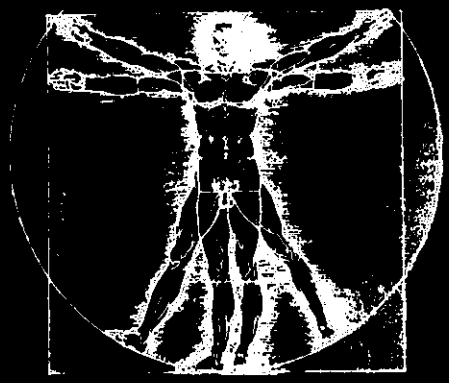
*VOLUME*

*I*

# DOCUMENTATION

*OF*

*IL GRANDE GRIDO*



Ruggero Maria Santilli

# DOCUMENTATION
# OF
# *IL GRANDE GRIDO*

*Volume I*

**Ruggero Maria Santilli**

**— 1984 —**
**Alpha Associates**
**Rome, Italy**

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# DOCUMENTATION
# OF
# IL GRANDE GRIDO
# VOLUME I

by

Ruggero Maria Santilli

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# PART I:

# HARVARD

# UNIVERSITY

PART IA:

ACADEMIC
YEAR
1977–
1978

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS

October 24, 1977

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Fellow in Physics

to serve from September 1, 1977 to June 30, 1978 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

*Secretary to the University*

Ruggero M. Santilli

STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305

DEPARTMENT OF PHYSICS

December 28, 1976

Dr. Ruggero Maria Santilli
Center for Theoretical Physics
M.I.T.
Cambridge, MA 02138

Dear Dr. Santilli:

Your papers look very interesting, and I will be very glad to meet with you to discuss these matters during your trip to California. If you call me when you arrive, I'm sure we will be able to arrange a convenient time to meet. My office phone number is (415) 497-2687.

Yours very truly,

Steven Weinberg

SW/dal

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 23, 1977

Prof. Ruggero Maria Santilli
Room 6-405A
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear Professor Santilli:

Thank you for your resume. We will almost certainly be able to offer you a position as an Honorary Research Fellow. The department does not meet again as a group until September, so I cannot say anything official, but we do not expect any trouble with your appointment. We look forward to having you in our group.

If you have any further questions, please contact me.

Howard M. Georgi

Howard Georgi

HG/dt

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

July 1, 1977

Professor Ruggero Maria Santilli
Room 6-405A
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Santilli:

I am writing just to let you know that it has been possible to get a vote of this department, so that your position as Honorary Research Fellow is now official.

I look forward to getting to know you next fall.

Sincerely,

Steven Weinberg
Higgins Professor of Physics

SW/bm

September 5, 1977

Professor S. WEINBERG
Harvard University

Dear Steven,

I would gratefully appreciate your help in regard to the recent governmental invitation for supporting my research. Permit me to indicate, if at all needed, that I have no aim of remaining at Harvard. I am essentially interested in having Harvard administer the contract the first year and then move it to another campus where I have possibilities of tenure. Your help in the administrative steps to bring the invitation into a reality would be gratefully appreciated. As you know: I am currently unemployed; I have two children in tender age to feed and shelter; my wife is a graduate student; our savings are non-existent; and the unemployment benefits last only a few weeks.

Sincerely yours

V Ruggero

Harvard University, Lyman Laboratory
Informal Seminar Course

by Ruggero Maria Santilli

September 1977

| | |
|---|---|
| Title | **The Inverse Problem in Newtonian Mechanics and Field Theory** |
| Objective | a study of Lagrange's and Hamilton's equations |
| Means | 1. integrability conditions for analytic representations of systems with couplings not necessarily derivable from a potential,<br>2. methods for the computation of a Lagrangian or, independently, of a Hamiltonian, when they exist, from given arbitrary systems,<br>3. outline of the applications to Newtonian mechanics, space mechanics, optimal control theory, plasma physics and classical field theory. |
| Prerequisites | knowledge of mechanics and differential equations |
| Duration | 25 lectures of 1.5h each |
| Organization | (a) 50% of time dedicated to the methodology of the Inverse Problem for ordinary differential equations (Newtonian mechanics, etc.),<br>(b) 30% of time dedicated to the extension of the methodology to partial differential equations (field theory),<br>(c) 20% of time dedicated to applications and open problems,<br>(d) several illustrative examples will be either worked out in class or distributed,<br>(e) a number of homeworks will be assigned. |
| Reference | a limited number of copies of two forthcoming monographs on the Inverse Problem by R.M. Santilli (to be published by Springer-Verlag) will be made available. |
| Outline | enclosed in a tentative form |
| Organizational Meetings | Time: 4:30 p.m., October 3, 1977. Place: Room 267 (alternatively, the interested persons can contact R.M. Santilli at Room 437, Lyman Lab, Harvard University, office 495-3212, home 969-3465). |

# The Inverse Problem in Newtonian Mechanics and Field Theory

## Course Outline

**1. Introduction.** Significance of systems with arbitrary couplings in Newtonian Mechanics, Space Mechanics, Optimal Control Theory and other disciplines. Need for the applicability of known analytic methods to the broader systems considered. Identification of the Inverse Problem.

**2. Elemental Mathematics.** Review of the existence theory for implicit functions, solutions and derivatives in the parameters. Review of the calculus of differential forms, the Poincaré-Lemma and its converse. Review of the calculus of variations.

**3. Variational Approach to Self-Adjointness.** Equations of variation, adjoint system, conditions of self-adjointness for quasi-linear second order systems. Reduction to first order systems and extension to arbitrary order.

**4. Lagrange's and Hamilton's Equations.** Hilbert Differentiability Theorem and basic properties. Equations of variation of Lagrange's and Hamilton's equations and their properties. Self-adjointness of Lagrange's and Hamilton's equations. Characterization of the ordered analytic representations of arbitrary systems.

**5. Fundamental Theorems.** Theorems on the necessary and sufficient conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. Methods for the computation of these functions from the given equations of motion. Analysis of their structure from the viewpoint of the interactions and classification of the admissible couplings.

**6. Application to the Newtonian Transformation Theory.** Algebraic and geometrical significance of the conditions of self-adjointness. The new class of equivalence transformations of a Lagrangian characterized by its integrability conditions (isotopic transformations).

**7. Application to Symmetries and First Integrals.** Noether's theorem, its inverse and its generalization to higher orders. Use of the isotopic transformations for the computation of new first integrals. The concept of isotopically related symmetry groups, algebras and brackets.

**8. Extension of the Inverse Problem to Field Theory.** Conditions of variational self-adjointness for quasi-linear partial differential equations and relation to the conventional concept of self-adjointness in linear spaces. Fundamental theorems on the necessary and sufficient conditions for the existence of a Lagrangian density and its construction. Analysis of its structure and the relation with chiral Lagrangians. Application of the methodology to the transformation theory.

**9. Outline of Applications.** Electric circuits inclusive of losses. Nonlinear nonconservative plasma equations. Spinning top with drag torques. Space mechanics with drag forces for interplanetary dust. A missile trajectory problem. Nonconservative equations in continuum mechanics. Variational analysis of the Weinberg-Salam model of unified gauge theories of weak and electromagnetic interactions and the problem of its extension for the inclusion of strong interactions.

Springer
Springer-Verlag
Berlin Heidelberg New York

Dr. R.M. Santilli
Department of Physics
Jefferson Physical Laboratory
Harvard University
Cambridge, MA 02138

USA

October 11, 1977
WB/mc 2153

Dear Dr. Santilli,

Mr. Borsodi from Springer-Verlag New York sent me copies of your manuscripts: "The Inverse Problem in Newtonian Mechanics" and "Generalizations of the Inverse Problem in Newtonian Mechanics." He also sent me some information on Volume III, "The Inverse Problem in Field Theory," and your outline of September 1st referring to Volumes IV and V.

As editor of the series "Texts and Monographs in Physics" publihsed by Springer-Verlag, I think that at least parts I and II of your manuscript should be published. The opinions of various referees have been rather favorable and some of them even feel that your work might become a kind of standard reference on the subject. At the moment, almost nothing can be said about Volumes III-V and so I propose to offer you a contract for Volumes I and II only with the understanding that we will publish the following ones if they, too, stand up to the criticism of our referees.

In the case that you have any questions, please contact Mr. Borsodi in New York as you have in the past. He will also discuss the details of the contract with you. If you have any problems concerning the scientific part of your book, please do not hesitate to write to me.

Sincerely yours,

Dr. W. Beiglböck
(absent after dictation)

cc: Mr. Borsodi

Springer-Verlag KG
Postfach 105280
Neuenheimer Landstraße 28-30
D-6900 Heidelberg 1

Telephone: (06221) *487-1
Extension: (06221) 487-.....
Telex: 04 [illegible]
Cables: Springerbuch

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TO: Professors S. Weinberg, M. Tinkham and H. Georgi October 20, 1977
FROM: R.M.Santilli
SUBJECT: Second progress report [NOTE OF 5/25/1984: THE FIRST REPORT IS MISSING FROM THE FILE]

I am now in a position of informing you of my studies on strong interactions as promised in my first report. In essence, I have been interested since the time of my graduate studies at the University of Torino (1963-1966) in studying the old idea that strong interactions are not derivable from a potential (e.g., Enrico Fermi) but with particular reference to the problem of the hadronic structure.

I have prepared for your consideration five highly condensed summaries of my efforts which are here enclosed. Besides submission, these papers have not been released. As for my first Harvard paper, my status is identified in the acknowledgments.

These are my first papers on the hadronic structure despite the fact that I have been literally working at the problem for over a decade. I think you might be interested at the reasons for such a delay as well as know in more detail my research program.

Monographs. Soon after the decision to study the problem indicated, I realized that the methods for the treatement of (local) forces not derivable from a potential were virtually ignored in the existing literature (of 1963-1964). It still is as of today. I therefore decided to undertake a long term and laborious program aiming at the identification of the methods needed for these forces.

As you know, I have worked since 1973 at three monographs on the so-called Inverse Problem of Analytic Mechanics (two monographs for the Newtonian aspect and one on the field theoretical aspect). They essentially identify the integrability conditions for the existence of analytic representations with conventional Lagrange's and Hamilton's equations (without external terms) of system with local but arbitrary couplings, the methods for the computation of these functions and a study of the significance of the methodology for other aspects (e.g., transformation theory, symmetries and first integrals for systems with arbitrary local couplings).

You will be pleased to know that these monographs have been officially accepted for publication by Springer-Verlag in their series "Monographs in Physics" under the title "Foundations of Theoretical Physics", Volumes I, II and III. This is a result of over one year of inspection of the manuscripts by European, US and USSR experts which appeared advisable from the novelty of the presented analysis. Regrettably, I had to decline offers from US publishers for several reasons.

This does not mean instant publication, as we know. And indeed I intend to release the manuscripts for publication only when I am sure that I have done a fine job. The informal seminar course I am currently delivering on these techniques is proving invaluable for my reaching the utmost possible maturity.

My true interest, however, is not the Inverse Problem per sé. The line of study in which I am actually interested is the so-called Lie-admissible problem. It essentially consists in the study of the same systems of the Inverse Problem (those with forces not derivable from a potential) but this time with the equations originally conceived by Lagrange's and Hamilton's, those with external terms. The intriguing aspect is that the presence of these external terms induces a covering of current analytic mechanics at all its level, analytic, algebraic, geometrical etc. For an outline

of my laborious efforts see Tables 3-7 of the enclosed note 4. You might be interested to know that the study of the Inverse Problem became mandatory in 1973 for a central technical difficulty I had identified at that time(and subsequently solved)in the construction of a genuine Lie-admissible covering of Lie's theory.

Springer-Verlag is aware of these efforts and interested in publishing them as Volume IV of my series if I have the time of organizing my published, submitted and unsubmitted notes which I have accumulated through the years.

Despite the availability of these new techniques, I was still unable to confront the contruction of an actual model of structure of the hadrons based on more general forms of the hadronic forces owing to the need (for technical reasons which you can identify in the enclosed papers) of estending the methodology to systems with generally nonintegrable subsidiary constraints. This called for a third stage of labor.

In 1976 I reached sufficient maturity to deliver a seminar course on the methods for the treatement of local systems with arbitrary forces and subsidiary constraints. My lecture notes are available in an untyped form. The essential aspects of these notes are: (a) the study of arbitrary constrained systems with the most poweful analytic tools known to me: the integrability conditions for the existence of Lagrangian or Hamiltonian formulations but this time reformulated on the hypersurface of the constraints, (b) the embedding of the currently available constrained systems of Dirac type into a more general class of Bolza type (as available from the Calculus of Variations and of particularly intriguing significance for short range, rather than electromagnetic, interactions) and (3) the identification of a yet broader class of the system considered (local, constrained, nonselfadjoint) which is only directly treatable with my Lie-admissible techniques.

Springer-Verlag is again interested in publishing my notes as Volume V of my series, provided that some time in the future I have the time to finilize them.

I should add that in all these manuscripts (for a total of some 4,000 pages), the terms "strong interactions" are ignored. The techniques are presented for their arena of unequivocal applicability: Newtonian Mechanics and Continuum Mechanics.

Research. Once the technique for the classical treatement of forces not derivable from a potential were sufficiently clear in my mind, I initiated my efforts aiming at the identification of the rudiments of their quantization. The use of Lie-admissible techniques turned out to be the most intriguing research topic in which I had been involved. Only after I had achieved such rudiments, I was finally able to confront the problem I had decided to attack a decade earlier: a tentative construction of a structure model of the hadrons with forces not derivable from a potential. The enclosed papers present a concise summary of my latest efforts.

It is an easy prediction that your inspection of these papers will demand your best patience, scientific vision and genuine interest in basic research. The reason is that, irrespective of a significant number of alternative presentations I had considered, the central nature of the research remained highly delicate:

the study of whether established relativity and quantum mechanical laws are applicable or not within a hadron. Neverthless, for the arguments presented in the papers, I believe that this problem must be seriously confronted sooner or later.

I would like also to add that at an initial inspection the content of these papers appear as incompatible with current trends in hadronic physics. In my opinion this is not the case. For instance, one of my central objectives is the study of the generalization of the Weinberg-Salam model of unified theory of weak and electromagnetic interactions with the inclusion of strong interaction as local couplings not derivable from a potential (technically this would be an SU(2)xU(1)-admissible embedding/breaking of the SU(2)xU(1) structure). I should also add that, since I had already worked out (and published) an SU(3)-admissible embedding/breaking of the SU(3) structure in 1966 with very intriguing results, I could have published preliminary studies on the indicated generalization of the SU(2)xU(1) structure a number of years ago. In my opinion, this would have been simply a scientific cheat. The reasons is that the use of local couplings not derivable from a potential either as a generalization or as a symmetry breaking mechanism of existing models has, in my opinon, profound methodological implications which are not transparent in Lie-type tools, but which are directly expressed by Lie-admissible tools. A central objective of the enclosed papers is to identify possible methodological implications of the forces considered even before considereding whether to study the Lie-admissible embedding/breaking of existing unitary gauge models.

Almost needless to say, the amount of efforts I have put in these studies has no bearing on their maturity. I still need a long way to cover to achieve even a preminary maturity.

<u>ERDA support</u>. ERDA has followed me through several years in these efforts. As you know, ERDA is now interested in supporting these studies, provided that I have Harvard sponsorship (an initial consideration for Boston University sponsorship was subsequently declined).

Therefore, I am hereby applying for <u>any</u> position of your choice at your Department which allows me to apply for an ERDA research grant either as principal investigator or, at least, as co-investigator.

In closing I would like to add that, despite my best efforts, no financial support from any other US campus is even conceivable (this is, in essence, an indication of the status of basic research in the USA). Despite my best dedication to this Country, and since I have a family of four to support, the decision which I must take in the near future is whether to continue my studies in the USA or accept an offer from a European institution interested in both the experimental and theoretical implications of my studies. Your solicit action on my request would be gratefully appreciated.

Office No. 495 3212, Home No. 969 3465. Sincerely R M Santilli

c.c: Professors S.R.Coleman, S.L.Glashow, A.M.Jaffe, R.V.Pound and C. Rubbia;
Drs. J.S.Kane, B.Hildebrand, W.A.Wallenmayer and D.C.Peaslee (ERDA).

I have a number of reviews of my studies by collegues. As an indication of their assessement I enclose a review letter by Abner Shimony (now at the Univ. of Geneva).

July 25, 1977

Dear Ruggero,

I have spent the last three evenings reading the papers which you sent me, which of course is not time enough to do even partial justice to them. But at least I know much better what your program is, and how the parts of your work which I have examined before fit together. I see, in particular, what the long range goal of your monograph on mechanics was. I can only say that I am immensely impressed by what I have read. You have taken on one of the major physical problems of our times, you have proposed an unconventional but reasonable solution to it, and then -- and this is most impressive -- you have developed the intricate and wide-ranging array of tools needed to explore the consequences of your hypothesis. If your hypothesis turns out to be correct, you will have made a major contribution to human knowledge and you will be a very famous man. But even if your hypothesis does not turn out to be the correct explanation of hadron structure, you will have done great service developing these tools. For example, your proof that dynamical equations in situations where the forces are not derived from a potential require a Lie-admissible structure seems to me to be a great contribution to the understanding of classical mechanics. I am convinced that Lie-admissible algebras will be one of the powerful standard tools of theoretical physics, as your footnote on p.94 of paper VII (on the Gell-Mann-Okubo mass formula indicates). You have succeeded in beautifully unifying algebraic, analytic, and geometrical treatments of mechanics, which also contributes to an understanding of the subject. Incidentally, the amount of mathematical and physical literature which you had to master in order to do this seems to me truly astonishing.

Now let me comment on the physical plausibility of your main idea. The methodology of seeking a covering theory to current quantum mechanics and relativity theory in order to explain the physics within the hadrons seems to me thoroughly plusible. There is no a priori reason why concepts which have been succesful on the atomic and nuclear level should be ~~XXXXXX~~ equally successful within the hadrons.

Is it possible that a complete and detailed explanation of hadron structure requires a microscopic treatment of space-time, and that your hypothesis -- the role of a force not derivable from a potential -- is a kind of phenomenological consequence of the correct microscopic space-time theory? This way of looking at things would throw light upon one of the important considerations in your theory: that invariance under the Poincaré group holds for phenomena involving the hadron as a whole though not for its components. For deviations from Einstein-Minkowski space-time could be expected inside the hadron, but would be rathersmall outside it (except in very condensed states of matter, such as neutron stars).

I am delighted that you will be Weinberg's guest next year. Not only does that help you out for the coming year, but it may open doors for the future. He is knowledgeable enough to appreciate what you are doing, and has enough prestige that a recommendation from him may obtain a permanent position for you. What you have accomplished is so impressive that you deserve a great award.

With best wishes

[redacted]

RESEARCH GRANT PROPOSAL SUBMITTED TO THE NATIONAL SCIENCE FOUNDATION

Proposed Amount: $ 99,212; Proposed Effective date: Jan. 1, 1977
Proposed duraction: 24 Months

Title: NECESSARY AND SUFFICIENT CONDITIONS FOR THE EXISTENCE OF A LAGRANGIAN IN NEWTONIAN MECHANICS AND IN FIELD THEORY

Principal Investigator: Ruggero Maria Santilli
Social Sec. No.: 032-46-3855

Submitting Institution: Boston University
Department: Physics
Branch: Graduate School
Address: 111 Cummington St, Boston Mass. 02215

Endorsements:

Principal Investigator

Name : Ruggero Maria Santilli

Signature: [signature]

Title : Associate Professor

Tel. No. : (617) 353 2187

Department of Physics

Name : George O. Zimmerman

Signature: [signature]

Title : Chairman

Tel. No. : (617) 353 2623

B.U. Administrative Official

Name : _____

Signature: _____

Title : _____

Tel. No. : _____

TO: Professors S. Weinberg, M.Tinkham and H. Georgi December 4, 1977
FROM: R.M.Santilli
SUBJECT: Last progress report

As you have eventually noted, the five short papers on my independent studies on the problem of the hadronic structure I gave you with my second report of October 20, 1977, were highly condensed. In essence I prepared them to let you have a full knowledged of my current research activities in the shortest possible time. Since these notes tentatively bear the name of your department, no copy has been circulated besides those as stated in my second report.

What I did circulate rather widely and in a confidential way prior to my arrival at your department was a series of papers (for over 1,000 pages) giving full technical details of my studies. Predictably, the publication of this series turned out to be practical impossible, despite the acceptance of the technical content by referees, simply because their publication costs would have exceeded $ 25,000.

I therefore submitted this series of papers for publication as monographs several months ago. You might be interested to know that these monographs have been accepted for publication (with quite encouraging referee's reports) by Hadronic Press, Inc., Nonantum Massachusetts 02195 (a new press recently organized by local physicists) according to the following reorganization of the material:

"Lie-admissible approach to the Hadronic structure",
Volume I: "Nonapplicability of the Galilei and Einstein relativities", 537 pages. This is a technical treatement, by using the methodology of the inverse problem, of the content of the note # 2 (for 10 pages) in your possession concerning the possible inapplicability of the established relativity ideas to strong interactions in general and the strong hadronic forces in particular when assumed as local but not derivable from a potential.

Volume II: "Covering of the Galilei and Einstein relativities", 487 pages. This is a technical treatement, by using this time the complamentary Lie-admissible problem, of the content of the note # 4 (for 12 pages) in your possession concerning my efforts aiming at the identification of the Lie-admissible covering of established relativity laws for the strong hadronic forces assumed as local but nonderivable from a potential. The volume also contains the basic ideas of the Lie-admissible embedding/breaking-generalization of current symmetry breaking models.

Volume III: "Identification of the hadronic constituents with physical particles", 343 pages. This is a technical treatement of the content of the note # 5 (for 26 pages) in your possession. It deals first with my rudimentary efforts aiming at the quantization of local hadronic forces nonderivable from a potential by using the methodologies of the inverse problem and the Lie-admissible problem. An explicit structure model of the light hadron is then constructed and the predictions compared with the available experimental data. Finally, the volume touches on the problem of the future experimental orientation of high energy physics, with particular reference to the problem of the validity or invalidity of Pauli's exclusion principle and Einstein's special relativity within a hadron, which appears to be needed to provide a physically effective selection among an ever increasing number of models on the hadronic structure.

Volume I will be printed soon. Volume II will be released in spring 1978 and Volume III will be released subsequently. Hadronic Press, Inc. is currently preparing a brochure on the appearance of these volumes for world wide distribution, with comments by a number of scientists.

I have managed to have a copy of Volume I made at my expenses for Howard Georgi. I would be happy to provide copies to any interested collegue but, quite candidly, I do not have the money to do it at my expenses.

It is appropriate here to stress that these monograph, by no means, are intended to indicate that my approach to the hadronic structure is better than others. This comparative study is entirely left to interested independent researchers. Their objective is merely to provide a report on the results of my laborious and solitary journey which, as you know from my report of October 20, lasted for over a decade: the current state of the art on the hadronic structure is such that other approaches are equally conceivable at this time.

I am, of course, fully aware that the content of these monographs is contrary to the rather vast financial enterprise which has been constructed through the years by the governmental-academic complex on the concept of quark and related unitary models. Neverthless, their finalization is, for me, a question of scientific integrity which is, in this instance, contrary to a career oriented attitude. To be quite candid on this point, I believe that the problem of the hadronic structure must go beyond the interests of currently supported groups of research and, to have a well balanced community of researchers genuinely dedicated to basic studies, all conceivable alternatives must be investigated and subjected to a comparative confrontation with the physical reality. The current restriction of the studies on the hadronic structure to only these trends which are supported by governmental agencies and their referees, in my opinion, is contrary to the spirit of basic research.

I hope that these remarks will not be misrepresented, but simply accepted with benign patience and understanding of my "stato d'animo" due to the extreme difficulties which I have found in the conduction of these studies in this Country.*) I hope, however, that you are by now aware that these difficulties have strenghtened, rather than weakened, my determination in their conduction.

The finalization of my monographs on the Inverse Problem with Springer-Verlag is proceeding on schedule. The first volume will be released on January 1978 and the second volume will be finalized during the summer of 1978..

The remaining period of my stay at your department will be devoted to the finalization of these projects. Therefore, I see no need of further progress reports.

In closing, I would like to note with regret that my application of October 20, 1977 for "any" (not necessarily supported) position which allows me to apply for a research grant, has remained unacknowledged. I am therefore proceeding toward the acceptance of a position from a receptive European Institution.

c.c.: Professors S. Coleman, R. Glauber, S. Glashow, A. Jaffe, R. Pound and C. Rubbia.

---

*) Up to the current status of being unemployed with a family of four and wife at the graduate school.

STATEMENTS PRINTED BY
HADRONIC PRESS INC
on promotional leaflets regarding
the research monographs

*Lie–admissible Approach to the Hadronic Structure*

*Vol. I: Nonapplicability of the Galilei and Einstein's Relativity? (1978)*

*Vol. II: Coverings of the Galilei and Einstein relativities? (to appear)*

*Vol. III: Identification of the Hadronic Constituents with Physical Particles? (to appear)*

**Ruggero Maria Santilli**
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138

*"The monographs by Professor Santilli constitute a landmark in the study of the inverse problem in classical mechanics and field theory emphasizing its effectiveness and the significance of the underlying methodology for the problem of interactions."*
*R. MERTENS, Professor of Theoretical Mechanics and Director, Instituut voor Theoretische Mechanica, Rijksuniversiteit, Gent, Belguim.*

*"Professor Santilli's undertaking is a courageous quest which demonstrates endless enthusiasm and profound familiarity with a vast and generally neglected literature."*
*P. ROMAN, Professor of Physics, Department of Physics, Boston University, Boston, MA, USA*

*"Professor Santilli's monographs systematically and imaginatively explore the hypothesis that the hadronic interactions are not derivable from a potential. In the course of his exploration he makes important investigations on the necessary and sufficient conditions for the existence of a Lagrangian and of the properties of Lie–admissible algebras. He has possibly developed a very fruitful new approach to hadronic structure and he has certainly made great contributions to the mathematical methods of theoretical physics."*
*A. SHIMONY, Professor of Physics and Phylosophy, Boston University, currently visiting The Département de Physique Théorique of the Université de Géneve, Switzerland.*

December 13, 1977

Professor Shlomo Sternberg
Chairman
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Dear Professor Sternberg,

This is to acknowledge my appreciation for your sponsorship of the research proposal with ERDA. I would also like to indicate that in the case that the proposal is funded for a two year period, the Department of Mathematics at Harvard University, you and/or any of your colleagues would in no way be accountable for either a continuation of my stay or my relocation after June 30th, 1979.

Very truly yours,

Ruggero Maria Santilli

RMS/mjm

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

JEFFERSON PHYSICAL LABORATORY
CAMBRIDGE, MASSACHUSETTS 02138

December 14, 1977

Dr. R. M. Santilli
Physics Department
Harvard University

Dear Dr. Santilli:

I am writing to acknowledge receipt of your progress reports and to indicate why I had not made any earlier response.

I had brought your request before our faculty on several occasions. However, none of them felt that your work was close enough to their active research interests to allow them properly to serve as Principal Investigator. And, I am sure that you have had the Harvard rules made clear to you which allow research grants to be requested in Harvard's name only by those holding substantial academic appointments, of a level which we could not offer to you. Such appointments must be reviewed and approved by the Dean of the Faculty, and they are made only for normal academic reasons.

When last I had brought up your case, I was told that there was a good chance the Professor Sternberg of Mathematics might feel able to serve as Principal Investigator. We all thought that that might form a satisfactory resolution of the difficulty, and accordingly I thought it appropriate to defer my response while that possibility was being explored.

Sincerely yours,

M. Tinkham

M. Tinkham
Chairman

MT/ew
cc: H. Georgi, S. Weinberg, R. V. Pound

January 15, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I am in the process of releasing the enclosed material related to my monographs with Springer-Verlag and Hadronic Press.

Please inspect the expressions of appreciation for your hospitality I have included in the acknowledgments and feel free to recommend any modification you desire.

Best Personal Regards

Ruggero Maria Santilli

c.c. Professors S. R. Coleman, H. Georgi, S.L.Glashow, R. J. Glauber, A. Jaffe and S. Weinberg.

P. S. I also enclose copy of the announcement which has been recently distributed concerning the organization of the Hadronic Journal.

# Acknowledgments

I simply have no words to express my gratitude to A. Shimony. It is a truism to say that, without his encouragement, support, and advice, this work would not have been completed.

I would also like to express my sincere gratitude to A. C. Hurst, S. Shanmugadhasan, and P. L. Huddleston for carefully reading an earlier version of the manuscript and for numerous suggestions.

I am also sincerely grateful to my graduate students J. Eldridge and A. Sen for carefully studying an earlier version. Their various suggestions were very valuable in the overall improvement of the manuscript.

Correspondence on several historical or technical points with H. Corben, P. Dedecker, D. G. B. Edelen, G. M. Ewing, A. Harvey, P. Havas, R. Herman, M. R. Hestenes, C. B. Morrey, L. A. Pars, E. G. Saletan, D. C. Spencer, P. Stehle and K. R. Symon must be acknowledged.

Special thanks must be acknowledged to:

R. Mertens, E. Engels, F. Cantrijn, and W. Sarlet. Their frequent assistance has simply been invaluable for the entire project.

H. Rund and D. Lovelock. Their advice on the use of the calculus of differential forms has been invaluable for the proof of the main theorems.

A. Thellung and J. Kobussen. Their suggestions and comments have also been invaluable.

L. Y. Bahar and R. Brooks. Their critical reading of the manuscript has been invaluable.

C. N. Ktorides. His penetrating critical comments have been equally invaluable.

It is a pleasure to thank F. E. Low, H. Fesbbach, and R. Jackiw for their hospitality at the Center for Theoretical Physics of the Massachusetts Institute of Technology in 1976–1977, where part of this project was conducted.

It is a pleasure to thank S. Weinberg, M. Tinkham, and H. Georgi for their hospitality at the Lyman Laboratory of Physics, where this project was completed. In particular, I would like to express my appreciation for the opportunity of delivering an informal seminar course on the Inverse Problem at the Lyman Laboratory during the fall of 1977, which proved to be invaluable for the finalization of this project.

D. Nordstrom gave generously of his time and experience in assisting with the editorial preparation of the manuscript.

I would like also to express my gratitude to the Editorial Staff of Springer-Verlag for their invaluable assistance in the finalization of this project.

Almost needless to say, I am solely responsible for the content of this volume, including several modifications implemented in the final version of the manuscript.

It is a pleasure to acknowledge support from the U.S. Department of Energy under contract no. ER-78-S-02-47420.A000.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 2, 1978

Dean RICHARD G. LEAHY,

Harvard University

Dear Professor Leahy,

You will be pleased to know that my research grant application to the Department of Energy (formerly ERDA) with Professor Shlomo Sternberg as principal investigator has been accepted by the High Energy Physics Division of DE and sent to the DE Administrative Office with a recommendation for funding. Copy of a letter in this respect is enclosed.

Subsequently, the DE Administrative Office entered in contact with Ms. Cheryl Peyton, ORC, for the finalization of the procedures. A revised front page and budget of the contract has been prepared as per DE specification. A copy is enclosed.

I would appreciate the courtesy of setting up an account number on my name and sending it to Ms. Cheryl Peyton for approval. This is needed so that I can receive a salary beginning from April 1, 1978.

Best Personal Regards

Ruggero Maria Santilli
honorary research fellow

c.c.: Prof. M. Tinkham
Ms. C. Peyton

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

April 6, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University
Cambridge, Ma 02138

Dear Professor Tinkham,

I am here respectfully applying for a change in my current status of honorary research fellow to research associate from March 1, 1978 until May 31, 1979.

As you eventually know, the formalities for the research contract with DOE have either been concluded or are in the process of being concluded, according to the enclosed material. This change in title is apparently needed so that I can receive a salary from this contract (I have been told that a "honorary" position does not allow reception of salary).

On more specific grounds, it appears that I need an appointment as research associate from March 1, 1978 until May 31, 1979 (which is the duration of the contract) at a salary of $ 30,000.00 payable in monthly installements of $ 2,000.00 as per approved budget (copy is enclosed) to be charged to the Harvard code no. 33|966|7131-2 (as per enclosed letter from ORC).

I would appreciate the possibility of keeping my desk in room 437, Lyman Laboratory, for the same period of time. All expenses (xerox, etc.) will however be charged to the DOE contract. Therefore, I do not expect direct expenditures on my behalf. The monthly accounting procedures for non-salary expenses will be administered by the Department of Mathematics.

Almost needless to say, my request is for a temporary, terminal, research appointment. Under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after May 31, 1979 or for my relocation.

Very Truly Yours

Ruggero Maria Santilli

C. C.: Professor S. Sternberg, Dept. of Math.,
Ms. Cheryl Peyton, ORC

**HADRONIC JOURNAL**
NONANTUM, MASSACHUSETTS 02195, U.S.A.
A Publication of
HADRONIC PRESS, INC.

Ruggero Maria Santilli
Editor In Chief

April 15, 1978

Professor S. COLEMAN,
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Sidney,

The enclosed manuscript has been reviewed or is currently under review by a number of collegues (physicists and mathematicians). The general consensus is quite encouraging for publication. Some collegue is even enthusiastic (e.g. A. Shimony, as you know). I have received numerous critical comments of details for the finalization of the manuscript which I will take in full account in the final version (jointly with the due additional acknowldegments).

Rather than being pleased, I am somewhat dissatisfied (on scientific grounds) because I did not received truly penetrating CRITICAL remarks, that is, candid, open, strong criticism on the conceptual foundations of the conjectured covering of the Galilei relativity.

Owing to this situation, I would be truly grateful whether you can inspect the conceptual lines of the project and let me know your critical remarks in any form you prefer (verbal or otherwise). You can rest assured that your possible assistance will meet with my utmost discretion as well as sincere gratitude. Without doubt, this is one of the most delicate research steps of my academic life for which I need all the qualified advice I can get. The manuscript is supposed to be released for printing on May 1, 1978.

In relation to the stile of presentation, let me indicate that it is the result of numerous draftings and redraftings. By specific intent the paper is written in a form to be provocative in the hope to stimulate a scientific debate (hopefully, also in the interest of the Hadronic Journal). However, I discovered that it is difficult to be provocative in a moderate, well balanced way. Your advice whether the current stile need serious changes would be very much appreciated. I am here referring to Sections 1, 3.9 and 5.

In relation to the content, please take into consideration that one of the objectives of the Hadronic Journal is that of being as far as possible detached from the sea of minute incremental papers or of stuffy research which, in my opinion, has now reached suffocating proportions. To be candid, it appears that the courage, imagination and vision of the founders of contemporary theoretical physics is simply dead these days. What I see instead, with the due exceptions, is a pletora of followers resisting even the consideration of fundamental issues. One of the objectives of the Hadronic Journal is to stimulate new ideas which, as such, are purely conjectural. But this, of course, cum grano salis: the conjecture must have a well defined degree of plausibility. Along these lines, the content of the enclosed paper is purely conjectural. What I would appreciate is some assistance in the evaluation of the degree of plausibility.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ls

Rudimentary draft for confidential communications

# ON A POSSIBLE LIE-ADMISSIBLE COVERING OF THE GALILEI RELATIVITY IN NEWTONIAN MECHANICS FOR NONCONSERVATIVE AND GALILEI FORM-NONINVARIANT SYSTEMS.*

Ruggero Maria Santilli •
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

Submitted on January 16, 1978
Revised version submitted on April 3, 1978
Final version submitted on

## ABSTRACT

In order to study the problem of the relativity laws of nonconservative and Galilei form-noninvariant systems, two complementary methodological frameworks are presented. The first belongs to the so-called Inverse Problem of Classical Mechanics and consists of the conventional analytic, algebraic and geometrical formulations which underlay the integrability conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. These methods emerge as possessing considerable effectiveness in the identification of the mechanism of Galilei relativity breaking in Newtonian Mechanics by forces not derivable from a potential. Nevertheless, they do not exhibit a clear constructive capability for a possible covering relativity. For this reason, the second methodological framework is presented. It belongs to the so-called Lie-Admissible Problem in Classical Mechanics and consists of the covering analytic, algebraic and geometrical formulations which are needed for the equations originally conceivedby Lagrange and Hamilton, those with external terms. These formulations are characterized by the Lie-admissible algebras which are known to be genuine algebraic covering of Lie algebras, and which in this paper are identified as possessing (a) a direct applicability in Newtonian Mechanics for the case of forces not derivable from a potential, (b) an analytic origin fully parallel to that of Lie algebras, i. e., via the brackets of the time evolution law, (c) a covering of the conventional canonical formulations as classical realizations, (c) an implementation at a number of levels of Lie's theory, (d) a fundamental realization as universal enveloping nonassociative algebras, (e) a generalization of symplectic and contact geometry as geometrical backing and (f) the capability of recovering conventional formulations identically at the limit of null external forces, here interpreted as relativity breaking forces. A covering of the Galilei relativity, called Galilei-admissible relativity, is then conjectured for independent scrutiny by interested researchers. A number of potential implications, particularly for hadron physics, are then briefly considered for future detailed treatment.

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* To be published in the HADRONIC JOURNAL, Volume 1 (1978).

• Supported in full by the U.S. DEPARTMENT OF ENERGY under contract number ER-78-S-02-4742. A000.

April 24, 1978

TO: Professor M. TINKHAM
FROM: R. M. SANTILLI
SUBJECT: Removal of the word "honorary" from my title, as per application of April 6, 1978

This is to inform you that a duly executed copy of the research grant

DOE number ER-78-S-02-4742

has arrived. A copy of the formal notification by the Department of Energy is enclosed for your consideration.

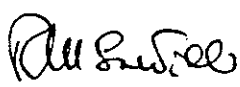
I would be truly grateful whether a decision on my application of April 6, 1978 can be reached in the near future, so that I can receive the salary allocated to me by the DOE grant.

I would like to take the personal liberty of indicating in this respect the expectation by Professor S. STERBNERG that, whether possible, the removal of the term "honorary" from my title be conducted within a physics institution.

You might be interested to know that the topic of the research (which I have sumbitted to numerous collegues as rudimentary drafts for confidential communications) is meeting with a quite encouraging interest which, in any case, is beyond my rather cautious expectation.

If I can be of any assistance, please do not hesitate to contact me.

Thank you for your consideration and time.



c. c.: Professors S. Coleman, H. Georgi, S. Glashow, R. Pound and S. Weinberg.

TO: Professor M. TINKHAM April 26, 1978
FROM: R. M. Santilli
SUBJECT: Application of April 6, 1978, for the removal of the term "honorary" in my title.

It is not without considerable embarassement that, following our phone conversation of this afternoon, I release copies of some of the letters of comments by collegues on my studies related to the grant DOE-ER-78-S-02-4742 (which is now in full effect, to my understanding). Please feel free to contact these individuals, if you so desire. Neverthless, I would appreciate your discretion on the disclosure. A number of additional letters from distinguished collegues is also in my file, but I cannot disclose them prior to the formal authorization by their authors.

As you can see, some of the enclosed letters of comments are excessively optimistic. I hope that you simply consider them as an expression of the genuine enthusiasm which is growing on these studies (the first issue of the Hadronic Journal will publish three papers on Lie-admissible algebras- by three different authors- and truly intriguing papers are forthcoming).

It is my impression that my senior collegues are basically noninformed of my studies. Actually, few physicists are informed at this moment. As you know from my progress reports of the past fall, my studies are presented in two series of monographs, one with Springer-Verlag (now in print) and one with Hadronic Press (which will be printed when I find the courage to release the manuscripts). Any inspection of my studies, to have sufficient value, demands the inspection of all these volumes because they contain the proofs (or, I should say, my efforts at the formulation and proof of) a number of crucial theorems and a tentative elaboration of their possible physical relevance. By today's standard (according to which contributions are assessed in 60 or, maximum, 90 seconds of inspection ) the study of these manuscripts is a possibility of few.

These two series of monographs are devoted to two different but complementary, classical and quantum mechanical, possible methods for the treatment of local forces not derivable from a potential (the Inverse Problem and the Lie-admissible Problem). They are basically presented for the arena of their potential direct physical relevance: nonconservative systems in applied physics, mathematics and engineering (you might inspect the enclosed comments by Professor L. Y. Bahar, an engineer, on the applications to energy-related issues). Equivalently, these studies are inspired by the original conception by Lagrange and Hamilton of preserving external forces in their analytic equations to avoid an excessive approximation of physical reality. And indeed, in my teaching of mechanics, I stress that the customary treatment of, say, the spinning top under gravity is noting but a conservative abstraction fully equivalent to the acceptance of the "perpetual motion" (for your amusement, I have enclosed a "vignetta" depicting the operation of truncation of the analytic equations originally conceived by Lagrange and Hamilton which has occurred in recent times).

Neverthless, since my initial letters of application to Steven Weinberg of the spring 1977, I have candidly confessed that the reason which stimulated this laborious and solitary journey is to be able to study, in due time, the old idea that the strong interactions in general, and the strong hadronic forces in particular, are (partially) nonderivable from a potential. You are aware that this is a line of study which was strongly suggested by the founders of contemporary physics (e.g., by Enrico Fermi even before I was born). With an understanding that this approach is substantially unconventional by today's trends, the epistemological argument is quite simple. Unlike the corresponding occurrence at the nuclear level, the charge volume of hadrons does not sensibly increase with mass and it is of the same order of magnitude of any other, physically known, charged particle. If the hadronic constituents are assumed as being non-point-like, that is, of possessing a finite charge volume, the hadronic structure emerges as being substantially different than the atomic and the nuclear structure. In rudimentary words, the dynamical evolution demands a continuous status of penetration of the charge volume of each constituent with that of the others, i.e., a type of motion which likely calls for nonlocal forces. However, local forces not derivable from a potential are known to be a good approximation of these forces. The net effect is that a rudimentary, primitive, Newtonian limit of this hadronic structure can be conceived as a state which is conservative as a whole, but the constituents are in highly nonconservative conditions. The possible direct applicability of Lie-admissible techniques is then consequential.

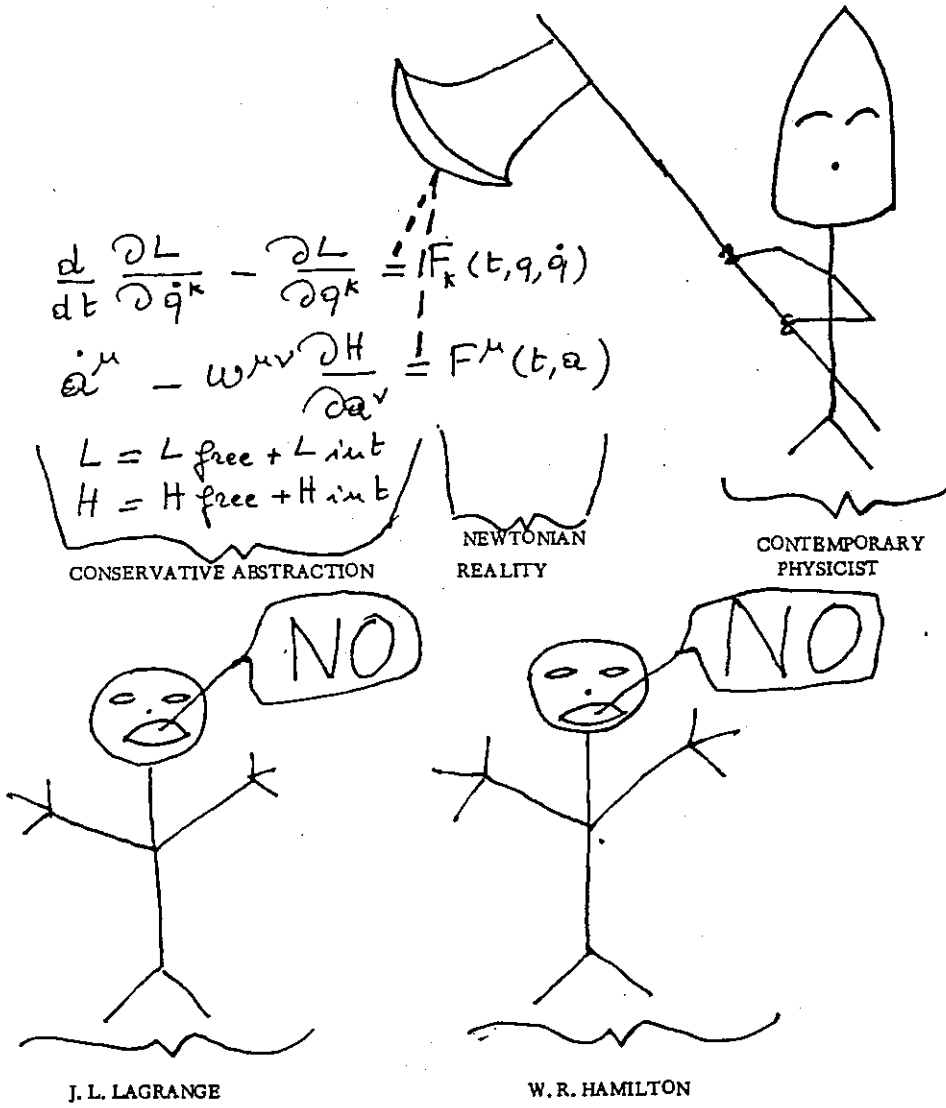
No physicist (and, first of all, myself) can today establish whether this picture of the hadronic structure is correct or grossly erroneous and, in my opinion, the resolution of this issue will perhaps demand generations. The following three aspects, however, deserve a comment.

