the only comment I could make informally to you as the author is that it may be useful, at the end of the paper, to summarize the specific experiments you would urge. As the paper stands now, an experimentalist reading it would be awed but would probably be unable, on his own, to glean experimental guidance out of it.

My best regards

Michael J. Moravcsik

Copy: Professor Gary Goldstein

Mailing address: Institute of Theoretical Science. University of Oregon. Eugene. Oregon 97403 USA
Phone: (503) 686-5207 or (503) 344-1137. Messages at (503) 686-5204
Telex: 510 597 0354

THE PHYSICAL REVIEW 719 D

PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES

21 June 1983

Dr. Ruggero Maria Santilli
The Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Re: Comments on ``Polarization experiments and the isotropy of space''

By: Ruggero Maria Santilli

DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by one of our referees.

Comments from the report are enclosed. We are returning the manuscript for your consideration of these comments.

Yours sincerely,

Stanley G. Brown

Stanley G. Brown
Editor
Physical Review D

F2

ICAN PHYSICAL SOCIETY

Referee's Report on MS No. DDR231D, by R.M. Santilli.

This paper hardly qualifies as a "Comment" on the work of Goldstein and Moravcsik, Ref. 1, since the subject matters of the two papers have very little to do with each other, hence, it has to be viewed as a separate article.

The paper under review, however, contains no new material. Rather, it is a summary of some of Santilli's and collaborators' works, published elsewhere, on their modification of the rotation group. The author also claims that the experiments of Eder et al, also published elsewhere, support his theory of rotations; however, the claim is made without a detailed analysis of those experiments or without proposing new types of experiments to further test this claim.

Where the paper does contain specific comments regarding the work of Goldstein and Moravcsik, those are based on a misunderstanding of the framework in which the latter authors obtain their results. Contrary to Santilli's statement (see e.g. "Comments D"), the results of Goldstein and Moravcsik are not based on potential scattering or on the assumption of structureless target and projectile. The authors of Ref. 1 merely assume that a differential cross section is the modulus squared of a scattering amplitude; the latter, in turn, possesses all the invariance properties of the underlying theory. This approach is perfectly general and it is independent of the details of a theory as long as the theory in question is a quantum theory in which the principle of superposition holds.

In conclusion, this paper does not contain new results or comments relevant to the subject dealt with by Goldstein and Moravcsik, Ref. 1. Hence, it is not suitable for publication in Physical Review.

July 7, 1983

COMMENTS ON REFEREE'S REPORT ON THE PAPER NO.DDR231D, by R.M.Santilli, ACCEPTED AND RELEASED BY PHYSICAL REVIEW D

The statement by this referee
"....the subject matters of the two papers have very little to do with each other"
is manifestly false. Both papers (by Goldstein-Moravcsik and by Santilli) deal exactly with the same, single,issue: the tests of the rotational symmetry in particle physics.

The additional statement by this referee
"The paper under review, however, contains no new material."
is also manifestly false. The paper is the first to treat jointly and on a comparative way all (and not only some) possible tests of the rotational symmetry, as an essential pre-requisite for the future conduction of the tests themselves.Furthermore,the paper presents for the first time the main ideas of the Lie-isotopic lifting of the rotational symmetry and contains several other advances which need not to be identified here.

The additional statement by this referee
"...the claim is made ... without proposing new types of experiments..."
is also manifestly false. The paper proposes specifically and in all sufficient details three varieties of experiments, identified in page 8 and recalled in the final statements.

The additional statement
"... experiments by Eder et al.."
is also manifestly false. Eder is a theoretician. The experiments referred to (interferometric measures of the apparent, quite natural, deformation of the spherical charge distribution of neutrons in the intense fields of Mu-mtal nuclei, with consequential rotational-asymmetry) have been conducted since 1975 by H. Rauch et al, as repeatedly stated in the paper.

The additional statement by this referee
"This approach [by Goldstein-Moravcsik] is perfectly general and it is independent of the details of the theory in question..."
is also manifestly false. As explicitly stated in the paper, Goldstein-Moravcsik assume the conventional associative algebra with trivial associative product of matrices, functions, etc of type AB. But this is the SIMPLEST POSSIBLE (rather than the most general possible) realization of the associative product. The hadronic mechanics assumes instead the most general possible associative product of operators with realizations of the type $A*B = AgB$ where g is fixed (and verifies certain restrictions). In turn, it has been shown in the literature that an isotopic lifting of the enveloping algebra (with a parallel one for the Hilbert space) implies a generalization of the current"abstract"formulation of the scattering theory, including nontrivial deparures from the cross sections used by Goldstein-Moravcsik.

All these and other elements suggest rather strongly that the review is of nonscientific nature, that is, of the political character which is rendering the journals of the APS sadly known world wide. At any rate, the absolute, total, and complete lack of any constructive comment or suggestion to improve the paper establishes quite clearly the fact that this referee OPPOSES the experiments suggested in the paper and the appearance of the paper IRRESPECTIVE OF POSSIBLE IMPROVEMENTS. In short, we are evidently facing a situation of academic dances totally deprived of any scientific content whatsoever.

The basic motivation for the preparation of the paper and its submission to Phys. Rev. D must be recalled here. The submission resulted from the fact that the paper by Goldstein and Moravcsik failed to quote a rather massive literature in the topic pf their paper (test of the rotational symmetry), which, when combined with theoretical and mathematical efforts exceeds the 10,000 pages of published research!

It is evident that the leaving of this situation uncorrected will damage, first of all, Goldstein and Moravcsik. Second, it will damage the reputation of the PR at an international level, and last but not least, it will not serve the pursuit of novel physical knowledge.

It should also be stressed that the occurrence is PRIMARILY AN EDITORIAL PROBLEM, THAT IS, THE PRIMARY RESPONSIBILITY OF THE MASSIVE LACK OF REFERENCES RESTS IN THE EDITORS OF THE PHYSICAL REVIEW D.

Two possibilities can be foreseen for the solution of the problem.
ALTERNATIVE I. Goldstein and Moravcsik publish an Errata Corrige OR Addendum to their paper indicating the missed references. In this case the paper by Santilli will be withdrown, rewritten,and submitted to another (European) journal.
ALTERNATIVE II:Phys. Rev. D selects a true referee, that is, a referee interested in doing physics in the traditional way: submission of ideas and presentation of CONSTRUCTIVE criticism for their improvement. Reference is made here to the uncompromisable need that referee's reports indicate in all specific details the aspects that must be improved to reach maturity of publication. Complete silence on this point implies that the referee opposes the line of study of the paper. To be even more specific, the referee should indicate whether paper DDR231D should

- elaborate in more details the three varieties of experiments suggested;
- enlarge the novel parts on the isotopic lifting of O(3) and its capability to leave invariant all ellipsoidical deformations of the spherical charge distributions;
- modify in any desired/suggested way any other aspect.

On one point is is essential that Phys. Rev. reaches a clear understanding. Everybody is entitled to his/her own little politics. But there MUST be limits, even in the current absence of a Code of Ethics in Physics. In the case of the paper by Goldstein-Moravcsik, the missed quotations are simply too huge to be left unchallenged.

The continuation of the formal acceptance of nonscientific referees of the type accepted and released by Phys. Rev. D. will be taked for its face value: a provocation to turn the issue into a legal fight.

cc. Dr. Lazarus, Editor in Chief
Drs. Goldstein (Tufts Univ.) and Moravcsik (Univ. of Oregon)
Attorney J. Grassia, Boston

Encls.: Revised version of the paper.

MICHAEL J. MORAVCSIK
• Theoretical Physics
• The Science of Science
• Science Policy and Development
particularly in the Third World

July 14,1983

Dr. Ruggero Santilli
Institute of Basic Research
96 Prescott Street
Cambridge, Mass 02138

Dear Dr. Santilli,

Thank you for sending me a copy of your comment on the referee's report on your paper that you were kind enough to send me a copy of earlier. I was sad to hear that you have had some difficulties with Phys. Rev. Let me know how things work out, but for the moment let me just make a few comments which may help to resolve the difficulties.

I see that one of the main points in the argument is the presumed lack of references in our original article. As you recall, that paper simply contained a rather simple point, pertaining to the experiments and their interpretation, and we did not feel it would be appropriate in that note to make a mountain out of a molehill and drag in the 10,000 pages of research on symmetries which are not really directly relevant to the article or contain results on which our note was built. We still think so, but of course this is a matter of opinion, and therefore we would by no means be opposed to submit an erratum or an addendum, containing a modest list of references supplied by you, and we would be happy to acknowledge your help in preparing that list.

It would, however, be preferable if your article, or some version of it, could be published in Phys. Rev. or some other journal, since it summarizes the background much more effectively than a list of references could in an addendum.

I have not had a chance to discuss the content of this letter with Gary Goldstein, who is at the moment at the Rutherford Laboratory, and in fact by the time he returns in late August, I will have left for Europe, to return only at the middle of September. So this letter is only my personal opinion, though I would expect Gary to concur with its content.

Sincerely

Michael J. Moravcsik

Michael J. Moravcsik

Copy: Gary Goldstein

Mailing address: Institute of Theoretical Science, University of Oregon, Eugene, Oregon 97403 USA
Phone: (503) 686-5207 or (503) 344-1137. Messages at (503) 686-5204.
Telex: 510 597 0354

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

July 19, 1983

Dr. M. J. MORAVCSIK
University of Oregon
EUGENE
Oregon 97403

Dear Dr. Moravcsik,

Please accept my appreciation for the courtesy of your letter of July 14, 1983. I am also grateful for your positive attitude. You can therefore trust on my own best possible attitude.

MISSING REFERENCES. They are the following.

FIRST PRIORITY REFERENCES: All the experimental papers by H. Rauch and his associates on the rotational symmetry of neutrons conducted since 1975. They are ref.s 25 through 29 of my note on your paper. These references (particularly the last one, ref. 29, on the latest results) refer specifically to the experiments you suggest. Their quotation is of utmost importance for all papers on the rotational symmetry, whether yours or mine.

SECOND PRIORITY REFERENCES: They are given by the theoretical studies by G. Eder on the apparent deformation of the spherical symmetry of the charge distribution of hadrons (ref.s 12 through 14 of my note on your paper). They provide a model of deformed nucleons for which SO(3) is manifestly broken. As such, they are directly related to your paper.

LAST PRIORITY REFERENCES. Are my own studies in the field, and you should not feel obliged to quote them. To put it explicitly, I have contacted Phys. Rev. on the issue as a representative of a scientific group, rather than for myself only. Perhaps, rather than quoting my papers (and those of additional researchers), you should consider quoting the Bibliography by M. L. Tomber (ref. 4 of my note on your paper), as well as the Proceedings of the Orléans International Conference (Ref. 5).