(A) This line of study MUST be conducted, of course, as a minor complement of the current major stream of studies, in the traditional spirit of unsolved physical problems. Your are aware of the growing, severe judgement by an increasing number of physicists on any attempt of restricting the studies on the hadronic structure along only one line. This would be equivalent to scientific loss, because deprived of the primary function in basic research, as well as for human knowledge, of scientific debates. You are also aware of my determination in the continuation of these studies. Finally, you are aware of my eagerness and sincere gratitude toward any scientifically productive criticism or collaboration. I believe I have given proof to all my referees of providing a genuine implementation of all their valuable suggestions. The complexity of the problems to be confronted and, hopefully solved in due time, demands the most serious consideration of any viewpoint. In any case, I have found the courage to release a first paper for publication on Newtonian relativity aspects (this is my first formal release to outsiders-other than confidential reviewers ).

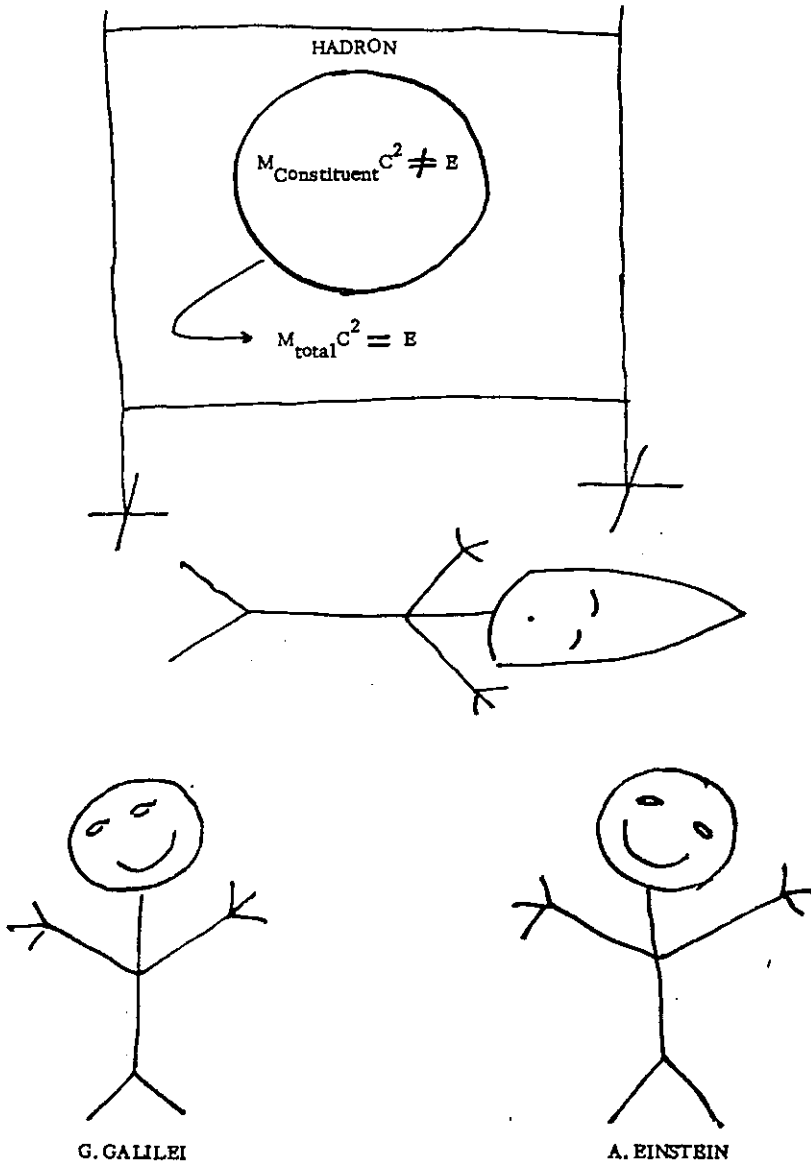
(B) At the risk of jeopardizing my personal reputation and of even being considered demented by superficial collegues, it is my moral and scientific duty, as a researcher, to point out in a way as clear as possible the technical implications of the assumptions that the strong hadronic forces are not derivable from a potential, as they appear to me. Simply stated, it appears that while our current knowledge unequivocally applies to hadrons as a whole (and as experimentally established) the applicability to the hadronic constituents under (and only under) the assumed forces is in doubt. For instance, field (Newton's) equations can apparently be proved to be noncovariant under the Poincaré (Galilei) group; conventional quantization procedures (up to the Wightman's axioms) can apparently be identified as inapplicable; the customary concept of perennal value of quantized spin (and Pauli's exclusion principle) appear undefinable; etc. It is my duty to release my findings on these issues, so that they are reinspected by independent, interested researchers for a possible future resolution of the technical issues. Regrettably, however, these issues appear to generate an understandable emotional behaviour by the average physicist, in which case the profile is basically non-scientific. The reason is that we are here not confuting established trends, but simply studying the possible implications of a conjecture on the nature of the strong hadronic forces (for your amusement I have enclosed a second "vignetta" depicting a true episode-the genuine fainting spells of a relativist that resulted during a "friendly conversation" when he was finally cornered by my technical arguments).

(C) In short, if the old idea that the strong hadronic forces may be (partially)nonderivable from a potential has to be studied to any physical depth, it appears to necessarily demand the attempt at the construction of a new generation of physical theories specifically constructed for the approach and in much the same way as it occurred at the atomic level. This is the aspect which is exiting my collegues and friends beyond my most optimistic expectations and, perhaps, this is the profile for which my enthusiam at such an intriguing physical problem has became contagious. This is also the area of the hopes for covering Lie-admissible formulations (a third vignetta, this time from my forthcoming paper, is enclosed also for your amusement). Of course, this is the most speculative part which will demand time, a long time, for any assessement of any value. What I can safely state as a researcher, as well as a co-editor of the Hadronic Journal, is that this search for possible covering formulations can by now be considered as initiated.

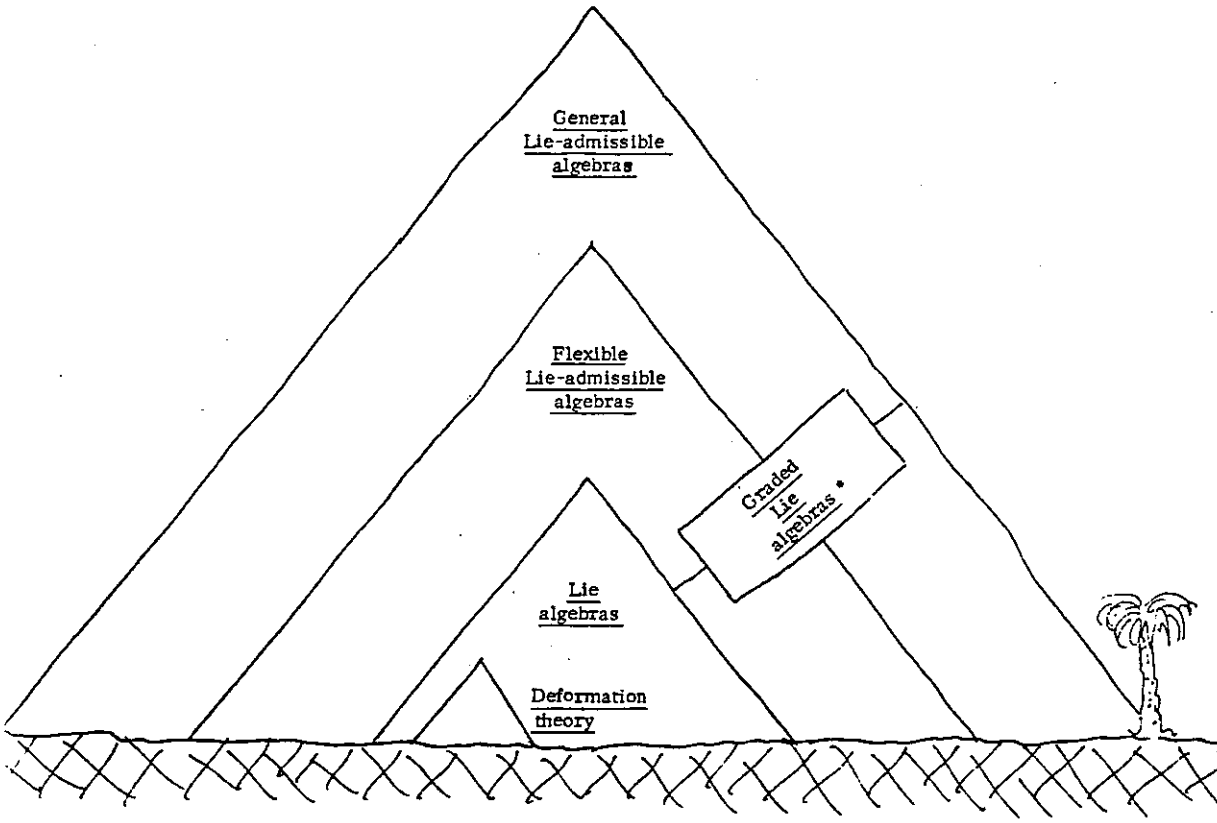
I understand the reservations by my senior collegues on such a delicate undertaking, but I believe that it is only the result of the lack of knowledge of my person and of my study. In any case, I am at their complete disposal for any question. Jointly, I hope that my senior collegues understand the rather severe difficulties I have in being the recipient of the allocated salary via the Department of Mathematics. After all, I have only applied for noting more than the removal of the word "honorary" from my title. Again, please accept the sen timents of my sincere gratitude for your courtesy, consideration and time.




THE TRUNCATION OF LAGRANGE'S AND HAMILTON'S EQUATIONS




RELATIVISTIC FAINTING SPELLS




*) Secret passage to bigger pyramids

Apr 27, 1978

Professor S. Coleman
Harvard University

Dear Sidney,

I have no words to express the fact that the manuscript I gave you was truly rudimentary.

I have now implemented numerous technical improvements in virtually all sections, following the help I generously received by quite a few collegues.

I have also provided my best efforts to temp.er the style of presentation, particular in crucial points.

However, please keep into account that this paper has been written during the worst period of my life (without a regular salary since September 1977 with a family of four and with my wife at the graduate school). This is inevitably reflected in the style of presentation.

I do not know whether you will have the time of going through this project. In any case I would like to indicate that it is supposed to be printed on May 1, 1978.

Sincerely

Ruggero

April 29, 1978

Professor M. TINKHAM,
Chairman,
Department of Physics
HARVARD UNIVERSITY

Dear Professor Tinkham,

I have separately put in your mail box a note dated April 26, 1978 elaborating on my current research with copies of some of the letters of comments by collegues. I have taken the liberty of giving copy of this material to each senior collegue. Additional copies are at your disposal.

I would like to report to you an open conversation I recently had with Shlomo Sternberg. In essence, it appears virtually unfeasible that I can receive the salary of the DOE grant via the Department of Mathematics prior to its expiration, owing to the need of searching for mathematicians willing to study my research (an impossible task). In any case, since I am a physicist, I do not think I should even apply for a position at any mathematics department.

In conclusion, a possible negative vote by my senior collegues at your Department implies the inability for me to receive the allocated salary of the DOE grant.

I am sure that, as an administrator, you realize the implications of such a possible occurrence. I trust in your judgment to properly present them to the senior collegues in case needed.

Thank you.

Sincerely

Ruggero Santilli

Ruggero Maria Santilli

P.S. I hope you have enjoyed the "vignette" of my note of April 26.

In case needed, I can be reached Monday either in my office (5 3352) or at the printer, 492 5600 (where I will be supervising production of the HADRONIC JOURNAL).

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 5, 1978

Professor M. TINKHAM,
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I would like to acknowledge your verbal, informal communication of a negative attitude by the senior departmental members toward my appointment as research fellow.

After due consideration, it would be inappropriate for me, being a physicist, to apply for a position at the Department of Mathematics. In any case, since my studies are essentially of conjectural high energy nature (as most of the current high energy research is), a negative decision by the Department of Physics renders simply inconceivable my application at the Department of Mathematics.

The net result is that a negative decision by your senior departmental members on my appointment as research fellow is technically equivalent to the decision that I should not receive the salary allocated by the Department of Energy under contract no. ER-78-S-02-4742 for me.

I am sure you realize the implications of such a rather delicate, possible occurrence.

I am confident that you are aware of my sincere committment toward the pursuit of physical knowledge, as well as of my eagerness and receptive attitude toward any critical review and inspection of my studies.

Therefore, please take into account that I would be simply pleased to submit any paper during the period of the contract to any senior member for inspection and approval prior to any release or submission to outsiders, whether for formal submission for publication or for confidential review. Similarly, I am receptive toward any scientifically productive procedure for the conduction of the studies related to contract DOE-ER-78-S-02-4742 which might be requested by the department.

In consideration of the above I hereby submit my last appeal for a reconsideration of the case, hoping that the wisdom by the senior departmental members will produce a sensible solution.

I look forward to receiving from you a formal communication on the final decision.

Very Truly Yours

Ruggero Maria Samtilli

Ruggero Maria Samtilli
Honorary research fellow

RMS|cgg
c.c.: Professors S. Coleman,
S. L. Glashow, R. J. Glauber,
A. Jaffe, R. V. Pound and
S. Weinberg.

May 5, 1978

Professor S. GLASHOW,
Harvard University

Dear Shelly,

I believe that a negative decision on my case is substantially against the interest of the department, as well as of the individual senior members. Permit me, respectfully and candidly, to express my viewpoint and propose a solution inspired to the utmost possible moderation.

WHY A NEGATIVE DECISION IS AGAINST THE DEPARTMENTAL INTERESTS.

(1) The DOE grant is now a reality (whether for good or bad reasons). I have no doubt that a negative decision on my case will negatively affect the future relationship between Harvard and the Department of Energy (for this reason I have absteined from communicating to DOE the current difficulties and kept a complete silence until now). I do not know whether you are aware of the pressures on Govenmental Agencies to diversify the topics of research in hadron physics, that is, more explicitly, to begin minor support of quark nonoriented research (to my knowledge, they originate from the highest rank in the Administration and in the Congress). I am sure you realize the difficulties which such a decision would create at the DOE and the consequential implications for Harvard's interests.

(2) The line of study on Lie-admissible algebras is, by now, also a reality. I can assure you that a significant number of physicists and mathematicians are by now conducting active research in this topic (you should not be surprised at some of their names, and keep into account that the names I have disclosed are only part of the complete group). It appears likely that no group of researchers of contrary interest can by now stop this line of study. The current departmental decision literally implies the release of this line of study to outsiders. I am sure you realize the potential implications one to two years from now. Most importantly, Harvard would be simply confronted with articles when they arrive at your library and would be basically outside of the decisional process. With my most sincere respect, permit me to indicate that the number and quality of physicists who do not believe in the quark conjecture (whether colored or not) is considerable. Some of them told me that they have been silent until now because no alternative of any value was on the horizon. I have no word to express my recommendation to you of giving serious consideration to the negative implications for Harvard of the free spinning of these ideas entirely outside of departmental inspection.

(3) The Hadronic Journal is, by now, also a reality, with adequate financing, subscribers all over the world, a regular flow of papers submitted for publication and a very encouraging support from so many collegues. You are aware of the inspiration according to which the journal was born: the presentation of any valuable alternative to the problem of the hadronic structure for the sole pursuit of physical knowledge. The sentiments of my sincere esteem and, if you permit me, consideration in your person, suggest me to recommend to you a careful consideration of this vehicle if you force it completely outside of Harvard's touch.

In conclusion, I sincerely hope that the senior members give full and adequate consideration of the above points prior to reaching a negative decision on my case. They are in Harvard's interest, not my own.

MY PROPOSAL.

I have provided my best efforts (as a researcher by instict) to analyze the difficulties of my case as seen from a departmental viewpoint, even tough I receive no formal or informal information. My proposal is therefore inspired by my sincere intent to comply with the departmental rules and general interests. I believe that I have given proof in the past of being a responsible person capable of complying with departmental structures. Also, you can trust that I will follow all my committments ad litteram.

My most serious deficiency, as it appears to me, is that I do not have a senior departmental member as a supervisor. Shlomo is literally burdened by so many duties and responsibilities (as I can testify) that he simple does not have the time at this moment. You know that, since my arrival at Harvard, I have searched for a senior supervisor with my sincere intent of submitting all my scientific production for inspection and approval prior to any release to anybody. But I understand that everybody is quite busy. Lately, I have asked Sidney whether he can act as my supervisor. If he cannot for any reason, I would be very happy whether you can act as supervisor. Almost needless to say, I would be honored to collaborate with any collegue. But wat I believe is most needed is supervision.

In relation to time, please keep into account that I am not prolific (contrary to a different initial impressions). During the year of the grant I intent to write one or maximum two papers. This boils down to hours per one years. My next step is to study a possible relativistic formulation of Lie-admissible algebras. I will need 4-to-5 months just to get oriented.

In essence, I am offering a full departmental supervision and inspection of my studies with the understanding that they should not be released until approved.

In this respect permit me to candidly stress that I offer this strict supervision beginning from the date I receive my first check. My family needs food and shelter, like yours. My lack of regular salary since September 1977 has forced me into a series of decisions and initiatives which I would never have made, if regularly salaried as you are. I verbally told you that the primary reason why I accepted the job of organizing the Hadronic Journal is financial. I also told you that the primary reason why I am in the process to release my manuscripts with the Hadronic Press is because I need the royalties and advance payments. Most of what has happened is strictly motivated by my financial needs. If I were regularly salaried, the Lie-admissible algebras would have remained in my desk for a number of additional years. Further delays will force me into further actions.

In closing, permit me to openly discuss the editorial situation of the Hadronic Journal. You know that we are sincerely committed to scientific values. You also know of our awareness that, to achieve this objective, we need qualified advice by senior members of the scientific community. You also know that senior departmental members received the offer to be editor of the journal. I had numerous possibilities of appointing outstanding physicists for the needed third position of editor. I have abstained from doing so waiting for a more careful consideration by you and your senior collegues.

I would be honored whether you can become the third editor of the Hadronic Journal and, as such, the senior editor (as an incidental note, you know that I have a budget of few thousands dollars for the honorarium, depending from the final number of subscribers).

I understand that you might decide not to officially let your name be printed in the second page as "Editor". In this case, I am also authorized to an informal appointment of a "Silent Editor" (with the same honorarium), in which case you name will not be printed in the second page.

Whatever possibility you select, in case you decide to accept my offer, you can rest assured that I would be happy to submit to you each and every paper prior to formal communication to the authors. Time-wise, we are talking of some 4-to-9 papers (maximum) every two months.

In conclusion, my proposal is inspired by my genuine desire to comply with departmental regulations and general interests. I would of course welcome any scientifically effective alternative solution. My rationale is quite simple: I believe that by doing so the quality of my scientific production and activities would immensely benefit. Jointly, Harvard would avoid a quite risky decision.

Sincerely

Ruggero

c. c.: Professors S. Coleman,
M. Tinkham and S. Weinberg.

 May 5, 1978

Professor S. Coleman
Harvard University

Dear Sidney,

I am sure that, within your own soul, you recognize that the Lie-admissible algebras can indeed write a page of physics, provided that they are properly developed and presented.

I am fully aware that my difficulties in front of the department are also due to the fact that I am alone and, as such, substantially handicapped toward the proper development and presentation of the studies. Regrettably, Shlomo, with his knowledge, could make a real breakthrough. But he is too busy and burdened by some many duties and responsibilities (as I can testify after being his guest for few weeks) that he simply has no time at this moment.

You are the only scientist at Harvard (and, I can safely say, in the Boston area) with the necessary knowledge and vision to understand and techni cally assess the research.

I am therefore asking for your supervision. I would, of course, be delighted and honored to collaborate with you. But if this is incompatible with your current interests, I would be grateful to receive only your occasional supervision, with the strict understanding that I should take a truly minimal amount of your time and that no paper, information, material or otherwise is released to other persons prior to the necessary time for your inspection and approval.

I am making this request in relation to my last proposal to the department related to a solution of the available grant. An additional copy is enclosed. It essentially calls for a supervisor. I believe I have given proof in my life of being a man of honor and you can trust my committments ad litteram.

In practice this means supervising my work for essentially one (or maximum two) papers I can (possibly) write during the period of the contract. You have not seen the final version of my paper with the Hadronic Journal, but the old reference 33 on my forthcoming papers has been reduced to only one, the initial studies of a possible relativistic formulation of Lie-admissible algebras.

In relation to time, I will need 4-5 months only to get oriented at the problem. Thus, during the period of the contract your time can be reduced to the order of hours. Of course, I would be happy di discuss with you alternative topics, if you have any preference (there is so much and so much fascinating work to be done). In relation to the "color-committed" collegues, I am sure that this is a better solution than forcing me angrily loose to fight color and similar conjectures (you know how easy it is to get qualified "anti-color" support).

As an incidental note, I feel obliged to disclose to you on a strictly confidential basis that the copies of letters of support I distributed were only partial. As indicated in my letter to Prof. Tinkham, I have not disclosed the support from "distinguished collegues". You should not be surprised at some of their names.

Hoping to hear from you, I remain, sinceramente tuo

Ruggero

May 9, 1978

Professor R. BOTT
Department of Mathematics
Harvard University

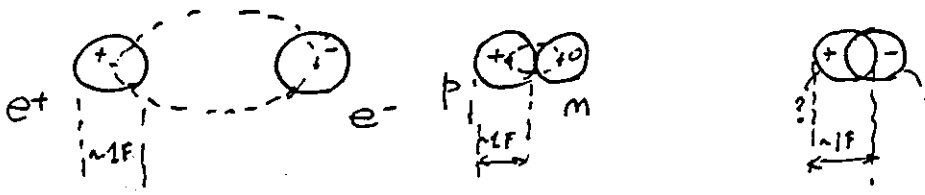
Dear Raoul,

I am in a delicate moment of my academic life. I would appreciate the courtesy of your confidential advice whether I should apply for the position of research fellow (or any other suggested position) at the Department of Mathematics or not. You can rest assured that your consideration and kindness will meet with my utmost discretion.

My situation is rather delicate on a number of technical and academic aspects. Permit me the liberty of presenting an outline as it appears to me and presented in a form as candid as possible. This is only motivated by my desire to provide elements of some possible value for the consideration of the issue.

The conjectural physical nature of my research. I have been involved for some time on the study of the problem of the nature of the forces of the hadronic constituents, the methods for their treatment and the conjectural implications for possible models of structure. In the most rudimentary language possible, the central conceptual argument is as follows. You are aware of the current trends in hadron physics (quarks, color, asymptotic freedom, etc. etc.). They are essentially based on the (for me) abstraction of point-like constituents and the assumption that the strong hadronic forces are derivable from a potential. This is equivelent to say that the hadronic structure can be represented with the conventional structure of a Lagrangian, i.e., $L_{tot} = L_{free} + L_{int}$. This assumption, in turn, implies the direct applicability of conventional analytic formulations (Hamilton's equations, etc.), algebraic formulations (Lie algebras, etc.) and geometrical formulations (symplectic geometry, etc.), as well as conventional quantization procedures in both discrete and continuous cases.

The epistemological argument of my studies is quite simple. Again in rudimentary language, a most intriguing experimental data on hadrons is that their charge volume does not increase with mass (contrary to the corresponding occurrence at the nuclear level) and it is of the same order of magnitude of any other physically established, charged particle ($\sim$1 Fermi). If the hadronic costituents are assumed as physical particles, that is, possessing a finite charge volume, a picture of the hadronic structure substantially different than that of the atomic and nuclear structure emerges. In short, starting from the atomic structure of very large distances as compared to the charge radius of the constituents, we go to the nuclear structure of very close distances and, finally we have a hadronic structure in which, according to this line of thinking, there is a "penetration" of the charge volume of each constituent with that of the others.




positronium deuterium

If such a view of the hadronic structure is plausible, it appears to demand nonlocal forces. This sufficient to indicate the possibility that the complexity of the problem of the hadronic structure might go beyond the most vivid imagination of contemporary physicists.

I am unable to treat, both classically and quantum mechanically, nonlocal forces. I will remain to do so even if I spend the rest of my life on the problem to attempt a rigorous treatment. With full knowledge of my limitations, I have therefore worked on a rudimentary approximation of such a structure which is solely intended as an intermediate step of mainly qualitative nature, prior to possibl future, more adequate treatments.

The idea of this (rather crucial) approximation is again, quite simple. We know from mechanics that local forces not derivable from a potential constitute a good approximation (in a physicist's language) of nonlocal forces. I have therefore concentrated my efforts on the classical and quantum mechanical methods for the treatment of these forces. The emerging systems, at a primitive and rudimentary level, can be conceived as being the local, class $C^{\infty}$ nonconservative systems of our everyday experience, e.g., the nonconservative (rather than conservative) spinning top with drag torques responsible for the nonconservation of the angular momentum.

After a number of years of isolated labor, I am now in the process of presenting my studies on the treatment of local forces not derivable from a potential in two series of monographs. The first is in print at Springer-Verlag and it is within the context of the Inverse Problem of the CV. The second will be printed by Hadronic Press(when I find the courage to release the manuscripts)and it is within what I have tentatively called the Lie-admissible problem.

I enclose copy of the table of contents of the monographs with Springer-Verlag. I have mailed a copy to Dr. Brooks at Stony Brook who has generously accepted to read the galleys. I also enclose a leaflet on the monograph with the Hadronic Press. Both series of manuscripts have been inspected by numerous collegues, mostly physicists and few mathematicians. I also enclose copy of my articles on the field theoretical extension of the Inverse Problem. Finally, I enclose a copy of the first issue of the Hadronic Journal with a rudimentary presentation of the Lie-admissible problem and some conjectural remarks on possible relativistic implications.

The idea of this dual methodological profile for the treatment of the same systems is again simple.

(1) local, class $C^{\infty}$ Newtonian systems with forces not derivable from a potential can also be represented with conventional Lagrange's equations (without external terms) under certain integrability conditions. This, however, implies the loss of the conventional structure $L_{tot} = L_{free} + L_{int}$, in favor of an arbitrary functional dependence. The important point is that the knowledge of a Lagrangian or a Hamiltonian renders fully applicable to the broader systems considered conventional analytic, algebraic and geometrical techniques.

(2) The same systems can also be represented in the way originally conceived by Lagrange and Hamilton, that is, by representing the forces not derivable from a potential with external terms in the analytic equations. Apparently, this implies the need of generalizing the conventional formulations by stimulating mathematical problems which, for me, are truly intriguing, as I point out below.

Back to the hadronic structure, if you go to the case of more than two constituents, the structure which emerges is essentially the following, again, in rudimentary language. The state as a whole is conservative and obeys established relativity and quantum mechanical laws, as experimentally established. However, the constituents are in a high degree of nonconservative notion on an individual basis, that is, each constituent exchanges with the other all its physical characteristics (energy, linear momentum, charge, etc.). The intriguing point is that the description of such a structure apparently demands the efforts of generalizing our physical knowledge (essentially based on $L_{tot} = L_{fre} + L_{int}$). To state it explicitly, it appears that this view of the hadronic structure

demands a generalization of available formulations in much the same way as it occurred at the atomic level. For instance, the conventional quantum mechanics, essentially emphasizing the stability of the orbits of the electrons in an atomic structure (for which, after all, was conceived) apparently presents difficulties to describe the motion under consideration because now all the emphasis is in the nonstationarety of the orbits of the constituents. Similarly, conventional relativity ideas appear to exhibit difficulties because, for instance, the equations are not form-invariant under the Galilei transformations by central requirement (to ensure a nonconservative behaviour-I am here referring, again, to the case of the individual constituents and not to the hadron as a whole).

The reaction by collegues (physicists) to this approach to Hadron structure can be divided into two opposite groups with no common grounds. A first group, essentially composed by physicists who do not believe in quarks, color, etc. are strongly in favor for the conduction of this line of study. A second group, essentially composed by physicists academically and financially committed to quarks, is either openly against this line of study (e.g., because of the "style" of presentation of my paper, which, in my opinion, has noting to do with the scientific issue) or is silent.

In my papers I claim no result, while stressing the conjectural nature of my studies. On one point I am firm. I stress the need of subjecting to an experimental verification the validity of the quantum mechanical laws for the hadronic constituents which, in virtually all current papers, are tacitly assumed as valid. I am not trying to convince people that they could be invalid. No. My central objective is to create the awareness in the physics community on the need to verify these laws with experiments prior to claiming that they constitute a scientific truth. Since quark oriented collegues cannot attack me on this angle, this is perhaps the point which renders them most unhappy.

In any case, it appears that a quite intriguing scientific debate is under way. The first issue of the Hadronic Journal, as you can see, already presents three papers on Lie-admissible formulations and truly intriguing papers will be likely published in subsequent issues. If things go as planned, this debate could bring into active role a significant segment of the community. In any case, it cannot be resolved either way by isolated researchers.

The possible mathematical aspects of my studies. Raoul, permit me to candidly confess that, in my view, IT IS EXTREMELY UNLIKE THAT THE PROBLEM OF THE STRUCTURE OF THE HADRONS WILL BE RESOLVED BY PHYSICISTS ALONE WITHOUT THE PARTECIPATION BY QUALIFIED MATHEMATICIANS. My rationale is quite simple. The technical complexities of the problems to be confronted and hopefully solved in due time, simply goes beyond the knowledge and capability of the best groups of best physicists. I have made this appeal to a number of mathematicians and few have positively reacted until now. The best case is that of Prof. H.C. Myung, a mathematician of the University of Northern Iowa and the leading expert on Lie-admissible algebras. He is now fully active and involved in research (he is a member of the Editorial Council of the H.J.). I hope that other mathematicians will undertake active research efforts on some of the many aspects which, to the best of my knowledge, are still open on grounds of pure mathematics. Without this technical contributions by mathematicians, the physicists will not only be unable to districate the issues, but will end up doing noting more than a "grossa confusione".

Unless I am grossly mistaken in my view, it appears that there are truly intriguing, apparently new mathematical problems to be contronted. If you are interested and have the time, please briefly inspect Section 3 of my paper on the Hadronic Journal on the "Lie-admissible Problem". You will see then the apparent need of generalizing Lie's theory into a form of Lie-admissible type, or the need of studying the possible existence of broader geometrical approaches capable of characterizing (in my rudimentary language) the Lie-admissible algebras (I had to discart in my efforts both, the symplectic and the Riemannian geometry).

4.

The role of the Department of Energy. As you know, the DOE grant No. ER-78-S-02-4742.A000 is now arrived, duly executed. The money is sitting in Harvard's bank and I cannot draw the salary allocated by DOE for me because the university regulations prohibit a person with my title, "honorary research fellow", to receive salary.

The DOE is, of course, fully informed of my studies. In turn they had them submitted to several top physicists and (I understand) mathematicians before allocating the money (in these days, no agency gives to money away without due consideration). The net outcome has been that, to the best of my knowledge, DOE appears to be determined to financially support my research provided that I have a qualified university to administer the grant.

Permit me to disclose on a strictly confidential basis that, after this initial grant for two years, DOE appears receptive for a second rather substantial grant. As a final notice, you should also considered that I applied for this grant after two written, sequential, invitations by the DOE. I did not solicit it because I knew I could not apply from Harvard as a principal investigator.

My future research program. After the enclosed first paper in the H. J., I am now writing a second paper of conjectural high energy physics nature (as any theoretical study of this type is today). It is essentially a critical anamnesis of the quark model in light of possible generalization aiming at the stimulation of the awareness of the physics community that, after all, different models could be conveivable.

This paper, which I will release while being still at Lyman, will complete my conjectural papers. After that, I would like to dedicate myself to study and technical papers. Next year I will essentially write two papers with Myung (a mathematician) on Lie-admissible algebras and spend most of the time in studying mathematics. As I indicated to Shlomo, my primary interest for being here next year was to be able to follow a few graduate courses. I am fully aware that my mathematical knowledge is noting but rudimentary.

To summarize, I would appreciate your confidential advice whether it is appropriate for me to apply for any position acceptable by the department of mathematics. My last academic position was that of associate professor of physics. However, any position (of research, nonteaching, nontenured and terminal nature) would be fine for me. I am aware that I am a theoretical physics and, as such, not fully qualified for such an application. Neverthless, I believe that my studies have a possible mathematical significance. Also, the DOE support could be of some value to the Department.

In closing, permit me to stress that, if such a position is granted, the nature of my papers will change, in the sense that I will not write papers of conjectural high energy nature. Instead, I shall provide my best efforts for joint collaborations with mathematicians for technical contributions (this is the reason why I am now hurrying to complete all conjectural papers while being under the name of a physics department).

If you need any further element, please do not hesitate to call me (office 5 3352 and home 969 3465).

Please accept the sentiments of my sincere esteem and gratitude for your courtesy.

Sincerely

Ruggero

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

JEFFERSON PHYSICAL LABORATORY
CAMBRIDGE, MASSACHUSETTS 02138

May 10, 1978

Dr. Ruggero Maria Santilli
Lyman Laboratory of Physics

Dear Dr. Santilli:

I regret to inform you that the Physics Department has declined to change the decision which I had communicated to you earlier and informally. That is, it reconfirmed its view that, since the DOE contract in question has been made with Professor Sternberg in the Mathematics Department, and because your work is in an area of mathematical methods in theoretical physics which is as suitable to that Department as to Physics, therefore your paid appointment should be made through the Mathematics Department. This view is consistent with the fact that no member of the Physics Department felt close enough to your work to be willing to accept the responsibility of serving as Principal Investigator. Moreover, at the time Professor Sternberg was considering whether to agree to serve as Principal Investigator on this contract, our Director of Laboratories, Professor Pound, made it very explicit to him that he expected the Mathematics Department to assume the responsibility for all further appointments related to it if he did agree to serve in that role.

In communicating this decision to you, I feel that I should make it clear that we do not consider removing the "Honorary" from your title to be a trivial technicality. Honorary Research Fellow appointments are made as a courtesy to visitors, with minimum scrutiny, since no financial commitment is involved, and they are for only one year; as a result they carry little institutional responsibility. By contrast, ordinary (i.e., paid) Research Fellow appointments are made only when specifically requested by a senior faculty member who will pay the salary for work related to a contract for which he is fully responsible. Moreover, the supporting documentation for the appointment must be approved by the Dean's office, etc., and it normally must involve comparisons with other candidates turned up in the search mandated by federal Affirmative Action procedures. (This search can be waived only in certain cases where the appointment is limited to a non-renewable term of no more than 12 months.) Harvard University has very restrictive rules governing these matters of appointments and grant solicitation in an attempt to assure appointments of the highest quality and to retain faculty control and responsibility for all research activities. Other universities may be less restrictive, but insofar as they are, their institutional endorsement inevitably carries less weight. In choosing to come to Harvard, you effectively elected to accept the governing standards and principles of the institution.

Sincerely yours,

M. Tinkham

M. Tinkham
Chairman

MT:ag
cc: S. Sternberg
R. V. Pound

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 15, 1978

Professor M. Tinkham
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

Please let me know at your convenience the date of termination of my honorary appointment (May 31, 1978 or August 31 1978 ?).

Sincerely

Ruggero Maria Santilli

RMS|is

Research Grant Proposal submitted to the
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION

by

President and Fellows of Harvard College
c/o: Office for Research Contracts
Holyoke Center 458
Cambridge, Massachusetts 02138

entitled

# INTEGRABILITY CONDITIONS FOR THE EXISTENCE OF A LAGRANGIAN IN NEWTONIAN MECHANICS AND FIELD THEORY

Principal Investigator

Shlomo Sternberg
Professor of Mathematics
Soc. Sec. No. 090-34-7895
Science Center
One Oxford Street
Cambridge, Massachusetts 02138

Co-Investigator

Ruggero Maria Santilli
Research Fellow
Soc. Sec. No. 032-46-3855
Lyman Laboratory
Harvard University
Cambridge, Massachusetts 02138

Proposed Starting Date: March 1, 1978
Proposed Duration : 15 Months
Amount Requested : $50,000.00

Endorsements

Principal Investigator

Shlomo Sternberg
Professor of Mathematics
(617) 495-2170

Co-Investigator

Ruggero Maria Santilli
Research Fellow
(617) 495-3212 / (617) 969-3465

Departmental Officer

Shlomo Sternberg
Chairman
Department of Mathematics
(617) 495-2170

Institutional Adm. Officer

Mary Marquebreuck
Director
Office for Research Contracts
(617) 495-5501

DEPARTMENT OF PHYSICS

*MEMORANDUM*

To Ruggero Maria Santilli

DATE May 22, 1978

FROM M. Tinkham

SUBJECT

In reply to your letter of May 15, I can inform you that your honorary appointment runs through June 30, 1978.

July 19, 1978

Dr. M. TINKHAM,
Department of Physics, Harvard University

Dear Dr. Tinkham,

Permit me to express my gratitude for your exhaustive letter of May 10, 1978 communicating the decision by your senior collegues in relation to my affiliation for the DOE grant. I am particularly grateful for elaborating aspects which were basically unknown to me. You can rest assured that I understand and respect this decision in full.

I would like to inform you that I have absteined from even mentioning your consideration of the case to the Department of Energy, as well as to any collegue. You can trust that this silence will be kept in its entirety and that, in case requested to disclose the case by DOE, I will take the liberty of consulting with you to select the most appropriate form of disclosure.

Permit me to comment on my correspondence with you and your senior collegues. You have eventually noticed the use of the conventional academic language in my formal letters with Harvard stationary and the use of a nonacademic, candid language in my personal notes such as this. I understand that you might not be accostumed to nonaccademic languages in accademic matters. Permit me to indicate that, under no circumstance you should identify in such open language my intent of being offensive. If I failed to convey this point, please accept my most sincere apoligies.

In essence, I felt the need of using a candid language because of my high concerns on the grave status of high energy physics, which calls for the attempt of the identification of the current problem in a way as clear as possible. You might inspect in this respect my enclosed letter to Prof. Panofski, Director of the Stanford Linear Accelerator Center.

Also, I was under the erroneous impression that your senior collegues had judged my studies as without physical interest, contrary to an extensive analysis by DOE (no agency gives money these days without extensive consideration) and the support by quite numerous collegues, some of whom outstanding and recipient of the Nobel Laurea. Also, I was under the erroneous impression that Harvard University, after letting me formally file a research grant application with a U.S. Governmental Agency, and after letting this application be formally funded, had decided not to execute the proposal.

But perhaps my state of spirit was the result of a major disappointment I received from Sidney Coleman. In essence, I asked to visit your Department in the hope of receiving criticisms on my delicate studies. On April 4 I visited Sidney indicating that I was in need of qualified advice on an intriguing but truly delicate paper (my second of the enclose formal letter, on the proposal for a generalization of Galilei's relativity). He indicated interest after filing his tax forms. On April 15 I formally submitted the paper for his review via the Hadronic Journal in the form then available, a "rudimentary draft for confidential communications". Most of the critical, technical and linguistic passages of the paper were hand marked for his advice. On April 27 I wrote a note to Sidney in which I indicated the existence of a revised version incorporating suggestions by numerous collegues and the projected time of printing of the article (May 1). This printing (of the first issue of the Hadronic Journal) was then delayed in the hope that Sidney would kindly help me in the finalization of the paper. On May 5 I discovered that Sidney, while he had kept a complete silence with me, he had been quite generous of criticisms on this paper, as a result of a detailed study he had conducted for department use. This was reason for extreme disappointment for me because contrary to centuries of scientific traditions to which I have been educated and contrary to the confidentiality of the formal referee process. My note of May 5 to Shelly was written under the reaction of that moment.

In any case, I want to reassure you that I have no animosity whatsoever with Sidney. Perhaps, he was afraid that I could misuse his help. In this respect permit me to indicate that I have received help for this paper from truly outstanding physicists. I have respected their confidentiality to the point, as you know, of refusing to release their names when requested by some of your collegues during the consideration of my case.

In closing, let me indicate that, being the genuine temperamental Italian you by now know, I am capable of genuine sentiments of respect and gratitude. I have nothing but these sentiments towards you and all your collegues.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS|cgg

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 30, 1978

Professor M. TINKHAM,
Chairman
Department of Physics
Harvard University

Dear Professor Tinkham,

I complete today my honorary visit at your Department. It is my pleasant duty, as my last act, to express the sentiments of my sincere gratitude to you and all your collegues for the kind hospitality I have received.

For your information, during my visit I have released for printing the following monographs

- Foundations of Theoretical Mechanics, Volume I (with Springer-Verlag); and
- Lie-admissible approach to the hadronic structure, Volumes I and II (with Hadronic Press).

The remaining volumes of these two series will be released at some later time under my new association.

During the same visit I have also released, either for preprint distribution or for printing, the following articles as technical summaries on parts of the above monographs.

- Isotopic breaking of gauge symmetries, preprint HUTP-77|A066;
- On a possible Lie-admissible covering of the Galilei relativity in Newtonian mechanics for nonconservative and Galilei form-noninvariant systems (HADRONIC JOURNAL 1, 223-423 (78
- Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle (HADRONIC JOURNAL 1, 574-901 (1978)).

Copies of the latter articles are enclosed for your file. Complimentary copies of my monographs will be mailed to you, in case available. You will recall that I also wrote few additional nontechnica notes. But, as verbally indicated to Steven Weinberg, they were not intended for outside release and simply for internal use.

Permit me to indicate that the above articles are not written in the conventional style for conventior topics in conventional journals. Instead, their style of presentation is provocative as a result of a specific and laborious search. In essence, the hope of these articles is that of stimulating the consideration of fundamental issues, according to the traditional priorities of basic research which have been regrettably abandoned in recent times. Also, the style of presentation was selected after consideration of a number of alternatives, in the hope of focusing the current grave situation of basic studies.

As you eventually know, my research grant with the Department of Energy has been executed by Harvard and beginning from tomorrow I will be formally associated with Harvard University Science Center.

Very Truly Yours

Ruggero Maria Santilli
Honorary Research Fellow

RMS|cgg

RUGGERO MARIA SANTILLI
Information for Annual Report

April 14, 1978

A. Summer 1977: Visiting fellow at

- International Centre for Theoretical Physics, Trieste Italy (invited by Prof. P. BUDINI)
- Institut fur Theoretische Physik, Universität Zürich (invited by Prof. A. THELLUNG)
- Instituut voor Theoretische Mechanica, Rijksuniversiteit Gent (invited by Prof. R. MERTENS).

B. Research grant

coinvestigator of a research grant with Professor S. STERNBERG as printipal investigator supported by the U.S. Department of Energy Number ER-78-S-02-4742.A000 and entitled "Integrability conditions for the existence of a Lagrangian in Newtonian Mechanics and field theory". The grant has been approved by the high energy physics division of DOE and recommended to the DOE administration for support. The grant is expected to be executed before May 1, 1978.

C. Lectures outside of Harvard

- "Classical symmetry breakings", talk delivered at the Department of Physics of Northeastern University (invited by Prof. E. J. SALETAN), November 1977;
- "Methodological treatment of nonconservative systems", talk delivered at the Department of Physics, Queens College of the C. U. N. Y. (invited by Prof. K. R. RAFANELLI), October 1977;
- "The Inverse Problem for partial differential equations", informal talk at the International Centre for Theoretical Physics to a group of physicists from Mid-East and Africa; August 1977;
- "The existence theorems of Analytic Mechanics", talk delivered at the Instituut voor theoretisch Mechanica of the Rijksuniversiteit, Gent (invited by Prof. R. MERTENS), July, 1977;
- "The conjecture of a Lie-admissible covering of the Galilei relativity for nonconservative Newtonian systems", talk delivered at the Institut fur Theoretische Physik der Universität Zürich (invited by Prof. A. THELLUNG), July 1977.

D. Lectures at Harvard

- "The Inverse Problem in Newtonian Mechanics and Field Theory", informal seminar course for graduate students in physics and engineering (all from MIT) presented in the fall semester

E. Publications

- Foundations of Theoretical Mechanics, Volume I (The Inverse problem in Newtonian Mechanics), currently in press by Springer-Verlag in the series "Textbooks and monographs in physics"
- Foundations of Theoretical Mechanics, Volume II (Generalizations of the Inverse Problem in Newtonian Mechanics), accepted for publication by Springer-Verlag. To be published in 1978|79.
- Lie-admissible approach to the Hadronic Structure, Volume I (Nonapplicability of the Galilei and Einstein Relativities ?) currently in press by Hadronic Press, Nonantum, Ma;
- Lie-admissible approach to the hadronic structure, Volume II (Coverings of the Galilei and Einstein relativities?), currently in press by Hadronic Press, Nonantum, Ma;
- Lie-admissible approach to the hadronic structure, Volume III (Identification of the hadronic constituents with physical particles?), accepted for publication by Hadronic Press. To appear.
- "Isotopic breaking of gauge symmetry" preprint HUTP-77|A066. Submitted for publication.
- "Need of subjecting the validity of Pauli exclusion principle within a hadron to an experimental verification", preprint HUTP-77|A085. Submitted for publication.
- "Need of subjecting the validity of Einstein special relativity within a hadron to an experimental verification" preprint HUTP-77|A086. Submitted for publication.