To avoid misunderstanding, none of us consider you and Dr. Goldstein directly responsible for the occurrence. In fact, we believe that you were in good faith, and that you simply were unaware of the amount of publications directly related to your paper. The entirety of the responsibility of the occurrence is seen to belong to the editors of Phys. Rev. D who were fully aware of the references.

POSSIBLE SOLUTIONS. Your indication of the possibility of publishing a brief Errata–Corrige or Addendum at Phys. Rev. D is seen as a confirmation of your good faith. In fact, a few lines would be sufficient, indicating that, following the publication of the paper, a number of references had been brought to your attention, and then quote the experimental papers first, followed by the theoretical ones. In case you publish these lines, I shall withdraw my own paper from Phys. Rev. D, rewrite it, and submit it to another journal as indicated in my recent letter to Phys. Rev. D.

There is another possibility you should consider. We can publish jointly a follow up paper. As you know well, following our meeting here at the I.B.R. and this correspondence, there are a number of technical aspects on your paper that need clarification, such as:

- The possibility that space is and remains isotropic even for a broken rotational symmetry. In fact, the breaking my indicate motion of extended particles within an isotropic hadronic medium without any connection whatsoever with the isotropy of space (as considered in your paper);
- The possibility that the breaking is due, quite simply and trivially, to a conceivable deformation of the spherical charge distribution of hadrons, as suggested since 1978;
- The possibility that, even in case the symmetry under conventional rotations is broken, the SO(3) symmetry is still exact. In fact, our isotopic lifting $\widehat{SO}(3)$ of SO(3) provides the invariance of all possible ellipsoidical deformations of the sphere, while being isomorphic to SO(3). Thus, the abstract rotational symmetry can be exact EVEN FOR UNISOTROPIC MEDIA AND DEFORMED SPHERES.

The purpose of my note on your paper submitted to Phys. Rev. D is to bring to the attention of the experimenter these and other facts. It is evident that, lacking their knowledge, the maturity of the formulation of the experiments you suggest is questionable. It is a question of scientific accountability.

Rather than publishing these comments on my own, I would be glad to join forces with you and publish them together. In this way, rather than appearing as a form of insufficiency of your work, the remarks acquire the meaning of further developments.

In case you are interested in this joint collaboration, simply rewrite and modify my note submitted to Phys. Rev. D in the way you wish, and let me have a copy. Additional papers on the isotopic lifting of rotations are enclosed. Additional information will be available at our summer workshops, where the issue will be discussed in considerable experimental, theoretical, and mathematical detail (a formal invitation for you and Dr. Goldstein to attend the workshops was mailed a time ago).

Sincerely,

Ruggero M. Santilli

RMS/mlw

cc: Dr. Goldstein and Phys. Rev. D

P.S. I shall leave soon after our workshops (on August 8) for a tour of lectures in Europe, and I contemplate to be back sometime in September 1983.

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS

EDITORIAL OFFICES
1 RESEARCH ROAD BOX 1000 RIDGE NEW YORK 11961
Telephone (516) 924-5533

12 September 1983

Dr. Ruggero Maria Santilli
The Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Re: Comments on ''Polarization experiments and the isotropy of space''
By: Ruggero Maria Santilli

DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by one of our referees.

We are enclosing comments from the report, and are returning the manuscript.

Yours sincerely,

Stanley G. Brown

Stanley G. Brown
Editor
Physical Review D

F2-R

REPORT OF SECOND REFEREE

REPORT OF REFEREE

Manuscript Number: DDR231D

Author: Ruggero Maria Santilli

Title: "Comments on 'Polarization experiments and the isotropy of space'"

1. This manuscript is a comment on the paper of Goldstein and Moravcsik only in the limited sense that it points out literature citations apropos of the subject but omitted in the paper of Goldstein and Moravcsik.

2. Manuscript DDR231D is, for the most part (but not exclusively), an extended discussion of the work of the author and his group on theories of Lie-admissible extensions of mechanics and associated criticisms of rotational symmetry. This discussion is difficult to follow since the terms are not defined, and the treatment is not self-contained (referring to citations for key results). The material in this part of the manuscript is not suitable for publication.

Recommendation: Publication is <u>not</u> recommended.

September 27, 1983

Dr. S.G.BROWN, Editor, Physical Review D
P.O.Box 1,000, Ridge, New York 11961

RE: Comments on the second referee report of paper DDR231D entitled
"Comments on 'Polarization experiments and the isotropy of space'
by G.R.Goldstein and M.J.Moravcsik"

Dear Dr. Brown,

I believe that the dishonesty of the first referee report was beyond any reasonable doubt .

This second report must be praised for the use of clean language and arguments. Nevertheless, its end results raise the same doubts of the first: partisanship with established scientific interests; suppression of due scientific process; and insufficient scientific accountability of the journals of the APS vis-a-vis the American taxpayer.

The doubts on partisanship are evident. In fact, the second referee essentially rejects the paper because the basic aspects are defined elsewhere, by therefore preventing complete comprehension of the issue via only the paper submitted. IF the same editorial rule is applied to ALL papers submitted, it would lead to the suppression of the virtual entirety of papers published in PRD. In fact, NONE of the papers published (or, at best EXTREMELY FEW) are completely selfsufficient. Only review papers are conceived to be entirely self-sufficient, but then they are not published in PRD.