- "On a possible Lie-admissible covering of the Galilei relativity in Newtonian Mechanics for nonconservative and Galilei form-noninvariant systems", Hadronic Journal 1 (1) (1978), in press.
- Lie-admissible covering of the Galilei relativity (200 pp), Hadronic Press. In press.
- "The conjecture of a Lie-admissible covering of Einstein's special relativity for strong interactions". In preparation.

F. Editorial

Conducted referee work for:
- Physical Review Letters,
- Physical Review D,
- Annals of Physics.

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Texts and Monographs in Physics

Ruggero Maria Santilli

# Foundations of Theoretical Mechanics I

## The Inverse Problem in Newtonian Mechanics

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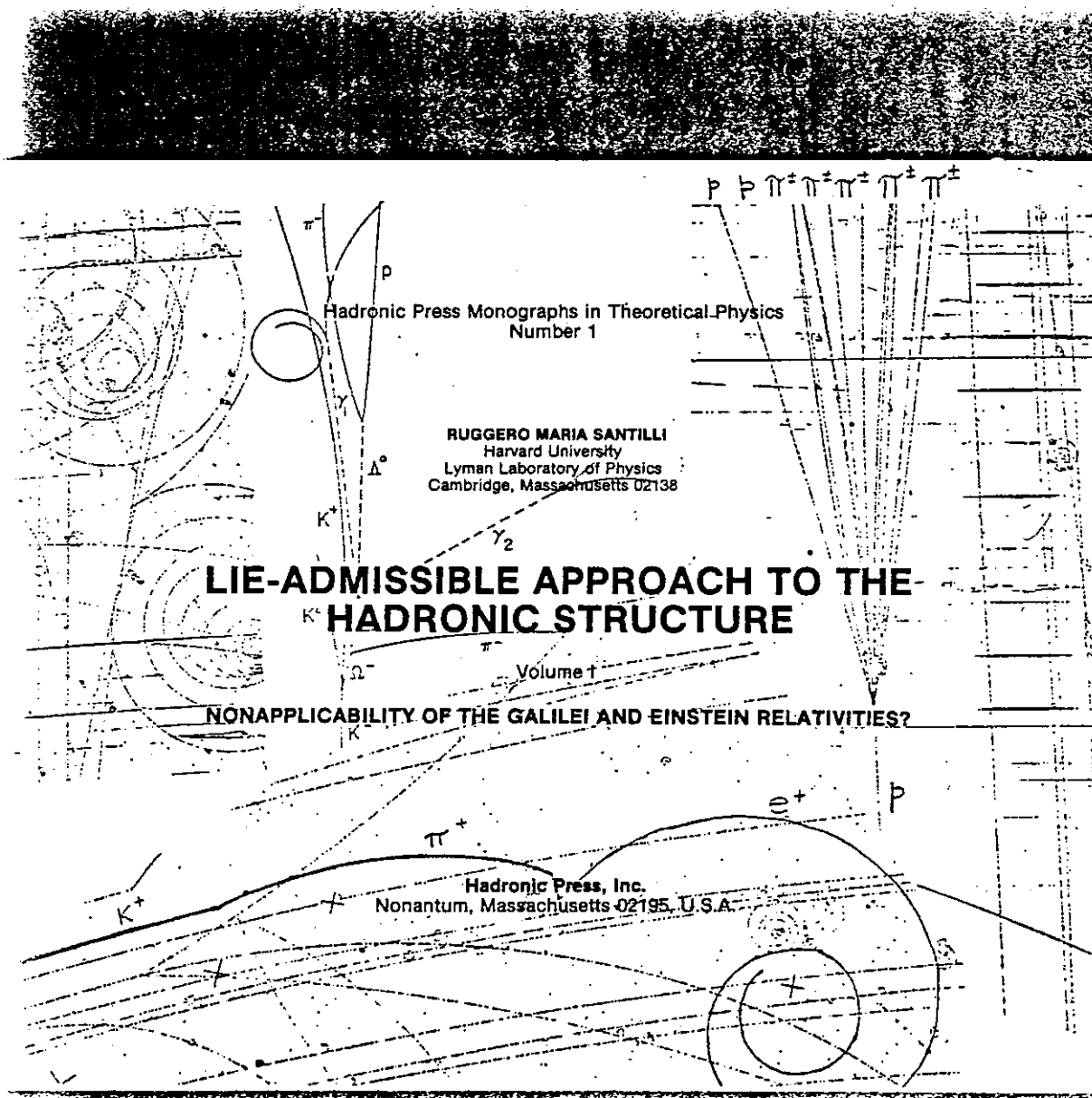

Hadronic Press Monographs in Theoretical Physics
Number 1

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# LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE

Volume 1

## NONAPPLICABILITY OF THE GALILEI AND EINSTEIN RELATIVITIES?

**Hadronic Press, Inc.**
Nonantum, Massachusetts 02195, U.S.A.

HUTP-77/A066

# ISOTOPIC BREAKING OF GAUGE SYMMETRY*

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ABSTRACT

By using the integrability conditions for the existence of a Lagrangian, a classical gauge symmetry breaking mechanism is introduced whereby: (a) the original gauge Lagrangian is replaced by an equivalent chiral Lagrangian without changing the local variables and conserved currents, (b) the original gauge symmetry is broken by the equivalent Lagrangian and is replaced by a different symmetry which leads to the conservation of the original charge current, and (c) this different symmetry generally results to be a mixture of space-time and internal transformations.

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10/77

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HADRONIC JOURNAL 1, 223-423 (1978) 

# On a possible Lie-admissible covering of the Galilei relativity in Newtonian Mechanics for nonconservative and Galilei form-noninvariant systems

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Cambridge, Massachusetts 02138

Received January 16, 1978
Revised version received April 3, 1978
Final version received April 27, 1978

Abstract

In order to study the problem of the relativity laws of nonconservative and Galilei form-noninvariant systems, two complementary methodological frameworks are presented. The first belongs to the so-called Inverse Problem of Classical Mechanics and consists of the conventional analytic, algebraic and geometrical formulations which underlie the integrability conditions for the existence of a Lagrangian or, independently, of a Hamiltonian. These methods emerge as possessing considerable effectiveness in the identification of the mechanism of Galilei relativity breaking in Newtonian Mechanics by forces not derivable from a potential. Nevertheless, they do not exhibit a clear constructive capability for a possible covering relativity. For this reason, the second methodological framework is presented. It belongs to the so-called Lie-Admissible Problem in Classical Mechanics and consists of the covering analytic, algebraic and geometrical formulations which are needed for the equations originally conceived by Lagrange and Hamilton, those with external terms. These formulations are characterized by the Lie-admissible algebras which are known to be genuine algebraic covering of Lie algebras, and which in this paper are identified as possessing (a) a direct applicability in Newtonian Mechanics for the case of forces not derivable from a potential, (b) an analytic origin fully parallel to that of Lie algebras, i.e., via the brackets of the time evolution law, (c) a covering of the conventional canonical formulations as classical realizations, (d) an implementation at a number of levels of Lie's theory, including a fundamental realization as enveloping nonassociative algebras, (e) a generalization of symplectic and contact geometry as geometrical backing and (f) the capability of recovering conventional formulations identically at the limit of null external forces, here interpreted as relativity breaking forces. A covering of the Galilei relativity, called Galilei-admissible relativity, is then conjectured for independent scrutiny by interested researchers. A number of potential implications, particularly for hadron physics, are then briefly considered for future detailed treatment.

*Supported by the U.S. Department of Energy under contract number ER-78-S-02-4742.A000.

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HADRONIC JOURNAL 1, 574-901 (1978) 

# Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle.

**Ruggero Maria Santilli***
Lyman Laboratory of Physics
Harvard University
Cambridge, Massachusetts 02138

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Revised version received June 5, 1978
Final Version received June 19, 1978

## Abstract

This paper is a call for theoretical and experimental studies on the problem whether the relativity and quantum mechanical laws which have proved so effective for the atomic as well as the nuclear constituents are truly verified also for the hadronic constituents. For the intent of stimulating these studies, this paper is devoted to the problem whether a violation of the laws considered in the arena considered is conceivable, plausible and quantitatively treatable on grounds of our current knowledge. This problem is studied according to a number of sequential steps.

First, we conduct a critical analysis of the quark models on the hadronic structure to the effect of indicating that, perhaps, their known problematic aspects are only the symptoms of a much more fundamental problem of consistency at the level of the basic laws. By noting that the available unitary models produce a Mendeleev-type classification of hadrons of unequivocal physical effetiveness and of virtually conclusive character, we search for a compatible but fundamentally different model of structure along much of the differentiation between the problem of classification and that of structure which resulted as necessary at the atomic level.

* Supported by the U.S. Department of Energy under contract number ER-78-S-02-4742.A000

We then enter into the study of a conceivable new model of hadronic structure which is capable, on one side, of achieving compatibility with the established models of unitary classification and, on the other side, of resolving the fundamental problematic aspect of available models of structure, the identification of the hadronic constituents with physical particles. According to our priorities, we assume as fundamental the problem of the nature of the forces of the hadronic constituents. The second problem in our priorities is that of the disciplines capable of treating the assumed type of strong hadronic forces. The last problem in our priorities is that of the construction of a structure model of hadrons and of its confrontation with physical reality.

A crucial experimental data of the hadronic phenomenology is that the charge volume of hadrons does not appreciably increase with mass (contrary to the correspondent occurrence at the nuclear level) and it is of the same order of magnitude of that of any other know, massive and charged particle. It then follows that, if the hadronic constituents are massive, charged and physical particles, that is, non-point-like, they are bounded according to a state of penetration of their charge volumes (or wave packets). This yields a realization of the strong hadronic forces as being nonlocal and nonderivable from a potential, that is, a type of force which is beyond our current knowledge at this time for any effective, quantitative treatment. We therefore approximate these forces with local forces nonderivable from a potential. This yields forces which, at the primitive Newtonian level, are the nonconservative forces of the systems of our everyday experience. The fundamental physical character of the assumed strong hadronic forces is therefore that of being nonconservative. These forces essentially constitute the simplest conceivable analytic generalization of the Lorentz force, in the sense that the Lorentz force is linearly dependent on the velocities and derivable from a potential (variationally selfadjoint forces), while our strong hadronic forces are dependent on the velocities in a generally nonlinear way and are non-derivable from a potential (variationally nonselfadjoint forces).

We then enter into the study of the quantization of nonconservative Newtonian forces in general and of strong nonselfadjoint hadronic forces in particular. For this purpose we briefly recall the dual methodologies for the classical treatment of the forces considered, as presented in details by the author in preceding papers and forthcoming monographs, those of the Inverse Problem and of the Lie-Admissible Problem. The paper essentially presents a study for the quantization of these methodologies which results in a proposed dual covering of Schrödinger's and Heisenberg's equations. A central result is that, under the condition that the quantum mechanical algorithms at hand (r, p, H, M, etc.) possess a direct physical significance, the brackets of the time evolution law must violate the Lie algebra identities as the fundamental condition for mathematical and physical consistency for the case of nonselfadjoint forces. Instead, the brackets considered can characterize a covering Lie-admissible algebra, in precisely the same way as it occurred at the classical level. Intriguingly, there is the emergence also of the Jordan algebras, which therefore acquire an apparent fundamental methodological role for the quantum mechanical treatment of nonconservative forces, perhaps equal to that of Lie algebras. Indeed, the brackets of the proposed covering of Heisenberg's equations result to be, jointly, Lie-admissible and Jordan-admissible. The epistemological lines for a possible covering of conventional quantum mechanics, here called hadronic mechanics, are presented. It is then pointed out in details, either via the generalized algebraic structure of the theory or via direct analysis of the dynamical behaviour, that the inflexible laws of quantum mechanics (here called atomic mechanics) for the treatment of selfadjoint forces are fundamentally inapplicable to the broader physical context constituted by strong nonselfadjoint hadronic forces. Instead, the familiar quantum mechanical laws appear to be replaced by covering laws capable of identically recovering the former at the limit of null forces non-derivable from a potential.

As a necessary complement to the above dynamical analysis of the problem, we then study the relativity laws which are applicable in nonconservative quantum mechanics (the hadronic mechanics in our terminology). This objective is achieved by quantizing the Lie-admissible covering of Galilei's relativity for nonconservative Newtonian mechanics proposed by the author in a recent paper. This results in the proposal of a quantum mechanical covering relativity for the hadronic constituents, under the assumed broader forces, which is Lie-admissible in algebraic character and, as such, capable of identically recovering the conventional relativity of atomic mechanics at the limit of null nonselfadjoint forces. It is pointed out that established relativities (Galilei's, Einstein's special and Einstein's general relativity for the interior problem) are inapplicable to the considered more general nature of the strong interactions. In particular, the proposed covering relativity results to be of non-Lie, non-conservative, non-inertial, non-linear, non-geodesic, non-symplectic and non-Riemannian character to technically characterize physical systems which are non-derivable from a variational principle. In conclusion, our studies on the dynamical profile of the problem of quantization of nonselfadjoint forces result to be in full agreement with the corresponding studies on the relativity profile.

We then enter into the study of an apparent dichotomy of physical laws for the hadronic phenomenology: the unequivocal validity of established laws for the behaviour of a hadron as a whole under ut most electromagnetic interactions and the conceivable applicability of covering laws for the hadronic constituents. This problem is studied via the use of nonintegrable, classical and quantum mechanical subsidiary constraints. In figurative terms, the established laws for the total physical quantities of a hadron are imposed as subsidiary constraints to the covering laws for the individual constituents. The emerging overdetermined systems of differential equations result to be consistent (that is, admitting a physically meaningful solution) under the proper selection of nonselfadjoint forces. It is this property which, in the final analysis, has allowed the presentation of the analysis of this paper. According to this approach, established laws are valid by construction for a hadron as a whole, and possible departures are admitted only for the hadronic constituents. In particular, the violation of these laws at the level of the constituents (only) emerges as a necessary condition for the existence of more general structure forces in order to attempt a real departure of the hadronic from the atomic structure. For instance, the imposition of Galilei's relativity at the structure level would imply conservative strong forces. We then argue that, under these conditions, the atomic and hadronic structures are dynamically equivalent.

As the last step in our priority, we finally consider the problem of the construction of a new structure model of hadrons based on this dichotomy of physical laws, and its confrontation with experimental data. The study essentially indicates that the identification of the constituents of unstable hadrons with suitably selected massive particles produced in their spontaneous decays, while prohibited by the conventional relativity and quantum mechanical laws of strict Lie algebraic character, becomes admissible under the proposed covering, relativity and quantum mechanical laws of joint Lie-admissible and Jordan-admissible algebraic character. The case of mesons is considered in detail and it is indicated that the model is capable of producing a quantitative representation of all the intrinsic characteristics of the particles, while offers some genuine hope for a quantitative interpretation of the decay modes and related fractions. Thus, the proposed new model of hadronic structure appears to resolve the fundamental problematic aspect of the quark models, the identification of the constituents with physical particles, while reaching full compatibility with the established unitary models of classification.

As we all know, our current theoretical knowledge can be interpreted as characterized by suitable implementations of the experimentally established knowledge for the electromagnetic interactions which preserve the underlying basic laws. Pending the verification by interested researchers, our analysis essentially indicates that such knowledge can be considered as applicable, provided that the forces (or couplings) are local and derivable from a potential, that is, the system is represented in its entirety via the simple Lagrangian structure $L_{tot} = L_{free} + L_{int}$. If the strong interactions are assumed as dynamically nonequivalent to the electromagnetic interactions and their forces are realized in a form analytically nonequivalent to the Lorentz force, they demand the abandonment of the virtual entirety of our current theoretical knowledge (such as: Galilei's and Einstein's relativities; Heisenberg's equations and Pauli's exclusion principle; scattering amplitude and Feynman diagrams; canonical field quantization and spin-statistics theorem; etc.). Instead, under the conditions indicated, the courageous construction of covering disciplines must be undertaken for the strong interactions in general and for the hadronic structure in particular, in exactly the same way as it occurred for the electromagnetic interactions in general and for the atomic structure in particular.

The paper concludes with remarks concerning the future orientation of experimental high energy physics which is needed to provide means for a physically effective selection among an ever increasing number of hadronic models. It is argued that, until the experimental efforts are essentially restricted to the identification of new particles, the problem of the hadronic structure will likely remain fundamentally unresolved, because the knowledge of new particles adds informations which are certainly useful for the classification of hadrons, but not necessarily for the structure. It is submitted that, jointly with the continuation of these valuable experiments, the fundamental problem of the validity or invalidity of established relativity and quantum mechanical laws for the hadronic constituents is confronted. In the final analysis, if the laws considered will eventually result to be valid in the arena considered, the quark models are likely to emerge as the only conceivable models at this time. On the contrary, if the laws considered will eventually emerge as being violated in the arena considered, the concept of quark as the constituent of hadrons is likely to be ruled out in a final form.

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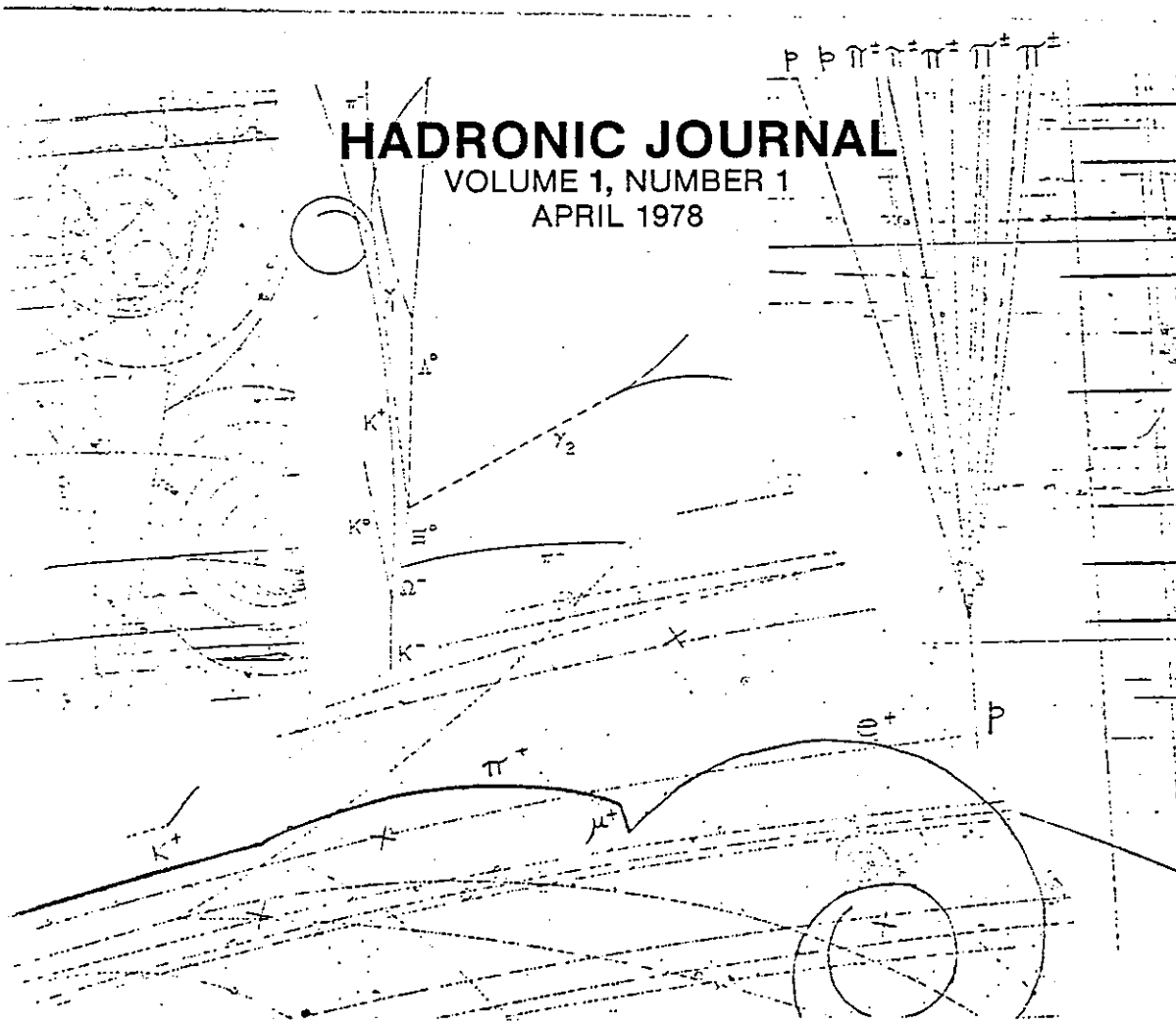
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# HADRONIC JOURNAL


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The particle events printed in half tone in the front page of the Journal are derived from 80" bubble chamber photographs kindly provided by BROOKHAVEN NATIONAL LABORATORY

PART IB:

ACADEMIC
YEAR
1978–
1979

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS
June 17, 1978

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Associate in Mathematics

to serve from March 1, 1978 through May 31, 1979 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

*Secretary to the University*

Ruggero Maria Santilli

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 16, 1978

Professor S. STERNBERG,
Chairman
Department of Mathematics
Harvard University

Dear Professor Sternberg,

I am here respectfully applying for any nonteaching, nontenured, terminal position at your Department of your selection from March 1, 1978 until May 31, 1979 which allows me to conduct research as per DOE grant no. ER-78-S-02-4742.A000.

I would also appreciate whether my salary of $ 30,000 for the period March 1, 1978 until May 31, 1979 and payable in monthly installments of $ 2,000 allocated by the DOE grant is formalized and charged to the Harvard code number 33|966|7131-2.

Copies of the formal notification of execution by DOE of the indicated grant and of the Harvard ORC office for the code number should be in your file. Additional copies are at your disposal.

As verbally indicated, you can count on my committment that any possible paper which might originate from my research will reach departmental standards prior to any outside release. Also, I would be happy to comply with any specific guidelline for the conduction of the research as per the DOE grant you might consider appropriate. Almost needless to say, I would be honored to collaborate with any departmental member.

Finally, I would like to stress that my application is only for a terminal appointment. Under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after May 31, 1979 or for my relocation.

Yours, sincerely

Ruggero Maria Santilli
Honorary research fellos

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

June 2, 1978

Professor SHLOMO STERNBERG
Chairman
Department of Mathematics
Harvard University

Dear Professor Sternberg,

I have just received copy of the execution by Harvard University of the DOE grant no. ER-78-S-02-4742.A000. Permit me to express the sentiments of my sincere appreciation for the possibility of conducting research under this grant at the Science Center of Harvard University.

I would like to take this opportunity to confirm that this is a nonteaching, nontenured and terminal research appointment which, as such, cannot be counted either directly or indirectly, for tenure considerations. Also, permit me to confirm that under no circumstances, either direct or indirect, should Harvard University be responsible for my salary after termination of the appointment on May 31, 1978 or for my relocation. Finally, I expect to have no formal affiliation with Harvard University after the aforementioned date.

I have already initiated the search for a job at another campus and I hope to complete it sometimes by late 1978 or the early part of 1979. Permit me to indicate that, even in case I am unable to find such a job, I do not intend to apply for financial support at Harvard University under any form, at the expiration of the current DOE grant.

Hoping that the research activity of this DOE grant can be scientifically productive, I remain

Very Truly Yours

Ruggero Maria Santilli
Honorary Research Fellow

Professor S. STERNBERG
Tel-Aviv University
Department of Mathematics
RAMAT-AVIV, TEL-AVIV, ISRAEL

July 10, 1978

Dear Shlomo,

I would appreciate your help for the following rather unusual occurrence.

By separate air mail parcel, I have mailed to you a record on the music by the late John BOROS. After listening to this music, if you so desire, I would appreciate whether you can donate this record to some public collection of records in Israel of your choice (that at Tel-Aviv University, if any, would be fine).

John was my truly best and intimate friend. He was Assistant Professor and composer of modern music at the Dept of Music of Brandeis. I was close to him during his laborious composition of this music and I considered him truly promising. Regrettably, as you eventually recall, he died with his wife Emy in a tragic car accident in Waltham during a police chase some time ago. I have provided my best possible help for a fund raising effort organized by his collegues at Brandeis to have John's music performed by professionals and recorded. We have succeeded in this effort by raising all the necessary money and the record has been entirely payed. Therefore, there is no need of additional donations.

I believe that it would be nice if John's music is present in the Country of his Ancestors.

Here everything is quite and peaceful during this summer period. I shall soon air mail you the second issue of the journal. If there is anything I can do in your absence, please let me know.

I wish you a happy stay in Tel-Aviv (and I miss your presence in the department).

Sincerely

Ruggero

CONFIDENTIAL - July 23, 1978

Dear Shlomo,

I have mailed on July 22 (Saturday) a complimentary copy of the Hadronic Journal for your amusement. The issue contains my second article which, in essence, is a direct and open attack at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in the subsequent issues of the Journal by independent authors. It appears that, in case these experiments result to be negative, the quark conjecture will likely be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appeal to Panofsky (director of SLAC) for the conduction of the proposed experiments in due time. As a background information, you should know that experiments on the verification of the validity (or invalidity) of Einstein's special relativity for the hadronic constituents have already been proposed years ago. However, since the mere considerati of these experiments is a profound disturbance to quark people (let me say it, because of money-academi aspects related to research grants), these experiments have been completely ignored.

Owing to this situation, and after due consideration, I decided to use the open, clear and candid language in my letter to Panofsky you can see, as a necessary prerequisite, jointly with its formal submission to officers of Governmental Agencies, to prevent the repetition of the traditional procedure to avoid unwanted lines of study: complete ignorance. Thus, the language of this letter (as well as the provocative language of my papers) is not the result of an emotional state, but instead of a meditated decision. Of course, this could render my guilt even greater. I am therefore left with only one alternative: hope in your benevolent understanding if I misjudged some of these steps.

Please note the letter head and my biographical notes of the letter to Panofsky. As you can see, I have carefully avoided any disclosure whatsoever of my association with the Dept. of Math. and with you. You can rest assured that this procedure will be followed in the future ad litteram.

As I indicated earlier to you, this second paper in the Journal is my last of conjectural, speculative, high energy physics nature. I have no words to express my gratitude to you for the possibility of spending one year at the Science Center. Indeed, I intend to study mathematics as much as possible. Beginning from September, I would like to quietly and silently listen to all your graduate courses and lectures, as well as to others at the Dept. of Math. It is a unique opportunity for me to improve my grossly deficient knowledge of mathematics and I do not intend to miss it.

In case you are interested, my next research objectives are the following. First of all, I am not contemplating to write papers for quite some time. Secondly, I would like to give all my necessary or useful assistance to experimentalists interested in the study or actual conduction of the experiments I have proposed (the most critical one, in my view, is the verification of Pauli's principle for strong interactions restarting at the nuclear level). But this will take a minimal amount of time and it is not expected to result in papers.

My long term theoretical objectives are the following. First, I am interested in studying the possible existence of a covering of Einstein's special relativity in classical field theories with nonlinear derivative couplings (or essentially nonselfadjoint in my language), i.e.,

$$\left\{\left[\left(\Box + m^2_{(E)}\right)\phi_K - f\left(\phi; \frac{\partial\phi}{\partial x}\right)\right]_{\underline{SA}} - F\left(x; \phi; \frac{\partial\phi}{\partial x}\right)\right\}_{\underline{NSA}} = 0 \qquad (1)$$

The central objective is to see whether it is possible to construct a covering of the conventional Lorentz covariance law

$$\phi'(x') = S(\Lambda)\phi(x) \qquad (2)$$

with an understanding that this law unequivocally applies for essentially selfadjoint field equations, that is, systems with ut most linear derivative couplings (this includes the models of constructive field theories, and of unified gauge theories of weak and electromagnetic interactions ONLY, the inclusion of the strong interactions being highly questionable and a mere opinion, as you can see from my paper, particularly Section 5.20 ).I am expecting that, if a covering covariance law exists for Eqs. (1), it will be nonlinear. I am also expecting a possible primary constructive role of Lie-admissible algebras, on the basis of their crucial role (to the best of my understanding at this time) for the explicit construction of the strictly nonmanifest transformations for the form-invariance in Newtonian mechanics of the same class of systems (nonconservative, nonlinear in the velocity and explicitly time dependent).

If you are interested, my truly long range theoretical objective is to attempt the construction of a covering of Einstein's general theory of gravitation for the interior problem only (I am a beliver of Einstein's general theory for the exterior problem), along the lines outlined in pages 873-875 of the HJ.The idea is to abandon the use of mass terms in the equations for the interior problem; to confront instead the structure problem at least at a rudimentary classical level; to allow for the existence of more general structuresof the strong interactions (essentially nonselfadjoint strong hadronic forces, in my language which, as such, appear to be incompatible with Einstein's equations for the interior problem only on a number of counts); and finally and most importantly, to construct the gravitational field for the exterior problem, according to exactly the experimentally verified Einstein's approach, with the sole use of the structure fields. In this way, the gravitational field is not superimposed as an independent entity to other fields. Instead, it emerges as a direct consequence of the structure fields (electromagnetic and strong) of matter. In different terms, the idea is to attempt the very removal of the problem of a unified theory which, in any case, has escaped so many studies. I have conducted a preliminary study of this approach a number of years ago (Ann. Phys. 83, 108 (1974)) and it appears truly promising and, to me, quite intriguing. Again, the experimentally established Einstein's theory for the Riemannian characterization of the exterior gravitational problem remains in full effect by construction. However, at this moment I see no way of preserving the use of the Riemannian geometry for the interior problem, when the structure of matter is included as the central aspect and essentially nonselfadjoint strong hadronic forces are admitted. Equivalently, the approach is based on the traditional speculation of basic aspects, that via criticism of the experimentally unverified validity of the Riemannian geometry for the interior problem, besides its physical character of providing a first, crude approximation of physical reality for the interior gravitational problem of massive objective (I have seen no experiment conducted in the interior of a star....).

You can easily see that, for even an initial, orientational conduction of these studies, I need a number of years of study of Geometry. I would be truly grateful whether, during next year, you can guide me for an effective study of this discipline. It would take only seconds of your time on an occasional basis to indicate valuable references for me to digest.

Almost needless to say, in case you are personally interested in this delicate attempt to initiate a direct study of conceivable coverings of Einstein's ideas, I would be simply honored to work for joint papers. Also, I would be happy to work for joint projecu with you in any other topic in which I can produce a meaningful contribution.

In closing, you might be interested in few other aspects. The Hadronic Journal has attained financial selfsufficiency owing to the very positive outcome of subscriptions (still growing on a dayly basis). For this objective I have put my previous corporate experience at work in its entirety (I have been Chairman of the Board of a U.S. corporation from 1970 until 1974). In essence, as a condition for my acceptance of the job to organize the Journal I have asked and obtained a post in the board of directors of the publisher, which I obtained. Then, I have asked and obtained the strict implementation of extremely conservative business practices.

In particular, by formal board policy, the printed has no accounts payable at all (a true rarity in U.S. business enterprises). All bills are payed cash on delivery. Most importantly, all production is prepayed and no production is order for stock, a part for few samples. Since the cost of printing the Journal in its current form (mainly offset and few typeset pages) is minimal and the individual runs and reruns of each issue can be for small amounts ( 150 to 200 copies), the company can effectively and proficiently operate under such conservative practices.

I am happy to report that we have already achieved the smallest period of time between submission and distribution of the trade. Our average now runs around 45 days (again, between arrival of the papers and distribution, that is, mailing, and therefore including refereeing, typesetting, printing and mailing). Notice that this is for papers without regards to length, as you know, and without publication charges, which are also rather unique in the journal trade.

My sole concern is for the quality of the papers. I see no way of achieving the desired quality via only papers in the current hadron physics (which is not a science, like Mathematics, in my view, but the mere expression of opinions by individual physicists). The need of clean papers by mathematicians is therefore a must.

I would appreciate whether you could kindly indicate the HADRONIC JOURNAL to some friend in Israel or abroad. They might be interested because of the rapidity of production, the lack of restrictions on length and the lack of publication charges (reprints can be produced at a charge to the publisher). This, of course, includes also possibly interested friends in Russia or anywhere with potential difficulties, say, of publication charges. The topic, as you know, is any aspect of math and physics.

Almost needless to say, as it was for the first issue, any additional paper of yours you might decide to publish with the Journal, will have utmost priority and will be the first of the issue.

Incidentally, the Journal is now regularly reviewed by Math. Review and Curr. Math. Publ. and this should help mathematicians, jointly with the availability of the Journal in the general libraries.

I have made 750 reprints of my second article in the second issue which are now being mailed. I have made this at my own expenses (an invoice is enclosed). I can afford it because Carla has a summer job and (finally!) I have a regular salary. I have also instructed Donna to let me know any eccess expenditure on our grant on my behalf in excess of the agreed $ 300, because I intend to repay it immediately either personally or via small funds I have from the Journal.

Finally, I enclose copy of my formal thanks to Tinkham and all Lyman collegues for the hospitality they have provided for me during 1977/78.

Sinceramente Tuo

Ruggero

P.S. A good news! just received. It appears that experimentalists are interested in considering my proposed experiments. Even though this is for a long term possible outcome, FINALLY it is the first time that physicists acknowledge the need of subjecting to direct experiments the basic physical laws for strong interactions. It was time indeed! I shall keep you informed of relevant events.

Prof. D: KAZHDAN

July 24, 1978

Dear David,

I enclose for your amusement a complimentary copy of the second issue of the HADRONIC JOURNAL. It contains my second article which, in essence, is an open, direct attach at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in subsequent issues by independent authprs. There are indications that, in case these experiments are negative, the quark conjecture could be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appear to PANOFSKY (director of SLAC) for the implementation of the proposed experiments. As a background information you should know that experiments on the verification of the validity or invalidity of Einstein's special relativity for the hadronic constituents have been proposed years ago. However, since the mere consideration of these experiments is disturbing to quark people (primarily for money-academic and grant related issues), these proposals have been completely ignored until now. The open, candid and clear language of my letter was therefore a necessary prerequisite to avoid (at least I hope) the repetition of the traditional procedure to bypass unwanted lines of studies: complete ignorance.

It is only with regret that I must acknowledge that Mathematics is a Science. Hadron physics, instead, is an opinion. At least at this time.

I have no words to express my gratitude for the possibility of spending one year at the Science Center. Indeed, I intend to study as much mathematics as possible. Beginning from September, I would like to follow, quitely and silently, graduate course in mathematics. It is a unique opportunity for me to improve my grossly deficient knowledge of mathematics and I do not intend to miss it.

I also enclose copy of my formal thanks to M. Tinkham for the hospitality they have provided for my visit, which expired on June 30, 1978.

Have a happy summer with your famility.

Sincerely,

Rufino

P.S. Please notice in the enclosed letter heads and curriculum that, as promised to Shlomo, I indicate my association only with the Science Center and exclude any release whatsoever of my association with the Dept. of Math. as well as with Shlomo. You can rest assured that this procedure will be followed ad litteram.

Prof. R. Bott

July 24, 1978

Dear Raoul,

I enclose for your amusement a complimentary copy of the second issue of the HADRONIC JOURNAL. It contains my second article which is, in essence, a direct, open attack at the jugular vein of the quark games: the basic laws. A number of experiments are proposed and others will appear in subsequent issues by independent authors. There are indications that, in case these experiments will result to be negative, the quark conjecture may be ruled out in a final form.

I also enclose for your amusement copy of a candid, passionate appeal to Panofsky (director of SLAC) for the implementation of the proposed experiments. As a background information, you should know that a number of authors have proposed (years ago) the experimental verification of Einstein's relativity for the hadronic constituents. However, since the mere consideration of such fundamental experiment is disturbing to quark people (primarily for money-academic and grant related aspects), these proposals have been completely ignored until now. The open, candid and clear language of my letter to Panosfky, jointly with its formal release to officers of Governmental Agencies, was a necessary prerequisite to avoid the repetition of the traditional tool (at least I hope) for bypassing unwanted lines of studies: complete ignorance.

Dear Raoul, you see, Mathematics is a Science. Hadron Physics is, instead, an opinion. This is why, as anticipated in my former note to you, the second paper in the Hadronic Journal is my last of conjectural high energy physics nature.

I have no words to express my gratitude for the possibility of spending one year at the Science Center. Indeed, I intend to study as much mathematics as possible. Beginning from September, I would like to follow quitly and silently as many graduate courses in mathematics as possible. It is a unique opportunity to improve my grossly insufficient knowledge of mathematics and I do not intend to miss it.

I enclose copy of the front page and acknowedgments of my first monograph with Springer. Robert Brooks did a simply excellent review job (jointly with a number of other collegues). I have managed to let him have via Springer a check for $ 100 (and probably more at some later time).

Finally, I enclose copy of my formal thanks to M. Tinkham for the hospitality I received at Lyman, which ended on June 30, 1978.

Have a happy summer.

Sincerely

Ruggero

P.S. Please note in the enclosed letter head as well as biographical notes, that I indicate my association only with the Science Center, as promised to Shlomo. No disclosure whatsoever with the Dept. of Math. and with Shlomo is released. You can rest assured that I will follow this procedure ad litteram.

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

December 27, 1978

Professor DEREK C. BOK, President,
Harvard University, Massachusetts Hall 1.

STRICTLY CONFIDENTIAL

Dear Professor Bok,

I feel ethically and scientifically obliged to bring to your personal attention certain delicate events at the Lyman Laboratory of Physics. According to information available to me, it appears that there exists a realistic risk whereby the Lyman Laboratory of Harvard University might be directly involved in a potential public action for [redacted] by malcontent U.S. physicists.

This letter is motivated by my sincere desire to provide you and Dean LEAHY with all the necessary background information. I would like, of course, leave to you the decision whether a preventive action to keep this risk under control is appropriate at this time or not.

The candid language I have selected for this letter is solely intended to avoid possible misrepresentations, in the sole benefit of clarity. Also, the topic is such to stir up great emotions in the scientific community, as you will see. I would like to beg for your benevolence if I have failed to control my own emotions, or if I have failed to properly interpret the information.

THE SCIENTIFIC SECTOR UNDER CONSIDERATION. The sector of basic research in theoretical and experimental physics under consideration is that of the strongly interacting (subnuclear) particles, called hadrons (e.g., proton, neutron, pion, and a multitude of others). Predictably, the understanding of this subnuclear layer of the physical reality turned out to be substantially more complex than that for the broader nuclear and atomic layers. Among the variety of problems, most notable are the problem of the classification of hadrons into families (the equivalent of the Mendeleev classification of atoms) and the complementary but different problem of the structure of each element of a given family (the equivalent of the Bohr or the Thomas-Fermi model of structure of atoms).

You are aware that this sector of research is nowadays conducted at the cost of billions of dollars per year. Therefore, you should keep into account a considerable financial profile at each and every level under analysis.

NONTECHNICAL OUTLINE OF THE CURRENT STATE OF THE ART. As you know, physics is a discipline with an absolute standard of values: the physical reality. Until theoretical ideas have not been proved via incontrovertible experiments, they constitute conjectures, hypotheses, or beliefs, but not manifestations of the physical veritas. In line with this standard, I can confidently state that the problem of the classification of hadrons of Mendeleev type is nowadays accomplished as the result of brilliant contributions by M. GELL-MANN and G. ZWEIGH OF 1964, as well as numerous others. In line with the same standard, I can with equal confidence state that the problem of the structure of hadrons is, at this moment, fundamentally open. We simply have a pletora of models

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each of which is controversial, conjectural and without final experimental backing.

The majority of research has been devoted until now to the study of the conjecture that the so-called quarks are the constituents of hadrons. These efforts are essentially based on the hope that one single model (the so-called SU(3) model) can resolve both, the problem of classification and that of structure, contrary to the corresponding differentiation of these aspects which resulted as necessary at the atomic level.

As you are eventually aware, despite a laborious search and investments of the order of billions, it has been impossible to physically establish the quarks as the actual constituents of hadrons. This has induced quark-believers to attempt the construction of a so-called model of confinement whereby the constituents of hadrons (contrary to all other occurrences in the microscopic world) cannot be produced as free. Despite additional huge investiments in money and human resources, an effective model of confinement which can be accepted by the scientific community at large has not been achieved as of now.

To complicate the problematic aspects, certain technical needs have forced quark-believers to multiply the number of different, unidentified quarks. The net result is that the (unitary) models of hadron structure along the quark conjecture are based on a complex topology of assumptions, each of which is of fundamental physical character, but experimentally unestablished.

I would like to add, for your amusement, that quarks are sometimes called the "Yeti particles", or that the sea of studies along the quark conjecture has been compared to the historical episode of the ether, or that the strong dominance of the scientific scene by the (rather powerful) quark-believers has been compared to the control of science by priests during the times of GALILEO GALILEI. In any case, the very inventor of quarks, M. GELL-MANN, has not yet stated whether he believes that quarks exists or not.

THE INDICATIONS FOR A DANGEROUS LEVEL OF MALCONTENT IN THE PHYSICS COMMUNITY.

To the best of my knowledge and reconstruction, there has been a continuous evolution of the reaction by the scientific community to the financial investements for quarks which, starting from a scientifically effective debate, has evolved up to an apparent dangerous level of malcontent due to a number of social factors.

In essence, the period 1964-1970 (again, to my simplistic and perhaps erroneous understanding) was that of the great hopes for the experimental detection of quarks. Jointly, this was the period in which numerous outstanding scientistis had expressed serious doubt as far as the validity of the quark conjecture is concerned. But they were silenced by vigorous and equally powerful quark-believers.

The period 1970-1975 has seen a marked increase of the concern of valuable physicists and the first timid expression of reservation as far as the amount of money invested in quark conjectures is concerned, as well as the lack of a well balanced condition and conduction of studies in the sector. Subsequent events have moved much faster. In essence, the substantial financial restrictions on the community of basic research have implied the direct consequence that quark-nonbelievers could not benevolently tolerate huge sums of money invested in the quark conjecture, while they were without research support at all, or even unemployed with families to support.

According to information available to me, this level of malcontent has now reached such a proportion to justify the consideration by administrators such as you and Dean LEAHY. The current situation (again, to my perhaps erroneous understanding and reconstruction) can be divided into a number of aspects.

There is first a group of moderate (please read here: tenured,employed,salaried) physicists who are performing a vital moderating function and attempting to implement an orderly adjustment of situations. Then there is a second group of highly malcontent (please read here: without the dream of tenure, or unemployed, etc.) physicists who have lost hopes for an orderly improvement of the dispersal of funds in basic research without grave gestures.

Apparently, the first group has expressed the concern directly and independently to President CARTER. The rationale of this group is the following. The United States is a (beautiful but) technologically oriented Country with a rather cloudy long term future. This future VITALLY depends on our capability to implement NOW a well balanced condition and conduction of basic studies. In particular, as it was the case for the achievement of the structure of the atom and the nucleus, the achievement of the final solution of the problem of the hadronic (subnuclear) structure is expected to play a crucial, if not a vital role for survival, owing to a knowledge of primary relevance for energy related issues. It is therefore essential that any monopoly of funds in this sector is cathegorically avoided and that, instead, ALL conceivable approaches to hadron structure are studied, and subjected to a comparative confrontation with physical veritas, in the true pursuit of human knowledge. On financial grounds, this first group of concerned physicists essentially recommend that studies along quark conjectures are indeed funded, but that jointly, studies along fundamentally different approaches begin to receive support. The proportion of diversification of funds in the sector in subsequent years should then be a clear reflection of a clear interpretation of experimental data. If quarks are experimentally established, they should receive 100 % of the funds If experiments disprove the possible existence of quarks, all funds along these conjectures should be truncated.

I personally share this position in its entirety. Besides ethical aspects, the future of my children is at stake here.

The action of the second group of highly malcontent physicists is, in my perhaps erroneous, personal view, potentially explosive and, as such, deserving the utmost possible consideration. Oddly, this situation has been literally created by the quark-beliebers, owing to an often presumptuous attitude from their status and the methodic realization of their personal beliefs via the use of scientific power (rather than veritas). In essence, a number of physicists is unemployed. This is partially due to the unavailability of tenure and still partially to the rejection of proposals (particularly by the National Science Foundations and apparently) of non-quark-inspiration. What has rendered this situation intolerable to these physicists (again to my understanding and reconstruction) is that in certain instances these technical applications for support have been rejected by quark-believers WITH A FLATLY OFFENSIVE LANGUAGE. I have seen personally some of these reports and, although I am not an attorney, these referee reports are, in my view, an expression of shear irresponsibility by tenured physicists currently under the "quark-money-tree".

As a result of these last events, I have been informed of specific accusations of monopoly of funds by studies along quark conjectures, as well as of scientific corruption, that is, the use of scientific power to prevent the allocation of funds to any study on hadrons other than quark-oriented, as well as to prevent the realization of experiments that might disprove the validity of the quark conjecture.All this allegedly occurring at the academic and not the agency level.

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I feel also obliged to report the (actually uncontrollable) rumor that the submission of a documented petition for a Senate Hearing on alleged [redacted] has been avoided by the partecipation of moderate, concerned physicists (at least temporarily).

MY EDITORIAL ACTIVITY. I have recently organized a new journal in theoretical physics, called the HADRONIC JOURNAL. A number of informative leaflets are enclosed. The undisclosed function of this journal is that of attempting a moderating action in this delicate moment of the research sector, jointly with the conduction of a promotional action for all valuable studies on the hadronic structure, whether quark-oriented or not, because this problem is fundamentally open at this time and (in GALILEI's word) "the authority of a thousand" is not sufficient to establish a scientific truth.

After the first year of operation, with a successful financing and completion of the Volume 1 (for over 1,600 pages) I am happy to report to you that the HADRONIC JOURNAL is already considered in the scientific community as one of best evidences of the LACK of existence of a monopoly by quarks, at least on editorial grounds (the more insidious financial profile is, of course, out of my reach). Indeed, virtually all issues the journal publish papers along quark conjectures jointly with papers along fundamentally different approaches to hadrons, in the genuine pursuit of human knowledge, as well as in substantial disrespect of any financial or academic interests of any specific group of researchers.

My position as Editor in Chief of this "scientifically liberal" journal, as well as the fact that I am the recipient of one of the first research grants of non-quark-inspiration (see below) has put me in a rather peculiar situation, which somewhat indicates the reasons for my being a recipient of the indicated delicate information.

In turn, this has allowed me to perform my intermediary action to the best of my judgment and possibility. For instance, in July 1978 I wrote a letter to Professors PANOFSKY, WILSON and VINEYARD, Directors of the SLAC, FERMILAB and BROOKHAVEN Laboratories, respectively, denouncing the current status of condition and conduction of research and expressing my personal concern. A copy of this letter is at your disposal (also for your amusement). In essence this letter presents itself as a vigorous call for a moment of reflection. In actuality, it was used to calm down excessive malcontent. Even though the treatment of the financial profile was carefully avoided, and the aspects were mainly technical, the ultimate motivation of this letter was the malcontent I am reporting to you here.

HARVARD'S ROLE IN HADRON PHYSICS. S. COLEMAN, S. GLASHOW and S. WEINBERG of the Lyman laboratory of Physics are three of the utmost leading exponents of the quark conjectures. Their scientific status and credibility is quite high, and they have an active involvement in virtually all sectors of hadron physics, from research- editorial to funding functions. In addition, there is a quite valuable experimental group (but I am not an experimentalist and I would like to abstein from commenting on this profile). As a result, the Lyman Laboratory of Physics is considered as one of the leading centers of studies along quark lines on a world wide basis.

These occurrences should be solely reason of pride under normal circumstances. Regrettably, they should also be seen as reason for concern, particularly from an administrative profile. This is due to the total absence of any hadron study whatsoever at Lyman Laboratory other than quark oriented. The net effect is that the Lyman Laboratory has been depicted to me by outsiders as a typical

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representative of the monopolistic condition and conduction of research for hadrons, as a clear hint for a potential direct implication of Harvard in possible public actions for alleged scientific corruption.
I urge you and Dean LEAHY to conduct an independent verification of these personal remarks. Again, I present them in good faith, but they can be erroneous.

THE EPISODE OF MY VISIT AT LYMAN. I joined the Lyman Laboratory on September 1, 1977 with a one-year appointment as "honorary research fellow". On December 1977 I was authorized to file a research grant application to the DEPARTMENT OF ENERGY (DOE) jointly with Professor S. STERNBERG of Harvard's Department of Mathematics, with Lyman Laboratory being my affiliation of the application. On March 1978 the application was funded and the contract was sent by DOE to Harvard for signature.

I discovered at that time that I could not draw a salary at Harvard under my grant because of the term "honorary" in my title. I therefore submitted a formal application to Lyman Laboratory for (A) the removal of the term "honorary" from my title, so that I could at least draw a salary until the termination of my appointment on June 30, 1978, and (B) the extension of such a "research associate" position for a non-tenured, non-teaching, terminal, additional year , with all expenses supported by my grant.

After an elaborate and rather long departmental consideration (rather oddly restricted to only the senior members, despite its research character) my applications for points (A) and (B) was REJECTED with a final vote of the senior members in early June 1978.

This rejection is, per sé, innocuous. It is the way in which my case was handled and the subsequent events, that are reason for concern.

In essence, with the assistance of the Lyman Laboratory I could have moved easily this grant to another Department at Harvard or, in case impossible, to another university. This assistance was clearly crucial because my application had been filed and funded with my affiliation with the Lyman Laboratory.

Instead, S. COLEMAN, S. GLASHOW and S. WEINBERG made it a point in indicating to a number of collegues that my application had been rejected because the research under my grant has "no physical value". This directly created a number of predictable technical and nontechnical problems, including self-evident legal implications. The return of the grant contract to the DOE, unsigned, was avoided thanks to the invaluable partecipation by Professor STERNBERG and Dean LEAHY. I received a one-year research appointment at the Department of Mathematics, although I qualify myself as a member of the "Science Center", as you can see from my letterhead. This is due to the fact that I am not a mathematician and I do not intend to become a mathematician. I am a theoretical physicist involved in conjectural studies on hadrons.

The episode was aggravated by a number of side elements. For instance, in the occasion of my first and most delicate paper with the DOE grant, I consuted S. COLEMAN expressing to him my need for qualified advice. He expressed interest to help, but after the IRS returns. On April 15 I formally submitted this paper to S. COLEMAN (as editor of the HADRONIC JOURNAL) for his confidential review. I subsequently kept him informed via written notes of the progress of this paper. Finally, I was later told that, while S. COLEMAN had kept a total silence with me, he had been very generous of criticisms on this paper during the numerous meetings on my case. Quite frankly, I find this behaviour in strict violation of centuries of scientific tradition, as

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well as in violation of the confidentiality of the formal referee process. The judgment of the ethical aspect is here left to you.

Similarly, I did attempt to express the potential implications of this negative attitude to the senior members, but my action was misinterpreted as "threats". I was left with no other conceivable route than that of truncating any contact with the senior members at Lyman. Copy of the correspondence is at your disposal.

To understand the meaning of the judgment of "no physical value" by S. COLEMAN, S. GLASHOW and S. WEINBERG you should know that one of the primary objectives of my research under the DOE support is that of conducting a critical analysis of quark models in the traditional spirit of unsolved physical problems. Actually, my grant appears to represent (to my understanding) an indication of the desire by the DOE Officers (Drs. HILDEBRAND, PEASLEE and WALLENMAYER,HEP, as well as Dr. KANE, Director of the Division of Physics and Dr.DEUTCH, Director of the Division of Energy) to implement the well balanced condition and conduction of research at Harvard University on hadron structure, as indicated earlier (or, in my language, to avoid a monopolistic restriction of research on hadrons at Harvard along only quark conjectures).

As a result, the judgment of "no physical value" by the indicated quark committed collegues was referred to studies of strict non-quark inspiration. Also, and more insidiously, it was referred as a form of opposition (or it can easily be interpreted as such) to the DOE against the initiation at Harvard of a nonmonopolistic conduction of studies on hadrons.

In defense of my studies permit me to indicate that the central (novel) methods I have identified for my own treatment of the strong interactions have been accepted for publication(since November 1977, and, thus, much before the consideration of my case at Lyman) in one of the most prestigious series of monographs in physics, that by SPRINGER-VERLAG of Heidelberg, West Germany, under the title: FOUNDATIONS OF THEORETICAL MECHANICS, Volumes I and II. The first volume under my DOE grant has already been distributed and I enclose exerpts for your consideration. The second volume is scheduled for the second year of my grant. My papers, also under this DOE grant, have apparently stirred up a considerable form of interest and I have,for your inspection, a rather voluminous file of letters of support for my studies by outstanding physicists from all over the world.A few abstracts of my papers are enclosed.

In case you are interested to inquire for an independent assessement of the physical value of my studies, I recommend you to consult [redacted] Professors [redacted] and [redacted], as well as all the distinguished members of the EDITORIAL COUNCIL of the HADRONIC JOURNAL. A list of additional outstanding scientists of proved ethical standard is at your disposal.

<u>HARVARD'S CURRENT RISK</u>. The rejection of my application at Lyman for the conduction of my DOE grant, the breaking of the confidentiality for the referee process by S. COLEMAN, the judgment of "no physical value" of my research by S. COLEMAN, S. GLASHOW and S. WEINBERG are all INNOCUOUS occurrences for the simple reason that, owing to the current delicate moment of our community, I have kept a complete silence and I have cathegorically absteined from even alluding their existence to outsiders.

The risk for a potential implication of the Lyman Laboratory in a possible public action by malcontent physicists originates from a much more substantial, delicate, and clear occurrence.

The true, ultimate objective of my DOE grant, as well as of my editorial involvement with the HADRONIC JOURNAL, is the following. The most distressing aspect of the current status of hadron physics IS NOT, in my view, the pletora of models and our inability to select the right one. It is due, instead, to the complete lack of experimental verification of the basic physical laws in an incontrovertible form. Specifically, all studies along quark conjectures are based on the supplementary, much more fundamental conjecture that Einstein's special relativity applies under strong interactions in general and within a hadron in particular. The point is that this relativity has been experimentally established only for the electromagnetic interactions and its validity under the strong is a mere BELIEF as of now.

My primary objective is therefore that of studying and promoting the experimental verification of basic physical laws, as currently used in hadron physics. Only after these laws have been verified, the question of quark or non-quark models may acquire a scientific value beyond that of exercises of curiosity.

I should here indicate that I am not the first to stress the need of verifying basic, conventional, physical laws for the strong interactions. Actually, the Founders of Quantum Mechanics suggested it already for nuclear physics. Lately there have been initial proposals of specific tests and even initial attempts of experiments, although with insufficient energies. As of now, it appears that we have indeed achieved the technology to test Einstein's special relativity at extremely small distances, within a subnuclear particle, and I enclose an article by Professor D.Y. KIM from Cambridge-England I have (proudly) published in the HADRONIC JOURNAL. It is understood that our current knowledge for these crucial experiments is at the very beginning and a long labor of trial and error, presentation of ideas and their constructively critical examination by independent researcher, is expected.

Permit me to confess that the aspect of the current status of our community of basic studies which distresses me most is a caparbious, emotional, at times violent OPPOSITION BY QUARK-BELIEVERS ON THE CONDUCTION OF THESE EXPERIMENTS OF SUCH FUNDAMENTAL CHARACTER FOR HUMAN KNOWLEDGE. To understand this aspect you should first know that quark models are fundamentally incapable of resolving the problem of the validity or invalidity of basic laws within a hadron (owing to the complex topology of assumptions in which they are based, none of which experimentally established). Secondly, you should know that these quark conjectures are vitally dependent on Einstein's special relativity. To be specific on this truly crucial point, and as elaborated in my papers in all necessary details, if Einstein special relativity is invalid in the interior of a hadron (as it is already known to be in the interior of a star .....) the quark conjectures are inconsistent in their very formulation , let alone their treatment and intended physical content.

In conclusion, if the experiments studied under my DOE grants are actually conducted in due time and establish the invalidity of Einstein special relativity within a hadron, THE COMPLETE SCIENTIFIC PRODUCTION ON STRONG INTERACTIONS CONDUCTED AT LYMAN LABORATORY FOR HADRON STRUCTURE IS INVALID.

This is the legacy of Einstein's ideas in the first centennial of his birth. These ideas are truly fundamental in contemporary theoretical physics, but they have been proved only for the electromagnetic interactions (for the special case). I can understand the personal emotions by S. COLEMAN, S. GLASHOW and S. WEINBERG at the even remote possibility that their numerous papers on hadron structure could ALL be invalid. But this is a direct consequence of their assumption of Einstein's ideas in an area in which they have not been established experimentally, without a critical analysis of the profile. A possible invalidity of quark-oriented studies is than nothing

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but a fact of scientific life.

I am now, finally, in a position to express to you in good faith the true risk that Harvard University is currently exposed to, as it appears to me.

In essence, it appears that a number of physicists from the U.S.A. and from abroad have contacted the senior faculty members of the Lyman Laboratory in relation to my papers and my studies on hadrons to the effect of inquiring their view on the feasibility of the indicated experiments. This is quite predictable because all my papers in the subject bear the name of "Lyman Laboratory". It is only with extreme concern that I have to report the indication that these senior collegues have dismissed the studies and experiments under consideration with the routine statement of "no physical value".

I am sure you are are aware of the fact that your senior members at Lyman have many outside admirers, but an equally numerous number of enemies.... Some of the latters are quite friendly on the surface. As a result, the statement of "no physical value" made by your quark committed faculty at Lyman, on fundamental experiments which could invalidate the quark conjecture, apparently has been a too easy an opening for nontrivial attack by outsiders.

In conclusion, until my episode, my studies and Lyman's opposition have remained within our Yard, I saw no reason for concern. I have now felt obliged to inform you because of the release to potentially dangerous outsiders of potentially dangerous statements by the senior members of Lyman Laboratory.

MY RECOMMENDATION. The problem of the experimental verification of the validity or invalidity for the strong interactions of Einstein's special relativity (as well as other quantum mechanical laws) is of such fundamental character for human knowledge to go beyond the personal interests of individual researchers, whether those by S. COLEMAN. S. GLASHOW and S. WEINBERG or mine. I am sure you are by now aware of my extreme determination to pursuit this scientific objective at whatever personal price or humiliation.

I beg you for your help in avoiding, whether possible, unnecessary aggravations in the conduction of this function, as well as unnecessary, risky, exposure of Harvard in a rather delicate moment of our community of basic studies.

In particular , I would like to respectfully submit the following options.

(1) To identify sensible but effective means of recommendation to S. COLEMAN, S. GLASHOW and S. WEINBERG to use their invaluable knowledge and scientific status in favor of these fundamental tests, possibly, in a openly stated form.As of this moment, the totality of papers by these physicists assume in a TACIT FORM the validity for the strong interactions of the basic physical laws experimentally established for the electromagnetic interactions only, without any mention of the need of their experimental study. If, for any reason, this constructive attitude is not realizable,

(2) To identify sensible but effective means of recommendation to S. COLEMAN, S.GLASHOW and S. WEINBERG to use extreme scientific caution when consulted by outsiders on these fundamental experimental tests (or, better, be silent). This is clearly essential to avoid unnecessary risks of accusation by outsiders of scientific corruption. And, finally,

(3) To implement at Harvard University a well balanced condition and conduction

of research in hadron structure, in such a way that not only quark conjectures, but also all valuable conjectures are comparatively studied, in the genuine pursuit of fundamental human knowledge. This is not only essential to avoid accusations of monopolistic restrictions of research to only quark oriented studies, but also to continue the fulfilment of a by know historical, primary contribution by Harvard to human knowledge.

In this latter respect, permit me to frankly express my personal opinion that the implementation of this well balanced condition of research is impossible at this moment at Lyman Laboratory, owing to the personality of the theoretical physicists currently in control of the sector of research, as well as their personal research interests. Neverthless, I do see certainly possible the implementation of these conditions of research at Harvard, via the joint use of Lyman Laboratory, as well as other divisions, or via the study of other alternatives (the formation of a Center for Hadron Physics as a subnuclear complement of the existing Center for Astrophysics ??)

In closing this letter, permit me to clarify a few points. First of all, permit me to reassure you that I have no animosity whatsoever toward S. COLEMAN, S. GLASHOW and S. WEINBERG. As a matter of fact, I have only sincere gratitude for their hospitality, as I have formally stated in my monographs with SPRINGER-VERLAG as well as all my papers (see the enclosures). I simply understand their emotions at the mere thought that Einstein might be violated within a hadron .....

Secondly, permit me to stress that I have dedicated my life to basic studies and, as such, I am committed to their orderly conduction. As a result, you can rest assured that I am committed to continue my intermediary or preventive action to avoid even the risk of unnecessary crises.

Thirdly, you have my formal committment that, until I am a member of Harvard University, I shall provide my utmost loyalty to this campus. I am therefore at your disposal for any assistance you might need.

In relation to my future, you might be interested to know that I do not intend to apply for a renewal of my research appointment at the Department of Mathematics for the conduction of the second year of my DOE grant. This is due to a number of reasons, including my utmost consideration and respect for the ethical and scientific status of Professor S. STERNBERG, as well as to avoid even the risk of a repetition of the indirect humiliations he had to tolerate from the Lyman collegues.

Neverthless, I shall attempt to identify a non-tenured, non-teaching, terminal, one-year position at some other department, completely supported by my grant. In principle, the experimental high energy group at Jefferson is the ideal candidate. Neverthless, this group has operated in the past in a sort of symbiotic condition with quark-oriented studies. Therefore, I do not know whether they are interested in being even indirectly exposed to the study of experiments of truly fundamental physical character. Owing to this situation, I intend to avoid any formal contact with the experimental high energy group and abstein from applying for the position indicated (of course, in case I am invited to join, I would be simply honored).

After due consideration of other alternatives, I would like to submit an application for the position indicated to Professor G.B.FIELD at the Center for Astrophysics. Jointly, I intend to consult with Professor MARTIN at the Division of Applied Sciences to inquire about the advisability of my submitting the application indicated. Hoping not to be of any inconvenience, I shall send you copy of the related correspondence.

page 10.

If none of these possibilities materializes, my formal association with Harvard will terminate, by contract, on June 30, 1979. I only hope that S. COLEMAN, S. GLASHOW and S. WEINBERG, as well as the other senior members of Lyman Laboratory, WILL NOT INTERFERE with these contacts with their, by now, routine statement of "no physical value".

In closing, permit me to confess my deep sadness, after numerous years of sincere dedication to basic studies at a substantial personal and family sacrifice. I first came to realize that, being an independent theoretical physicist, the mere idea of applying for a regular academic job in the U.S.A. is sadly laughable. These jobs simply do not exist, at least not at my level (nontenured associate professor). They have to be created. But then, they are created at the highest possible human price: your brain. Now I have come to realize that, even with full financial support by a U.S. Governmental Agency, it is extremely difficult to resolve the academic entanglements created by preexisting, vested, academic and financial interests of the type reported in this letter. WHAT WILL BE NEXT ? Where is the creativity of this Country leading to, without the intervention of academic administrators with a genuine vision ? Are we already engulfed in a pattern of "scientific suicide" as far as the genuine pursuit of fundamental human knowledge is concerned ? Will the historians of, say, the 29th century be in a position to express the easy judgment that the (now feared) collapse of the U.S. economy was due to the failure by the academic administrators to implement in time an effective conduction of basic studies ?

In relation to the latter question, permit me the liberty of expressing a vigorous form of support for the Governmental administrators. I can personally testify of their genuine committment and mature scientific vision for the needs of this Country. In my humble view, the current shadows exist ONLY at the academic level. In any case, it is for me inconceivable that they have to be faced with academic entanglements of the type reported in this letter during the actuation of their functions.

I am confident that you will treat this letter and its content with utmost confidentiality. I would be particularly grateful whether its existence can be kept confidential as much as possible. On my part, I have even avoided secretarial partecipation by typing it on my own (as I do, after all, for all my papers and manuscripts). You can trust my utmost discretion.

Please do not feel obliged to acknowledge this letter. I am confident in your wisdom to differentiate its good part from the bag, to put the former to good use and benevolently ignore the latter. Whatever your decision will be, you have my gratitude for your consideration and my sincere esteem.

V E R I
T A S

Ruggero Maria Santilli

c.c. Dean RICHARD G. LEAHY, Faculty of Arts and Sciences
University Hall 20

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

January 11, 1979

Professor DEREK C. BOK, President,
Harvard University, Massachusetts Hall 1,

STRICTLY CONFIDENTIAL

Dear Professor Bok,

Dr. DAVID C. PEASLEE of the Department of Energy, Division of High Energy Physics (tel. 301 - 353 3624) has informed me in a phone conversation of January 10, 1979 of the existence of considerable difficulties in his search for a possible accomodation for the second year of my grant No. ER-78-S-02-4742.A000.

These difficulties were predicted in my report to you of December 27, 1979. Almost needless to say, no member of the Department of Energy has been informed of this report nor of this letter.

After due consideration of the situation, I would like to humbly recommend you the study of the formation of an independent division for the conduction of studies such as mine under the suggested title of

"Center for Hadron Physics"

Owing to preexisting, vested interests in other divisions, this appears to be the most effective way to implement at Harvard the well balanced conductic of studies on hadron structure I indicated to you in my report of Dec. 27.

I enclose for your consideration a "rudimentary draft" of this project. As you can see, I recommend that the position of Director for this possible Center be assumed by a professional administrator (and not by a physicist)

I am here formally asking your authorization to proceed for a feasibility study for the organization of this Center. In case you are in a position to grant such an authorization, I would need the indication of the administrator for me to contact, possibly, with the understanding that the same administrator might become the first Director of the Center (initially on a part-time basis).

I have no word to express to you the human and scientific value for the formation, at this time, of such a Center. I also am firmly convinced that such a Center would be a scientific and financial success for Harvard, because I am fully confident of the implementation of the "mature synthesis between genuine scientific vision and sound administrative practice" I recommended in my draft.

Looking forward to hearing from you, I remain

Very Truly Yours

Ruggero Maria Santilli

C.C.: Dean R. C. LEAHY, Univ. Hall 20

Rudimentary draft for the possible organization of the

# C E N T E R  F O R  H A D R O N  P H Y S I C S

by RUGGERO MARIA SANTILLI, Science Center Room 435, ext. 5 3352

FUNCTION. The Center should conduct a well balanced theoretical and experimental study on the problem of the strongly interacting elementary particles (hadrons), with particular reference to the problem of the experimental verification of the fundamental physical laws currently used in this sector of research (e.g., Einstein's special relativity, Pauli's exclusion principle and the spin-statistics theorem). This objective should be pursued by coordinating the efforts by leading mathematicians, theoretical and experimental physicists and engineers.

ORGANIZATION. The Center should be constituted by a Director, a Board of Advisors and the staff members under the position of Research Assistant, Associate and full professors. No member of the Center, including the Director, should be tenured and all members should be on a contractual, research, terminal basis. The Director should be appointed by the President or any administrative body suggested by the President. It is strongly recommended that the position of Director should not be taken by a physicist, and be taken instead by a professional administrator. The Research Professors of the Center should be appointed by the Director, following the recommendation by the Board of Advisors. This latter Board should be composed by senior scientists of proved scientific vision by different Institutions.

FUNDING. Rather than costing money, the Center should bring money to the University. This objective can be realistically attempted via a sound administration, and also in view of the energy related potential of the primary function of the Center which is expected to grow in the near future. All activities should be funded via research grants of governmental or corporate nature. Applications should be filed jointly by the Center and by individual scientists. It is recommended that any scientist, from all over the world, should have the possibility of filing an application via the Center, of course, upon review and approval by the Board of Advisors, and that possibility be advertised in The Physics Today as well as in other scientific news media to fulfill an increasing need of such a function in the scientific community.
Initial funding of the Center may be provided by the grant of R.M.SANTILLI from the Department of Energy, under the assumption that such a relocation is possible.

AFFILIATION. It is suggested that the Center be affiliated to the Department of Mathematics, in such an administrative form to create the minimum possible burden, if any, to such Department. This affiliation appears recommendable on the basis of the primary function of advanced mathematical knowledge for the activities of the Center. All members of the Department of Mathematics (as well as of any other Department with the necessary technical qualification) should be encouraged to spend part of their time at the Center, under the Center's support. Initial housing should consists of a few rooms (1-or-2) made available at the Science Center for close contacts with the Dept. of Math.

GROWTH FORECAST. The first year of operation should be mainly orientational and operated under minimum conditions (part time Director, and one or two full time and/or part time research members). Subsequent growth should be the result of a mature synthesis between genuine scientific vision and sound administrative practice.

January 3, 1979

TO: Professors BOTT, HIRONAKA, KAZHDAN, MACKEY and STERNBERG
FROM: Ruggero M.Santilli

We have closed today Vol. 1, 1978 of the HADRONIC JOURNAL for a total of 1,598 pages,plus the Index. I would like to thank you (as well as all the members of the Department of Mathematics) for the logistic hospitality, whithout which this Journal could not have been possible. Also, the possibility of being in contact with you has been simply invaluable for me, as well as for the Journal, owing to the central objective:

Presentation of rigourous, advanced mathematical tools → Conjectural application to hadron physics → Formulation and promotion of basic experiments

More specifically( as you know) the central objective of the Journal is to appeal to all possible mathematical resources to formulate, in due time, the experimental verification for the strong interactions in general and for the hadron structure in particular of the BASIC physical laws (Einstein's special relativity, Pauli's exclusion principle, the spin-statistics theorem) which are experimentally established until now ONLY for the electromagnetic interactions. The idea is that only after such experimental resolution, the problem of the structure of hadron can be truly confronted in a final form.

You will be amused to know that this attitude has stirred up a genuine form of interest by outstanding physicists, as well as considerable "pain" by others (I understand that the sale of ALKA SELTZER has considebrably increased near quark-centers....).

Outstanding physicists such as A.SALAM (Director of the ICTP in Trieste, K.S.THORNE (Director of the Center for Astrophysics at CALTEC), B.DeWITT (Director of the Center for Relativity at the Univ. of Texas at Austin) as well as the Nobel Laureates YANG and PRIGOGINE and all members of the Editorial Council of the Journal and numerous other physicists have openly supported the study of the experiments in question.

Governmental Agencies (both the DOE and NSF) have also formally expressed their interest to support these studies. To my knowledge, a number of research proposals have already been filed and other are intended for filing in 1979. This support is apparently intended for the entire line, from the pure mathematical studies to the experimental proposals, and I tought you might be interested to know it. As far as myself is concerned, DOE appears to be serious in continuing (actually increasing) the support of my studies.

I enclose for your amusement copies of my recent two papers (written for The Phys. Rev. D, which means, in a non-provocative style). I also enclose the table of contents of my first volume with SPRINGER-VERLAG which is now in regular distribution. Regrettably, I do not have a complymentary copy for all of you (my personal copy is however at your disposal).

Please accept the sentiments of my sincere gratitude and my best wishes for a VERY GOOD 1979.

Ruggero

HARVARD UNIVERSITY
UNIVERSITY HALL 20, CAMBRIDGE, MASSACHUSETTS 02138

FACULTY OF ARTS AND SCIENCES
OFFICE OF THE ASSOCIATE DEAN

January 24, 1979

Dr. Ruggero Maria Santilli
Mathematics Department
Science Center
1 Oxford Street

Dear Dr. Santilli:

I am writing with regard to your recent correspondence to President Bok. He has forwarded this to Dean Rosovsky and the Dean and I have had an opportunity to review the issues you raised.

Unfortunately, the matters you discuss in your long memorandum of December 27, 1978, and in the more recent letter, are questions which, in the first instance, must be resolved at the departmental level. Thus, should you wish to pursue your proposal, you should ascertain whether or not there is any support for it within the members of the faculty of the appropriate academic department. It would then be up to that department to formally endorse and forward that proposal to the Dean for further consideration.

Sincerely yours,

Richard G. Leahy

Richard G. Leahy

RGL:bec

cc: President Derek Bok
Dean Henry Rosovsky

HARVARD UNIVERSITY

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

January 25, 1979

Professor HEISUKE HIRONAKA, Chairman
Department of Mathematics, Harvard University

Dear Heisuke,

After due consideration of the current status of basic research in physics, I have recently taken the liberty of proposing to President BOK, Dean ROSOVSKY and Associate Dean LEAHY the consideration for the possible setting of a new center of research at Harvard called CENTER FOR HADRON PHYSICS. Associate Dean LEAHY has recently informed me that I should ascertain whether ot not there is any support for such a proposal within the members of the faculty of the appropriate academic department. It would then be up to that department to formally endorse and forward the proposal to the Dean for further consideration.

I enclose copy of the rudimentary proposal, as originally submitted. As you can see, I recommend that the Center be affiliated to the Department of Mathematics. I would therefore appreciate the courtesy of your consideration of this proposal.

My recommendation to affiliate this possible Center to your department is based on the need of advanced mathematical knowledge for the treatment of rather fundamental physical problems which is only available at your department. Also, because I believe that an effective dialysis between mathematics and physics is much needed and would constitute a valuable scientific service.

The proposal itself was motivated by my sincere concern of the current status of basic research in physics, with regard to both, its short term and long term impact. I am here referring, in particular, to the experimental verification of the validity or invalidity of basic physical laws for strong interactions, and a number of related aspects, some of which predictably related to energy issues. In short, I believe that these experimental verifications are of such complexity as well as physical relevance, to warrant the organization of a Center specifically conceived for that function. In any case, these problems have been rather vigorously posed to the scientific community in 1978 and I believe that they will not be ignored by other campuses. The long term economic forecasts for this Country are, in my view, so cloudly that we simply cannot afford the luxury of overlooking fundamental issues in basic research.

In case you consider this proposal of any value, I would be happy to discuss it with you in more detail. As you can see, I had in mind a quite modest beginning, possibly funded via my DOE grant with Shlomo, if at all possible, and then a controlled growth as scientifically and financially appropriate.

I am sending a copy of this letter to Shlomo, but I have abstained from informing other collegues. I am also taking the liberty of informing Dr. DAVID C. PEASLEE of the Department of Energy (tel. 301 353 3624) of this initiative. I am sure that, in case you desire to consult him, he will be most cooperative.

Sincerely

HARVARD UNIVERSITY

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

February 14, 1979

Professor H. HIRONAKA, Chairman,

Department of Mathematics, Harvard University

Dear Professor Hironaka,

My function as editor of the HADRONIC JOURNAL calls that I clarify as soon as possible my address upon the expiration of my current appointment as research associate at your department on June 1979.

Pending a study of my case, I would be truly grateful whether you could kindly write me a letter indicating that I can be your personal guest with the understanding that

- such guest status will carry no salary and can be terminated at any time of your election;
- I shall pay on my own all logistic expenses (telephone, xerox, etc.); and
- I shall continue the use of my current stationary (as per this letter) as well as my formal address at the"Science Center" to avoid any unnecessary connection of your department with the editorial activities of the Journal.

Such a guest status would be fully sufficient to clarify my whereabouts after June 1979 with the administration of the HADRONIC JOURNAL, as well as with the distinguished scientists of its EDITORIAL COUNCIL.

Subsequently, I would appreciate your courtesy of exploring with Dr. D. C. PEASLEE of the Department of Energy, during your meeting at the end of this month, the possibility that my research associate position at your department be renewed for 1979/80 with my salary and expenses fully supported by the second year of my grant.

I have no words to express my appreciation and gratitude.

Sincerely Yours

Ruggero Maria Santilli

RMS/se

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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CAMBRIDGE, MASSACHUSETTS 02138

February 15, 1979

Professor R. GIACCONI,
Dept. of Astronomy

Caro Giacconi,

vorrei prendermi la liberta' di informarti delle attivita' dell' HADRONIC JOURNAL, che ho organizzato qui ad Harvard con HOWARD GEORGI (del dipartimento di fisica). L'indice del Volume 1 e' accluso come una prima informazione, mentre mi tengo a tua disposizione per copie dei numeri e per ogni altra informazione puoi desiderare.

Il "piccolo vento nuovo" che abbiamo tentato e' la riconsiderazione della vecchia idea che le interazioni forti non sono della struttura triviale $f = \partial V/\partial r$, ma bensi' sono strutturalmente piu' complesse, e plausibilmente di typo locale non-derivable da potenziale, come una approximazione di una realta' fisica di typo non-locale (questa pare fosse l'opinione anche di Fermi).

Per questo obiettivo, abbiamo promosso lo studio, come primo passo, di tutti i metodi per lo studio delle forze considerate (variationally non-self-adjoint) ed a livello sia classico che quantistico che della teoria dei campi quantizzati.

Allo stato attuale (leggi rudimentale) questi metodi si possono classificare nei seguenti due gruppi.

"Inverse Problem", che consiste nella riduzione delle equazioni ad una forma trattabile mediante algebre di Lie, equazioni di Hamilton ed Heisenberg ecc.;

"Lie-admissible problem" che consiste nel tentative di generalizare la struttura analitica delle formulazioni teoriche attuali, senza trasformare le equazioni di studio, e mediante una generalizzazione delle algebre di Lie chiamata algebre Lie-ammissibili. Infatti, le transformazioni di equivalenza per ridurre un sistema non-derivabile da potenziale ad uno che lo sia, spesso implicano l'impossibilita' di usare le coordinate usate nell'esperimento.... o l'uso delle algebre di Lie implica un sistema altamente non-inerziale, ecc. Questi problemi vengono a mancare nell'uso delle tecniche Lie-ammissibili che sono formulate per le coordinate di uso pratico, ma il prezzo da pagare e' abbastanza alto: le algebre Lie-ammissibili sono incompatibili con molta della conoscenza teorica attuale.

Un quadro abbastanza inquietante per alcuni e stimolante per altri e' quindi emerso da questa azione del 1978: l'ipotesi che le interazioni forti non sono derivabili da potenziale sembra essere incompatibile con le idee di Einstein, sia per la relativita' ristretta che per quella generale per il solo problema interno.

Come tu ben sai, al livello della fisica delle particelle elementari, oltre mezzo secolo di studi delle interazioni forti e' fondamentalmente basato sulla relativita' ristretta. Ma tu sei ben al corrente dell'esito di questi ingenti sforzi e finanziamenti: la congettura che i quarks sono i costituenti degli hadroni e la QCD (fondamentalmente dipendente dall'ipotesi che $f_{strong} = \partial V/\partial r$).

Il punto rimane che la validita' della relativita' ristretta per le interazioni forti e' solo un vango sentore a questo momento, senza nessuna verifica sperimentale sia pure parziale (a meno che la congettura dei quarchi, complementata dalla congettura che confinano, complementata dalla congettura della liberta' asimptotica, ecc. ecc., costituisce prova della validita' della relativita' ristretta per la struttura degli adroni....).

In aggiunta, questi studi sui quarchi sono cosi' dipendenti dalla relativita' di Einstein speciale che, se questa e' invalida per la structura adronica, l'ipotesi dei quarchi e' inconsistente nella sua formulazione, lasciamo stare il trattamento (tecnicamente. le forze forti non-autoaggiunte implicano una rottura del concetto di spinore e molte delle leggi di base; l'aspetto della carica frazionaria e irrilevante).

Lascio a te immaginare le implicazioni di una situazione del genere, inclusi gli investimenti attuali nei quarchi da parte delle agenzie governative che sono entrati d'un tratto in una condizione di limbo.

In ogni case, il dado e' stato tratto. Ti accludo copia di un volantino per due volumi di reprints su queste cose che e' stato stampato e spedito in circa 100.000 campioni (sic), in tutti i dipartimenti di matematica, fisica ed ingegneria. Ti accludo anche copia di altro materiale di tuo possibile interesse.

Se sei interessato ad essere messo al corrente di piu' dettagli, ti prego di farmelo sapere e faro' del mio meglio per farti avere copie dei volumi.

Ti fara' piacere sapere che tutto questo programmd e' stato lanciato con l'assistenza del Dipartimento dell'Energia (DOE) sotto il mio grant No. ER-78-S-02-4742.A.000. Attualmente sto decidendo il Dipartimento di Harvard a cui fare domanda per il rinnovo del mio secondo anno del contratto (i fondi sono gia' stati allocati, secondo mia conoscenza).

Ti sarei grato se potessi considerare la cosa. Nonostante una opposizione prevedibile (e strenua) da parte dei "quark-believers", la agenzie governative e molto dell'ambiente scientifico "limpido" sono pienamente a favore della verifica sperimentale della relativita' ristretta per le interazioni forti , per cui la cosa appare inarrestabile ora.

In linea di principio, un programma del genere dovrebbe essere di interesse diretto per il Center of Astrophysics. Comunque, mi sono astenuto dal fare una domanda, e questo e' il mio primissimo contatto, perche' non conosco affatto l'ambiente. Nota che non mi riferisco ad una richiesta di salario. No. Anzi, e' il contrario, nel senso che il mio salario piu' le overheads sono interamente pagate dal mio grant, come puoi verificare contattando direttamente l'ufficiale del DOE in carica del mio grant, Dr. D. C. PEASLEE, 301 353 3624.

Per concludere questa lunga lettera, ti prego innanzitutto di scusare ogni disturbo. Se sei interessato come collega a quello che sta succedendo nell'ambiente scientifico in forma confidenziale, ti prego di farmelo sapere. Puoi contare nel mio pienc rispetto della confidenzialita'. Infine, se credi che il programma in corso e' di interesse per il Center of Astrophysics ed un mio appointment a questo Center sia pieno che"joint" sia di vantaggio per questo centro, ti prego di farmelo sapere. Da parte mia sarei felicissimo di una associazione del genere.

Sperando di avere il piacere di conoscerti, rimango

[illegible] Ho

**Center for Astrophysics**
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

February 23, 1979

Professor Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

UNIVERSITY MAIL

Dear Professor Santilli:

I was most interested to read your letter and consider your thoughtful remarks about the desirability of applying the most advanced mathematical techniques to the study of physics. Unfortunately, much of the discussion is beyond my specific expertise and therefore I will have to remain a sympathetic bystander.

Sincerely yours,

Riccardo Giacconi

/s

HARVARD UNIVERSITY

Area Code 617
495-3352

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

February 28, 1979

Professor RICCARDO GIACCONI
Center for Astrophysics
60 Garden Street

UNIVERSITY MAIL

Dear Professor Giacconi,

I would like to express my appreciation for the courtesy of your letter of February 23, 1979. I hope you did appreciate my effort to be as candid as possible.

You might be interested to know that the initial reaction to our reprint series on Lie-admissible formulations has been rather encouraging. As a Government Officer recently put it to me, the problem of the experimental verification of the validity or invalidity of Einstein's special relativity for the strong interactions "simply cannot be ignored".

It will be a pleasure to keep you occasionally informed of most salient research developments at the HADRONIC JOURNAL, of course, on a fully informal basis.

Also, it would be a pleasure for me to meet you sometime.

Sincerely

Ruggero Maria Santilli

RMS/se

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

February 28, 1979

Professor RICHARD G. LEAHY, Associate Dean
Faculty of Arts and Sciences

UNIVERSITY MAIL

Dear Professor Leahy,

Following your letter of January 24, 1979, I have submitted the proposal for a feasibility study on the setting of a new center of basic research to Professor HIRONAKA, Chairman of the Department of Mathematics.

Subsequently, I arranged for a meeting between Professor HIRONAKA and Dr. DAVID C. PEASLEE of the High Energy Physics Division of the U.S. DEPARTMENT OF ENERGY on February 27. I am confident that Professor HIRONAKA will report to you the outcome of this meeting. On my part, I would like to reassure you that all the necessary precaution was taken to stress the informal, noncommittal nature of this feasibility study, as well as the need for confidentiality.

I also had two meetings with Dr. PEASLEE on this subject. Their outcome is essentially the following. Understandably, DOE intends to avoid any participation whatsoever in the decisional process at Harvard, whether to proceed with this feasibility study or not and eventually set the center or not. Nevertheless, in case Harvard decides favorably for the project, DOE appears genuinely interested to provide all possible assistance. It is understood that possible initial operations should be minimal, so that DOE has the necessary time to budget the potential research support without affecting current engagements. It is also understood that all possible efforts would be provided to avoid conflicts with other divisions at Harvard. For instance, the names suggested are "Center for basic research" or "Center for applied mathematics", rather than "center for Hadron physics".

My personal impression is that DOE is quite receptive to the projected primary function of the center. I am here referring to a coordinated, effective dialysis between advanced, pure, mathematics and physics, with priorities on energy related issues. DOE also appears to be quite receptive to the needed, advanced mathematical knowledge for the theoretical and experimental study of the validity or invalidity of basic physical laws for the strong interactions (Einstein's special relativity, Pauli's exclusion principle, etc.). After all, these laws are currently assumed as valid in all funded research on the controlled fusion. You can easily figure out the administrative and scientific implications in case these laws, in the ultimate analysis, need implementations for the strong interactions, as by now believed by numerous qualified physicists. To put my personal impression in a vivid language, DOE (as well as other governmental agencies) simply cannot ignore these issues.

page 2.

During his visit at Harvard, Dr. PEASLEE has also explored the possibility for the continuation of my research under full DOE support for a second year. I understand that Professor HIRONAKA will explore the possibility of a renewal for a second year of my nonteaching, nontenured, terminal appointment. As a result, I have absteined from submitting an application to other divisions at Harvard. I am confident that Professor HIRONAKA will report to you on this matter too in due time.

In closing, you might be interested to know the recent scientific activities at the HADRONIC JOURNAL. Volume 1, 1978 has been completed on schedule for a total of 1,603 pages. An informative leaflet is enclosed. Volume 2, number 1 (February) 1979 has also been completed (today) on schedule. The Table of Contents is enclosed. As you can see, the Journal continues the nonmonopolistic presentation of qualified research on hadron by quark believers as well as quark nonbelievers.

The call for a moment of reflection on the basic physical laws for the strong interactions launched via Volume 1, 1978 of the Journal has seen a truly encouraging response. All papers on these topics are being reprinted in two first volumes of a yearly series entitled "Applications of Lie-Admissible Algebras in Physics", Edited by Professors H.C. MYUNG (the current leading mathematician on Lie-admissible algebras), S. OKUBO (an outstanding authority on hadron physics) and myself.

A blue leaflet on these reprint volumes is enclosed. You might be amused to know that this leaflet has been printed and distributed in 60,000 copies (sic) to the world wide mathematics and physics communities.

Very Truly Yours

Ruggero Maria Santilli

RMS/se
c.c.: President DEREK BOK
Dean HENRY ROSOVSKY
encls.

TO: THE SENIOR MEMBERS OF THE DEPT OF MATH April 29, 1979
FROM: R.M.SANTILLI

As you are eventually aware, the U.S.DEPARTMENT OF ENERGY has formally approved the renewal of the grant for the support of my salary during 1979-80 with my affiliation to your department, under the new grant number AS02-78ER04742. I understand that there are difficulties for the renewal of my research associate position at your department for an additional terminal year.

I believe that these difficulties are due primarily to my lack of keeping you sufficiently informed of my research activities, as well as of their implications for the promotion of the development of mathematics. I have therefore interrupted my research, to prepare for you a review paper on the rather intriguing status of strong interactions. A copy of this paper is enclosed with the understanding that it is the result of a few days of work and, as such, of preliminary character.

I hope that, among your many duties and activities, you will find the time to look at this paper. You may then see why so many scientists all over the world (including Nobel laureates) consider as of final character the Mendeleev-type classification of hadrons via unitary groups. Yet they question the conjecture that quarks are the constituents of hadrons. More importantly, I hope that this paper will indicate to you that the current search for a more adequate treatment of the strong interactions has all the ingredients to constitute a genuine thrust for the development of numerous branches of mathematics.

I also hope that you will see with benevolence my intemperances with your Lyman collegues. The disagreements in our community of basic research are not of minute technical character. Instead, they are related to fundamental issues. As such, they stir up great emotions on all sides. The roots of my disagreement with your Lyman collegues are quite simple. On my part, I respect studies on quarks and, as an editor, I routinely accept them for publication. However, the doubts on quarks are too numerous and too substantial to justify the restriction of research on the fundamental problem of hadron structure to quark conjectures only. As a researcher and editor, I therefore favor a more balanced and mature conduction of studies on the sector in which quark lines are considered jointly with all other conceivable lines of clear scientific promise. As such, I vigorously oppose any more restrictive view, whether of quark or nonquark inspiration.

In any case, I hope you understand and respect my scientific convictions and determination. I decided long time ago to pursue knowledge in physics, rather than a career in physics, with full cognizance that this is often unrewarding. In essence, I simply cannot compromise with my scientific ethics, at whatever personal cost.

You should be informed that the DOE is interested in having me spending one additional year at your department. The reason is due to the fact that my research calls for applications of advanced mathematical tools. In turn, these tools are expected to play a crucial function for the formulation and execution, in due time, of experiments of truly crucial physical character: the experimental resolution of the validity or invalidity for the strong interactions of currently used laws and principles (Einstein's special relativity

page 2.

and Pauli's exclusion principle, in particular), which are experimentally established until now only for the electromagnetic interactions.

Permit me to be candid on this issue. Tenured physicists can afford the luxury of expressing personal beliefs. Governmental officers cannot. They are responsible to the taxpayer. I am confident you will see that the U.S. Department of Energy cannot continue to invest indefinitely truly large amounts of money on hadron physics, all based on the mere belief of the validity of the basic laws, without jointly promoting their experimental verification.