The doubts on lack of due scientific process are equally self-evident. In fact, the report is ONLY NEGATIVE, and FAILS TO INDICATE SPECIFICALLY THE IMPORVEMENTS UNDER WHICH THE PAPER MIGHT BE PUBLISHED. This is a quite widespread disease of the review process at the Journals of the APS, with the understanding that it is implemented only for papers of potential novelty, that is, papers potentially against established vested interests.

The doubts on insufficient scientific accountability are equally evident. The facts treated in the paper are incontrovertible and leave no room to academic dances.

1) Extended charge distributions (such as hadrons) are expected to be deformable under sufficiently intense external fields, as a consequence of which the magnetic moments of the particles are altered, and the conventional rotational symmetry is manifestly broken [results of ref.6 of paper].
2) Quantitative calculations of the effect have been conducted by Eder, leading to the expectation of about 1% rotational asymmetry for low energy (thermal) neutrons within the fields in the vicinity of Mu-metal (or similar) nuclei [ref. s12-14].
3) Direct experimental tests on the intrinsic rotational symmetry of neutrons have been conducted by Rauch and his associates since 1975 via neutron interferometry. Even though still preliminary, the latest and best available measures CONFIRM THE BREAKING OF THE CONVENTIONAL ROTATIONAL SYMMETRY EXACTLY IN THE 1% RANGE [25-29].

The APS has somehow managed to suppress the appearance of facts 1), 2), 3) in its journals. This has been achieved via referee reports of the type under consideration here (first and second). The creation of doubts on sufficient scientific accountability are then evident. In fact, how can topics of such fundamental nature be left without due scientific process, that is, without their PUBLICATION and subsequent critical examination, experimentally and theoretically, in other publications?

The implications are evident, not only for the entirety of the scientific and financial profile of basic research [evidently, because of the breaking of the rotational symmetry due to deformations of extended objects], but also for possible applications [evidently, because of the implications, say, for the attempts to reach magnetic confinement of nucleons whose intrinsic magnetism can CHANGE with the approaching of the fusion conditions...].

I have repeatedly communicated these problems to the highest levels of the APS in other occasions. It is my opinion that, the later the existence of these problems is acknowledged, the bigger and more explosive will be an inhevitable crisis.

In fact, lacking any valuable scientific content in the referee reports, the only aspect left is the question: for how long can the suppression of facts 1), 2), and 3) be continued at the Journals of the APS?

Also, lacking any scientific content in the reports, the paper is resubmitted without modifications, jointly with a paper appearing elsewhere, in the flimsical hope that at least some members of the APS are indeed interested in due scientific process, e.g., to see better why mutation of shape and magnetic moment-and breaking of conventional rotational symmetry-may occur while hadrons conserve their conventional values of spin.

Very Truly Yours

Ruggero M. Santilli
96 Prescott Street
Cambridge, Massachusetts 02138

cc. Dr. D. Lazarus, APS
Dr. G.R.Goldstein, Tufts University
Dr. M.J.Moravsik, Oregon State University

THE PHYSICAL REVIEW

AND

PHYSICAL REVIEW LETTERS



EDITORIAL OFFICES - 1 RESEARCH ROAD
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December 8, 1983

Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Re: Manuscript No. DDR231D

Dear Dr. Santilli:

The above manuscript has been reviewed by Dr. Gordon L. Kane, in his capacity as a member of the Editorial Board of Physical Review D. We regret that in view of his comments (enclosed), we cannot accept the paper for publication. We are therefore returning the manuscript.

Sincerely yours,

Stanley G. Brown

Stanley G. Brown
Editor
Physical Review D

SGB/di
Enc.

Editorial Board Report on DDR 231D, Santilli

The reviewing of this manuscript seems to have been done in a responsible way by informed reviewers. I see no reason to modify their conclusions. One solution to the conflicting viewpoints seems to have been acceptable to all parties, and it solves the substantive problems, so I also recommend it--namely, that Goldstein and Moravcsik publish an erratum listing a set of references; it would be suitable to cite several recent references, with a remark that earlier work can be traced from those.

Gordon Kane

PHYSICAL REVIEW D VOLUME 25, NUMBER 11 1 JUNE 1982

(Ho) space only isotropic - Plate related - can be unisotropic - Test shift of today

# Polarization experiments and the isotropy of space

Gary R. Goldstein
*Department of Physics, Tufts University, Medford, Massachusetts 02155*

Michael J. Moravcsik
*Department of Physics and Institute of Theoretical Science, University of Oregon, Eugene, Oregon 97403*

(Received 5 May 1981)

It is shown on an example that sensitive tests of the isotropy of space (i.e., of rotation invariance) in strong-interaction particle reactions are almost identical to tests of parity conservation, and hence the two can be confused without some additional experiments which we specify.

The test of various conservation laws connected with symmetries is a central concern in nuclear and particle physics both because of cosmological implications and because theories of particles themselves depend on such conservation laws. Rotation invariance (i.e., the isotropy of space) is a symmetry that is relatively rarely studied. Our present belief, for example, that space is isotropic with respect to strong interactions is not based on experimental information of very high precision.[1] The aim of this article is therefore to explore the type of particle reaction experiments which can test rotation invariance in strong interactions. The conclusions of the investigation can be summarized in three points:

Best

(1) One can construct tests, by using polarization quantities that lend themselves to "null experiments," which can be performed to a reasonably high degree of accuracy, such as one part in $10^7$.