I would like to stress here that the sole objective of my grant with DOE is the study and promotion of these basic experimental verifications. On my part, I would be honored to collaborate with your Lyman collegues on these crucial physical issues, and it is regrettable that this collaboration did not materialize. It is also regrettable that there exist a number of physicists opposing these crucial tests. But they are a minority by now and, in any case, they cannot arrest a scientific program of the United States Government already in motion.

Please take all the necessary time to reach your final decision. I am at the disposal of all of you for any additional information you might desire. Nevertheless, please take into consideration that I have not applied for a position elsewhere and, more importantly, that I cannot apply without the prior consultation with the Department of Energy.

I would like to suggest to you most warmly that you communicate directly to the Department of Energy your final decision on my appointment. This is recommendable to avoid an unnecessary repetition of the Lyman episode of April-June 1978 (The DOE had funded my proposal with my affiliation to the Lyman Laboratory, but Lyman failed to report the negative decision on my appointment to the DOE and created unnecessary aggravations).

I am confident that your final decision will be a reflection of your scientific ethics, as well as of the traditional scientific freedom at Harvard. If your final decision will be favorable, I will be honored to spend another year with you. If your final decision will be negative for whatever reason, I would like to confirm what I indicated verbally to Eisuke, that you can count on my best collaboration for an orderly transfer of the grant.

I am and I will be always grateful to Shlomo, because what I have learned from his lectures I treasure most, to Heisuke, because of his genuine committment to science and mature conduction of my case, and to all of you, for the hospitality during the current academic year.

Sincerely

Ruggero

c.c.: President DEREK BOK, Dean HENRY ROSOVSKY and Associate Dean RICHARD LEAHY.

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.

## ABSTRACT

The objective of this paper is to present an outline of the rather numerous criticisms on the quark models of hadron structure which have been lingering in our community of basic research for some time.The main line of the paper consists of the presentation of the various arguments according to which the unitary models provide a Mendeleev-type classification of hadrons of unequivocal physical value and of virtually conclusive character. Nevertheless, the quark models are not expected to provide a joint model of structure of each individual element of a unitary multiplet, nor quarks are expected to exist as physical particles. The hope of this paper is that quark supporters will inspect these criticisms, and present possible counter-arguments, for a scientifically effective resolution of these issues in due time. In particular, this paper is an appeal to experimenters in high energy physics to provide means for an effective selection among an ever increasing number of hadronic models. It is submitted that the problem whether quarks exist or not as physical particles necessarily calls for the prior experimental verification of the validity or invalidity of the basic physical laws used in quark models, with particular reference to Einstein's special relativity and Pauli's exclusion principle, which are experimentally established until now only for the electromagnetic interactions. A number of proposals of specific tests exist in the literature, although in a predictable preliminary form, and numerous other tests are conceivable, all apparently feasible with the current technology. In any case, it is unlike that research in the sector can be effectively continued without these basic experimental resolutions. But, most of all, this paper is an appeal to young minds of all ages. The possible invalidity of conventional physical laws for the strong interactions, rather than constituting a scientific drawback, represents instead an invaluable thrust toward the search for covering laws specifically conceived for the strong interactions, which has already been initiated by a number of researchers. In turn, this situation has all the ingredients for a scientific renaissance, that is, the pursuit of genuine non-incremental advancements in mathematics and physics.


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o o o

May 3, 1979

Professor H. HIRONAKA
Dept. of Math.

Dear Heisuke,

I am taking the liberty of enclosing copies of personal, confidential letters (from my file) on my work, in case they may be of some assistance for the proceedings of my possible reappointment.

Please notice that these letters are unsolicited and, as such, are a spontaneous expressions by their authors on my work. You will also notice that some of them are eccessively genererous on my grossly incomplete work. Please consider them nothing more than an expression of the genuine enthusiasm around the "scientific rainaissance" which has been touched in the paper you received yesterday.

Finally, I have selected the letters enclosed from my personal file, to give you an indication of the reaction to my work by mathematicians, physicists and engineers. In case I can be of any further assistance, please do not hesitate to let me know.

I simply have no words to express my gratitude for your consideration.

Sincerely

Phillips

President Bok,
Best and sincere regards,
Ruggero Santilli

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

President DEREK BOK,
Harvard University

May 6, 1979

Dear Mr. President,

A number of regrettable circumstances has forced me to write the enclosed paper of review on the rather numerous criticisms on quark models in hadron physics moved by distinguished scientists all over the world, including Nobel laureates. The wide distribution of this paper (15,000 copies via Hadronic Press) is intended to indicated to quark-committed physicists that a critical process of examination of quark conjectures is in motion on a world wide scale. Therefore, their corridor-type of opposition to these studies has little scientific value. If they have technical arguments to disprove these criticisms, they should present them in scientific papers.

This paper is the result of indications in my possession that the situation in this crucial sector of research has deteriorated considerably, both at Harvard and outside, since the time of my confidential report to you of December 27, 1978.

Here at Harvard, the Department of Energy has formally approved the renewal of my grant with my affiliation to the Department of Mathematics, as filed, for the study of the experimental verification of the validity or invalidity for the strong interactions of the currently used, basic, physical laws (with particular reference to Einstein's special relativity and Pauli's exclusion principle), which are experimentally established until now only for the electromagnetic interactions. But considerable difficulties have lately emerged for the renewal of my research associate position at the Department of Mathematics for one terminal year under this grant. I heard rumors that these difficulties are due to lobbying by senior, quark-committed physicists at Lyman. But I am not interested to know whether this is indeed the case or not. Copy of my recent letter to the senior members of the Department of Mathematics, as well as copy of confidential letters of comments on my work by collegues, are enclosed for your file.

In any case, it appears that there is a tense moment on campus which calls for defusing. In my perhaps erroneous, personal, view, it is essentially due to the lack of graceful acceptance by quark-committed physicists of critical studies on quark conjectures. I would like to stress here again that I have no animosity whatsoever against these physicists. Yet, I consider essential the conduction of critical studies on current trends on the fundamental problem of hadron structure and their assumed laws. Without this scientific process we are pursuing power, trather than truth.

page 2.

Outside Harvard, there has been an apparent increase, to my knowledge, of the number of physicists who believe that the truncation of the alleged monopoly of funds and human resources in the sector by quark conjectures is a matter of primary national interest. Again, these are responsible scientists. They favor the continuation of funding of quark conjectures. Nevertheless, they vigorously oppose the restriction of research in the sector to quark conjectures only. What has deteriorated the situation is again the lack of graceful acceptance of these sound requests by quark committed physicists, currently in control of the sector. There is noting personal against them. It is a mere fact of scientific life. After a decade of undisputed control of the scientific scene and investments of the order of billions of dollars, the quark models on hadron structure are more controversial than ever. Any scientist with a genuine committment to the pursuit of knowledge can see that it is time now to search, promote and fund studies alone new ideas (promising ones exists in the literature), jointly with the continuation of studies along old ideas.

The point of potential rupture is that indicated in my report to you of December 27, 1979, the apparent opposition by some (but not all) quark-committed physicists against the conduction of experiments on the validity or invalidity of the basic physical laws used in quark conjectures. Indeed, such an alleged attitude can only invite a crisis.

Permit me to say that, whatever the future will bring, I am in peace with my soul. With my report to you of December 27, 1978 I have accomplished my moral duty: to provide Harvard administrators with all necessary elements of judgement, as seen from one side of this scientific dispute. The idea of the delineation of a scientific crisis without any cognizance at all by Harvard administrators was contrary to my ethical code. On my part, you can rest assured that I have kept utmost confidentiality on this report, to the point of avoiding the indication of its existence to my wife, let alone collegues and outsiders.

Permit me the liberty of indicating again that, in my view, the only way to defuse this deteriorating situation is via the intervention of administrators. I simply do not believe that physicists can effectively administer themselves under the current circumstances.

If I can be of any assistance, please do not hesitate to contact me.

Sincerely

Ruggero Maria Santilli

c.c.: Dean H. ROVOVSKY and Associate
Dean R. LEAHY.

Encls.

May 21, 1979

Dear Heisuke,

Shlomo has difficulties for my keeping the current office (room 435). Please consider the possibility that I occupy room 515 (Shlomo secretarial office), as soon as he makes it available for his leave. Perhaps, this office is considerably less exposed than my current one and could solve this (rather unusual) problem.

In case difficulties persist, please advice me whether it is appropriate that I apply for a joint position at the Center for Astrophysics in the intent of moving there my Editorial Office of the HJ, in case accepted.

I am sorry to be of any inconvenience to you. Unfortunately, I live in a two bedroom flat with a family of four. It is absolutely impossible for me to work at home.

In relation to the DOE grant budget I would like to confirm to you what verbally indicated to Shlomo, that I shall accept and endorse whatever he decides.

Again, please accept the sentiments of my sincere gratitude,

Sincerely

Pupperis

# HARVARD UNIVERSITY

OFFICE OF THE PRESIDENT

MASSACHUSETTS HALL
CAMBRIDGE, MASSACHUSETTS 02138

May 23, 1979

Dear Mr. Santilli:

This is just a note to thank you for sending me a copy of your preliminary draft on quark models in hadron physics.

With best wishes,

Sincerely,

Derek C. Bok

Mr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

# PART IC:

## ACADEMIC YEAR 1979–1980

HARVARD UNIVERSITY

OFFICE OF THE SECRETARY
17 QUINCY STREET

CAMBRIDGE, MASSACHUSETTS

September 7, 1979

SIR,

I beg to inform you on behalf of the University and the Dean of the Faculty of Arts and Sciences that you are appointed

Research Associate in Mathematics

to serve for one year from June 1, 1979 subject to the Third Statute of the University (*overleaf*).

Your obedient servant,

*Secretary to the University*

Ruggero Maria Santilli

HARVARD UNIVERSITY
FACULTY OF ARTS AND SCIENCES

OFFICE OF THE DEAN

5 UNIVERSITY HALL
CAMBRIDGE, MASSACHUSETTS 02138
(617) 495-1566

June 6, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street

Dear Dr. Santilli:

Thank you for your letter of June 4th. I will be leaving for England shortly after Commencement and consequently I really don't have any free time before I go. However, I appreciate your courteous note and trust you will understand why I can't now schedule an appointment for you.

Yours sincerely,

Henry Rosovsky

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

June 26, 1979

Professor FRED L. WHIPPLE
Harvard University
Department of Astronomy

University Mail

Dear Professor Whipple,

Following a suggestion by THORNTON PAGE (see enclosed letter), I am contacting you for assistance in the technical and editorial finalization of a recent paper of mine entitled "An intriguing legacy by Albert Einstein, etc". I am enclosing the copy of the paper with the corrections and comments by Dr. Page up to p. 14. As you can see, my knowledge of the English language is truly limited, and an editorial control is much needed. A technical control or criticism would be much appreciated too. I am referring in particular to Section 3, on the problematic aspects of the special relativity for the strong interactions as well as of the general relativity for the interior problem(only). Your candid criticisms of these speculations would meet with my sincere gratitude.

Almost needless to say, you can count on my confidentiality for any possible assistance. In particular, I shall include your name in a possible acknowledgment section only upon your inspection and autorization. But, if you do not have the time to look at this paper, simply return it to me. I will be equally obliged for your consideration.

I have not yet decided the Journal for submission, pending the preparation of a more mature version. In any case, I would like to abstein from publishing this paper in the HADRONIC JOURNAL, owing to my position of editor.

Permit me to confess that this has not been an easy paper for me to write. The primary objectives were: (1) to stimulate a moment of reflection on quarks; (2) to promote the experimental verification of the basic physical laws currently used in strong interactions; and (3) to stimulate the conduction of nonincremental research, jointly with more conventional studies.

A difficult part was to minimize the expected negative reaction by quark committed colleagues. For this purpose I have presented only criticisms on the topic at large, and I have avoided the presentation of additional criticisms which would have called for specific reference to specific papers. Nevertheless, quite candidly, I do not know whether I did indeed achieve my objectives in a scientifically balanced way.

page 2.

Also, I do not know whether I properly conveyed my respect and favor for the continuation of studies along quark lines. As a matter of fact, I routinely accept (and often invite) for publication in the HADRONIC JOURNAL papers along these lines. My paper was simple an expression of my personal uneasiness and concern on the virtual complete restriction of studies on strong interactions along quark lines which has occurred in recent times, with considerable stagnation (also in my view) of genuine advancements in mathematical and physical knowledge in the sector. In essence, I favor a more balanced conduction of research in the sector in which all valuable lines, of both quark and non-quark inspiration, are pursued.

Finally, your council on Section 5 on Einstein's criticism for quantum mechanics would also be appreciated. I have received mixed comments on it. A first group favors the emphasis on this "legacy" in a specific section and in the title, because it has been only academically discussed until now, that is, without the actual conduction of research. A second group (mainly composed by quark committed friends) is against this emphasis, and suggests only a mention, because the strong interactions were unknown when Einstein's conceived his special relativity (see the remark of p.25, last line). I personally favor some form of emphasis on this "legacy" because I believe it is time to consider it seriously. But, again, I do not know whether I did achieve a well balanced presentation.

You might be intrigued to know that a rather feverish research activity is going on on the issues touched in this paper. As you can see in the enclosed copies of the content of the HADRONIC JOURNAL, virtually every issue presents papers either directly or indirectly related to the problem. Additional papers are appearing in other Journals and in conference proceedings. The contributions in 1978 have been reprinted in two volumes (see the enclosed flier) and a complimentary copy is at your disposal upon request. A copy of my monographs on the classical profile (with Springer-Verlag) and on the quantum mechanical profile (with Hadronic Press) is also at your disposal upon request.

Most importantly, a number of experimentalists in high energy physics, following the distribution of the preliminary draft of my paper, have expressed the awareness of the need to initiate a serious experimental study on the validity (according to some) or the invalidity (according to others) of conventional atomic laws for the hadronic structure.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encls.
P.S. Please feel free to call me, if you so desire, I would be happy to visit you at any time of your convenience.

June 26, 1979

*From the desk of:*
FRED L. WHIPPLE
Director, Smithsonian Astrophysical Observatory } Emeritus.
Professor of Astronomy, Harvard University

Dear Dr. Santilli:

I do not consider myself able to be of help on the theoretical aspects of this paper.

Since I am starting immediately on vacation, I could do little on other aspects of the paper until September anyhow.

So I return the paper to you

Regrettfully,

Fred L. Whipple

[redacted]

[redacted]

14 June 1979

Reply to Attn of Code SN

Dr. R.M. Santilli
Science Center
Harvard Univ.
Cambridge, Mass. 02138

Dear Dr. Santilli:

Your goals for this paper are good but your use of the English language needs improvement. I have marked suggested changes in red.

I would question your doubt of Special Relativity for strong interactions on p. 12.

After p. 13, I gave up editing your English. You should get help from some of your colleagues at the Science Center. Fred Whipple at the Center for Astrophysics (60 Garden St.) would surely be willing to help. Tell him that I know his English is good! Dave Layzer would also help.

I am not competent to judge your arguments against quarks in hadrons, but I hope your paper will straighten out the pro-quark and anti-quark groups.

Sincerely,

[redacted]

HARVARD UNIVERSITY

Area Code 617
495-3352

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

July 26, 1979

Professor G.B.FIELD, Director,
Center for Astrophysics
Harvard University

University Mail

Dear Professor Field,

I am here applying for a position at your Center commensurate to my qualifications. I enclose for your consideration my resume, as well as informative material on my recent research, teaching and editorial activities. I am also enlcosing copies of my monograph "Foundations of Theoretical Mechanics", Volume I, with Springer-Verlag, as well as copies of the reprint volumes "Applications of Lie-admissible algebras in physics", I and II with Hadronic Press. The courtesy of returning these volumes to me at your convenience would be appreciated. Finally, I enclose my list of references. I would appreciate the courtesy of indicating whether you prefer to contact directly these colleagues, or I should solicit letters of recommendations.

I am currently the corecipient of a research grant with the Department of Energy with Shlomo Sternberg of the Dept. of Mathematics as principal investigator. Apparently, DOE is interested in the continuation of this support. Please feel free to contact Dr. D. E. Peaslee of the DOE, tel. 301 353 3624, if you so desire. I would like to have the opportunity, in case I join your Center, of applying for the continuation of this grant, either as principal investigator, or as coinvestigator, jointly with interested colleagues. Since the current grant expires on June 1, 1980, the new application should be filed by September 1979. Your consideration of this aspect during the consideration of my application would be appreciated.

I am currently the editor in chief of the Hadronic Journal and I would appreciate the possibility of continuing this editorial function. The journal has completed Volume 1, 1978 for some 1,602 pages, and volume 2, 1979 is now at an advanced stage. As you can see in the enclosed informative material, our journal is attempting a dialysis between advanced mathematics and open physical problems in strong interactions. The Hadronic Journal, in particular, has initiated the treatment by mathematicians and physicists of a generalization of Lie algebras known under the name of Lie-admissible algebras, because of their applicability for the classical and quantum mechanical treatment of forces nonderivable from a potential. You are eventually aware that the strong interactions have been long suspected as being of this type by the founders of contemporary physics (Enrico Fermi and others).

page 2.

I am also the co-organizer, jointly with a mathematician, of a yearly meeting called "Workshop on Lie-admissible formulations". The meeting of this year will be held here at the Science Center from August 1 until 4. Participation is restricted to a few mathematicians and to a few physicists working in the field. This workshop is organized in the intent of conducting research without prejudices, as much as humanly possible. Also, the workshop is devoted to the treatment of fundamental, open, mathematical and physical problems (studies of minute incremental character are gently diverted to other meetings). In case I join your Center, I would appreciate the opportunity of continuing this meeting. No expense on behalf of your Center or of Harvard University is expected (the direct costs for the 1980 meeting will likely be paied by the DOE).

I am currently a nontenured member of the Department of Mathematics. Nevertheless, as you can see, I do not disclose this association in my letter head owing to my editorial function. In case I join your Center, you can rest assured of my best cooperation in regards to this issue.

The primary objective of my research is to study the problem of the experimental verification of the validity (according to some) or invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The search for maturity of formulation of this problem is conducted via (1) my series of monographs with Springer-Verlag and the Hadronic Press (see my curriculum); (2) research papers I am currently writing with mathematicians and physicists; (3) the coordination of the efforts by independent researchers via the Hadronic Journal; (4) contacts with qualified colleagues (experimentalists in particular); and (5) the yearly workshop on Lie-admissible formulations. All valuable contributions in the study of the problem considered are reprinted in yearly volumes, the first two of which have been included. Two additional volumes are under preparation.

This project has now passed the predictably nebulous,orientational phase and is now entered in the second phase of technical treatments of specific issues, thanks also to an increasing participation by mathematicians. Almost needless to say, the complexity of the topics to be confronted is such that a considerable way remains to be covered to achieve maturity of formulation of actual experiments. Nevertheless, a number of experiments have already been proposed, and a rather feverish research activity is now going on, as a result of the efforts initiated at the Hadronic Journal.

I believe that these studies have a rather direct astrophysical implication. My application to join your Center has been submitted in this spirit.

RMS/ml
encls.
c.c.: President Bok, Dean Rosovsky and Associate Dean Leahy (to fulfill an old promise).

Yours, Very Truly
Ruggero Maria Santilli
Ruggero Maria Santilli

**Center for Astrophysics**
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

1 August 1979

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

Dear Dr. Santilli,

Thank you for your letter of July 26. Clearly you are very active in your chosen field. As regards your possible association with the Center for Astrophysics, I must respond that such an association would not be appropriate. It is our policy to encourage research at the Center which is directly related to astrophysics and to resist the temptation to go into fields which are not directly related.

Sincerely,

George Field

P.S. I return your published materials herewith.

GF/dr

cc: D. Bok
H. Rosovsky
R. Leahy
H. Hironaka

HARVARD UNIVERSITY

Area Code 617
495-3352

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

August 10, 1979

Dr. GEORGE FIELD,
Center for Astrophysics

Dear Dr. Field,

Following your letter of August 1, 1979, I would like to reassure you that I understand and respect your position and that, on my part, I do not intend to pursue any more my association with your Center. Unfortunately, since I was expecting a normal consideration jointly with other candidates, I did solicite letters of recommendation on my behalf, and you might receive them in the near future. I would appreciate the courtesy whether you can simply keep these letters in your file without returning them to their authors.

You might also be interested to know that, unless suggested by senior members at Harvard, I do not intend to pursue my association with other divisions of this campus. I am grateful to the Department of Mathematics for the possibility of receiving my DOE grant for two years. Nevertheless, I am not a mathematician , nor I intend to become a mathematician. As a result, I do not intend to abuse of the hospitality of this department by asking for a further extension.

I sincerely have no intention of disagreeing with you. Nevertheless, permit me the liberty of indicating that the studies I am conducting with a growing number of mathematicians and physicists are indeed directly related to astrophysics. As director of a center for astrophysics, I believe that you should be informed of their existence, for whatever their value is.

I am taking the liberty of outlining here a few points in nontechnical language. The technical profile is now treated in too numerous papers in different languages and techniques to be effectively summarized.

The root of the current situation in theoretical physics which has propagated directly into astrophysics (in my view) is the fact that the virtual totality of classical, quantum mechanical and quantum field theoretical methods were conceived for and remain capable of treating only point-like approximations of particles, whether the peripheral electron of a hydrogen atom or a proton created in the core of a neutron star.

page 2.

This is, after all, a consequence of the strict locality of the theories available. My monographs with Springer-Verlag inspect this situation at its conceptual foundations, Newtonian Mechanics. In particular, they provide a technical treatment of the fact that variationally selfadjoint physical models, whether in Newtonian Mechanics or in Riemannian geometry can only characterize action-at-a-distance interactions among point-like approximations of objects. You should keep in mind that these studies have identified as being variationally selfadjoint all current models used in astrophysics, with the possible exception of only a few cases in which technical difficulties (e.g., singularities) render debatable the computation of the adjoint systems.

We believe that these selfadjoint models have a clear place in physics of clear physical value, such as, for the exterior problem of test particles in astrophysical objects. Nevertheless, these models are not believed to be of terminal character, nor that astrophysics will stop at the current views, irrespective of the number and scientific authority of the current supporters. Indeed, the same models, when applied to the interior problem of astrophysical objects, and are truly identified in their physical foundations, provide a model, say, of the interior of a neutron star as composed of point-like constituents with action-at-a-distance forces only. This is contrary to experimental evidence that hadrons have an extended size ($\sim$1F).

Even though pursued by only a few, gaunt researchers at this time, and against all sorts of oppositions (sometimes strictly intended to prevent the conduction of these studies), what we are attempting is the reinspection of physics in the intent of achieving an initial, simple, but genuine representation of the extended character of particles in interation, whenever such a representation is physically appropriate (e.g., the dynamics of a proton within a neutron star). The methods we are searching are subjected to what we call an "uncompromisable condition" of being a covering of conventional formulations for point-like abstractions (that is, capable of recovering the latter at the limit of point-like objects). To avoid the human error of most of our colleagues, we do not claim that our results are of terminal character. They are intended only as a first step in a problem which will likely call for generations of researchers.

This objective is now pursued at a number of methodological levels, with particular reference to the algebraic, geometrical, quantum mechanical and quantum field theoretical levels.

The astrophysical, direct, implications of these studies have clearly transpared at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, that we have just completed. The proceedings (by mathematicians and physicists) will be published sometime in December 1979. A number of the articles are specifically devoted to initial steps in astrophysical problems (mainly the algebraic, geometrical and quantum mechanical treatment of

page 3.

extended hadrons within the core of astrophysical objects).The idea is that we should first try to understand the behaviour of one such extended particle , before attempting the construction of models for a collection of such particles. Initial attempts for this transition have been set for the THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, scheduled for August 1980.

Particularly intriguing has been the following outcome of the workshop. It consists of a possible link between the vexing problem of quantization of conventional gravitational models and the van Hove no go theorem of (pre) symplectic quantization, jointly with genuinely new possibilities of bypassing this impass via our broader tools (Lie-admissible algebras and symplectic-admissible geometry). Of course, this topic is under study at this moment, and it will take some predictable time before it will appear in print.

It is for me difficult to indicate all the rather numerous aspects of strict astrophysical inspiration.This is mostly due to the novelty of our mathematical methods. For instance, the Lie-admissible generalization of the Lie algebra is virtually unknown in the mathematical community at large, let alone the physical community.*

Perhaps, a representative episode (which will amuse you) occurred recently within a Newtonian context. While Skylab was falling, NASA was somewhat embarassed because of the inability to predict with even a minimum of indication the location of impact (a few days before the fall, NASA was unable to make any prediction, that is, it could have fallen everywhere on the projection of the trajectory on the entire Earth). Owing to this situation, as well as the known potential political implications, the virtual entirety of our theoretical knowledge was feverishly inspected in the hope of gaining some insight. In turn, this episode provided a clear identification of the limitations of current theoretical views.

I was contacted by NASA because they had just learned of my studies for forces more general than $f = -\partial V/\partial r$, as well as of the rigorous analytic techniques of the Inverse Problem for the treatement of these more general forces (my monographs with Springer-Verlag). I was unable to make any contribution, understandably, because of only a couple days notice... Yet my contact with NASA established the following point.

The virtual totality of current theoretical tools in classical mechanics resulted applicable only to point-like abstractions and action-at-a-distance forces (variationally selfadjoint methods).On the contrary, the dynamics of Skylab was strictly nonselfadjoint, because of drag forces necessarily nonderivable from a potential, as well as the necessary extended character of the object, as well as its orientation.

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* In case you are interested, you may inspect some of the papers I wrote in astrophysics and general relativity, as per my curriculum.

page 4.

As one NASA man vividly put it to me, in case a theoretical physicist comes here proposing to treat SKYLAB with his conventional relativities and inherent point-like approximations, "he would be likely chased out of NASA premises".

In my humble view, the current level of knowledge in astrophysics for the treatment of the interior problem is fundamentally equivalent to the point-like abstraction of the motion of Skylab in Earth athmosphere. The differences are merely technical (curvature, quantization, etc.). Indeed, once the physical reality is finally confronted, the motion of an extended hadron within superdense hadronic matter (the core of a star) is indeed conceptually equivalent to the the motion of the extended Skylab in dense Earth's atmosphere. But then the need for a severe inspection of current theoretical views in astrophysics is simply unavoidable. The difficulties for attempting a genuine advancement in astrophysics, which cannot be disjoint (in my view) from strong interactions, are of human and not of technical character: the desire by the majority of physicists (with numerous exceptions and, I believe, you included) to remain attached as much as possible to old ideas.

As I indicated earlier, this letter terminates my search for a possible continuation of my studies at Harvard. I would like to take this opportunity to stress that my loyalty to Harvard will remain untamed, as the only expression of gratitude for the hospitality received which is compatible with my ethics. Jointly, however, benevolently allow me to express my sadness for the apparent lack of interest at Harvard in the studies of these fundamental physical problems, as well as the rather strong opposition I have encountered here during their conduction.

Sincerely

c.c.: Drs. D.BOK, H.ROSOVSKY,
R. LEAHY and H. HIRONAKA

Ruggero Maria Santilli

# HADRONIC JOURNAL

VOLUME 2, NUMBER 6
DECEMBER 1979


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HELD AT THE SCIENCE CENTER OF HARVARD UNIVERSITY
FROM AUGUST 1 to 8, 1979

## PART A: REVIEW PAPERS

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# HARVARD UNIVERSITY

OFFICE OF THE PRESIDENT

MASSACHUSETTS HALL
CAMBRIDGE, MASSACHUSETTS 02138

August 13, 1979

Dear Mr. Santilli:

This note is simply to acknowledge the copy of your letter to Professor Field and to return the supporting materials.

Best wishes,

Sincerely,

Derek C. Bok

Mr. Ruggero M. Santilli
Science Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

CONFIDENTIAL

Sept. 4, 1979

Prof. H. HIRONAKA

Dear Heisuke,

I am in a delicate moment of my academic life. Your advice and, whether possible, help, would meet with my sincere gratitude and confidentiality.

As you know, my DOE grant expires on June 1, 1980. The DOE has verbally indicated to me the decision to continue the support of my studies. To keep continuity, I must apply sometime during this semester. In turn, this situation calls for the identification of my new institution within the next two months or so.

I have therefore initiate the search of an outside institution willing to administer my grant. However, this has immediately met with problems. For instance, a colleague put it to me: "If your salary is covered by your grant, why Harvard does not want to continue its administration?".

In conclusion, the transfer of my grant from Harvard to a new institution is resulting to be much more difficult than I expected.

Owing to this situation, I am also looking at the possibility whether there is any way according to which Harvard can administer my grant for the next two years (the second term). It is in this respect that your council would be appreciated. But, if you cannot advice me for any reason, simply ignore this letter.

I shall keep my promise of non-applying to the Dept. of Math. I am sincerely grateful for the hospitality I received for two years, but I am not a mathematician, nor I intend to become a mathematician. Besides, my studies are now converging not only on applied physical problems, but actually in the formulation of experiments. I do not see much of a compatibility with a Dept. of Math. and I shall, therefore, not apply (unless some senior member recommends me the contrary). It remains without saying that in these experimental studies there is considerable new mathematics (Lie-admissible algebras), yet unexplored at the mathematical level.

I did apply to the Center for Astrophysics on July 26, 1979. On August 1 I received the enclosed letter by Dr. Field in which, as you can see, my application was denied even consideration on grounds that the policy is

"to encourage research at the Center which is directly related to astrophysics and to resist the temptation to go into fields which are not directly related".

Permit me to express my great surprise when I received this letter. I simply could not believe to my eyes. My application was for what I am now doing, that is, theoretical and experimental studies on the problem of the validity or invalidity of conventional relativities within a hadron and, thus, within the interior problem of astrophysical objects.

page 2.

This research program, not only is "directly related to astrophysics", but, in the opinion of a growing number of physicists, verts on THE fundamental open problem of contemporary astrophysics. To be specific on this rather crucial point, my studies vert on the future, experimental resolution whether conventional geometries of current use in physics (i.e., of non-integral type) apply or not within hadronic matter, whether a hadron or a star. Until this problem is experimentally resolved, decades of studies at Field's Center, and millions of taxpayer's money risk to be spent in scientific hand-wavings.

Owing to this situation, I was expecting a normal consideration, jointly with other candidates, and then the customary rejection at the end of these formal processes already decided at the beginning .... I am confident you will now see the reason of my surprise. After all, since my application was in astrophysics,I have written papers in astrophysics, and I do qualify as an astrophysicist, the rejection by Field to even consider my application jointly with others might be, at the extreme, considered even against the Act for Equal Opportunity/Affirmative Action Employer.

Almost needless to say, I have absteined from bringing these aspects to Field's attention and I shall continue to do so. I answered with a moderate letter of August 10 in which I reassure him that "I understand and respect your position and that, on my part, I do not intend to pursue any more my association'with your Center".

Despite this, my application to Field's Center for a fundamental open problem in astrophysics supported by the DOE, and Field's refusal to even consider the application,remain.

Could you kindly consider the possibility of contacting Dr. Field and finding the proper way of bringing to his attention the delicate aspects of the current situation?

Another possibility for me is to apply to

Professor Peter J. Huber, Department of Statistics, Science Center room 609

according to the opening announced in the August issue of the Notices of the AMS (see ecnlosed copy).

Indeed, part of my studies that will be intensified in the next two years consists of the possible construction of a generalization of the conventional statistics of current use in physics (based on Lie algebras) for the case of the strong interactions, via the Lie-admissible coverings of the Lie algebras. I believe that this too is a rather fundamental problem of contemporary statistics and, therefore, I believe that I do qualify for this opening as far as consideration is concerned.

page 3.

Nevertheless, I have absteined from applying, waiting your return from Japan, in the hope that I can consult with you and, jointly we can avoid the duplication of Field's episode.

Could you kindly consider the possibility of contacting Dr. Huber? Could you kindly advice me whether it is appropriate to apply to this opening ?

Again, I am not asking for money from Harvard. On the contrary, I would like to bring money, as I did in the past two years. I am simply looking for the possibility that Harvard administers my DOE grant for the next two years. This essentially calls for a nontenured, nonsalaried, terminal, nonteaching appointment at a division which is compatible with my research. The Center for Astrophysics is THE most compatible. The Department of Statistics is equally compatible, and so are others (e.g., Applied Sciences).

The primary reason why I have been looking for this possibility is related to the difficulties and questions of various nature I have encountered in my recent attempts to transfer this grant to a new institution. Also, I still hope that the opposition at Harvard against my studies will one day terminate.

I enclose for your convenience copies of my curriculum, as well as of informative material on my past teaching-research-editorial activities.

Hoping that I did not abuse of your courtesy, time and patience, I would like to take this opportunity to express again the sentiments of my sincere esteem and gratitude.

Yours, Sincerely

Ruggero

P.S. On the first week of August we had an excellent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS. Half of the participants were pure mathematicians and half physicists. A number of participants were from abroad (Switzerland, France, Israel, W. Germany, Italy, Australia and the USSR, this including corresponding members who could not attend because of lack of funds for travel). I believe that we made rather crucial advances on both mathematical and physical grounds. The PROCEEDINGS of this workshop will be published in the December 1979 issue of the Hadronic Journal. I have asked to the publisher for a number of complimentary copies of this issue and I shall do my best to let you have a copy. It is expected to be available in late January 1980 and it will comprise some 12-14 articles.

HARVARD UNIVERSITY

Area Code 617
495-3352

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

September 23, 1979

President DEREK BOK,
Harvard University

Dear Mr. President,

I am concerned for the implications of a recent administrative decision on my case and, until I am a member of this community, I feel obliged to express this concern to you. I am also under the impression that this decision is due to lack of adequate information. Therefore, I am taking the liberty of outlining here the most salient aspects, while I remain at your disposal for any additional information you might need.

As you know, I am a research associate at the Department of Mathematics and the recipient of a research grant from the Department of Energy which supports my salary as well as all my research and logistic expenses (including telephone, xeroxing, etc.). My appointment and grant expire on June 1, 1980. The DOE is apparently pleased with the scientific outcome of my studies and has the best intention to continue my grant. Please feel free to contact, if you so desire, the DOE officer in charge of my case, Dr. D. C. PEASLEE, tel. 301 353 3624.

To keep continuity of grant, I have to identify a qualified research institution willing to administer my grant, and in such a way that I can apply for a renewal before the end of this calendar year. Beginning with this past summer, I have therefore applied to numerous institutions in fields related to my studies, which are: High Energy Physics, Astrophysics, and Statistics. I am also considering institutions in Engineering, because of certain engineering applications of my methods that are already under way by independent researchers.

Here at Harvard I have applied only to Dr. FIELD with an application of July 26, 1979, for a position at the Center for Astrophysics. I have absteined from applying to other Divisions of your campus, such as for the opening in the Department of Statistics announced by Dr. P.J.HUBER in the August issue of the Notices of the American Mathematical Society, lacking advice by senior members.

A copy of my letter of application to Dr. FIELD was sent to you. As you can see, my application essentially consists of theoretical and experimental studies on the problem of the validity (according to some) or the invalidity (according to others) of basic, conventional, physical laws, geometries, relativi ties and statistics for the interior of a strongly interacting particle (a hadron) or, much equivalently, for the interior of a star. It is almost pointless to indicate that the validity of these conventional settings for the exterior problem (outside a hadron or a star) is unquestionable. The studies are strictly referred to the interior problem (inside a hadron or a star) where no meaningful experimental information is available at this time.

With my considerable surprise, on August 1, 1979 I received a letter from Dr. FIELD essentially indicating that my application had not been submitted to the customary faculty consideration, because it is the policy of the Center to consider only research which "is directly related to astrophysics". Since Dr. FIELD was informed that copy of my application had been sent to you, Dean ROSOVSKY and Associate Dean LEAHY, I am lead to conclude that the decision not to consider my application has been essentially reached by Harvard's administrators.

The purpose of this letter is to provide you with information related to this decision.

MY APPLICATIONS ELSEWHERE. Permit me to stress that I have applied and I am continuing to apply to several institutions. With a full research support and with a research program of rather fundamental physical character, I have no major problem in finding a receptive Institution, despite the typical aspects involved in any grant relocation. As a matter of fact, I have already received an offer. On my part, I therefore have no need to remain necessarily at Harvard. With full candor, permit me to say that the issue is whether it is in Harvard's interest to keep my research program or not, as I shall elaborate below.

THE DIRECT RELATIONSHOP OF MY STUDIES TO ASTROPHYSICS. As communicated to Dr. FIELD in my answer of August 10, 1979, I understand and respect his decision. However, I cannot accept his view that my studies are not directly related to astrophysics. But, again, I believe that this view is only the result of lack of adequate information.

In terms as simple as possible, all currently available astrophysical models, irrespective of their most advanced implementations (e.g., gauge) are based on the conjecture for the interior problem that the geometry is always locally Lorentz (that is, Einstein's special relativity applies in the neighborhood of a point of the local variables). My studies are essentially intended to subject this conjecture to experimental verification within a hadron. If the special relativity is invalid within a hadron, it will be consequentially invalidated in the interior of astrophysical bodies, because they are nothing but a collection of hadrons. In turn, this would call for a genuine, nonincremental, advancement in astrophysics, that is, the construction of coverings of current astrophysical models for the in terior problem, which has already been initiated by a number of researchers( see below).

Irrespective of personal theoretical view by individual researchers, I stressed in my answer to Dr. FIELD that a failure of the current theoretical views in astrophysics of the interior problem has been recently identified in rather visible terms and for one of the simplest possible interior systems: the motion of SKYLAB while within Earth's atmosphere. In essence, SKYLAB had rather complex forces (nonlocal forces approximated via polynomial expansions in the velocities) which cannot be all "geometrized" via current tools. In particular, the use of conventional geometries yielded only a point-like approximation of SKYLAB with only part of the actual forces.

I would like to stress that this view is not new. In the final analysis, this view is nothing but Cartan's point that the Riemannian geometry cannot recover the entirety of Newtonian Mechanics, but only that part compatible with Galilei's relativity (again, only geometrizable forces).

But there is a further aspect in which I disagree with Dr. FIELD, the separation between astrophysics and hadron physics implied in his decision. The gravitational field, in my view, literally originates in the interior of astrophysical bodies. At a deeper analysis, the interior of these bodies is not the conventionally used notion of mass (which is a technical expedient to avoid our ignorance on the problem of structure), but instead, a collection of fields. Among them, the strong fields (interactions) are expected to play a crucial role for the very initiation of the study of the "origin" of the gravitational field. For technical details, you might consult my article in Ann. Phys. 83, 108 (1974), where a model consisting of the (conventional) geometrical "identification" of the gravitational field in the interior problem with the structure fields is proposed (as a first step, prior to the availability of more suitable geometries), jointly with an explicit experimental proposal.

You should also be informed that I have proposed a generalization of Galilei's relativity for Newtonian systems with forces nonderivable from a potential (i.e., precisely those forces that are outside of Riemannian geometry). This proposal, presen ted in the Hadronic J. 1, 223 (1978) was the result of about a decade of prior studies devoted to the methods for the treatment of the broader forces considered. Even though predictably under examination by independent researchers, this generalized relativity is the first that provides a form-invariant description of Newtonian systems with arbitrary forces, while jointly characterizing the time rate of variation of physical quantities (par contre, Galilei's relativity provides a form-invariant description of only systems with conservative forces, and the characterization of the subcase of conserved quantities).

I subsequently used this generalized relativity, after working out an initial quantization, for the problem of structure of hadrons, resulting in the proposal of a new model of structure which is now under study by a number of researchers. I enclose to this effect a paper by Dr. Jiang from the People's Republic of China. The key point is, again, the realization of strong interactions with forces more general than those $f = \partial V/\partial r$ of current use in high energy physics and astrophysics. For technical details, you may consult my paper Hadronic J. 1, 574 (1978).

Currently, I am working on a relativistic extension of this relativity or, equivalently, a generalization of Einstein's special relativity for physical conditions of particles not treated in the books, e.g., the motion of the extended proton ( radius $\cong 10^{-13}$ cm) within dense hadronic matter, such as the core of a neutron star, and expected, consequential, nongeometrizable forces. The gravitational implications of this step are crucial. I am, therefore, working in astrophysics.

In conclusion, and against the view of numerous colleagues, I do not see a qualitative difference between the problem of structure of the lighest known hadron, the $\pi^0$, and that of the most massive star. The only difference I see is a matter of size. This is the reason why I cannot accept a separation between hadron physics and astrophysics. In any case, an interplay between these two profiles of the same physical reality can only be beneficial to both.

THE HISTORICAL REASONS OF DOUBTS. By no means my doubts on the validity of conventional theoretical tools of contemporary physics for the interior of hadronic matter are new. On the contrary, I have simply followed the teaching, not of my contemporary colleagues, but of the Founding Fathers of contemporary physics. I quoted earlier Cartan. Permit me to recall that are numerous, additional, authoritative, historical voices of doubts.

(1) Fermi made it quite clear in his "Lectures in Nuclear Physics" in relation to the strong interactions and their short range that "there are doubts as to whether the usual concepts of geometry hold for such small regions of space". Notice that doubts on conventional geometry imply doubts on conventional relativity and quantum mechanical laws. After a considerable search and inquiry among his colleagues, I have come to the reconstruction that Fermi expected the strong interactions to be nonderivable from a potential (exactly the nongeometrizable forces of Cartan's point), as one way to differentiate them from the electromagnetic interactions, by therefore implyin g the possible existence of generalized formulations. On my part, I have taken seriously Fermi's teaching, and my studies on the problem of the structure of hadrons indicated earlier are based on this point by Fermi. Par contre, quark believers realize the strong forces via the simplistic structure $f = \partial V/\partial r$ up to their most advanced formulations (e.g. quantumchromodynamics), without a genuine critical inspection, and in the apparent disregard of Fermi teaching.

(2) Pauli clearly indicated in his historical lectures and papers that his exclusion principle ( a pillar of contemporary quantum mechanics) had been conceived for the lack of overlapping of the wave packets (the atomic structure). Again, I have taken seriously this teaching by Pauli. Indeed, most of my efforts are centered on the experimental verification of the validity or invalidity of Pauli's principle under the conditions of overlapping of the wave packets (see below) which are necessary for the constituents of hadrons. Par contre, quark believers apply rather lightly Pauli's principle as a pillar of their views, without a genuine critical examination, and in the apparent disregard of Pauli's teaching.

(3) Einstein's doubts on quantum mechanics, and Heisenberg's undeterminacy principle (another pillar of quantum mechanics) in particular, are famuous. He believed in the good approximation that this mechanics provides for the description of the electron's orbits in atoms. But he refused to believe up to his death in the final physical character of this theory. Incidentally, Einstein's doubts are, in my view, most touchingly and effectively reported in Heisenberg's memoires "Physics and Beyond". On my part, I have taken seriously Einstein's doubts. Indeed, my efforts

are also verted toward a theoretical proof that Heisenberg's undeterminacy principle is inapplicable under the conditions of overlapping of the wave packets, as well as toward the experimental resolution of the issue. Par contre, quark believers rather lightly assume Heisenberg's principle without a genuine critical inspection for the necessary conditions of overlapping of the wave packets of the hadronic constituents, and in the by now known disregard of Einstein's teaching.

(4) Jordan's doubts on the lack of terminal character of quantum mechanics and quantum statistics are also well known. These doubts resulted in the creation of a new (nonassociative) algebra known as Jordan algebra. In his joint celebrated paper of 1931 with von Neumann and Wigner, he put it quite clearly that "the need of such a generalization arises from the (probably) fundamental difficulties resulting when one attempts to apply quantum mechanics to questions in relativistic and nuclear phenomena". I have taken quite seriously the teaching by these three Founders of contemporary physics. Indeed, on algebraic grounds, my efforts are based on Jordan's view of generalizing the algebraic structure of quantum mechanics. In fact, my generalization of Galilei's relativity and my methods of quantization are based on more general algebras which are jointly Lie-admissible, as well as Jordan-admissible (thus, including Jordan's approach), in order to accomodate forces more general than $f = -\partial V/\partial r$. Par contre, quark believers preserve in its entirety the conventional algebraic structure of quantum mechanics, without a genuine critical inspection, and in apparent disregard of the teaching of Masters such as Jordan, von Neumann and Wigner, with very few exceptions (I am referring here to recent uses of the $M_3^8$ Jordan algebras for quark oriented models that have received rather moderate receptions within the same circle of quark believers).

(5) Wigner himself has clearly expressed in his memoir "Symmetry and reflections", and for different technical reasons, the statement: "let us not forget, the problem of interactions is still a mistery."

My list of historical reasons of doubts by authoritative voices could continue....

THE ROLE OF THE HADRONIC JOURNAL. When I decided to organize this new Journal in January of 1978 the situation was essentially the following. We had numerous authoritative and seemingly uncorrelated doubts on the validity of conventional views for the strong interactions, but we had:
(a) no comprehensive analysis on the reasons for such authoritative doubts;
(b) no organized initiation of the studies for their experimental resolution ; and
(c) no effective beginning of the studies aiming at the construction of possible generalized formulations, specifically conceived for the strong interactions.