(2) These tests are virtually identical with experiments which test parity conservation, and hence evidence for parity nonconservation can be easily mistaken for evidence for violation of rotaion invariance.

(3) There are feasible additional experiments which can distinguish between exidence for parity nonconservation and evidence for anisotropy of space.

It would be quite feasible to discuss this problem in the framework of a general formalism of polarization phenomena. For didactic reasons, however, it might be much preferable to select instead a simple reaction as an example. The nature of the discussion will be such that it should be evident to the reader that nothing essential hinges on the specific properties of the example reaction and that therefore the generalization to any other reaction is straightforward.

The reaction we choose as an example is $0+\frac{1}{2}\rightarrow 0+\frac{1}{2}$, where the 0 and $\frac{1}{2}$ denote particles with spins 0 and $\frac{1}{2}$, respectively. A specific instance of such a reaction may be elastic pion-nucleon scattering, but there are many other instances also throughout particle and nuclear physics. We will first discuss this reaction in the case when rotation invariance holds.

In that case, the $M$ matrix can be written in the following form:

$$M=a_0+a_1\vec{\sigma}\cdot\vec{q}_1+a_2\vec{\sigma}\cdot\vec{q}_1\times\vec{q}_2+a_3\vec{\sigma}\cdot\vec{q}_2\,, \tag{1}$$

where $q_1$ and $q_2$ are the initial and final center-of-mass momenta, the $a$'s are the reaction amplitudes which are complex numbers depending on kinematic factors, and $\vec{\sigma}$ is the usual Pauli spin matrix. This is one of the multiply infinite number of ways of writing the $M$ matrix. From the point of view of our discussion, it makes no difference which of the ways of writing the $M$ matrix we consider, and hence this one is used since it may be familiar to many of the readers.

The amplitudes $a_i$ are functions of the rank-zero tensors one can construct from the vectors that determine the kinematics. In the present case these vectors are $\vec{q}_1$ and $\vec{q}_2$, and hence the rank-zero tensors are $q_1{}^2$, $q_2{}^2$, and $\vec{q}_1\cdot\vec{q}_2$. The fact that these three are not independent of each other is of no concern to us in the present discussion. It is important to note, however, that all three of these rank-zero tensors are scalars and not pseudoscalars.

Now let us impose, in addition to rotation in-

~~RAPID COMMUNICATIONS~~ 733  DDR231D  D1-84

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PAC NUMBERS 11.30.–j; 11.30.–Er; and 21. 03.65.–w

quotes

COMMENTS ON ~~THE NOTE~~ "POLARIZATION EXPERIMENTS AND THE ISOTROPY OF SPACE", ~~BY G. R. GOLDSTEIN AND M. J. MORAVCSIK~~.

Ruggero Maria Santilli*
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

(RECEIVED APRIL 1983)

~~Abstract~~

*A valuable and courageous note by G. R. Goldstein and M. J. Moravcsik on possible tests of the rotational symmetry under strong interactions has been recently brought to our attention. In these comments we indicate possible additional tests, as well as references on the problem that were apparently unknown to the authors at the time of writing their note.*

NOTE OF JULY 7, 1983: THIS IS AN IMPROVED VERSION.

*Supported by the U. S. Department of Energy under contract number DE–AC02–80ER10651.A002.

# PART XIX:

# PHYSICS

# LETTERS

# (CORRESPONDENCE

# WITH HOVARD

# GEORGI)

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 22, 1983

Professor R. GATTO
Editor, Physics Letters B
CERN
CH-1211 GENEVA23, Switzerland

Dear Professor Gatto,

I submit the enclosed note entitled
"Use of hadronic mechanics for the regaining of the exact space-reflection symmetry in weak interactions"
for publications in your Journal.

The paper has not been submitted to other Journals nor will be submitted during your consideration. The copyrights on the note are assigned to North-Holland Publishing Company in case of publication.

The note complies with the restrictions on length set forth by your Journal, to my understanding. If this is not the case, Physics Letters is authorized to eliminate entirely footnote 11.

For your convenience, I enclose copies of the galleys of ref.s 1b and 1c that might not be available in Geneva at this time.

Any critical remark for the improvement of the presentation would be gratefully appreciated.

I am currently working on two additional notes:
- one on the use of hadronic mechanics in Kalnay's realization to achieve a "strict confinement" of quarks (identically null probability of tunnel effects for free quarks), while leaving current quark theories virtually unchanged; and
- one on the use of hadronic mechanics to achieve convergent perturbative series when divergent at the quantum level.

In case of interest by your Journal on these efforts, it would be a sincere pleasure to submit them to you.

Very Truly Yours

Ruggero Maria Santilli

PHYSICS LETTERS B

HOWARD GEORGI
*Physics Department*
*Harvard University*
*Cambridge, MA 02138*
*U.S.A.*
*Tel: 617-495-3908*

December 13, 1983

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

This paper draws so heavily on your earlier works (which are not widely known) that it cannot be made sufficiently self contained to warrant publication in the letter format. It is not suitable for Physics Letters B.