You should be informed that, beginning from the very first issue of April 1978, and thanks to the participation of scientists with genuine vision from virtually all developed Countries, the HADRONIC JOURNAL has made genuine contributions on these rather fundamental physical problems.

Today we do have an initial, but comprehensive analysis, linking rather deeply the seemingly independent doubts by Fermi, Pauli, Einstein, Jordan, von Neumann, Wigner and others. In essence, we have identified the property that, under the conditions of overlapping of the wave packets, we have nonlocal forces nonderivable from a potential which imply the inapplicability of conventional (symplectic and Riemannian) geometries (FERMI POINT). In turn, these forces, even under local approximation nonderivable from a potential, imply the breaking of the spin symmetry of particles and, thus, the inapplicability of Pauli's principle (PAULI POINT). Still in turn, these broader forces imply a departure from Heisenberg's principle (EINSTEIN POINT). Still in turn, in order to be algebraically treated, these broader forces call for a generalization of the associative enveloping algebra of quantum mechanics into a nonassociative form (JORDAN POINT). Still in turn, these broader forces imply rather fundamental, open problems for the strong interactions (WIGNER POINT). And so on. In conclusions, all the doubts by the Founding Fathers of contemporary physics turned out to be deeply and intriguingly related.

Today we do have initial formulations of experiments. I am referring here to:

- My proposal in the Hadronic J. 1, 574 (1978), subsequently elaborated in a number of articles, to test Pauli's exclusion principle in nuclear physics. At this level, we have experimentally established, statistically small, conditions of overlapping of the wave packets of the nucleons. Our methods then predict a statistically small deviation from the exact validity of Pauli's principle in atomic physics, which may have well escaped existing inspection, much along the (quantitatively similar) historical case of the discovery of the violation of discrete symmetries by LEE and YANG. The following aspects of this experimental proposal may have a relevance for your consideration of my case. First, the mechanism of inapplicability of Pauli's principle which is conjectured in nuclear physics is based on the breaking of the central part of Einstein's special relativity, the so-called SU(2)-spin part. Second, a small deviation from Pauli's principle in nuclear physics necessarily implies a bigger departure at the level of the structure of the hadrons (because of the higher state of overlapping of the wave packets), and an even bigger departure at the astrophysical level (because of the extremely high pressures and densities of hadronic matter). You should therefore be fully informed that this experiment is the first step toward the proof or disproof of a fundamental conjecture of contemporary astrophysics, that the geometries for the interior problem are Lorentz in local character.
- Kim's proposal in the Hadronic J. 1, 1343 (1978) to measure more accurately the mean life of unstable hadrons in flight. If the structure forces are more general than those currently believed, we have a deviation of the quantities predicted by Einstein' special relativity. This experiment too is, therefore, of direct astrophysical relevance and can be useful for the future resolution of the fundamental assumption of contemporary astrophysics indicated earlier, although from a complementary profile.

You should also be informed that numerous other experimental tests are under study.

Finally, we do have today a coordinated effort aiming at the construction of generalized formulations specifically conceived for the strong interactions, ranking the participation of senior mathematicians of the caliber of Tomber, as well as senior physicists of the caliber of Okubo, including Nobel Laureate Prigogine who received the Nobel price for his statistical studies on forces nonderivable from a potential. In particular, I am conducting my search of maturity in these studies via

(1) two series of research monographs on the methods for the treatment of forces nonderivable from a potential, one with Springer-Verlag under the title "Foundations of Theoretical Physics" (Volume I was published in 1978, Vol. II is in press, and Vol. III is scheduled for 1982), and one with the Hadronic Press under the title "Lie-Admissible Approach to the Hadronic Structure" (Vol. I was published in 1978, Vol. II is in press, and Vol. III is scheduled for 1980).

(2) A series of articles I have written or are under consideration in various Journals (Physical Review D, Foundations of Physics and the Hadronic Journal), either alone, or in collaboration with Professors Myung (from the USA), Kobussen (from the Institut für Theoretische Physik of Zürich), Ktorides (from Greece), Fronteau and Tellez-Arenas (from France) and Eliezer and his group (from Australia).

(3) An intensive effort of consultation with receptive colleagues of either mathematical or physical or experimental orientation in the various aspects of the problem.

(4) The coordination of efforts by independent researchers for articles in the Hadronic Journal; and

(5) A yearly congress, specifically devoted to these studies, called "Workshop on Lie-admissible formulations". The first was held in August 1978 and resulted in a number of papers. The second was recently held in August 1979 with the participation of mathematicians and physicists from the USA, France, Switzerland, Belgium, and Israel, as well as contributing participants (who could not attend for various reasons, but are sending in their contributions) from the USSR, and the People's Republic of China. The Proceedings will be published in January, 1980.

You are aware from preceding contacts of my determination to pursue these studies, at whatever personal sacrifice. In essence, I decided long ago to pursue knowledge in physics, rather than a career in physics, by being fully aware that this is more than often unrewarding.

THE SCIENTIFIC IMPLICATIONS. I am under the impression that Harvard administrators, again, I believe, primarily for lack of adequate information on my studies, are not actually aware of the implications of these studies. Permit me the liberty to candidly stress that we are here facing a coordinated and organized effort by numerous mathematicians and physicists to generalize the very foundations of contemporary physics.

In High Energy Physics we are feverishly working at the generalization of the fundamental tools, such as the relativity and the quantum mechanics, for the forces nonderivable from a potential, representative, as indicated earlier, of conditions of overlapping of the wave packets of particles. Even though available generalized tools are still at a rudimentary level (after all, Galilei's relativity was conceived circa 1638!), they have already been useful to identify the next technical problems to be solved, and they have already produced an alternative model to hadrons structure which, even though ignored by quark-committed physicists, is under active study by numerous young minds in various Countries (see the enclosed paper by Dr. Jiang from China).

In Statistics we have already identified a generalization of conventional settings (technically possessing a Lie-admissible algebraic structure, rather than the conventionally used Lie algebra structure) of course, under the presence of forces more general than $f = -\partial V/\partial r$. Also, a potential inapplicability of Pauli's principle under strong interactions has already stimulated the study of a generalization (of Lie-admissible character) of the fundamental statistics of contemporary particle physics, the Fermi-Dirac and Einstein-Bose statistics. A paper by an independent researcher is forthcoming in the Hadronic Journal on this crucial topic, and I will be glad to publish it whenever it reaches maturity.

In Astrophysics the possible inapplicability of the local Lorentz character of the geometries for the interior problem has already stimulated the study of generalized geometries by independent mathematicians (it is a geometry tentatively called symplectic-admissible and which is capable of accomodating the most general forces known at this time, the integrodifferential-nonlocal forces, rather than the limited capability of the Riemannian geometry of accomodating physical forces of nature). A critical inspection and assessement of Einstein's theory for the interior problem is under way by an independent researcher , expert in the field, and I will be glad to publish it in the Hadronic Journal whenever it achieves maturity.

We do not claim that our efforts are necessarily on the right track. All advances in physics are the result of a historically laborious process of trial and error. Also, in case our direction will be eventually proved via experiments to be valid, we do not claim that our approach has a terminal character. I firmly believe that Theoretical Physics will never achieve terminal descriptions. We are only participants and spectators (in Heisenberg's words) of a continuing scientific process.

Nevertheless, I believe that studies of such fundamental physical character should deserve a serious consideration, not only at the scientific level, but also at the administrative level. You are aware of the rather strong opposition I have found here at Harvard by quark-committed, senior colleagues, against the conduction of these studies. What I would like to add here is that these studies are now in motion on a world wide scale; the participation by mathematicians and physicists with a genuine vision is increasing litterally on a monthly basis; and these studies by now will be continued irrespective of whether I remain at Harvard or not, and whether I continue my contribution or not. What I am attempting to say here is that you should be fully informed of the scientific drive and momentum toward the experimental resolutions of theoretical beliefs, whether mine or those by others.

THE POLITICAL IMPLICATIONS. A final aspect I feel obligéd to bring to your personal attention is that, jointly with the scientific drive indicated earlier, a political drive at a number of administrative levels has been initiated, and it is now in motion. I would like to stress that this drive is substantially out of my control, and I try to abstein myself as much as I possibly can. I am simply not a politician, as proved by the candor of my writings. I am only interested in doing physics, and it is for me reason of particular regret when forced into the consideration of this profile.

The passwords of this political action are: SCIENTIFIC ACCOUNTABILITY.

B illions of dollars of taxpayers money are annually spent in this Country in research either directly or indirectly related to the strong interactions. Most of these public funds are nowaday spent in research conducted and based on the tacit, unquestioned, belief of the validity of conventional laws, geometries and statistics for the physical arena considered. This is certainly admissible on scientific grounds, and I personally favor the condution of these studies, as explicitly stated in my papers.

Nevertheless, the political action under consideration calls for
- the continuation of the study and funding of research in strong interactions based on the validity of conventional theoretical views, but
- jointly, the conduction and funding of theoretical and experimental studies on their possible invalidity, as well as generalization, to achieve a well balanced use of public funds.

If you meditate a little on the scientific scene emerging from this report, you will find the consideration of this point inessential. During the past two years we had a rather intensive promotion of a moment of reflection on the validity of conventional, basic laws for the strong interactions, backed by numerous, historical, authoritative voices. This action had to imply, sooner or later, a call on the scientific accountability.

Also, you should be informed that this political action has been and is being aggravated by the rather caparbi ous opposition by physicists academically and financially committed to quarks. Most regrettable is the complete silence, to my best knowledge, in the quark literature on the very existence of these problems at the level of the fundamental laws.

You should be informed also that, to my knowledge, this political action has been aggravated by the following additional aspect. The problem of the basic physical laws for the strong interactions is expected to have a deep (if not crucial) impact on energy related issues, with particular reference to the problem of the controlled fusion. After all, this fusion is nothing but a laboratory construction of bound states of nucleons. As such, the basic laws do have a primary role. Regrettably, this energy connection has been dismissed as non-sense by a number of quark-committed physists, to the best of my understanding and reconstruction. But this simply invites a process to the scientific accountability of these physicists, to the detriment of their supporting institutions.

THE IMPLICATIONS OF YOUR DECISION. At this moment, and in the view of outside observers, the scientific situation at Harvard in the sector is well balanced. It is true that the majority of funds and research is being devoted to the use of conventional laws, geometries and statistics. Nevertheless, you house the originator of the studies on their experimental finalization.

Your decision not to allow the continuation of my research here has the direct effect of producing, in my view, a significant scientific unbalance in your campus.

This letter will achieve its objective if Harvard's administrators will reach a final decision on my case, first of all, according to standard practices (an in depth assessement of the scientific merits of my studies), but also wi th due consideration of the implications for a possible unsubstantiated negative decision.

On my part, I can only confirm what indicated previously to you, Dr. Field and others, that whatever your final decision will be, you can count on my best understanding, as well as my best loyalty, as an expression of my appreciation for the hospitality received.

Very Truly Yours

Ruggero Maria Santilli

c.c.: Drs. ROSOVSKY, LEAHY and FIELD.
enclosures.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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495-2170

SCIENCE CENTER
ONE OXFORD STREET
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October 1, 1979

Professor R. M. Santilli
Department of Mathematics
Harvard University

Dear Professor Santilli:

After having read your letter of September 4, 1979 and talking with you, I now feel that I should make clear my opinion about your trying to continue your association with Harvard University. My opinion is that you can conduct your research much more efficiently and comfortably at some other institution than Harvard. I do not believe the transfer of your grant from Harvard to a new institution will create any difficulty either from the point of view of formality or from the point of view of the continuation of your own research activity. You asked me to contact Dr. Field at the Center for Astrophysics, but I can not believe that I can be of any service in persuading him or his colleagues to change his or their decision. The reason is simply that my understanding of your work is completely inadequate to make any reliable recommendation. The same applies to the Department of Statistics at Harvard. Simply, I am making neither any recommendation to either one of the two nor to any other department at Harvard.

I'm glad that you are keeping your promise of not applying to the Department of Mathematics. You should be aware of the departmental decision made during the last academic year that your appointment as Research Associate in our Department shall terminate at the end of May, 1980, irrespective of whether your D.O.E. grant is to be renewed or not.

I understand that you have an offer from some outside institution. I'm glad that you're making maximum effort to find a new institution in which to continue your line of research and I wish you the best success in your future career wherever you go.

Sincerely yours,

Heisuke Hironaka

Heisuke Hironaka
Chairman

HH/mjm

**Center for Astrophysics**
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

October 2, 1979

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

Dear Dr. Santilli,

Thank you for your letter of September 26. I read it with interest.

Sincerely,

George B. Field

GBF/ljc

HARVARD UNIVERSITY

Area Code 617
495-3352

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

October 2, 1979

Professor HEISUKE HIRONAKA,
Department of Mathematics

Dear Heisuke,

I would like to thank you for your letter of October 1, and reassure you that I am in full agreement with its contents. In particular, I am in full agreement with your view that I can conduct my research "much more efficiently and comfortably at some other institution than Harvard". Permit me to express my reasons for such a view (which may be different than yours).

You are familiar with my episode at Lyman. When, on September 1, 1977 I joined the Lyman Laboratory as "honorary research fellow" for the academic year 1977-1978, I was unemployed with a family of four to support and my wife at the graduate school. In particular, I was receiving unemployment benefits from the town of Newton, the office in Center Street. Despite my considerable experience and a nation wide search, I had simply been unable to find an academic job. Subsequently, I was authorized to file a research grant application with Shlomo, with Lyman being my affiliation. The application was accepted and the grant was sent to Harvard for signature on March 1978. I therefore applied at Lyman for: (A) the removal of the term "honorary" from my title for the remaining part of the appointment, so that I could draw a salary (according to Harvard's rules, a person with the term "honorary" in its title cannot receive salary); and (B) the consideration of the extension of my non-tenured, non-teachin g, terminal, research associate position for the duration of the grant (until June 1, 1979). I insisted repeatedly that I was primary interested in the removal of the term "honorary" for the remaining part of the appointment, so that I could draw a salary. The question for a possible extension was secondary for me. At that time, all my unemployement benefits had terminated (there is a maximum of 33 weeks in Massachusetts) and all my savings had terminated too. After a number of months of deliberation, the senior faculty at Lyman decided, on June 19, 1978, against the removal of the term "honorary" from my title, as well as against my appointment for the duration of the grant as research associate. In this way, I was forced into the rather paradoxical situation whereby I remained at Lyman until the end of the term, while I was prohibited to draw a salary from my grant, and while I was unemployed with two cildren to feed, without unemployment benefits, nor savings, nor the capability to look for a manual job to avoid conflicts with my federal grant. As you know, this situation was resolved thanks to the invaluable help by Dean Leahy.

You are also familiar how close we were to a duplication of the Lyman episode at your department. I am sure you realize that, after an experience of this type, I took all the initiatives to leave Harvard. Nevertheless, Shlomo reapplied for a one year extension of the grant, with my affiliation being this time the department of mathematics. The application was soon funded, and I therefore informed my outside contacts that I was not interested for 1979-1980. Nevertheless, in early April 1979 you informed me that the senior faculty had decided against the renewal of my terminal, one-year, research associate position covered by this already funded grant. As you recall, you further indicated to me that you saw "no possibility" of overcoming the situation. I am sure you realize what this decision meant to me: back to unemployment. Indeed, I saw no practical way of resolving the entanglements for a relocation of the grant under the circumstances. Also, in April all (the very few) openings in the academic world had been filled. The subsequent events are familiar to you. The impasse was resolved thanks, this time, to your invaluable help.

You should be informed that these occurrences have stimulated a predictable distorsion of my scientific programs. My article in the Hadronic Journal 1, 574 (1978) entitled "Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle" was released for publication on June 19, 1979, the day of the negative final decision on my case at Lyman. As you know, the article consists of a detailed critical analysis (for some 337 pages) of the validity of conventional laws for the strong interactions, that is, the basic laws used in the current quark conjectures. I had no plans at that time of publishing this article, nor I would have published it in case of my association with Lyman. Acually, after having spent a number of years of study of the topic, I was planning to spend a number of additional years to reach a maturity commensurate to the topic.

Similarly, my draft entitled "An intriguing legacy by Albert Einstein: the expected invalidation of quark conjectures" was written, as you know, after your communication in early April 1979 on the negative decision of my case at your department. As you also know, at that time I was deeply involved in completing my volume II with Springer-Verlag and I had no plans nor intention of writing this article. The article (distributed in some 15,000 copies) was this time a direct critical examination of the quark conjectures and presented only part of the reasons why, after studying these models for over a decade, I do not accept them. Nevertheless, the spirit of the article was not negative with respect to quark-oriented studies. All physical studies are valuable, even when they are proved as wrong. Physics is an approximation of reality. It is not an exact science such as mathematics. As such, quark models definitely provide a good approximation of the hadron ic world, whether quarks exist or are a mere imagination in the mind of physicists. The spirit of the article was different. Although not explicitly stated, the spirit of the article was to draw a moment of reflection on the politics surrounding quark conjectures. This spirit has been understood by a number of qualified readers, as attested by rather numerous letters and phone calls of support I received, some of them from quark-committed physicists. In short, it appears that the problems of our community, rather than being of scientific character, appears to be more of human character.

In conclusion, when faced with the rather unusual episodes I have recalled early, I selected the MINIMAL POSSIBLE REACTION: an aggressive scientific initiative, combined with the utmost confidentiality on my episodes here vis-a-vis with the outside world. You know that I have kept this confidentiality, and you can be confident of my mature attitude in the future. I am sure you realize that I could not have possibly remained totally passive. I am also confident you realize that no senior member here in true control of his mental capacity was expecting that I remained totally pass ve.

I hope you will now see that I am indeed actively looking for an outside department which, after authorizing me to file a research grant application, HONORS ITS COMMITTMENT AND DOES NOT SUBSEQUENTLY CHANGE MIND. I need this type of rather straighforward association, first of all, for my peace of mind, and, secondly, to prevent completely unnecessary, further scientific distorsions. You can therefore rest assured that I shall not apply to the department of mathematics here, nor at any other department, nor I shall seek, on my part, the association with the Center of Astrophysics. Nevertheless, you should not expect that my grant relocation will be easy.

I am also under the impression that my letter of September 24, 1979 has been misinterpreted (which does not surprises me owing to similar previous episodes). My sincere and only desire was with this letter to provide elements to Harvard administrators to keep themselves informed in a rapidly changing scientific scene, in a rather crucial sector of research, involving considerable amounts of taxpayers money. I am fully aware of the difficulties of their jobs, as well as their responsibilities. But this demands inside knowledge. I simply felt that, until I am a member of this community, I should provide this knowledge for whatever its value is. It appears that I was wrong in doing this, and you can rest assured that I see no reason for further disclosures in the future.

In closing, permit me to take this opportunity to confirm my sincere gratitude to you, as well as my sincere esteem in you as a person as well as a scientist. I appreciated your good wishes for a new job and, wherever I will be, I will remember you always with pleasure. Hoping that this letter will close the case, I remain

Sincerely Yours

Ruggero Maria Santilli

HARVARD UNIVERSITY

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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October 10, 1979

Dr. GEORGE B. FIELD
Center for Astrophysics
Harvard University

Dear Dr. Field,

I appreciated your note of October 2. I enclose "Chart 4.9" of my volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, now in press, dealing with the doubts on conventional geometries and laws for the interior problem, as well as with the historical voices of doubts (see part 9 of this chart, pp. 343-349). Perhaps you will prefer the conventional scientific language of this presentation (as compared to the informal language of my note to ANGAS HURST). Also, please note the formal acknowledgment to Lyman in p. 19.

Please take into account that the chart is written for the audience intended for my volumes, that is, graduate students or researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical treatment of the subject is presented elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be distributed in early 1980.

The key technical point is the no-go theorem of quantization via conventional approaches of the (pre)symplectic geometry, outlined in pages 309-315 (Part 6), and technically treated in the workshop. I believe that this no-go theorem should be brought to the attention of astrophysicists who are not aware of it (it is little known in physical circles).

You might be interested to know that my paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" has been accepted for publication in FOUNDATIONS OF PHYSICS. I am currently working in reaching the best possible scientific maturity of presentation with the assistance of a number of colleagues.

Sincerely,

Ruggero Maria Santilli

RMS/ml

HARVARD UNIVERSITY

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

October 10, 1979

Professor FRED L. WHIPPLE,
Center for Astrophysics
Harvard University

Dear Professor Whipple,

I would like to thank you for your consideration in regards to the editorial finalization of my paper "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures", and for the courtesy of returning to me the material prior to your vacation.

You might be interested to know that the paper has been accepted for publication in FOUNDATIONS OF PHYSICS, although I am still working to achieve the best possible maturity of presentation with the assistance of a number of colleagues.

These studies are acquiring more and more an astrophysical implication and flavor. In case you are interested, I enclose copy of the "Chart 4.9" of my volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, now in press. This chart essentially presents an outline of the doubts on the validity of conventional laws and geometries for the interior problem (only), whether a hadron or a star. The chart also recalls the authoritative, historical,reasons of doubts ("legacies", in my language) by Einstein, Fermi, Jordan, and other founders of contemporary physics, that have been largely ignored by contemporary physicists.

Please keep into acc ount that this chart is intended for graduate students and researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization. The technical presentation is elsewhere and, in particular , in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, we held here at Harvard from August 1 to 7, 1979, with the particopation of mathematicians and physicists from the USA, France, Switzerland, France, Belgium, and Israel, and with corresponding participants from the USSR and from the People's Republic of China. The proceedings will be distributed in early 1980.

The astrophysical implication which transpared quite clearly at this workshop is a doubt on the conventional assumption that the geometries of the interior problem are Lorentz in local character. In essence, we considered a proton within the core of a star. We abandoned point-

page 2.

like abstractions of this particle and consider it as it actually is in nature: an extended object with an extended wave packet. The high pressures and densities of the star force this wave packet to penetrate within those of the surronding particles. This results in forces more general than the simplistic $f = -\partial V/\partial r$, and in a dynamics which is fundamentally different than that conceived by Einstein for the special relativity (motion of the particle in vacuum under the electromagnetic interactions). The implications of these broader forces have been studied to a preliminary, but detailed extent. It essentially emerges that, under these broader forces resulting from the state of penetration of the proton within hadronic matter, not only the Poincaré symmetry is broken, but actually the breaking occurs at the level of its central part: the SU(2)-spin symmetry.

We are feverishly working at a generalization of conventional approaches via what we call the "symplectic-admissible" geometry (you might also call it a "Riemannian-admissible geometry"). This geometry was identified at the workshop as capable of representing the largest forces known at this time: the variationally nonselfadjoint, integrodifferential forces (superposition of local and nonlocal forces derivable and nonderivable from a potential).

By comparison, the Riemannian geometry, including its graded or gauge extensions, can accomodate a truly limited class of forces of nature. This point, of course, is not new. You will recall that Cartan made it quite clear that the Riemannian Geometry does not recover all of Newtonian Mechanics, but only that part with "geometrizable" forces (essentially that compatible with Galilei's relativity).

Irrespective of personal theoretical views, you will be amused that a visible example of the "collapse" of conventional geometries for the interior problem has been recently identified in Space Mechanics: SKYLAB had highly nongeometrizable forces. The Riemannian geometry essentially produced a point-like abstraction of this simplest possible interior system. This situation persists also for other systems, such as spinning tops, provided that one does not fall into the trap of representing them via Galilei's relativity (which would imply the perpetual motion), but treats them as they actually are in nature: extended rotational bodies in a resistive medium with drag torques for which the conventional SU(2) symmetry is meaningless (it would lead again to the perpetual motion). Apart technical profiles, this is conceptually our view of a proton within a star, and its possible departure from a local Lorentz character.

You might also be interested that we are feverishly promoting the experimental finalization of this situation, either in favor or against conventional views. In particular , we have experimental proposals at the nuclear level which, even though predictably delicate, are

page 3.

feasible with current technology. At this nuclear level we expect the possibility of very small deviations (because the condition of overlapping is here very small), which might have escaped inspection simply because not looked for. It is understood that these nuclear experiments have a direct astrophysical inspiration, conception and technical formulation. Again, they are conceived to test whether the geometry is SU(2) as well as SL(2.c) of local character or not.

In case you are interested in being kept informed of these studies, please let me know. I shall instruct my secretary to mail you selected, relevant,material.

The spirit of these studies can be best expressed via Heisenberg's words ("Physics and Beyond", p. 70)

> "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 hazardous leap forward."

To which he adds immediately after:

> "However, when it comes to entering 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

Ruggero Maria Santilli

RMS/ml
encls

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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October 10, 1979

Professor RICCARDO GIACCONI
Center for Astrophysics
Harvard University

Dear Riccardo,

I am taking the liberty of keeping you informed of our progresses, because they appear to have a deeper and deeper astrophysical flavor and implication.

I enclose copy of the "Chart 4.9" of my volume II with Springer-Verlag entitled"Foundations of Theoretical Mechanics", now in print. This chart outlines the reasons of doubt for the conventional geometries and laws with regard to the interior problem, whether that of a hadron or a star. The chart also presents an outline of the hystorical voices of doubts on the subject by Einstein, Fermi, Jordan, and others (you may see part 9, pages 343-349).

Please take into account that the presentation of this chart is that for the intended level of the audience, graduate students and researchers without a technical knowledge of symplectic quantization and of the broader Lie-admissible quantization.

The technical presentation is elsewhere and, in particular, in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS held here at Harvard from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Switzerland, Belgium and Israel, as well as corresponding participants from the USSR and the People's Republic of China. The proceedings will be distributed in early 1980.

In non-technical terms, the implications which clearly transpared at this workshop for astrophysics are the following. What appears to be in doubt is the rather crucial assumption of current models for the interior problem (only) according to which all admissible geometries are Lorentz in local character. We have conducted a preliminary, but extensive and detailed analysis of the dynamical behaviour of a particle (say, a proton) within hadronic matter (say, the core of a neutron star). Once point-like astractions are abandoned, and the particle is faced as it actually is in nature, an extended object

page 2.

possessing a wave packets of finite size, the physical picture appears to be different than conventional ones. The high pressures and densities of the core of a star force this wave packet to be in a state of penetration with those of surrounding particles. This results in generalized forces which break, to our understanding, not only the special relativity, but actually its central part: the SU(2)-spin symmetry, along much of the Newtonian breaking of the SU(2) symmetry of the spinning top in our environment which is necessary to avoid perpetual-type motions. A feverish activity is going on for the technical construction of covering notions via the Lie-admissible algebras, and the SU(2)-admissible covering of the SU(2)-spin algebra. The SU(2)-symmetry breaking forces are not geometrizable via conventional Riemannian approaches as well as conventional graded-gauge extensions of current consideration. This is not new. Cartan stated quite clearly that the Riemannian geometry does not recover the Newtonian Mechanics, but only that (minute) part with "geometrizable" (à la Riemann) forces (essentially that compatible with Galilei's relativity). A visible example of the "collapse" of contemporary views in the interior problem has been recently identified by NASA: SKYLAB had highly non-geometrizable forces (polynomial expansions in the velocities of nonself-adjoint character). The Riemannian geometry produces only a point-like abstraction for this simplest possible interior system.

You might be interested to know that I am feverishly working in astrophysics, but, by specific intent, not that conventionally done. In essence, I am working at a covering of Einstein's ideas for the dynamical conditions of an extended particle within the core of a star. The geometry I am using is, what we call, of symplectic-admissible type (you might call it also of "Riemannian-admissible" type). This geometry was proved at the recent workshop to be able to accomodate the largest forces known at this time: the variationally nonselfadjoint, integrodifferential forces (superposition of local and nonlocal forces derivable and not derrvable from a potential), as compared to the truly limited capability of the conventionally used geometries for the representation of the forces of nature. This work is, in essence, a "relativistic" extension of the Symplectic-admissible covering of Galilei's relativity I proposed in the Hadronic J. 1. 224 (1978), subsequently worked out and expanded by a number of independent authors.

A feverish promotional activity is also going on in the hope that experimentalists will finally put the machines in motion. We have the formulation of experiments at the nuclear level which are apparently feasable with current technonogy. These proposals predict very small deviations at the nuclear level (because of the very small condition of overlapping of the wave packets at this level). But, they would be sufficient to indicate larger deviations at the hadronic and the astrophysics levels. Again, the mechanics of these nuclear experiments is specifically intended to test the validity of invalidity of the conjecture that the geometries of the interior gravitational problem is Lorentz in local character.

page 3.

The technical ground of these expected deviations is essentially set by a no-go theorem of quantization via the conventional approaches of the (pre)symplectic geometry, which has been recently proved by mathematicians without any idea of the physical implications, particularly in gravitations. This theorem (outlined in Part 6 of the enclosed chart) appears to be little known in physics (and astrophysics) circles.

In case you are interested to be kept informed of these theoretical, and, now, experimental efforts, please let me know. I shall put you in our mailing list.

My paper (you eventually received in April 1979) entitled "An intriguing legacy by Albert Einstein: the expossible invalidation of quark conjectures" has been accepted for publication in FOUNDATIONS OF PHYSICS, although I am currently working on achieving the best possible maturity of presentation.

You might also be interested to know that in August I applied to Dr. FIELD to join your Center. But I received soon after an answer that the Center is interested only in research "directly related to astrophysics". In particular, my application was not for money. As a matter of fact, I bring money, in the sense that I am fully supported via my grant with the DEPARTMENT OF ENERGY. I simply asked for the possibility of reapplying for the renewal of this grant that expires on June 1, 1980, either alone, or in collaboration with senior, interested,colleagues.

I answered to Dr. FIELD that I respect his decision. In essence, I am under the impression that at the time of his answer he was not aware at all that our studies do have a direct astrophysical implication, and that I am indeed directly working in astrophysics.

In any case, as a result of this answer I have applied to other centers of astrophysics interested in the interplay between hadron physics and astrophysics, as well as in the experimental resolution of theoretical views. At this moment I have a couple of verbal offers, and I contemplate to reach a final decision by November-December.

Please do not feel obliged to intervene. I simply wanted to inform you. Nevertheless, in case you are interested in what we are doing, I would be happy to see you.

Sincerely

Ruggero Maria Santilli

RMS/nl
encls.

HARVARD UNIVERSITY

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RUGGERO MARIA SANTILLI
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December 18, 1979

Professor HEISUKE HIRONAKA
Chairman
Department of Mathematics
Harvard University

Dear Heisuke,

We are in the process of printing the PROCEEDINGS OF THE SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, and a draft of the table of contents is enclosed for your convenience.

As you can see, I would like to release my contributions to these Proceedings with my affiliation with the Department of Mathematics. The reason is that it is known in the scientific community that the studies of the Lie-admissible generalization of Lie's theory was launches while I was visiting your Department. I therefore believe that it would be scientifically correct, as well as in the interest of your department, to leave a formal record of the event.

In case you desire information on the quality of my contributions, please feel free to contact the following senior mathematicians who attended our workshop

Professor ROBERH H. OEMKE
Chairman
Department of Mathematics
The University of Iowa
Tel. 319-353 3669

Professor MARVIN L. TOMBER
Department of Mathematics
Michigan State University
tel 517 355 1000

The Proceedings are scheduled for print at the end of the year (around December 27-30). In case you have any objection, please let me know and you can count on my best understanding.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
c.c.: Associate Dean
R.G.LEAHY

January 2, 1980

Dear Heisuke,

I enclose copy of the table of contents and of the abstracts of the individual papers of the Proceedings of our Workshop on Lie-admissibility. Printing is expected to start sometime tomorrow. Again, in case you consider advisable that I change "Dept. of Math" into "Science Center" for my papers, please let me know, and I shall try to do my best.

There is a great exitement about these proceedings in mathematical and physical circles (outside quark financial interests) in the various Countries of the participants (USA, USSR, Switzerland, France, Greece, Israel, Australia, and others).

Perhaps, you might be interested to have an idea as a mathematician.

The starting point is provided by two theorems of invalidation of conventional quantization. One, proved by Abraham and Marsden in their recent edition of "Foundations of Mechanics", essentially states that there is NO MAP from functions A,B,... in the cotangent bundle T*M equipped with the Poisson brackets, to Hermitian operators $\tilde{A}$, $\tilde{B}$, ...in a Hilbert space equipped with Heisenberg's product

$$[A,B]_{ce} = \frac{\partial A}{\partial r^k}\frac{\partial B}{\partial p_k} - \frac{\partial B}{\partial r^k}\frac{\partial A}{\partial p_k} \longrightarrow \frac{1}{i}[\tilde{A},\tilde{B}] = \frac{1}{i}(\tilde{A}\tilde{B} - \tilde{B}\tilde{A}) \qquad (1)$$

The other, proved by a number of physicists, essentially states that Heisenberg's equations

$$\dot{\tilde{A}}(\tilde{r},\tilde{p}) = \frac{1}{i}[\tilde{A},\tilde{H}] \qquad (2)$$

are inconsistent on numerous counts (e.g., intrinsically inconsistent, violate the correspondence principle to Hamilton's equations, etc.) for all Hamiltonians that are of polynomial order in the canonical operators $\tilde{r}$ and $\tilde{p}$ higher than the second (that is, for all Hamiltonian vector fields that are nonlinear in the local variables).

The two theorems have resulted to be two sides of the same coin,

- one of geometrical character (general inconsistency of the symplectic quantization for canonical, that is, fundamental, symplectic two-forms); and
- the other of dynamical character (the quantization of Hamilton's equations into Heisenberg's equations is generally inconsistent).

The reasons for these rather serious inconsistencies have been unknown for some time. By spring 1979, the primary reasons had been sufficiently well identified. They have been then reverified at the workshop by the mathematicians and physicists who participated (either physically, or as corresponding participants because of lack of $ to travel). The most important one is that the Lie algebra of the Poisson brackets is the attached algebra to the

NONASSOCIATIVE LIE-ADMISSIBLE ENVELOPE WITH PRODUCT $A\cdot H = \frac{\partial A}{\partial r^k}\frac{\partial H}{\partial p_k}$ (3)

while the Lie algebra of the Heisenberg brackets is the attached algebra to the

ASSOCIATIVE LIE-ADMISSIBLE ENVELOPE WITH PRODUCT $\tilde{A}\tilde{H}$. (4)

As a result, mapping (1) is fundamentally inconsistent because it violates the algebraic structure of the underlying envelope. The validity of the Abraham-Marsden theorem is then selfevident. The valdiity of the other theorem is also selfevident.

This situation suggests, rather forcefully, the need of Lie-admissible algebras in quantum mechanics. In fact, as a necessary condition to attempt quantization, the time evolution law in quantum mechanics should be constructed via a nonassociative Lie-admissible algebra in exactly the same way as it occurs in Hamiltonian mechanics

| HAMILTON'S EQUATIONS | QUANTUM MECHANICAL EQUATIONS |
|---|---|
| $\dot{A} = [A,H]_{Class.} = A\cdot H - H\cdot A$ | $\dot{\tilde{A}} = \frac{1}{i}[\tilde{A},\tilde{H}]^* = \tilde{A}\cdot\tilde{H} - \tilde{H}\cdot\tilde{A}$ |
| $A\cdot H$ = nonassociative Lie-admissible | $\tilde{A}\cdot\tilde{H}$ = nonassociative Lie-admissible, e.g. = $\tilde{A}\tilde{R}\tilde{H} - \tilde{H}\tilde{S}\tilde{R}$; $\tilde{R} \neq \pm\tilde{S}$. |

These are precisely the equations proposed in the Lie-admissible literature (in the HADRONIC JOURNAL) since 1978. They have remained largely ignored until now. They emerge now rather forcefully, because no alternative is known at this time to attempt consistent quantization.

The physical implications of this situation are substantial. The workshop succeeded in identifying the most relevant ones because we succeeded in putting toghether mathematicians and physicists. In essence, the replacement of the conventional associative envelope of quantum mechanics with a nonassociative one implies the generalization of several crucial laws and principles, beginning with the generalization of the notion of spin.

The mathematical implications are equally intriguing. In my view, the following branches of mathematics

A- the theory of Lie algebras as conventionally studied by mathematicians until now, that is, in complyance with the Poincaré-Birkhoff-Witt theorem (associative envelope);

B- the symplectic geometry in canonical realization; and

C- the conventional theory of Hilbert spaces (that as one-sided modules)

are inapplicable for a consistent quantization, and should be replaced by

A' the theory of Lie algebra as the attached algebras of nonassociative envelopes (this is the key idea of Lie-admissible formulations);

B' the theory of "symplectic-admissible manifolds", to achieve a geometrization of the nonassociative Lie-admissible envelopes; and

C' the two-sided modular extension of the Hilbert space theory (nonassociative envelopes do not admit linear, one sided, representations on modules).

The energy-related implications are equally intriguing. Nonlinear effects in the controlled fusion are simply a reality. It was a general consensus of the experts that the current techniques (based on an associative envelope) must be replaced, in due time, with Lie-admissible (nonassociative) techniques.

Part A of the Proceedings identifies the state of the art in these topics at the beginning of the workshop. My contribution is a memoir of some 550 pages with the review of some 300 theorems, lemmas, etc. (without even touching the proof).

Part B of the Proceedings consists of the research papers along these lines. Most of them are of mathematical character (see the construction of the Lie-admissible algebras via a deformation of the Lie algebra by the Russians). By specific, planned, intent, the energy related aspect has been only touched in my paper with Fronteau and Tellez-Arenas of France. We are contemplating to have additional contributions by experts in the subsequent workshop with a progressive entrance into the topic.

Incindentally, generalizations A', B' and C' are some of the technical reasons why I propose to you the organization of a Center for Applied Mathematics (or other possible titles). In fact, in my view, they are so complex, intriguing, and valuable, to warrant the consideration of a center for coordinated study. These ideas were fully available at the time of my proposal, although I could not express them technically. The proceedings express these ideas in all needed details. Most intriguingly, it appears that the studies considered are unavoidable.

Please keep in mind this possibility. Whenever you consider appropriate or advisable to try again the organization of a Center for Applied Mathematics at Harvard, let me know. If, on my part, I shall organize this center at my new Institution next year, I shall let you know.

With my most sincere HAPPY NEW YEAR to you and your family, I remain

yours Pupre

P.S. The proceedings will be available within three weeks. The demand is quite great. Please let me know as soon as possible in case you desire a complimentary copy.

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RUGGERO MARIO SANTILLI
SCIENCE CENTER, ROOM 4-35
ONE OXFORD STREET
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January 8, 1980

Dr. G.B.Field, Director
Center for Astrophysics
Harvard University

University Mail

Dear Dr. Field,

To confirm our phone conversation, the DOE is interested in renewing my grant under the administration of the Hadronic Press, a Massachusetts Corporation producing the Hadronic Journal. Please feel free to contact Dr. D.C.Peaslee at the DOE, tel. 301 353 3624. Of course, this renewal is intended as an intermediate step of the current process of grant relocation.

In essence, I am currently considered by several institutions. One of particular interest is the Lawrence Berkeley Laboratory, where I have applied to Dr. R.W.Birge, Director of the nuclear division. Nevertheless, my current grant expires on June 1, 1980, while it appears advisable to avoid pressures for an early decision on rather intriguing but delicate topics. The intermediary solution of letting a Massachusetts non-academic corporation to administer my grant, has therefore emerged quite natural.

To facilitate a smooth transition in this predictably delicate grant relocation, I have asked you the courtesy of considering me for a possible guest status. In essence, I would need a letter, to be enclosed in the grant application, stating that

(1) I can be your guest for the academic year 1980/1981; and
(2) I can use the library and parking facilities.

In case you consider it appropriate, please feel free to consider the addition that my guest status can be terminated prior to the end of the academic year for administrative and other possible reasons.

A desk or office either at your Center or here at the Science Center would be welcome, but it is not essential, and I see no reason to mention it in the letter, although I would like to rely in your judgment.

Also, in case you consider it advisable, I can continue to use this stationary indicating my affiliation with the "Science Center" rather than with your Center. As indicated to you by phone, at the time of my appointment at the Department of Mathematics I decided to use this stationary in all my correspondence, including that of editorial character, as a form of my appreciation for the hospitality received.

On scientific grounds it is a truism to say that this is an intriguing and promising moment. I have just released for publication the Proceedings of the Second Workshop on Lie-admissible treatments of the strong interactions. It consists of two volumes (one of review, and one of research) for over 1500 pages by mathematicians and physicists from the USA, USSR, China, France, Switzerland, Australia, Israel, and Greece. It will be a pleasure to send you a complimentary copy as soon as available (within a few weeks). There are a number of astrophysical implications which will likely intrigue you (essentially related to the geometry for the interior problem which allows the representation of at least "simple" systems such as a satellite in Earth's atmosphere).

Whatever your final decision will be, permit me to express my appreciation and gratitude for your time and consideration.

Sincerely yours Ruggero Santilli

HARVARD UNIVERSITY

Area Code 617
495-3352

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

January 14, 1980

Dear Dr. Field,

I enclose some informative material on the series "Developments of the Quark Theory of Hadrons" organized by the Hadronic Journal, under the editorship of Don Lichtenberg and Peter Rosen.

It is the result of my considerable effort in funding the project, which lasted for several months. Happily, the project is now well under way.

This series of strict quark orientation is intended to complement the other series "Applications of Lie-admissible algebras in physics" (edited by Hyo Myung, Susumo Okubo and myself), which is of strict non-quark orientation.

The idea is to achieve a well balanced presentation of research on hadrons in which all valuable lines, whether of quark or non-quark orientation, are pursued, in the traditional, as well as most effective way of confronting open physical problems.

It is regrettable that our colleagues at Lyman do not apparently accept this scientific view.

Best and sincere regards

**Center for Astrophysics**
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

January 14, 1980

Dr. Ruggero M. Santilli
Science Center, Room 435
One Oxford Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli,

I am sorry that it will not be possible to fulfill the request made in your letter of January 8, 1980. There is a large unmet demand for our regular visitors programs, so it would not be fair to make exceptions for scientists who actually have no direct relation to our work in astrophysics.

Sincerely,

George Field

George B. Field
Director

GBF/ljc

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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January 15, 1980

Dr. H. HIRONAKA

Chairman
Department of Mathematics
Harvard University

UNIVERSITY MAIL

Dear Dr. Hironaka,

As you know, my appointment expires on June 1, 1980. Please let me know whether I should free my office at that date, or I can remain through the summer, up to but not later than August 15, 1980.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
c.c.: Dr. R. Leahy,
Associate Dean

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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SCIENCE CENTER
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January 25, 1980

Dr. H. HIRONAKA
Chairman
Department of Mathematics
Harvard University

Dear Dr. Hironaka,

Your secretary has indicated that I should outline my activities in regard to my note of January 15, 1980 (copy enclosed).

You are familiar with my editorial/research activities. During this summer I shall continue my function as editor of the Hadronic Journal for papers in pure mathematics and theoretical physics, and I shall work at my Volume II with Springer-Verlag, which, as you know, is a monograph in applied mathematics. A copy of an independent review of my Volume I is enclosed. I do not expect to have the time to write papers. On the last two-three weeks of August I shall take my vacations, and on September 1 I shall assume my new job outside Boston (for several personal reasons I cannot leave the Boston area this summer).

The primary reason for my note of January 15, 1980, is whether or not I shall continue to have my address "Science Center, Harvard University" in the cover of the Hadronic Journal during this summer (copy enclosed). There will be only two issues this summer, those of June and of August 1980.

Since the cover of the Journal is printed well in advance, I would appreciate an early decision on this matter. Also, I would gratefully appreciate a written communication of your decision.

Very Truly Yours

Ruggero Maria Santilli
Research associate

RMS/ml
encls.
cc.: AssociaTe DeanR.Leahy

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February 12, 1980

Professor Ruggero Santilli
Department of Mathematics
Harvard University

Dear Ruggero:

In the Departmental Meeting of February 1, it was discussed and voted once again to confirm that your appointment with the Mathematics Department is to be terminated as was previously planned and conveyed to you last year. It is our general policy that visiting positions should not be be continued too long so as to have constant flow of new research projects and stimulations within the Department.

By that departmental decision, your formal association with Harvard, Math Department or Science Center, will end at the end of May, 1980, and you have no right to use a Harvard office address in any official document. As to your moving out of you current office in the Math Department, we understand that you may find it difficult to move out immediately in June and wish to do it rather gradually (with reasonable effort to an early evacuation, of course).

Sincerely yours,

Heisuke Hironaka
Chairman

HH/mjm

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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

Ruggero Maria Santilli

RMS/ml
ecls.

c.c. Ass. Dean Leahy.

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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

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

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.

January 8, 1980 PLEASE POST

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

FROM: R. M. Santilli, Editor of the Hadronic Journal. Current address: Harvard University, Department of Mathematics, Cambridge, Ma. 02138 USA

SUBJECT: Theoretical and experimental papers on quarks and other topics which activate the theorems of inconsistency of Heisenberg's equations.

Dear Colleagues,

Recent studies by mathematicians and physicists have proved two theorems establishing that Heisenberg's equations are generally inconsistent, both intrinsically and with respect to the correspondence principle. A number of current applications of quantum mechanics activate the inconsistency theorems in a rather direct way. This poses the problem of the editorial processing of papers of this type. With this letter I would like to: fulfill my duty as regards rapid dissemination of the information; recommend a study of the problem; and solicite the possible achievement of a joint resolution. The technical aspects underlying this letter have been discussed in the informal sessions following the Second Workshop on Lie-admissible Formulations (held here at Harvard from August 1 to 8, 1979), and are reported in the Proceedings[1, 2]. To assume complete responsibility, I shall often quote my review[3] in the first volume of the Proceedings[1] and refer to it with the notation (3, pp. . .). The understanding is that the study of the original contributions, as well as of the complete Proceedings[1, 2] is essential for technical knowledge of the problem.

## THE THEOREMS OF INCONSISTENCY OF HEISENBERG'S EQUATIONS.

In their recent treatise[4], the known mathematicians Abraham and Marsden have proved a theorem within the context of the symplectic quantization essentially stating that **THERE EXISTS NO MAP** from the Poisson brackets of functions A, B, . . . in phase space (the cotangent bundle), to Heisenberg's product of Hermitian operators $\tilde{A}$, $\tilde{B}$, . . . on a Hilbert space ($\hbar = 1$).

$$[A,B]_{cl} = \frac{\partial A}{\partial r^k}\frac{\partial B}{\partial P_k} - \frac{\partial B}{\partial r^k}\frac{\partial A}{\partial P_k} \longrightarrow \frac{1}{i}[\tilde{A},\tilde{B}] = \frac{1}{i}(\tilde{A}\tilde{B} - \tilde{B}\tilde{A}) \tag{1}$$

Independently of these results, a number of physicists have proved a theorem via the theory of nonassociative algebras essentially stating that **HEISENBERG'S EQUATIONS ARE INCONSISTENT** for all Hamiltonians of at least polynomial order three in the canonical operators $\tilde{r}$ and $\tilde{p}$. Some of the inconsistencies are: (a) the Lie algebra product of Heisenberg's time evolution $(\tilde{A}\tilde{H} - \tilde{H}\tilde{A})|>$ is intrinsically inconsistent; (b) there is a violation of the equivalence with the (operator form of) Lagrange's equations; and (c) Heisenberg's equations in the Hamiltonian $\tilde{H}$ violate the correspondence principle to Hamilton's equations in the classical limit H of $\tilde{H}$. For a review of the inconsistency theorems, as well as of a number of additional lemmas and propositions I have omitted here for brevity, one may consult (3, pp. 1977-1782).

The implications of the theorems are rather delicate. First of all, the physical calculations are inconsistent (because the value of the product $(\tilde{A}\tilde{H} - \tilde{H}\tilde{A})|>$ is not unique, but depends on the selected application of the differential rule). Secondly, the theorems prevent a rigorous proof of the validity of conventional quantum mechanical laws, such as Heisenberg's indeterminacy principle, Pauli's exclusion principle, Einstein's frequency principle, etc. Finally, the lack of a consistent time evolution law prevents the achievement of a consistent formulation of Galilei's or Einstein's special relativity. For a review, the reader may consult (3, Section 2.3, pp. 1777-1905).

The technical identification of systems escaping the theorems is under study. It appears that all linear Newtonian systems (e.g., the familiar harmonic oscillator $m\ddot{r} + kr = 0$) can be consistently quantized via Heisenberg's equations because they admit **quadratic** Hamiltonians (e.g., $H = \frac{1}{2}mp^2 + \frac{1}{2}kr^2$). Also (under certain technical conditions still under study), the structure of atoms and the (nonrelativistic) electromagnetic interactions can be consistently quantized via Heisenberg's equations. As a result, the validity of conventional quantum mechanical laws for the atomic structure and the (long range) electromagnetic interactions in general, is unaffected.

The identification of systems activating the theorems is, in general, rather simple. For instance, **nonlinear** Newtonian systems (e.g., the anharmonic oscillator) cannot be in general consistently quantized because the Hamiltonian (when it exists) is of polynomial order higher than two (e.g., $H = \frac{1}{2}mp^2 + \frac{1}{2}kr^2 + \lambda r^4$). These **nonlinear effects** occur in a number of trends of contemporary research, although they appear to be more frequent for the strong interactions (see below).

The reasons for the inconsistency of Heisenberg's equations and related physical laws are intriguing. A rather important reason has been identified via the theory of nonassociative algebras. The envelope of the Poisson brackets $[A,H]_{cl}$ is a **NONASSOCIATIVE LIE-ADMISSIBLE ALGEBRA** with product $A \cdot H = (\partial A/\partial r^k)(\partial H/\partial P_k)$, while the envelope of Heisenberg's product $[\tilde{A}, \tilde{H}]$ is an **ASSOCIATIVE LIE-ADMISSIBLE ALGEBRA** with product $\tilde{A}\tilde{H}$[5]. Thus, the quantization

$$\begin{array}{c}\textbf{Hamilton's equations}\\ \left[\begin{array}{l}\dot{A} = [A, H]_{cl} = A \cdot H - H \cdot A\\ A \cdot H = \text{nonassociative}\end{array}\right]\end{array} \longrightarrow \begin{array}{c}\textbf{Heisenberg's equations}\\ \left[\begin{array}{l}\dot{\tilde{A}} = \frac{1}{i}[\tilde{A},\tilde{H}] = \frac{1}{i}(\tilde{A}\tilde{H} - \tilde{H}\tilde{A})\\ \tilde{A}\tilde{H} = \text{associative}\end{array}\right]\end{array} \tag{2}$$

despite its use for over half a century, is **INCONSISTENT** because it violates the character of the enveloping algebra (3, pp. 1787-1792).

A quantization currently under study to bypass the inconsistency theorems under nonlinear effects is given by (3, pp. 1800-1819)

$$\begin{array}{c}\textbf{Hamilton's equations}\\ \left[\begin{array}{l}\dot{A} = [A,H]_{cl} = A \cdot H - H \cdot A = \text{Lie product}\\ A \cdot H = \text{general (nonassociative)}\\ \qquad\text{Lie-admissible algebra}\end{array}\right]\end{array} \longrightarrow \begin{array}{c}\textbf{Quantum mechanical equations}\\ \left[\begin{array}{l}\dot{\tilde{A}} = \frac{1}{i}[\tilde{A},\tilde{H}] = \frac{1}{i}(\tilde{A} \cdot \tilde{H} - \tilde{H} \cdot \tilde{A}) = \text{Lie product}\\ \tilde{A} \cdot \tilde{H} = \text{general (nonassociative)}\\ \qquad\text{Lie-admissible algebra}\end{array}\right]\end{array} \tag{3}$$

where, e.g., $\tilde{A} \cdot \tilde{H} = \tilde{A}\tilde{R}\tilde{H} - \tilde{H}\tilde{S}\tilde{A}$, $\tilde{R} \neq \pm\tilde{S}$ and fixed, according to Santilli's proposal[6] which calls for the intermediary use of the Birkhoffian generalization of the Hamiltonian mechanics[7]. Quantization (3) at the level of the equation of motion is complemented by a quantization at the level of the enveloping (nonassociative) algebra proposed by Ktorides[8] (and extended to quantum field theory by the same author). More recently, quantization (3) for the flexible particularization of the general Lie-admissible algebras (e.g., $\tilde{A} \cdot \tilde{H} = \lambda\tilde{A}\tilde{H} - \mu\tilde{H}\tilde{A}$) has been worked out by Okubo[9].

The main idea of quantization (3) is that of generalizing the conventional associative envelope of Heisenberg's equations into a nonassociative form, as a necessary condition for preserving the algebraic structure of Hamilton's equations[3]. The implications of this generalization are nontrivial, mathematically and physically. Mathematically, quantization (3) calls for (i) the reformulation of Lie's theory, from its currently available version (as the attached algebra of an associative envelope according to the Poincaré-Birkhoff-Witt theorem) to a more general form (as the attached algebras of nonassociative envelopes according to Ktorides' generalization[10] of the Poincaré-Birkhoff-Witt theorem); (ii) the generalization of the symplectic geometry into a covering form capable of geometrizing non-associative Lie-admissible algebras (tentatively called symplectic-admissible geometry[11]); and (iii) the generalization of the conventional, one-sided, Hilbert space theory to a genuine, two-sided (left and right), bimodular form[12]. These and other problems were studied at the Second Workshop on Lie-admissible Formulations[1,2]. Physically, quantization (3) implies the need for a suitable generalization of the relativities, principles and insights of quantum mechanics (sometimes called "atomic mechanics"). A rather feverish study is now under way by a number of researchers for a possible covering mechanics (sometimes called "hadronic mechanics"), and I refer the interested colleague to (3, Part 2, pages 1682-1987).

Oddly, this generalization of quantum mechanics does not appear to interest the contemporary physics community at large. Yet, the generalization was advocated or expected by the Founding Fathers of contemporary physics. For example, the nonassociative generalization of the envelope of quantum mechanics was proposed by Jordan, von Neumann, and Wigner; the inapplicability of conventional geometries within a strongly interacting particle was predicted by Fermi; the lack of final character of Heisenberg's uncertainty was vigorously advocated by Einstein; etc. For these and other historical, authoritative, open legacies, one may consult (3, pp. 1700-1718; and 1865-1889).

## PAPERS IN NUCLEAR PHYSICS, CONTROLLED FUSION, QUANTUM GRAVITATION, AND OTHER FIELDS.

Polynomial Hamiltonians of order higher than two (representing nonlinear systems) are not uncommon in the fields considered. All these Hamiltonians activate in a rather direct way the inconsistency theorems, by therefore posing the problem of the editorial processing of papers submitted to our Journals. Needless to say, there exist numerous approaches that are expected to avoid the inconsistency theorems. This is the case, for example, with the non-conservative formulation of statistical mechanics by Prigogine and his collaborators[13], or the approach to the exterior problem of gravitation by Yilmaz[14].

PAPERS ON QUARKS.

Nonrelativistic quark models activate directly all theorems, lemmas, and propositions invalidating Heisenberg's time evolution law. This is due to a number of reasons, such as the fine structure terms $H_{fs}(r,p,s,M,\ldots)$ which are added to conventional Hamiltonians $H = T(p) + V(r)$ to attempt mass spectra. Again, what is at stake is not only the consistency of the calculations, but more insidiously the validity of conventional laws, and, thus, the consistent, quantitative definition of quarks as physical particles (3, Section 2.4).

Irrespective of this, a number of additional inconsistencies of these models have recently come to light, such as the fact that the models violate the conservation laws of the total, physical, angular momentum, the correspondence principle; etc. (3, pp. 1928-1943).

Also, it appears appropriate to recall a rather increasing uneasiness in one segment of our community in regard to the problem of confinement[15]. A growing consensus is that, since the spontaneous decays

$$\text{mesons} \longrightarrow q + \bar{q} + \ldots; \qquad \text{barions} \longrightarrow q + q + q + \ldots$$

do not exist according to available knowledge, papers on quarks should present explicit, detailed, and consistent calculations on the fractions of the spontaneous decays of hadrons into free quarks. If these fractions are not identically null, or, at least sufficiently small, the models are inconsistent[16].

Needless to say, the inconsistency theorems in their currently available (discrete) form do not affect quantum chromodynamics. Yet, their impact should not be underestimated. The theorems leave QCD without a consistent nonrelativistic limit. Also, their expected field theoretical extensions might well affect QCD directly (because all gauge, unitary, and relativity algebras of current use in QCD are based on associative envelopes[17]).

I should add that the inconsistency theorems *do not* affect unitary models in their original conception, that of providing a *classification* of hadrons. In fact, the theorems are activated only when quarks are assumed as physical particles, thus demanding specific dynamical equations and specific physical laws. In different terms, the inconsistency theorems are activated when, as implicit in current trends, one attempts the representation of the totality of the hadronic phenomenology (classification, structure, and scattering) via one single model, the quark model.

As a result of this occurrence, the inconsistency theorems are stimulating, via mathematical arguments, the consideration of a proposal of the Lie-admissible literature[18] which is rather natural on physical grounds. As it occurred for the atoms, it may well be that the hadronic phenomenology demands different yet compatible models: the established unitary models for the **Mendeleev-type classification** of hadrons into multiplets, and a different, **Bohr-type model of structure** for each individual element of a unitary multiplet.

EXPERIMENTAL PAPERS IN STRONG INTERACTIONS.

As we know well, all measurements (and, thus, all experiments) are approximate. The approximate character of experiments in strong interactions is compounded by the fact that a number of theoretical assumptions are made in the data elaboration (3, pp. 1697-1699). For instance, the experimenter may use a cross section as derived from the "potential scattering theory". The underlying assumption is that the strong interactions are derivable from a potential. But this assumption is rather controversial. In fact, the idea that the strong interactions are nonderivable from a potential (as a local approximation of nonlocal settings) is rather old, and dates back to the very first studies on strong interactions. As a result, we simply do not know at this time whether cross sections of current use by experimenters in strong interactions are correct or erroneous. Similarly, the experimenter often uses in data elaboration (either in a direct or in an indirect way) Einstein's special relativity, the spin-statistics theorem, and other physical laws whose validity for the strong interactions is also controversial. In fact, a rather intensive effort is being made to study quantitatively the possible invalidation of conventional physical laws for the strong interactions, because they expectedly imply point-like approximations of particles interacting at distances smaller than their size.

Owing to this situation, it appears advisable that experimental papers in strong interactions present the identification in as detailed a way as possible (e.g., via a list) of *all* the theoretical tools, conjectures, and assumptions used in the data elaboration. In this way the individual reader can reach his personal assessment on the approximate character of the results; the interested researcher can attempt the elaboration of the same data via different theoretical tools; and the editor can ascertain whether or not some of the theoretical tools used in the data elaboration activate the inconsistency theorems.

Needless to say, what is much needed is to resolve experimentally whether or not the inconsistency theorems are activated by the strong interactions, that is, whether the conventional laws of the electromagnetic interactions are valid or invalid for the strong. A number of proposals of specific tests are available (3, Section 2.5), and several others are conceivable, all apparently feasible with available technology. For instance, I am told that the proposal

of ref.[6,18] to establish whether Pauli's exclusion principle is valid in nuclear physics or small deviations are detectable (that is, whether the SU(2)-spin symmetry is exact or broken and, thus, whether the Lorentz symmetry is exact or only approximate under strong interactions) can be tested via suitable implementations of the current experiments of neutron beams on crystals[19].

## CONCLUDING REMARKS.

Basic research appears to be in an intriguing, but delicate phase, with a continuation of brilliant achievements, together with a proliferation of problematic aspects, particularly in the case of the strong interactions.

At present, we are apparently performing our editorial function independently, to the best of our capabilities. Nevertheless, this has created some disparities. For instance, some of us have implemented a moratorium on all papers on quark models and other fields which activate the inconsistency theorems. Others, perhaps unaware of these theorems, continue to accept papers which can be rigorously proved as being inconsistent.

Owing to this situation, I submit that we should conduct a coordinated study of the problem, and possibly reach a joint resolution. The most effective way to achieve the objective would be, in my view, the organization of a meeting which could be attended by all interested editors. The participation of researchers with a *technical* knowledge of the problem would, of course, be welcomed. Whether a formal meeting will be organized or not, you are welcome to attend our Third Workshop on Lie-admissible Formulations (tentatively scheduled here in Cambridge during the week of August 4 to 9, 1980), for which we are attempting to gather a number of mathematicians and physicists who are experts in the field.

If I can be of any assistance, please do not hesitate to contact me.

Very Truly Yours,

Ruggero Maria Santilli

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[redacted]

Department of Mathematics [redacted]

Feb 6, 1980

Dear Prof. Santilli

Thanks for your invitation to attend the 3rd Workshop on Lie-admissible formulations. Unfortunately I am already committed for this August; I otherwise would have been delighted to attend.

Thanks also for the information on the use of our theorem in Foundations of Mechanics. I'd like to remain informed.

I'd be happy to be a corresponding participant in your conference in whatever capacity you wish.

Sincerely

[redacted]

P.S. Can I suggest inviting Paul →

[redacted] of our department to your meeting. The result in F of M you mentioned was obtained in collaboration with him (see bottom of p. 434).

GM

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February 10, 1980

Professor J. MARSDEN
Department of Mathematics
University of California

Dear Professor Marsden,

I would like to express the sentiments of my appreciation for your kind letter of February 6, and for your interest. I am particularly grateful for your desire to be of assistance to us as a corresponding participant to our THIRD WORKSHOP ON LIE-ADMISSIBILITY. I believe that this would be equally invaluable.

I would like therefore take the liberty of suggesting that you prepare some informal notes (even hand written) on the problem and you mail them to me. I shall have copies of them distributed to the participants (only of our workshop). In case you have the time to subsequently finalize the notes and bring them up to the form of a paper, I would be simply pleased to publish them in the Proceedings of the Third Workshop.

For this purpose:
- the informal notes should reach me not later than August 1, 1980, and
- the formal paper should reach me not later than December 25, 1980.

What I am trying to express is that we would like to do our best so that your view is taken in due and full account at the workshop, particularly for the editorial, decision making, process.

The editorial problem is essentially the following. Recall the following occurrences:

(1) Lack of uniqueness of the value of Heisenberg's time evolution. As you know well, $\overline{A\rangle} = (AH-HA)|\rangle$ does not have a unique value for all (hermitian) polynomial operators A and/or H in the canonical operators r and p of at least order three. Instead, it has a number of values depending on the selected use of the differential rule. This creates considerable physical problems (actually an array of problems). It appears that we have a form of indeterminacy o/o which, as such, cannot be removed via substraction-type techniques. Also, the selection of one use of the differential rule implies the lack of necessary equivalence of physical numbers computed via Heisenberg's and Schrödinger's equations. The distressing aspect is that we simply do not know a way out at this moment capable of preserving old doctrines (that is, the associative character of the envelope).

As a result of this situation, as an editor I simply do not know how to handle papers based on Heisenberg's equations, and activating this inconsistency. This lack of capability to reach a mature judgment is shared by other editors. At the third workshop we would like to relay primarily on the advice of mathematicians experts in the field, and hopefully reach a decision. The alternatives are:
- reject all papers of this type;
- accept them; or
- continue the moratorium until the air is cleared, e.g., by experimentalists.

(2) Inequivalence of Heisenberg's and Lagrange's equations.* This additional inconsistency is also due to a form of break-down of the differential rule, this time that of chain type. Thus, problems (1) and (2) are related. Yet, there are differences. For instance,

* Please see the simple calculations of pages 1779-1780 of the Proceedings of the Second Workshop, Volume A (I mailed you early).

page 2.

the unharmonic oscillator activates (1) but not (2). On grounds of our truly limited knowledge,we do not understand this difference. Most importantly, we do not understand why quantum mechanics works for the Coulomb potential (atomic structure) which might activate (1), say, in polynomial expansions, while it does not activate (2).By recalling that the electromagnetic interactions are linear in the velocity, does this situation imply that the intrinsinc inconsistencies are activated only by systems that are nonlinear in the velocities (that is, activating jointly (1) and (2))?

In any case, we do not know how to handle editorially papers activating the inequivalence between Heisenberg's and Lagrange's equations. We cannot assume one equation more fundamental than the other. For instance, the assumption of Heisenberg's equations as the fundamental ones and the elimination of Lagrange's equations (by fiat!) implies fundamental problems for quantum chromodynamics (which, as you know, is based on Lagrange's equations). The inverse aprioristic assumption is equally problematic. Lacking a solution, we are forced into the current moratorium( that is,neither the acceptance nor the rejection because of lack of technically mature, final information).

(3) The invalidation of the correspondence limit of Heisenberg's equations into Hamilton's form.* This is a sort of inverse formulation of your crucial theorem 5.4.9. In principle, this occurrence could also be eliminated by fiat in the sense that one may assume the lack of existence of a meaningful map to "construct a true theory from a false one" according to another editor in pnysics. The problem is created because of the following occurrence.

Again, to our understanding, Heisenberg's representation (when consistent...) and Schrödinger's representation are equivalent. However, despite a considerable search, no Schrödinger's "image" of Heisenberg's invalidation of the correspondence limit has emerged. In different terms, under all needed condition (e.g.,boundedness from below) Schrödinger's equation is consistent not only intrinsically, but also with respect to the correspondence principle for all Hamiltonians activating (1) and (2). One reaches nicely and smoothly the Hamilton-Jacoby equation in the classical limit of the Hamiltonian, while we cannot reach Hamilton's equations in the same Hamiltonian when starting from Heisenberg's equations (dichotomy infinite/finite systems?).

In different terms, all the difficulties (1), (2) and (3) are restricted (to our limited understanding at this moment) to Heisenberg's equations, that is, to the quantum mechanical formulations in terms of the realization of Lie algebras via the trivial product ab-ba and the symplectic geometry in the canonical form.

The point remains that we do not know at this moment how to handle editorially papers activating (3) and, more specifically, papers for which the correspondence principle is fully valid for Schrödinger's equations and fundamentally invalid for Heisenberg's equations.

Notice that these problems are expected to multiply in time.**In fact, one researcher is already searching for corresponding problems for the Feynman path integral formulation.

With respect to your possible informal notes and participation as a corresponding participant, ANY comment,advice,or council on how to handle the editorial situations (1), (2) and (3) would be gratefully appreciated. Most welcome would be CRITICAL views of these statements, in case erroneous. In different terms, we are not looking for approval of our views, doubts and concerns. We are looking for advice in a truly intriguing, but delicate scientific moment.

* Please see the simple proof of pages 1779-1780 of Part A of the Proceedings.

** The difficulties for the map of the unity might intrigue you (see pages 1481-1483 of Part A of the Proceedings).

Page 3.

An additional area in which advice and critical inspections would be sincerely welcome is related to the possibilities currently under study to attempt the bypassing of troubles (1), (2) and (3). For instance, several of us (me included) are working at generalizing

- the Lie algebra product from the trivial AH-HA into (what we call the) isotopically mapped form ACH -HCA, C = fixed;
- the symplectic form from the fundamental to a local and exact, but otherwise unrestricted form.

Apparently, this remouves all the computational troubles*. But the "price to pay" is the abandonment of quantum mechanical notion of algebraic origin, such as the notion of spin. In fact, the familiar value $J_iJ_i|>$ is now generalized into the form $J_iCJ_i|>$.

It would be invaluable for us to know whether we do have mathematical consistency in this generalized setting, that is , whether it is true or false that:

(1') the inconsistency (1) is eliminated, thus achieving unique value of the time-evolution law;

(2') the inconsistency (2) is also eliminated, thus avoiding conflict with Lagrangian formulations of high energy physics; and

(3') the inconsistency (3) is also eliminated, by therefore achieving consistency of the correspondence principle in its algebraic/geometric and wave/Schrödinger formulations.

Needless to say, we are truly exited at these possibilities, and a feverish activity is going on. I am leaving next week for a trip to Europe (to deliver invited presentations on the situation), and I contemplate to be back by mid March. It would be a pleasure for me to visit you and discuss in an informal manner the situation in more detail.

In closing, I would like to express my appreciation also for suggesting the invitation of Professor PAUL R. CHERNOFF. A formal invitation is enclosed and it would be a pleasure for us to have him at our workshop. To facilitate communication, I am taking the liberty of sending him copy of this letter. Needless to say, any advice, criticism, suggestion that Professor Chernoff might have would be sincerely welcome.

Incidentally, the second workshop was conducted in a truly relaxed, informal atmosphere without speeches. We shall do our best to preserve this atmosphere for the third workshop. We essentially hope to have a few selected mathematicians, a few theoretical physicists,and a few editors, for a total number of say 20-23 participants (to avoid dispersal of energies).

The participations by mathematicians will be coordinates among experts in nonassociative algebras, experts in Lie's theory and experts in the symplectic geometry. As you can see from the Proceedings of the Second Workshop, Professors Tomber, Oehmke and Myung are the experts of our group in the theory of nonassociative algebras. Besides you, Professor Abraham and Professor Chernoff, we have not issued other invitations at this moment for experts in the symplectic/Lie quantization (Shlomo Sternberg is in Israel and I do not know whether MIT-or other-colleagues are interested). Any advice would be appreciated (I was personally considering Jedrzej Sniatycki, subject to the approval of the other members of our group). As far as the participations of theoretical physicists are concerned we give priority to those acknowledging the importance of mathematical rigour.

In closing, I would appreciate whether you could share with Professor Chernoff the two volumes of the Proceedings of the Second Workshop. I do not have another complimentary copy at this moment.

Sincerely

Ruggero Maria Santilli

* please see pages 1800-1814 of Part A of the Proceedings.

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SCHOOL OF PHYSICS

February 7, 1980

Professor R. M. Santilli
Department of Mathematics
Harvard University
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Thank you for your letter of January 8, 1980, on the question of the consistency of quantum theories. I am sorry I cannot entirely agree with your conclusions.

It is my understanding that what you are challenging is actually not the consistency of, say, the quantum theory of the quartic anharmonic oscillator, but of the prescription called canonical quantization for obtaining this theory. Let me know, please, if I mistake your thought.

Since I have considered canonical quantization part of the historical development, not a formal part of the theory, I am not surprised by the result and do not consider it a serious criticism. I don't understand why there should be a prescription for deriving new true theories from old false ones.

At the same time, I agree with much of your conclusion. I do not believe any of the field theories we publish, no matter what their origin, on the grounds of their unphysical assumptions about the small-scale structure of time space. In this region nothing like canonical quantization seems physically correct to me even in cases where it is, or can be made, mathematically consistent.

Forgive this note of discord, but I thought you would prefer even a slightly disputatious response to total silence.

Yours sincerely,

David

David Finkelstein
Director

DF:ah

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February 11, 1980

Professor DAVID FINKELSTEIN
Editor
International Journal of Theoretical Physics
Georgia Institute of Technology
ATLANTA Georgia 30332

Dear David,

I would like to express my sincere appreciation for the courtesy of your letter of February 7, 1980. I am particularly grateful for the critical content of your letter. In fact, I am seeking advice, not approval.

Perhaps my letter to colleagues-editors of January 8, 1930 is not completely clear. In fact, I am not challenging quantum mechanics, first of all, because I am not the author of the theorems quoted in the letter and, secondly, because I believe the problem is bigger than the capability of one isolated scolar.

In essence, I have been intrigued by the theorems because they constitute, in my view, such a new problem, to call for the attention of editors. Permit me to indicate a few points in more detail. I am taking the liberty of enclosing a xerox copy of my review (regrettably, I do not have copies of the complete work), as well as of the pages from Abraham and Marsden you certainly have.

As you can see, Heisenberg's time evolution law $\dot{A}|\rangle = (AH-HA)|\rangle$ is intrinsically "inconsistent" in the sense that it can take different values, depending on the selected use of the differential rule.

Permit me the liberty of confessing that, as an editor, I do not know how to handle papers activating this occurrence (whenever A and/or H are of polynomial order in r and p of at least order three). In fact, it is not a case of infinities, as in quantum electrodynamics. Instead, it is a case much along the indeterminacy o/o. Its removal by subtration would be a sort of mumbo jumbo hand waving merely intended to preserve old doctrines beyond the limits of rationale. The selection of only one use of the differential rule is also mumbo-jambo in my view because, e.g., the numbers in Heisenberg's and Schrödinger's representation would not necessarily agree. And so on. Should I accept or reject a paper activating this non-sense?

In addition, I do not know how to handle the inequivalence between Lagrange's and Heisenberg's equations. I have heard here numerous mumbo jumbos beyond the imagination. One fellows said that we should assume Heisenberg's equations as the true one. But then QCD is dead. Another fellow said the opposite. But then QM is dead. Again, I do not honestly know how to handle this point as an editor. Should I accept or reject?

Another clearly proved inconsistency is the correspondence limit to Hamilton (see the enclosed simple proof). Additional mumbo jumbos reached my ears in this respect. One fellow said that QM should not admit a classical limit. But then the QM anharmonic oscillator should not be called anharmonic oscillator. Also, Schrödinger's equation verifies beautifully (the selfconsistency as well as) the classical limit (all the difficulties are in the use of the Lie algebra with the trivial product ab-ba and the symplectic geometry in canonical realization).

Page 2.

Dear David, I hope you can see that the problems are more serious of what a first look might indicate, and, more insidiously, I do not know of any way out which preserves old doctrines.

At the third workshop we shall gather a restricted number of mathematicians and physicists (max 20-25) to look at this situation without any preconceived idea. During the second workshop we truly enjoyed ourselves (it was a relaxed, completely informal, friendly meeting without speeches). We shall do our best to preserve this atmosphere for the third. In case you can attend, you would be sincerely welcomed.

Needless to say, the problem is created by the familiar (to both of us) desire by physicists to preserve old doctrines as much as possible, or to react with skepticisms prior to a technical study of the literature. If QM is abandoned for nonlinear systems (read, in my personal view, strong interactions), and a covering approach specifically conceived for the situation at hand is constructed, we believe that the Lie-admissible formulations permit to bypass all the inconsistencies. But this is one aspect in which I am not expecting we can reach a general agreement. Regrettably, I do not have a complimentary copy of the Proceedings of the Second Workshop devoted to this latter aspect (two volumes for over 1,500 pages). The request was quite substantial, and all complimentary copies were committed months before their appearance.

I can however, mail to you my personal copy, with the understanding that you will return it to me at your convenience after a few weeks (you would be free to make xerox copies). In case you are interested in looking at the efforts of generalizing old stuff, please let me know.

Again, I would like to encourage you to express critical views, rather than approval of my own view. It is only in this way that we can reach a matury, joint, judgment, which is the primary objective of my letter of January 8, 1980. On one thing I hope we do not disagree: the problems are real indeed.

Sincerely

Ruggero Maria Santilli
Editor
Hadronic Journal

RMS/ml encls.

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February 11, 1980

Professor D. FINKELSTEIN
Georgia Institute of Technology

Dear David,

I read again your letter of February 7, and my aswer to you of this morning. Perhaps my asnwer was only partial to your sound question. The canonical quantization is only part of our concern. As a mattero of fact, it is the minor reason of concern. On the contrary, the primary problematic aspects are due to the intrinsic, quantum mechanical, inconsistencies of Heisenberg's time evolution, as indicated in my letter of this morning and in its enclosures. We are also interested in the canonical quantization for a number of reasons, the most intriguing (for us) being the fact that it identifies a possible reason for the problematic aspects (the lack of preservation of the nonassociative character of the envelope of the Poisson brackets), by therefore setting the way for generalized formulations which are capable of bypassing all the inconsistencies, including quantization.

I am in full agreement with you that there should not be necessarily prescriptions for the construction of new, true theories from falso ones. As a result, we are in full agreement on the main point of your letter, that troubles in canonical quantizations should not be reasons for major concerns when considered per se.

The reasons for concern are due to the fact that the problems with canonical quantization are only part of a recently emerged array of problems of consistency in the intrinsic, quantum mechanical, setting.

Also, problems of canonical quantization becomes problems of consistency because the same problems, to our (rather limited understanding at this moment) are absent in the allegedly equivalent Schrödinger's representation (that is, as indicated in my letter of this morning , no inconsistency has emerged in the correspondence limit of Schrödinger's equation).

Again, please accept the sentiments of my appreciation for your kind letter of February 7.

Sincerely

Ruggero

May 1, 1980

Dr. DEREK C. BOK, President, Harvard University

Dear Dr. Bok,

My research contract under DOE support expires on June 1, 1980. Before leaving your campus, I would like to express again my concern for numerous aspects of my case.

You will recall that the primary objective of my research, as well as of my editorship of the HADRONIC JOURNAL, is to study and promote the direct experimental verification of the validity or invalidity of conventional laws for the strong interactions. The need for such verification has been confirmed by numerous scientists of proved ethical standard, and it is due to the fact that conventional laws are expected to be assumed in the data elaboration of current experiments. These experiments, therefore, are not expected to test the assumptions, and new, direct experimental verifications are needed. At any rate, the mere existence of controversies in the issue is sufficient to warrant the direct experimental verifications, owing to the fundamental character of the problem.

As you will recall too, a number of senior Harvard physicists have expressed their personal view that these studies have "no physical value". These physicists are free to express their opinions. At the administrative level, however, the issue is different. In fact, Harvard is currently using several millions of dollars of taxpayer's money in research on strong interactions at the nuclear, hadronic, and astrophysical level. It is essential, in my view, that Harvard gives clear proof of a well balanced use of such large public funds. On the contrary, all available indications points toward the preference by Harvard administrators at this time of the personal opinions by senior physicist toward the termination of the study here of the problem of the basic laws.

In fact, you will recall the escalation of opposition here against my studies. This opposition initiated with initiatives by individual senior physicists, and it is now continuing with the apparent support of Harvard administrators. In particular, you should recall the proposal made by DOE to Dr. Hironaka of the Dept. of Math., to the effect that I should be considered for a guest status here during next year while we complete the transfer of my grant and research to another institution. After all, Harvard has created and is continuing to create considerable problems in the pursue of a knowledge which is fundamental for energy related issues, such as the controlled fusion. It is only fair to expect that Harvard will assit in their orderly solution.

But, quite frankly, the extreme of occurrences in my case appear to indicate an almost desire by Harvard administrator to prevent an orderly solution.

I feel obliged to bring to your attention a rather similar case. Recently, an internal controversy occurred at the CERN laboratories in Geneva, Switzerland. Individual permanent members expressed their personal opinions of rather doubtful inspiration. They are allowed to do so, and they are still there. Yet, Dr. Van Hove, director of CERN, had to resign. I doubt whether senior members here know the true reasons for this resignation.

As stressed to you a number of times, I have provided my sincere best efforts to be loyal to Harvard. This has meant for me to keep the utmost possible silence with outsiders, while informing Harvard administrators of the problems, and while resisting internal recommendation by apparently outraged senior members. Yet, the situation is now grossly out of my control due to the escalation of opposition here. I simply do not know what will happen when the study on the basic laws will be terminated here, and all the several millions of dollars of public funds will be restrictively administered along the academic views (and interests) of current senior members.

Owing to this situation I would like to ask for the possibility that I visit you at any time of your convenience. Besides the pleasure of meeting you, I would like the possibility of confirming verbally to you my sincere desire for an orderly solution.. I believe that our joint study of the situation and of possible solutions can only be beneficial to Harvard, as well as to both of us.

Very Truly Yours

Ruggero Maria Santilli

[redacted]

Tel 495 3352 (office)
[redacted]

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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
Chairman

HH/mjm

cc: Dean Richard G. Leahy

Enclosures

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

May 8, 1980

Dr. DEREK C. BOK, President
Harvard University

Dear Dr. Bok,

I am here respectfully applying to be your personal guest from June 1, 1980 until June 1, 1981. A guest status with any other member of the Harvard community designated by you would be equally appreciated.

Some of the understandings for this possible guest status are the following.

1. The guest status should be a gesture of generosity on the part of Harvard University to assist me in the transfer of my research to another university, but under no circumstance it is expected to be renewed or extended beyond June 1, 1981.

2. The guest status should be completely without compensation. In addition, I should pay out of my research funds all logistic expenses (such as xeroxcopies), in such a way that no direct or indirect expense should be encourred by Harvard University on my behalf.

3. The guest status should essentially allow me to use Harvard libraries and parking facilities, as well as serve as my scientific address in the form of this stationary or any other form recommended.

4. The use f an office anywhere on campus would be appreciated but it is not essential.

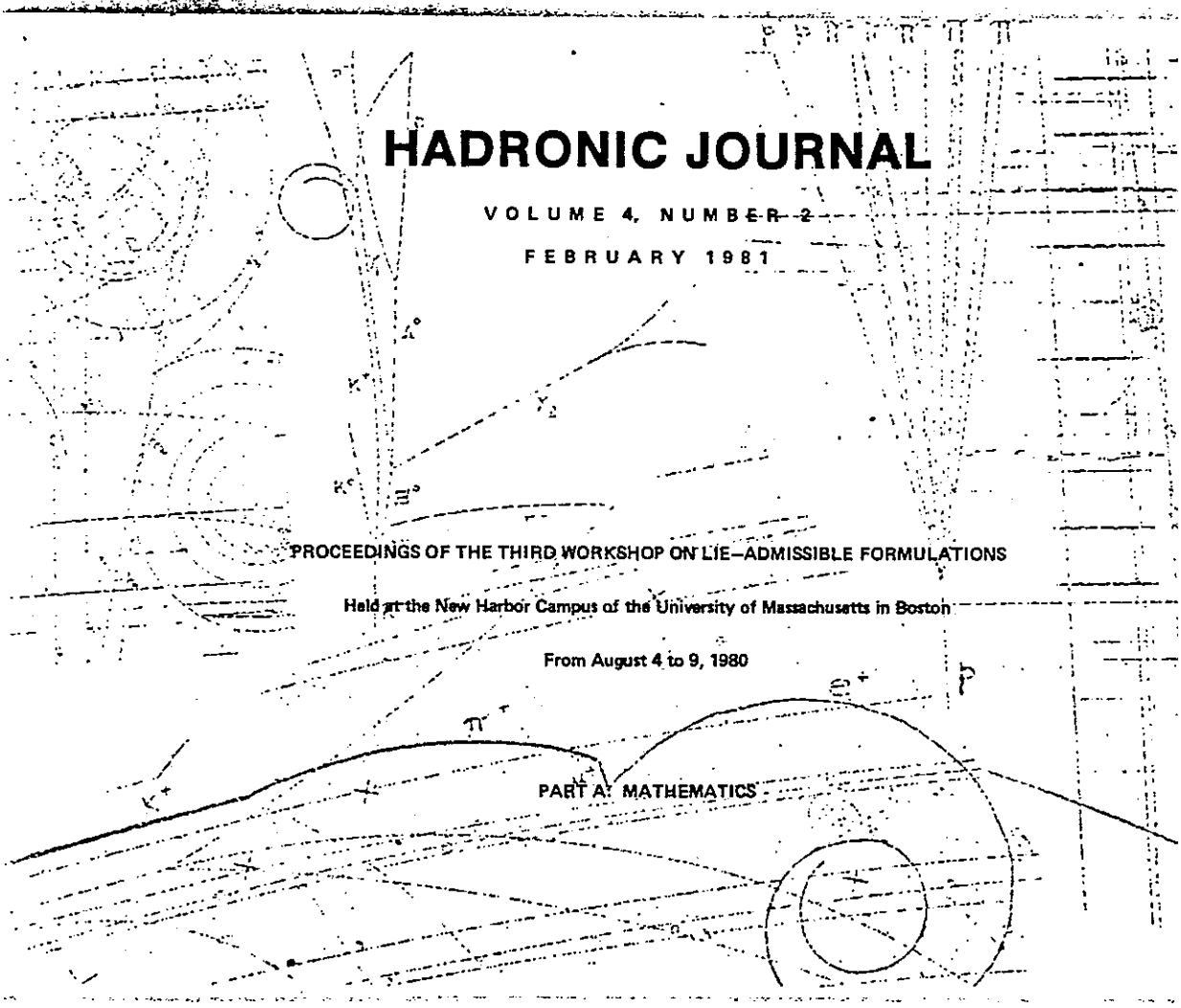
5. The guest status should allow the conduction at Harvard of the Third Workshop on Lie-admissible formulations tentatively scheduled from August 4 to 9, 1980 (but the Forth workshop scheduled for August 1981 should be conducted elsewhere). A list of selected, distinguished scientists interested to attend the Third workshop of this coming August is enclosed for your information.

I would consider it a personal courtesy whether a formal action on this application is reached before the expiration of my current research position here on May 31, 1980.

Very Truly Yours

Ruggero Maria Santilli

C.C. Dr. RICHARD G. LEAHY
Associate Dean



# HADRONIC JOURNAL

VOLUME 4, NUMBER 2

FEBRUARY 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 to 9, 1980

PART A: MATHEMATICS

# HADRONIC JOURNAL

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
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* Corresponding participants

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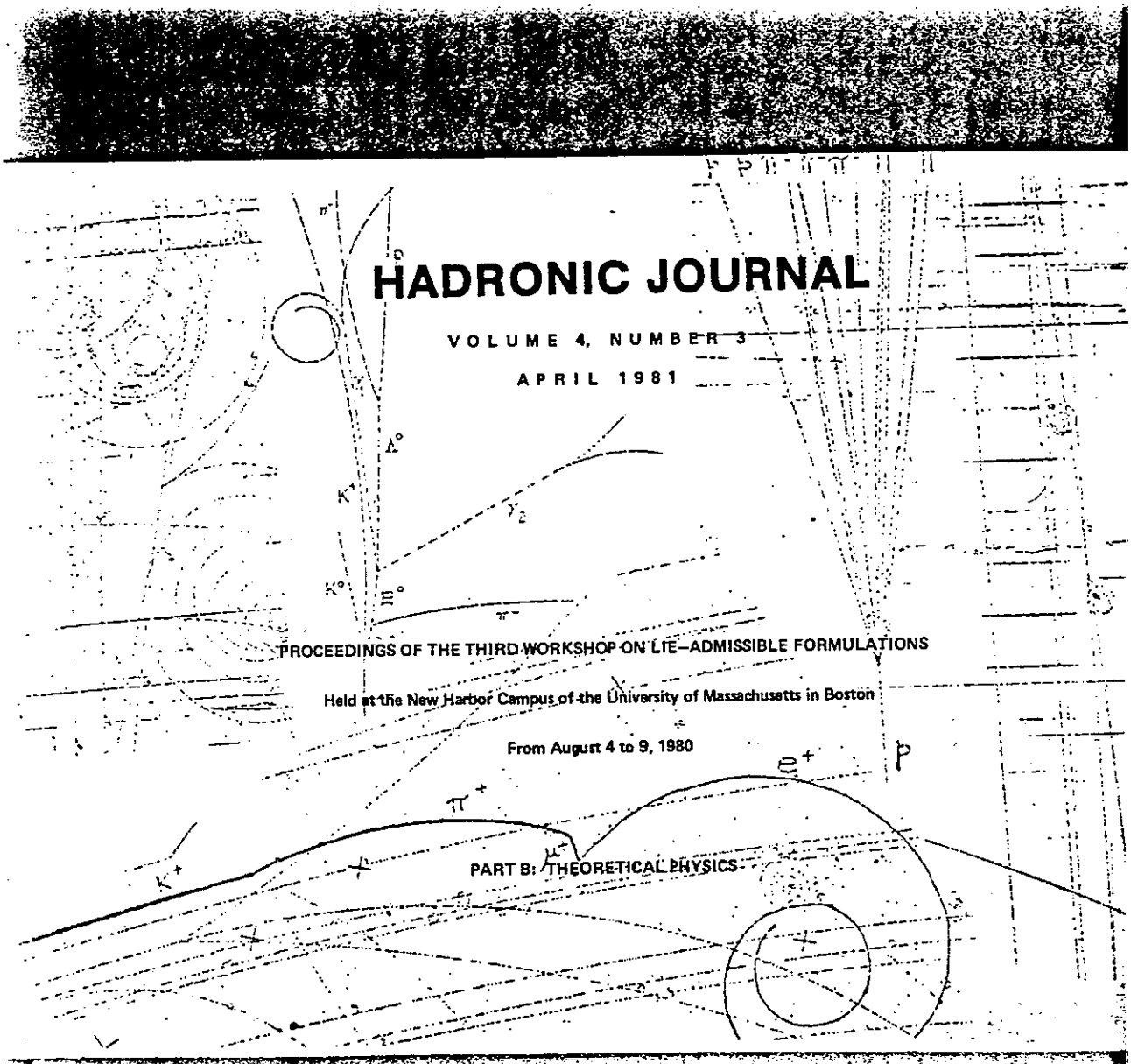

# HADRONIC JOURNAL

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PART B: THEORETICAL PHYSICS

# HADRONIC JOURNAL

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* Corresponding participants

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# HADRONIC JOURNAL

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Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-00185 Roma, Italy and
P. PELFER* and D. PROSPERI,* Universita degli Studi, Istituto di Fisica ed I.N.F.N., I-80138 Napoli, Italy and
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* Corresponding participants

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# PART II:

# TUFTS

# UNIVERSITY

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

January 9, 1980

Professor A.E.Everett
Department of Physics
Tufts University
Medford, Ma 02155

Dear Professor Everett,

I am currently seeking a tenured position in physics. In case your department is interested in my candidacy, I enclose my curriculum, a list of references, and some illustrative material on my recent teaching-research-editorial activities.

My salary is currently supported in full by my research grant with the Department of Energy. The main topic of study is to appeal to advanced mathematical and physical knowledge for the formulation of experiments on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic interactions. This expected long range support is at its second year, and it is possible that the funds will increase in time, depending on our progress toward achivment of maturity of formulation of experiments. The expected indirect, energy-related relevance of our studies is for the controlled fusion and other aspects.

The studies are now conducted by a group of distinguished mathematicians and physicists, besides myself, and they are coordinated via (a) a yearly workshop (now at its third year); (b) a yearly series of reprint volumes edited by Professors H.C.Myung, S. Okubo, and myself (Volumes 3, 4, 5 are in press); and (c) the Hadronic Journal of which I am the editor in chief.