Sincerely,

Howard

Howard Georgi
Editor

HG:pcc

enclosure

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THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
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Office of the President

December 15, 1983

Dr. H. Georgi, Editor
Physics Letters B
Department of Physics
Harvard University
Cambridge, Ma 02138

RE: manuscript # 1117

Dear Dr. Georgi,

Absolutely none of the papers you have accepted for Physics Letters B is "sufficiently selfcontained" to be understandable without a knowledge of the quoted literature, nor any letter can reach such a status these days. As a result, there exists absolutely no difference between the papers you routinely accept, and the paper submitted. In actuality, the latter paper requires the knowledge only of the literature quoted in ref. 1 [the papers printed in Lettere Nuovo Cimento], copies of which were enclosed with the original submission. Your rejection therefore has absolutely no visible scientific-editorial grounds.

Most regrettable are the implications of your rejection for a number of developments dependent on the paper submitted, such as the achievement of a strict form of quark confinement [identically null probability of tunnel effects for free quarks] via the use of Kalnay's quantization of Nambu's mechanics for the triplet case, that is emerged as being a particular realization of hadronic mechanics.

As communicated in the original letter of submission mailed to CERN, these latter developments were contemplated for submission to your journal. They are expected to constitute a primary topic of study at the forthcoming Second Workshop on Hadronic Mechanics [see copy of the announcement here enclosed]. In particular, they constitute one of the primary motivations for which the Hadronic Journal was founded.

A rejection of the paper without scientific-editorial grounds would imply a necessary revision of all these programs, for which you must assume the responsibility. Before doing that I want to give you a second, final chance of re-examining the paper and submitting it to a due scientific process. On my part I shall be glad to cooperate for all scientifically warranted revisions.

Very Truky Yours

R.M.Santilli

encls.

# PHYSICS LETTERS B

HOWARD GEORGI

*Physics Department*
*Harvard University*
*Cambridge, MA 02138*
*U.S.A.*

*Tel: 617-495-3908*

February 2, 1984

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Ruggero,

I have looked at your paper again, but I really don't know where to start trying to fix it. There are two problems, not unrelated. The first is the jargon. You have invented your own, which you have the right to do. But you have not tried hard enough to make connections to more conventional ideas. This makes the paper locally very hard to follow. The second problem is that even when the paper makes sense locally, it is not clear what is your overall plan. Unless the purpose and conclusions of the paper can be stated without reference your other works, it is not suitable as a letter.

Now let me write frankly, as a friend. I do not know whether your whole program makes any sense because I have not studied it deeply enough (although people I respect have studied it and claim that it doesn't). But I do know that if you really believe in it, then you are going about trying to convince others that it makes sense in the wrong way. Instead of basing your work on large papers full of jargon, you should start over completely from scratch. You should write a short self-contained <u>introductory</u> paper, completely free of jargon, historical references, etc.--concentrating on the physics which you are trying to address.

If you continue writing papers such as this one, you won't get anywhere. To any reader who did not already share your point of view, this paper would look like an elaborate mathematical ediface constructed out of random definitions. Of course, lots of things look like that at first which turn out to be interesting. Your paper may be one of them. But in its present form, it will only encourage readers to think that you are hiding behind jargon because you don't really have any thing to say. That doesn't do you or the readers or Physics Letters any good at all.

Sorry that I can't be of more substantive help, but I hope you will take my suggestions in the right spirit. They are well meant.

Sincerely,

Howard

Howard Georgi
Editor

HG:pcc

enclosure

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

February 7, 1984

Howard Georgi, Editor
Physics Letters
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Howard,

Please accept the sentiments of my sincere gratitude for your constructively critical comments regarding my note "Use of hadronic mechanics for the ...". Please be reassured that, when constructive and therefore performing a scientific process, I am sincerely grateful for any critical comment, no matter how harsh.

I am in full agreement with you that the letter is not suitable for publication in its current form, and needs rethinking and re-writing. However, I share only in part your view. In particular, I have difficulty is seen lack of discrimination between our scientific current [which is, by now, fully established no matter what other people say], and conventional trends when referring to the self-containing character of the letter and the absence of prior reference. If I have to do it, then exactly the same rule must be applied to, say, a paper on SU(5)!

Nevertheless, you are perfectly correct in asking that the physics to be addressed must be identified as clearly as possible. It is in this point where you can contribute significantly for a due scientific process. You are familiar with our objectives, but let's review them.

We believe that hadronic mechanics can:

(A) provide a strict confinement of quarks, that is, a theory with an identically null probability of tunnel effect for free quarks [see announcement of our second Workshop at Villa Olmo nest August];

(B) permit the identification of the quark constituents with ordinary electrons and positrons, although obeyind a generalized mechanics because of the generalized forces occurring from conditions of deep mutual penetration of their wave-packets [see the Hadronic J. Vol. 1 number 2, 1978]; and, last but not least;

(C) provide realistic hopes of re-establishing the exact character of space-time symmetries when quantum mechanically broken, via their more general Lie-isotopic formulation. Similar results are expected for internal symmetries. In particular, the conventional and isotopic symmetries result to be locally isomorphic as established for the rotational, Lorentz and parity in 1982-1983, and for SU(N) symmetry by Mignani very recently.

I believe that the paper submitted to Physics Letters should be restricted to its physical objective, as specifically identified beginning from its title. In fact, it is a mere individual link in our program. Its enlargment to include topics (A) and (B) would be inappropriate, in my view, although I might be wrong in such thinking. At any rate, an indication of aspects (A) and (B) as possibilities, prior to their actual achievement could be inappropriate.

By keeping these various aspects into consideration, I would like to re-write the letter along the following main lines

(1) Eliminate all past references, with the sole exception of ref. 3 on the Proceedings of the First Workshop on Hadronic mechanics, where the existence

and self-consistency of hadronic mechanics have been established, of course on formal grounds only, but beyond reasonable doubts;

(2) reduce the jargon to the truly, absolutely essential parts, which are three notions, those of isoenvelope, isofield and isohilbert space, by providing in footnotes information for their speedy identification in the current ref. 3; and

(3) elaborate in more detail the achievement of states with the right mixture of conventional parity nonconserving states, which is only indicated as possible in the current version via the use of the "isotopic element" of hilbert product [how can you call it with an old jargon if it does not exist?]. This last point would render truly visible the regaining of the exact P-symmetry, evidently, because the conventional and hadronic formulations would be equivalent for all practical computational needs. I might add comments on our future hopes to achieve objectives (A) and (B), but only if you advise me so.

But above all, the objective of the note is to focus the attention on the role of the unit operator which, in turn, is the true, ultimate basis for (A) and (B).

Kindly advice me whether a reworking of the note along points (1), (2) and (3) would make sense, or you would still disagree on the general lines. This would save me considerable time, and I would have additional reasons to be grateful to you.

Please feel free to call me, if you so desire. I could brief you on our progress in objectives (A) and (B).

Sincerely,

Ruggero M. Santilli

THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

February 7, 1984

Professor T.D.LEE
Columbia University
Department of Physics
NEW YORK, N.Y. 10027

Dear Professor Lee,

I would gratefully appreciate your comments for the improvement of the enclosed note entitled "Use of hadronic mechanics for the' possible regaining of the exact space-reflection symmetry in weak interactions".

The note has been submitted to Howard Georgi as editor of Physics Letters. Howard has rejected the note for insufficient maturity due to the use of excessive new jargon that is specialized in our line of inquiry, as well as insufficient focusing of the physical problem to be addressed. I agree with Howard that the note is immature in its current version and I have written him a note of sincere thanks for his constructively critical comments.

Nevertheless, I have difficulties in rewriting the note without our terminology and reference. It would be the same as asking the author of a letter in SU(5) to write it without any reference to past contributions in the field ! Similarly, I believe that the problem is fully identified in the note beginning with its title.

I was planning to rewrite the paper: (A) by eliminating virtually all references to our studies, except ref. 3 [on the Proceedings of the First Workshop on Hadronic Mechanics, where the formal, theoretical existence and consistency of hadronic mechanics has been established, I believe, beyond any reasonable doubt]; (B) by providing footnotes for the speedy identification in ref. 3 of all essential definitions [which are basically three, those of isoenvelope, isofield and isohilbert space]; and (C) working out the problem left open in page 4, to the effect that the states are indeed of the right mixture of conventional parity-nonconserving ones.

Do you think that such revisions make sense? Could you kindly express any criticism that has escaped both Howard and myself?

For your information, this note is a oreliminary steps toward a subsequent ongoing step, the proof that hadronic mechanics provides a strict confinement of quarks, that is, a true, identically null probability of tunnel effect of free quarks, while leaving the quark theory essentially unchanged.

Studies to this effect are in progress. Nevertheless, you can anticipate them from the enclosed note. In fact, the strict confinement is expected from the incoherence of the Hilbert spaces for the interior and the exterior problem, when the former is realized according to hadronic mechanics, and the latter is realized as in conventional quantum mechanics. The preservation of the quark theory as currently known is expected from the isomorphism of the conventional SU(3) and its image under isotopy.

In case you are interested in inspecting any of the existing literature, please let me know. I would be glad to let you have a complimentary copy of the Proceedings of our recent workshop.

Sincerely Yours

Ruggero M. Santilli

THIS LETTER WAS NEVER ACKNOWLEDGED.

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859

June 18, 1984

Dr. H. GEORGI
Editor for the U.S.A.
PHYSICS LETTERS
Harvard University
Department of Physics
Cambridge, Massachusetts 02138

Dear Dr. Georgi,

I must have a record of my doubts regarding the [illegible] of your recent refereeing for your journal.

YOUR FIRST REJECTION. Your rejection of my paper [1] is not credible. The paper presented a conjecture regarding a possible pulsating structure of the Coulomb law for electron pairs whose consistency has been proved beyond reasonable doubt for the nonrelativistic case. The motivation for your rejection was that the theory is not extendable to relativistic setting, in your view. This is not credible on a number of counts, such as, for instance, the fact that all known theories which are consistent nonrelativistically, have been proved sooner or later to admit a consistent relativistic extention. Besides, the job is under way. How can you claim it cannot be done before doing it? Perhaps, the true motivation of your rejection must be searched outside the pursuit of novel physical knowledge. At any rate, the paper you rejected was routinely accepted and published by a European letter journal.

YOUR SECOND REJECTION. Your second rejection is truly incredible by all standards. In substance, your letters of rejections of December 13, 1983 and February 2, 1984 state that you have rejected my paper because you have heard around in academic corridors that the hadronic generalization of quantum mechanics has no physical value. This is a sentence stated by senior physicist at your department since 1978, as you are well aware and know well from the extreme occurrences regarding my visit there in 1977-1980. The pertinent question here is the following: have you appraised the ethical standards of the colleagues you heard in academic corridors on the soundedness of the new mechanics? I do not believe you did, and there are reasons to expect you did not do it, particularly if you are finaňcially affiliated with them on grants and other matters.

The additional thing you ask is truly incredible. I am referring to your request that the paper be completely self-contained without any quotation of preceding work. It is evident that absolutely no paper you have passed for your journal has met these requirements even minimally. You therefore practice a selective kind of refereeing, with manifest leniency for certain types of conjectures aligned with your line of vested interests, and a different type of refereeing for conjectures and/or their authors outside said circle of interests. But then, under these premises, you are compelling even your best friends to enter into a severe judgment of your editorial work.

HARVARD'S APPARENT CONTROL OF PHYSICS LETTERS FOR THE U.S.A. In the name of our former friendship and associatiation, permit me to convey to you most candidly, primarily in your own interests, that the premises for your editorial post at Physics Letters are wrong. They are wrong for you in the long run. They will inevitably be wrong for Harvard, and they are definitely wrong for the printing house of your journal. I am referring to your totalitarial control of ALL publications in your journal originating in the U.S.A.

This situation is becaming more and more known in the trade, and is creating an increasing concern. It is established beyond a reasonable doubt in my case, as well as in numerous others. In fact, I did not want my second paper be refereed by you and therefore mail it to the editorial office of your journal in Geneva and, in particular, to the European editor Dr. GATTO. My failure to have the second paper considered by ANOTHER editor of Physics Letters OUTSIDE HARVARD UNIVERSITY establishes your absolute control of U.S. submissions to your journal.

This is wrong. It cannot be otherwise.

Best Regards and Good Luck!

Ruggero M. Santilli
RMS-mlw

PACS NUMBERS 03.65.–w; 036.65.Bz; 11.30.–j

# USE OF HADRONIC MECHANICS FOR THE POSSIBLE REGAINING OF THE EXACT SPACE–REFLECTION SYMMETRY IN WEAK INTERACTIONS

Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street, Cambridge, Massachusetts 02138

Abstract

It is shown that the isotopic lifting of the enveloping associative operator algebra, of the field and of the Hilbert space of quantum mechanics into those of the covering hadronic mechanics offers realistic hopes of regaining the exact space–reflection symmetry when quantum mechanically broken by weak interactions.

# PART XX:

# LETTERS

# IN

# MATHEMATICAL

# PHYSICS

LMP LETTERS IN MATHEMATICAL PHYSICS

*A Journal for the Rapid Dissemination of Short Contributions in the Field of Mathematical Physics*



*Postal address:*
Physique Mathématique
Université de Dijon, B.P. 138
F-21004 Dijon, Cédex (France)

M. GASPERINI
Istituto di Fisica Teorica
Universita di Torino
Corso M. D'Azeglio 46
10125 TORINO ITALY

Dijon, March 19, 1984

Dear author,

Your paper, entitled : "Lie osotopic lifting of general relativity" has been examined by one of our referees, who made the following remarks :
"This paper should be submitted to the Hadronic Journal because, it is based on the idea of "Lie isotopic generalization of Lie theory" developed in that Journal and incomprehensible to those who do not study the papers of R.M. Santilli".

Unfortunately, in view of these remarks, we cannot accept your paper for publication in LMP.

Sincerely Yours,

J.C.CORTET

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859

May 23, 1984

Dr. J.C.CORTET,
Editor
LETTERS IN MATHEMATICAL PHYSICS
Physique Mathematique
Universite de Dijon
DIJON, France

Dear Dr. Cortet,

I hereby respectfully submit for consideration by your journal the enclosed letter in three copies entitled

"Use of hadronic mechanics for the possible regaining of the exact space-reflection symmetry in weak interactions".

The note is not under consideration at other journals, nor it will be submitted to other journals during your consideration process. The copyrights of the letter, if published, will be granted to your journal.

Very Truly Yours

Ruggero M. Santilli

RMS-mlw

encls.

LMP LETTERS IN MATHEMATICAL PHYSICS

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*Postal address:*
Physique Mathématique
Université de Dijon, B.P. 138
F-21004 Dijon, Cédex (France)

Professor R.M. SANTILLI, Editor in Chief
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
CAMBRIDGE, Mass. 02138 USA

Dijon, June 22, 1984

Dear Professor Santilli,

Thank you for your letter dated May 23.
I am able to assure that the competency and the integrity of the referee are not suspicious.
I submitted your comments and your paper to the editorial staff of LMP.

Unfortunately his decision is to not consider it for publication in our journal.

Sincerely Yours,

J.C.CORTET

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138, Tel. (617) 864-9859

July 18, 1984

Dr. J. C. CORTET
Letters in Mathematical Physics
Physique Mathematique
Universite de Dijon, B.P. 138
F-21004 DIJON CEDEX, FRANCE

Dear Dr. Cortet,

I acknowledge receipt of your letter of June 22 declining the consideration of my paper "Use of the hadronic mechanics for the possible regaining of the exact space-reflection symmetry in weak interactions."

Unfortunately, facts speak for themselves:
1) the paper was particularly suited for your letter journal;
2) you declined consideration of the note; and
3) your declination was done via an absolute and total lack of any scientific content.

These facts point quite clearly toward mumbo-jambo academic politics as the most plausible explanation of the occurrence.

I shall reserve the option to disclose publicly and internationally all the correspondence on this case at the time I consider it most appropriate.

Very Truly Yours

Ruggero M. Santilli
RMS-mlw

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