The new mathematical tools stimulated by these studies have also seen applications in areas other than that of the basic laws of the strong interactions. For instance, the integrability conditions for the existence of a Lagrangian or a Hamiltonian I have presented in my volume "Foundations of Theoretical Mechanics", with Springer-Verlag, are now applied in space mechanics, plasma physics and engineering. Similarly, the Lie-admissible generalization of Lie's theory currently under way for (local and nonlocal) forces nonderivable from a potential, even though still at the beginning, has already seen applications to statistical mechanics and other areas.

Quite intriguing appear to be two theorems of invalidation of Heisenberg's equations for all Hamiltonians of polynomial order in the canonical operators r and p higher than two. These theorems (primarily studied by mathematicians and still largely unknown in physics circles) have rather predictable implications in several current lines of studies. Perhaps, these theorems may interest some of your physicists.

In case you are interested in a review of these new trends, please let me know, and I shall do my best to visit your Department at some time of mutual convenience. In the meantime, I remain at your disposal for additional information you might desire.

Best Personal Regards

Ruggero Maria Santilli

RMS|ml; encls.

TUFTS UNIVERSITY

Department of Physics

January 23, 1980

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

Thank you for your inquiry about the possibility of a position at Tufts. I regret to say that I do not foresee our being able to make a tenured appointment in the area of theoretical particle physics any time in the foreseeable future.

Sincerely yours,

Allen E. Everett

Allen E. Everett
Professor and Chairman
Department of Physics

AEE:rf

Medford, Massachusetts 02155
617 628-5000

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 5, 1980

Professor ALLEN E. EVERETT
Chairman
Department of Physics
TUFT UNIVERSITY
MEDFORD, Massachusetts 02155

Dear Professor Everett,

I would be interested in a position of unpaid visitor or guest at your Department for next academic year, possibly, beginning from June 1980. Besides expecting no salary, I can pay logistic expenses (xerox, etc.) from my own research funds, so that no financial expense is supported by your Department on my behalf. I do not expect participation to your faculty meetings, although I would be happy to provide services upon request, within the limitation of my time, including teaching.

As you eventually remember from our recent correspondence, I am fully supported by a grant from the Department of Energy. It appears that DOE has accepted my proposal to have my grant administered by a non-academic Institution (which, in my case would be much easier since I lack a permanent position). Therefore, I do not have salary problems. Yet, Il would be a pleasure for me to be associated with Tuft. Also, the production facilities of the Hadronic Journal (of which, as you will remember, I am the editor in chief) are quite close to your campus, with clear advantages for me as far as commuting is concerned. Jointly, it may be that the various scientific activities at our Journal could be of value for your Department.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

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

Office of the President

April 19, 1982

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

Office of the President

FINAL NOTICE

PLEASE POST PLEASE POST PLEASE POST

SEMINAR

FRIDAY, APRIL 30

2.30 p.m.

Algebraic identities, vector fields
and coordinate changes

Professor [redacted]
University of [redacted], Department of Mathematics
and IBR, Division of Mathematics

Abstract

The extension of Lie groups to analytic H-spaces (manifolds with multiplication having identity element) requires the use of general nonassociative algebras in their analysis. In this lecture, anticommutative algebras are emphasized relative to tangent algebras, generalized Campbell-Hausdorff formulas, and connections. Also, various algebraic identities extending associativity on H-spaces are discussed in terms of invariant vector fields and coordinate changes.

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

Office of the President

April 20, 1982

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
Medford, Massachusetts 02155

CERTIFIED

Dear Dr. Schneps,

I phoned this morning to Ms. CELIA MEES, Editor of the "Boston Area Physics Calendar" to ask the listing of a forthcoming seminar by Professor [redacted] who [redacted] you eventually know, is a distinguished mathematician of the University of [redacted] Copy of the intended announcement is enclosed.

To my considerable surprise, Ms. Mess informed me that she had received prohibition to list seminars from our Institute personally from you, and that I should contact you to know the reasons.

Following immediate consultations with our Board of Governors, I was instructed to write you this letter asking:

(1) The reasons for your prohibition that our Institute lists seminars in the Boston Area Physics Calendar;

(2) The clarification whether this was your personal action, or the action by Tufts University, or a joint action with other local colleges. In this latter case, please indicate the names of individual officers and colleges who participated in the decision.

(3) A detailed suggestion of procedures our Institute must comply with to have the listing of seminars, and the list of other Institutes currently excluded.

I do not know whether you realize the scientific gravity of the occurrence, or our determination to resolve it in the interest of the free and genuine pursue of knowledge, let alone to protect the dignity or the distinguished scientists who are damaged by the occurrence. I would like, therefore, to suggest that you take all the necessary appropriate actions. Lacking your answer in the near future, we will be forced to initiate our own actions without any further prior communication with you.

Very Truly Yours

Ruggero Maria Santilli
President
RMS-mlw
cc.: Professor [redacted]
WASSERMAN & SALTER, Law Firm of the IBR
Board of Governors, IBR
Ms. Celia Mess, TuftsUniv.

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

Office of the President

April 22, 1982

TO SELECTED MEMBERS OF THE IBR

CONFIDENTIAL

I feel obliged to report to you an expisode of rather vulgar academic greed typical of the Cambridge academic community, that has damaged the dignity of a member of our Institute, and will damage all members, unless prompt action is immediately undertaken.

On April 19, 1982, a distinguished mathematician, member of our Institute, was denied the listing of his seminar in the "Boston Area Physics Calendar". I subsequently contacted

Dr. JACK SCHNEPS, Chairman, Department of Physics, Tufts University, Medford, Ma 02155, tel (617) 628 5000, extension 3383

who is responsible for the editing of the Calendar. He indicated to me that he was acting under specific instruction by

Dr. KARL STRAUCH, Chairman, Lyman Laboratory of Physics, Harvard University Cambridge, Massachusetts, 02138, tel (617) 495 2844

as well as a number of faculty at the Lyman Laboratory. Schneps indicated that Strauch and his friends

opposed all listings of seminars from our Institute in the Boston Area Physics Calendar

This conclusion was reached on account of the facts that

(a) the rejection of the listings persists even when the seminars were for specifically indicated talks of physical orientation (the Calendar is for physical talks, whether theoretical or experimental);

(b) the rejection of the listings persists even under our best cooperation for the words of the listings. For instance, In the listings of the invited talk by Professors Myung and Schober we had in 1981 at our Institute, we indicated for logistic reasons that the talk would occur at "The Prescott House on Harvard Grounds". I therefore indicated to Dr. Schneps that if the indication of the location of the Prescott House would bother the academic greed at Harvard, it would be eliminated. The asnwer was that the prohibition to list the seminar persisted no matter what.

(c) The prohibition persists until Dr. Strauch and his [redacted] formally withdraw the opposition to the listings.

This situation, as you can see, implies great offense to the dignity of truly distinguished scholar who will visit out Institute (including experimentalists who are scheduled here within one or two months).

As a result of this situation, and after having exhausted all friendly attempts to solve it quietly, our Board of Governors has instructed me to initiate the study of a possible massive action against alleged [redacted] at Harvard University and at Tuft University which may involve:

- the filing of law suits for damages, currently under study by our Law Firm;
- an open written accusation of alleged [redacted] at Harvard University and at Tufts University by me as an individual to be mailed world wide;
- a formal request of initiation of investigations on the case (as well as numerous related aspects) to be filed by me as President of the IBR, under a formal vote by the Board of Governors, to the U.S.Senate, the American Institute of Physics, and other National Bodies.

To understand the episode in its true light, you should be aware of the extremes of academic greeds that eventually resulted in the creation of our new, independent Institute. We have absteined from disclosing them to you because, after all, they are of such gross humanity to be unbeliavable. This last episode is not, therefore, isolated. It is nothing else than a small episode in a much more serious chain.

For the very survival of the Institute, it is therefore essential that a halt to these occurrences be initiated once and for all. Now is the time.

This communication is intended to reassure you that, in case we do indeed decide for a massive, frontal attack, you will be informed individually with sufficient notice. Also, I would like to reassure you that your association with our Institute has not been disclosed by us, and that the confidentiality has been guarded to our best.

In case you are outraged of the enormity of the case, please feel free to write, or, better, call Drs.Schneps and Strauch and express your support for our Institute. However, I beg you not to feel obliged to do so. Also, it would be appropriate for you to contact me prior to any action, in order to be informed of latest developments.

The morale is the following. The studies of primary relevance for our Institute, that is:
- the Lie-admissible generalization of Lie's theory; and
- the consequential generalization of the atomic mechanics,

are against the "vested academic interests" of the departments of mathematics and physics at Harvard, MIT, Tufts and other universities. This has resulted in the truly unbelievable difficulties we have encountered in the past. Until now we have managed to go ahead with a tolerant, humanly decent response. Apparently, this has been counterproductive, in the sense that it has favored excesses, including alleged pressures in Washington to prevent the funding of our programs.

It is clear that, unless we succeed in containing this academic greed we will go nowhere.

Sincerely,

Ruggero Maria Santilli

The University of [redacted]

[redacted]

April 30, 1982

Professor Ruggero Maria Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Ruggero:

I intend, with your approval, to send the following letter to Tufts and Harvard.

> It has come to my attention that you may possibly have adopted a posture, relative to the Institute of Basic Research in Cambridge and the research efforts it supports, that runs counter to the traditional academic attitude towards open inquiry of all new ideas. I would appreciate very much in knowing if this is correct, and if so, on what grounds this position is predicated.

If any thing new has developed please let me know.

Sincerely,

[redacted]rman
Department of Mathematics

bs

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

Office of the President

May 20, 1982

Dr. JACK SCHNEPS
Chairman
Department of Physics
Tufts University
MEDFORD, Massachusetts 02155

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

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/miw

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.

NOTE OF 05/01/'84:
THIS SEMINAR WAS NOT LISTED

# THE BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is produced weekly during the Academic Year by the Physics Department, Tufts University. Announcements should be mailed to Tufts or called in to (617)-628-5000 x2295/3393 no later than 1:00pm on the Monday preceding the week of the talk.

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March 21 - March 25

NOTE OF 5/1/84: SAMPLE LISTING OF SEMINAR IN APPLIED MATH.

MONDAY, MARCH 21

Boston University

12:00 - Bag lunch, Room 235, 111 Cummington Street
12:15 - Center for Polymer Studies Seminar, Room 235
Experimental Determination of Critical Exponents near the Gelation Threshold
Prof. Izumi Nishio, Boston University

Boston University

4:00 - Astrophysics Seminar, Room 506, CLA Building
OH Masers in Compact H II Regions
Mark Reid, Center for Astrophysics

Harvard University

4:00 - Tea, Jefferson 461
4:30 - Physics Colloquium, Jefferson 250
Sources of Gravitational Radiation
Prof. Douglas Eardley, Harvard Astrophysical Laboratory

TUESDAY, MARCH 22

Brandeis University

3:30 - Coffee and cookies, Physics Building, Bass 333
4:00 - ***Please note new time**** Physics Colloquium, Nathan Goldstein Lecture Hall, Abelson 131
Science as Intellectual Property
Prof. Dorothy Nelkin, Cornell

Massachusetts Institute of Technology

3:45 - Refreshments, Marlar Lounge, 37-252
4:15 - Astrophysics Colloquium, Marlar Lounge
The Orion-KL Infrared Cavity
Prof. Reinhard Genzel, University of California, Berkeley

WEDNESDAY, MARCH 23

Harvard University

3:00 - Mathematical Physics Seminar, Jefferson 256
Supermanifolds Continued
J. Bernstein, Harvard

# PART III:

# BOSTON

# COLLEGE

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

Professor Ruggero Maria Santilli, President

October 31, 1983

Ms. S. LYNCH
Editor
The Boston Area Physics Calendar
Department of Physics
Boston College
CHESTNUT HILL, MA 02167

Dear Ms. Lynch,

This past Tuesday, October 25, 1983, I contacted Barbara at your office to see whether we could list a forthcoming seminar of Professor [redacted] of [redacted] University, New York, under the title

"Doubly–linked ring networks and computer communications"

to be delivered at our Institute on November 4, 1983.

Barbara informed me that the deadline for the appearance of announcements for the first week of November had passed, and that the Calendar had already been printed and mailed out.

I therefore asked Barbara for the courtesy of a copy of the mailing list of the Calendar, to be used by our Institute for the mailing of the enclosed announcement of Professor Hsu's seminar. I stressed that the cost of copying the mailing list will be paid by our Institute.

Barbara indicated that she had no authority on the matter and that she had to contact you. She also promised to let me know soon, owing to the urgency of the matter. Since that time (Tuesday morning) I have contacted Barbara a number of times without any result. Each time she repeated that she had to contact you, or that you were out for lunch, or the like.

The possibility of using your list for the announcement of Professor [redacted] talk is now lost. Nevertheless, the situation may well repeat itself in the future.

Please let me have a xerox copy of the mailing list, jointly with your bill for the related expenditures, so that we can expediciously use it in the future for the announcement of advanced talks in physics and applied mathematics by distinguished scholars.

In case, for any reason, you do not wish to release a copy of this list, please do not be afraid to say so. The important background issue you should be aware of is that the policy, whatever it is, is applied in exactly the same manner to all institutions, in order to prevent discrimination in the propagation of scientific information, particularly when conducted under governmental support.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

cc: Dr. R. Uritam, Chairman, Department of Physics, Boston College

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

PLEASE POST PLEASE POST PLEASE POST

SEMINAR IN APPLIED MATHEMATICS

FRIDAY, NOVEMBER 4, 1983

3:00 p.m.

Doubly—Linked Ring Netwoks and
Computer Communications

Professor [redacted]
[redacted] University
[redacted]

BOSTON COLLEGE
CHESTNUT HILL, MASSACHUSETTS 02167

DEPARTMENT OF PHYSICS
(617) 552-3575

November 9, 1983

Dr. Ruggero M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott St.
Cambridge, Mass. 02138

Dear Dr. Santilli:

In reply to your letter of October 31, 1983, the mailing list for the Boston Area Physics Calendar is not a matter of public record and is not available for distribution. The list represents the names of persons and institutions that have subsribed to the Calendar, and exists for the benefit of the Editor of the Calendar in his task of distributing weekly issues of the Calendar.

Sincerely,

Shirley Lynch
Administrative Assistant

SL/mr

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

Professor Ruggero Maria Santilli, President

November 22, 1983

Miss SHIRLEY LYNCH
Administrative Assistant
Boston College
Department of Physics
CHESTNUT HILL, Massachusetts 02167

Dear Miss Lynch,

I appreciated the courtesy of your letter of November 9, 1983. Please be assured that I respect your decision concerning the lack of availability of the mailing list of the Physics Calendar.

Permit me, however, to suggest, if at all needed, that the same policy be strictly inforced for all members of the Boston physics community. In fact, I have been hearing repeated rumors that discrimination of research under governmental support has apparently occurred in the editing of the Calendar by other universities and that the matter is under consideration for possible submission to the appropriate legislative body in Washington.

Very truly yours,

Ruggero M. Santilli

RMS/mlw

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

Professor Ruggero Maria Santilli, President

March 7, 1984

Ms. S. LYNCH, Editor
BOSTON AREA PHYSICS CALENDAR
Department of Physics
Boston College
Chestnut Hill, Massachusetts 02167

Dear Ms. Lynch,

please include in the calendar the following talk.

MONDAY, March 26
The Institute for Basic Research
96 Prescott Street, Cambridge, tel. 864 9859
12:30 - Theoretical Physics Seminar, IBR Hall
Problematic aspects of general relativity for planetary orbits
Dr. H. Yilmaz, Hamamatsu Photonics, Japan

Thank you

Very Truly Yours

Ruggero M. Santilli
President
RMS-mlw

cc. Dr. Yilmaz
105 Church Street
Winchester, Ma 01890

## THE BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is produced weekly during the Academic Year by the Physics Department, Boston College. Announcements should be mailed to Boston College or called in to (617) 552-3575 no later than 1:00 p.m. on the Monday preceding the week of the talk.

*-*-*-

THURSDAY, MARCH 29

Harvard-Smithsonian Center for Astrophysics

3:30 - Refreshments, Tea, Phillips Auditorium, 60 Garden Street

4:00 - Scientific Colloquium

From Interstellar Grains to Comets

Professor J. Mayo Greenberg, Huygens Laboratory, University of Leiden, The Netherlands

FRIDAY, MARCH 30

Tufts University

3:30 - Refreshments, Tea, Math-Physics Library

4:00 - Physics Colloquium, Robinson 152

Heavy Particle Production in QCD

Professor Louis Clavelli, Indiana University

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

Professor Ruggero Maria Santilli, President

March 27, 1984

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

Ms. S. LYNCH,
Editor
BOSTON AREA PHYSICS CALENDAR
Department of Physics
Boston College
Chestnut Hill, Ma 02167

Dear Ms. Lynch,

Please include in the calendar the following announcement

MONDAY, April 16
The Institute for Basic Research
96 Prescott Street, Cambridge, Tel. 864 9859
12:30 - Theor. Phys. Seminar, bring your own lunch

Problematic aspects of the general relativity for planetary orbits

Dr. H. Yilmaz, Hamamatsu Photonics, Japan

I understand that the preceding announcement of Dr. Yilmaz's talk, mailed to you on March 7, 1984 (via normal letter) did not reach you in time for the listing of the talk originally scheduled for March 26.

Owing to this apparent mixup, this communication is mailed to you via certified means. You should consider communicating to us reception and-or any decision as soon as possible.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

cc. Dr. Yilmaz
105 Church Street
Winchester, Ma 01890
Dr. R.A.Uritam, Chairman
Dept of Physics, Boston College

P 277 494 312

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(See Reverse)

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STREET AND NO. Dept of Physics

P.O., STATE AND ZIP CODE Boston Geek, Ma

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## THE BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is produced weekly during the Academic Year by the Physics Department, Boston College. Announcements should be mailed to Boston College or called in to (617) 552-3575 no later than 1:00 p.m. on the Monday preceding the week of the talk.

*-*-*-

WEDNESDAY, APRIL 18 (continued)

University of Lowell

3:30 - Refreshments, Coffee, Olney Science Ctr., OH-143, Physics Faculty Lounge

4:00 - Physics Colloquium, Olney Science Ctr., OH-115

The Bates Linear Accelerator Facility and Some Experiments

Dr. Padmanabh Harihar, University of Lowell

Boston College

3:45 - Refreshments, Coffee, Higgins Hall 354

4:15 - Physics Colloquium, Higgins 262

Novel Phenomena in Superlattices

Dr. Lionel Friedman, GTE Laboratories/Boston College

Harvard University

4:00 - Refreshments, Coffee, Lyman 330

4:30 - Joint Theoretical Seminar, Jefferson 256

Discrete Gravity

Prof. T.D. Lee, Columbia University

Boston University

8:00 p.m. - Philosophy of Science Colloquium, Rm. 314 of the George Sherman Union, 775 Comm. Ave., near Boston University Bridge

God and Coherence: On the Epistemological Foundations of Religious Belief

Kai Nielson, Philosophy, University of Calgary

THURSDAY, APRIL 19

Harvard University

2:30 - Physics Colloquium, Jefferson 250

Morris Loeb Lecture Series - Pulsars: Nature's Most Precise Clocks

Lecture #2 - Clock Stability, The Early Universe, and Millisecond Pulsars

Professor Joseph Taylor, Princeton University

Massachusetts Institute of Technology

3:30 - Refreshments, Coffee, 26-110

4:00 - Physics Colloquium, 26-100

Neutrino Exploration of the Earth

Dr. Alvaro de Rujula, CERN

Harvard-Smithsonian Center for Astrophysics

3:30 - Refreshments, Tea, Phillips Auditorium, 60 Garden St., Cambridge

4:00 - Scientific Colloquium

Neutron Stars: New Results from the Japanese X-Ray Satellite TENMA

Dr. Takaya Ohashi, Leicester University - Recently of the Institute for Space & Astronomical Science, Tokyo

THURSDAY, APRIL 19 (continued)

Northeastern University

3:45 - Refreshments, Coffee, Tea, & Cookies, Dana 114

4:00 - Solid State Seminar

Sliding Charge Density Waves

Dr. Peter Littlewood, ATT Bell Laboratories

FRIDAY, APRIL 20

Massachusetts Institute of Technology

3:30 - Refreshments, Coffee, Building 9, Rm. 150

4:00 - Center for Materials Science and Engineering Colloquium

Optical Transitions and Elementary Excitations in GaAs-AlGaAs Multiple-Quantum-Well Structures

Dr. Daniel Chemla, AT&T Bell Laboratories

Massachusetts Institute of Technology

3:45 - Refreshments, Coffee, Tea, & Cookies, NW14-2209

4:00 - Plasma Fusion Seminar Series

High Power Electron Cyclotron Heating in the Tara Tandem Mirror Experiment

Michael Mauel, MIT

Harvard University - Division of Applied Sciences

4:00 - Atoms, Molecules, and Condensed Matter Seminar, Pierce Hall, Rm. 209

Thinking About Solid State Structures That Have Not Yet Been Made

Roald Hoffman, Visiting Prof., Chemistry Dept., Harvard University

Refreshments after the Seminar in the Brooks Room

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

Professor Ruggero Maria Santilli, President

April 11, 1984

Father DONALD J. MONAN, President
Boston College
CHESTNUT HILL, MA 02167

CERTIFIED LETTER
RETURN RECEIPT
REQUESTED

Dear Mr. President,

I feel an ethical duty to bring to your attention the fact that the Physics Department of your College has refused the listing, in the Boston Area Physics Calendar, of a seminar by Dr. H. Yilmaz entitled, "Problematic aspects of the general relativity for planetary orbits".

Copy of the pertinent material is enclosed including: copy of an initial request for the listing dated March 7, 1984, addressed to Ms. S. Lynch and mailed via ordinary first class mail; copy of a subsequent letter dated March 27, 1984, addressed also to Ms. Lynch, with copy to Dr. R. A. Uritam, Chairman of your Physics Department, this time mailed via certified letter, return receipt requested; copy of the certifications by the U. S. Post Office; and, finally, copies of the Calendar printed by your Physics Department without the listing of the seminar by Dr. Yilmaz.

Permit me the liberty of recommending most respectfully: (a) the immediate initiation of an in depth investigation of the case; (b) the termination of the employment of all persons found responsible for the event; and (c) the conduction of the investigation in a way as open to the general public as possible.

The latter suggestion is based on the fact that, on our part, we shall take all the necessary initiatives to propagate the information of this incredible occurrence, as much as conceivably possible, on a worldwide basis.

The need for this action is evident. In fact, the occurrence has all the ingredients for a possible suffocation of America in its most vital jugular vein: freedom of scientific inquiry.

My respectful suggestion for you to conduct an investigation as public and open as possible is to prevent possible shadows of complicity of your entire College.

For your information, Dr. Yilmaz is an internationally renowned, senior scientist. He has been publishing, during the past quarter of a century, in numerous technical journals certain problematic aspects of Einstein's gravitation, which now constitute an historical open problem in the field.

It is evident from the very title of the proposed talk by Dr. Yilmaz, that his studies are damaging to the vested, academic—financial—ethnic interests built throughout this century around Einstein's ideas.

Nevertheless, in my humble view, excessive leniency on these vested interests constitute a clear threat to our free society.

I promise you that the refusal by your Physics Department to list the seminar by Dr. Yilmaz on constructive scientific criticisms on Einstein's ideas will indeed be brought to worldwide attention.

It is a marvelous test case to see whether or not America is a truly free society. We shall resolve the episode in due time while the world is watching!

Most Respectfully Yours,

Ruggero M. Santilli
President
The Institute for Basic Research

encls.
cc: Dr. H. Yilmaz, 105 Church Street, Winchester, MA

encls.: 1) Correspondence with Boston College; 2) A recent paper by Dr. Yilmaz; and, 3) A description of the I. B. R.

# PART IV:

# MASSACHUSETTS INSTITUTE OF TECHNOLOGY

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

May 7, 1979

Professor HERMAN FESHBACH, Chairman
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Herman,

You might be interested to know that the call for a moment of reflection on the basic physical laws currently used in hadron (and nuclear) physics, which I initiated while being a honorary guest at your Department in 1977-1978, has moved considerably ahead. I formally presented this call via a series of rather long papers in the Hadronic Journal, backed by two series of monographs, one with Springer-Verlag and one with Hadronic Press (the first two volumes were published in 1978, two additional volumes are in print and others are in preparation). This call was subsequently answered by independent researchers via papers already appeared in the literature or in print, all favorable.

Lately, I have released the enclosed review paper on the rather numerous and substantial criticisms on quark conjectures, which are moved by so many and outstanding physicists all over the world. You will be amused to know that I have released this paper for wide distribution (15,000 copies via the Hadronic Press) for the intent of indicating to quark-committed colleagues that a critical process of examination of quark conjectures is in motion on a world wide basis, jointly with the study of fundamentally different approaches. The idea is to indicate that their not so unusual corridor-type of opposition to quark-non-oriented studies on hadrons, nowaday, has no scientific value. If they have technical arguments to disprove these criticisms, they must present them in scientific papers.

In conclusion, it appears that the scientific scene in hadron physics is changing rapidly and drastically. Today, we have valuable physicists who do not believe in quarks and, actually, do not believe in the physical laws used in these models. In particular, the search for coverings of the basic physical laws of the electromagnetic interactions, specifically conceived for the strong, is in motion.

On administrative grounds, a new voice is being heard in our community, which I have attempted to reproduce in the enclosed paper. Studies along quark conjectures should indeed be continued (and funded). Nevertheless the restriction of all the studies on the fundamental problem of hadron structure to quark conjectures only may well result to be an "historical error". Thus, a more balanced conduction (and funding) of research in the sector is

advocated whereby all promising lines of study, whether of quark or non-quark inspiration, are conducted and confronted with physical veritas.

On technical grounds, the ultimate objective of this scientific effort is to promote the experimental verification of the validity or invalidity of the basic physical laws used in current trends on strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle. As you know, these basic laws are experimentally established until now only for the electromagnetic interactions.

The idea is that effective research in the sector cannot be any longer continued on the basis of the mere beliefs of the validity of the basic laws, however autoritative is their source. More particularly, we are currently spending truly large amounts of money in strong interactions, all based on the mere belief of the validity of the basic laws. In my humble view, if this situation is protracted any longer, without the joint experimental study of the basic laws, a process to our scientific accountability will be simply unavoidable.

You will be pleased to know that this call for a return to do physics via experiments, rather than beliefs, is being answered. Indeed, a number of experimenters have already expressed to me their desire to initiate a predictably laborious study.

The reason for writing to you is to candidly express my most sincere regret that the Massachusetts Institute of Technology is, at this moment, only a spectator of the study of these fundamental physical problems.

I would like therefore to recommend most warmly that the Massachusetts Institute of Technology initiates an active involvement on these issues. In particular, your Institute has all the human and technical resources to conduct an experiment for the verification of the expected small deviations or possible validity of Pauli's principle in nuclear physics, via low energy processes, according to my proposal in the HJ 1, 574 (1978) (see also the enclosed paper for an outline). It is for me regrettable that these invaluable resources should not be used for such a fundamental physical problem.

In closing, I would like to indicate that this letter is solely motivated by my gratitude for the kind hospitality I have received at your Institute, as well as my esteem and respect for all of your.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.
c.c.: Professors F.E. LOW, M. DEUTSCH, P. MORRISON, F. VILLARS and V. WEISSKOPF.

HARVARD UNIVERSITY

Area Code 617
495-3352

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

May 21, 1979

Professor HERMAN FESHBACH
Department of Physics
MIT
Cambridge, Massachusetts 02138

Dear Herman,

In case you might be interested to know more details about the possibility of testing Pauli's principle in nuclear physics, I enclose a copy of the joint papers with C.N.KTORIDES (Greece) and H.C.MYUNG (Iowa, USA) entitled "Lie-admissible approach etc.". Section 3, and Table 1 (p.37)in particular, of this paper present a tentative, conjectural study of the problem.

Apparently, a number of additional contributions on this problem are forthcoming by independent researchers. In case you desire to be kept informed of the progresses, please let me know. Similarly, it would be a pleasure to make available a complimentary copy of the reprint volumes I and II of papers of 1978 on related topics (H.C.MYUNG, S. OKUBO and myself, editors).

Almost needless to say, all these (and the forthcoming) studies have only an initial, preliminary character, and much remains to be done to reach the necessary maturity for actual experiments. Yet, the sentiments by a number of colleagues, which I share, are that the sooner we start, the better.

Again, I always remember all of you with sincere pleasure and gratitude.

Sincerely

Ruggero Maria Santilli

RMS/ml

c.c: Professors F.E.LOW, M. DEUTSCH, P. MORRISON, F. VILLARS and V. WEISSKOPF

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

(Mailed at the home address).

Professor Francis E. Low
Massachusetts Institute of Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Francis,

On June 1, 1980 the first phase of my grant with the DEPARTMENT OF ENERGY terminates. This phase essentially contemplated my exposure to advanced mathematics, which I did, thanks to the invaluable teaching of masters such as Shlomo Sternberg and Raoul Bott.

I am now entering into a new phase of studies which is specifically intended to achieve maturity of formulation of experiments. I am therefore seeking the association with a physics Institution.

The DOE is apparently pleased with the output of my studies and is interested in continuing my grant, provided that I identify a suitable Institution for its administration. Please feel free to contact, if you so desire, the DOE officer in charge of my grant, Dr. DAVIS C. PEASLEE, tel. 301 353 3624.

I have therefore applied to a number of selected Institutions in physics. In essence, I am seeking a physics Institution that allows me to apply for the renewal of my grant, as principal investigator, with the understanding that I will be considered for tenure after the needed number of years of service. This last point is particularly important for me. I am now 45 years old, and I have to give preference to an Institution where I have the possibility of being seriously considered for tenure at some time in the future.

As you know, I did not apply to MIT, nor this letter is an application. Nevertheless, I would appreciate the courtesy of your consideration of my case, and your advice whether it is appropriate for me to apply or not. You can rest assured of my confidentiality and gratitude.

Permit me the liberty of bringing you updated on my research. As you know, I am involved in the problem of the validity (according to some) or the invalidity (according to others) of conventional laws for the strong interactions.

After a predictably nebulous orientational phase, this problem is now studied by a coordinated group of mathematicians and physicists. Some of the mathematicians of our group are:

- Professor H.C.MYUNG of the University of Northern Iowa; Professor M. L. TOMBER ofthe Michigan State University; Professor R. OEHMKE of the University of Iowa; Professor G.P. WENE of the University of Texas at San Antonio, and others;

Some of the physicists of our group are:

- Professor S. OKUBO of the University of Rochester; Professor C.N.KTORIDES of the University of Athens in Greece (currently spending his sabbatical as my guest here at Harvard); Professors F. CANTRIJN and W. SARLET of the Instituut voor Theoretische Mechanics of the Rijksuniversiteit in Gent, Belgium (the latter spent his sabbatical 1978-1979 as my guest here); Professor J. FRONTEAU and M. TELLEZ-ARENAS of the Université d'Orléans; Professor J. KOBUSSEN of the Institut für Theoretische Physik der Universität Zürich; Professors J. LÕHMUS and L. SORGSEPP of the USSR Academy of Science in Tartu; Professor JIANG CHUN-XUAN of the People's Republic of Chine; Professor ELIEZER and his group at La Trobe University in Australia, and others.

It is for me rewarding to see that this group is now expanding in a promising way.

The efforts are coordinated via:

- a yearly workshop, called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS. The first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980. In addition a CONFERENCE IN LIE-ADMISSIBLE ALGEBRAS is currently under independent organization by the mathematicians of our group for spring 1981.
- the assistance to independent researchers for their contributions in the HADRONIC JOURNAL as well as in other Journals;
- my two series of monographs, "Foundations of Theoretical Mechanics" with Springer-Verlag (Volume I was printed in 1978 and volume II is in press); and "Lie-admissible Approach to the Hadronic Structure" with the Hadronic Press (Volume I was published in 1978, Volume II is in press and Volume III is scheduled for 1980).
- the reprint series "Applications of Lie-admissible Algebras in Physics" edited by Professors H.C.MYUNG, S. OKUBO and myself, which reprints <u>all</u> contributions in the field (Volumes I and II were printed in 1979, while we are working at two additional volumes, one on contributions prior to 1978 and one on the contributions of 1979).
- consultation and collaboration with colleagues involved in other lines of studies. For instance, I have recently completed the funding and editorial organization of a new series of reprint volumes "Developments in the quark theory of hadrons", edited by Professors D.B.LICHTENBERG and S.P.ROSEN (the first two volumes are under preparation, one on contributions prior to 1979; one on contributions in 1979; and then the series will continue with a yearly volume identifying the yearly

advancements in the field). In this way, we hope to have in the editorial programs of the HADRONIC JOURNAL representations of different trends in high energy physics that are effective for comparative analyses.

As you can see, the studies in the problem of the validity or invalidity of conventional laws for the strong interactions have proliferated considerably. Today, the achievement of a mature judgment calls for considerable reading. Lacking this technical study, any judgment is purely superficial. For instance, our notion of hadronic constituents (we call eletons and antieletons)-representative of a particle under joint electromagnetic and strong interactions (the latter realized via the condition of overlapping of the wave packets)-calls for the totality of the classical, quantum mechanical, algebraic, geometrical, field theoretical, statistical and thermodynamical contributions on Lie-admissibility which exists by independent mathematicians and physicists.

I am fully aware that you do not have the time of reading such a voluminous literature. To assist you for the possible achievement of a first understanding of what we are doing, I have enclosed copy of the "Chart 4.9" of my Volume II with Springer-Verlag.

This chart essentially outlines the problem in a way understandable for the intended audience: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization. In particular, I would like to bring to your attention the recollection, in Part 9 of this chart, pages 343-349, of the historical, authoritative, voices of doubt by Fermi, Einstein, Jordan, and others (that we call "legacies").

Permit me to stress that this is a non-technical presentation. The technical one is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, attended by all members of our group with the exception of a few who lacked travel funds and sent in their contributions. In case you are interested to have a complimentary copy of these Proceedings, please let me know and I shall do my best. They are scheduled for distribution in early 1980.

I hope you will see that there are indeed serious doubts on the validity of conventional laws for the strong interactions, backed by historical voices of the caliber of Fermi, Einstein, and Jordan. What we are doing is in essence consider their legacies seriously (rather than ignoring them, as done by the majority of our community); work out their implications as much as possible; and attempt the achievement of maturity of formulation of their experimental resolution.

I am fully aware of the inertia in our community toward these studies, which sometimes becomes straight opposition against their conduction (I have experienced a real hardship here at Harvard myself, that I shall tell you some day). Quite candidly, this is the reason why I did not

apply to MIT. I simply do not know whether MIT is interested in being associated with efforts toward the experimental resolution of the problem of the basic laws for the strong interactions, and leave theoretical beliefs (whether of quark or non-quark orientation) to the theoreticians.

An effective way to represent our studies is via Heisenberg's words ("Physics and Beyond", p. 70):

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

to which he adds soon after:

> "However, when it comes to entering 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."

Despite these predictable human aspects, I believe that a fundamental ethical rule of our profession is the resolution via experiments of theorecal divergences or controversies. When it comes to the problem of the basic laws for the strong interactions, I believe that the implementation of this rule is necessary, of course, upon achieving maturity of formulation and technical capability. In my view, there is too much at state to lightly overlook the issue. When, in addition, there are historical, unanswered legacies by the founding fathers of contemporary physics, we simply have a duty to perform.

Irrespective of whether I apply or not to MIT and whether I join or not the MIT, you should be perhaps informed that we are close to the needed theoretical and technological maturity.

I am referring here to the experimental test of Pauli's exclusion principle under strong interactions, beginning at the nuclear level, according to my original proposal in the Hadronic J. 1, 574 (1978 , subsequently elaborated in a number of articles, and treated again at the recent workshop on Lie-admissibility.

The idea is to ascertain whether the principle is valid in nuclear physics in the same quantitative amount as it is valid in atomic physics, or very small deviations exist , are experimentally detectable, and have escaped currently available studies simply because not looked for.

On more specific grounds, the proposal suggests the test via low energy scatterings of hadrons in nuclei selected in such a way that their charge volume is below the value predicted by the proportionality rule with the total number of nucleons. The objective is that of verifying the statistical character of the wavefunction of the identical nucleons of these nuclei, that is, whether it is totally antisymmetric, or small deviations

from this statistical character are detectable. This experiment is apparently feasable with current technology, with the understanding that it is predictably delicate, and that it will predictably call for further, coordinated studies by experimentalists and theoreticians.

For the nuclei selected, we have an experimentally established, statistically small, state of penetration of the wave packets of the nucleons, one within the others. Fermi's legacy is that, under these conditions, we have forces more general than $f = -\partial V/\partial r$. It is this legacy which has been studied to all possible extents. At a first look, under these broader forces (variationally nonselfadjoint forces nonderivable from a potential), there is the inability of treating the structure via the conventional Schrödinger's and Heisenberg's equations in $H = H_{free} + H_{int}$. At a second look, Heisenberg's equations have been proved to be <u>inconsistent</u> for the broader forces considered, via the no-go theorem of the (pre)symplectic quantization (See part 6 of the enclosed chart for a nontechnical outline). At a deeper look, the forces considered imply the breaking, in a very small amount, of the SU(2)-spin symmetry, in much of the conceptual way according to which we have to break the rotational symmetry of the spinning top in Newtonian mechanics to avoid perpetual-motion-type of academic abstractions. The studies predict in this way that, under very small conditions of overlapping of the wave packets, identical particles that are exact fermions under long range elm interactions (this is the <u>only</u> statistical character experimentally established now), are no longer exact fermions. An expected very small deviation from the applicability of Pauli's principle is then consequential.

I am sure you will see the impact of Fermi's legacy. But there are other legacies. Pauli made it quite clear in his historical lectures and papers that his principle was conceived for the case of lack of overlap of the wave packets. Indeed, under these latter conditions, he had "stronger" forces which prohibited him to separate the wave function, let alone to establish its totally antisymmetric character. This legacy by Pauli has not yet been resolved experimentally and, in my view, it calls for an experimental verification. If nothing else, it calls for due consideration.

But, perhaps, most important is Einstein's legacy. You are aware that he refused to believe up to his death on the terminal character of the conventional uncertainty of quantum mechanics. In Heisenberg's words, he could at most tolerate quantum mechanics as a "temporary expedient". It has been proved in the Lie-admissible literature that, under the conditions of overlapping of the wave packets and nonselfadjoint forces, the concentional uncertainty of quantum mechanics must leave the way to broader views. One way you can see it is via the fact that the time evolution law under these broader forces is strictly noncanonical, at the classical level, and strictly nonunitary at the quantum mechanical level. Assuming that the conventional undeterminacy holds at a given value of time, it is necessarily nonpreserved in time under nonunitary time evolutions. I am confident you will see the impact of Einstein's legacy on this issue, as well as its deep inter-relation with the seemingly uncorrelated legacies by Fermi and Pauli.

All these legacies are brilliantly expressed in a unified algebraic way by Jordan's legacy. As you know, he did not believe in the associative character of the central algebraic structure of quantum mechanics, the universal enveloping associative algebra. He therefore suggested a broadering of this structure into a nonassociative form, which he selected, for statistical considerations, of commutative character (the celebrated Jordan algebras). Jordan's legacy is at the foundation of the Lie-admissible formulations. We simply perform the transition to a yet broader non-associative algebra, as necessary to represent forces more general than $f = -\partial V/\partial r$. The algebra is Lie-admissible to achieve a covering of conventional Lie stuff. But the algebra we use is also Jordan-admissible. This means that Jordan's view in preserved in its entirety in our approach .

Once Jordan's legacy is taken seriously and worked out, you can see the direct inapplicability of conventional views in quantum mechanics under the condition of overlapping of the wave packets and forces nonderivable from a potential. For instance, the SU(2)-spin algebra becomes mathematically undefinable and physically meaningless, trivially, because of the lack of its primary structure, its enveloping associative algebra.

The reason why an increasing number of researchers is attracted into the problem is that the Lie-admissible algebras do not leave all these issues quantitatively open. No. They provide coverings of the Lie algebras. As such, they allow the quantitative formulation of coverings of the conventional notions definable via the Lie lalgebra, such as that of fermions. This is the reason why there is a feverish activity going on for instance, on the SU(2)-admissible covering of SU(2) via algebraic studies (Myung, and others), quantum mechanical studies (myself and others), statistical studies (Fronteau and others), functional studies (Ktorides and others), deformation-type studies (Lõhmus and others), etc.

The reason why I give utmost priority to the experimental verification of Pauli's principle in nuclear physics are numerous. First of all, this is the experiment most close to maturity. Secondly, it is expectedly less expensive than an equivalent experiment in high energy physics, and will requ re comparatively less time. Thirdly, the mechanics of possible deviations goes at the heart of conventional relativities. After all, the SU(2) spin is a vital part of Galilei's and Einstein's special relativities.

Most important, in my view, is the fact that the possible experimental detection of very small deviations from Pauli's principle in nuclear physics will have far reaching theoretical implications. For instance, at the hadronic level it will imply the expectation that the deviations from conventional laws are greater, if you abandon point-like abstractions of the hadronic constituents and represent them via wave packets of extended size which (from atomic and nuclear similarities) have the same dimension as that of the entire hadron and, thus, are in a much greater state of mutual penetration (on a comparative basis with the nuclear case). At the astrophysical level the implications are even greater. We would have the expected inapplicability of the Riemannian geometry for the interior

problem (only) and, actually, its collapse in the central part: the assumption that all geometries for the interior problem are Lorentz in local character. Irrespective of that, the problem essentially consists of the issue whether an extended particle such as a proton, while within the core of a star, has exactly the same statistical character as that when moving in vacuum under elem interactions or not. Permit me candidly to say that the belief that the proton, under these conditions of extremely high pressures and densities, is a conventional fermion, is nowday shared by a fastly decreasing number of physicists. For me, the idea that a proton is still a fermion under the conditions considered is mere academic hand waving essentially motivated by the customary inertia on established doctrine, and which see its historical origin on the notion of massive point by Galilei and Newton, preserved in full in the special relativity, and implemented in the most advanced contemporary theoretical views such as QCD or supergravity. These views are fully acceptable for systems of particles moving in vacuum and no overlap of their wave functions. When particles are in a necessary state of penetration of their wave packets, these views still produce excellent physics (as proved by QCD). Yet, they are a crude approximation of a much more complex physical reality.

What I am trying to convey to you is that the estreme conditions of penetration of the wave packets in the interior of astrophysical bodies have their intermediate realization in the hadronic structure, and a primitive realization in the nuclear structure, according to a decreasing order of complexity of the forces, dynamical effects, and impact in the basic laws and principles.

This is the reason why I consider as of fundamental character the resolution of the issue via experiments in nuclear physics.

Dear Francis, I believe that your scientific function in these studies can be invaluable, particularly after your acquisition of the post of director of the division of nuclear physics at MIT.

I hope that this letter will assist you in your independent assessement of the experimental test considered, including its scientific implications, and will stimulate an active involvement by your division.

Love

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

October 23, 1979

Dr. R. Santilli
367 Linwood Avenue
Newtonville, MA 02160

Dear Ruggero:

Thank you for your letter of October 10, and for sending me the notice of your publication.

I will inquire here as to the possibility of a position. If there should be any possibility, I will of course be in touch with you.

Please give my best regards to your wife.

Yours sincerely,

Francis E. Low

FEL:jad

P.S. We had a very nice visit with [redacted] last summer.

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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October 10, 1979

Professor HERMAN FESHBACK
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Ma 02139

Dear Herman,

My Volume II with Springer-Verlag of FOUNDATIONS OF THEORETICAL PHYSICS is now in press, and I though I should pay you the courtesy of an inspection of the Acknowledgments prior to their appearance in print.

Please inspect them (p. 18). You will see that I felt obliged to thank you again for the hospitality during 1976-77, as I did it for Volume I.

You might be interested to know that this effort is receiving a rewarding response. For instance, the enclosed book review on my Volume I by Professor LEIPHOLZ just came to me as a surprise: it calls my monograph "truly epoche-making". There is a feverish activity in the applications of these methods, particularly in engineering circles (but not at MIT, to my knowledge). As you know, they consist of rigorous analytic methods for the treatment of systems with forces more general than $f = - \partial V/\partial r$.

I enclose also copy of "Chart 4.9" of this Volume II. It essentially presents an outline of the doubts on the validity of conventional quantum mechanical laws and principles for the conditions of overlapping of the wave packets. The chart also presents a review of the historical, authoritative voices of doubt by Fermi, Einstein, Jordan, and others. (see part 9, pages 343-349).

If you do not have the time of looking at this material, you may pass it to a graduate student. Please keep in mind that this is a presentation for graduate students and for researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical treatment of the problem is presented elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, we held here from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Belgium, Svitzerland, and Israel, and with corresponding participants from the USSR and the People's Republic of China. These proceedings will be distributed in early 1980.

page 2.

I would like to bring to your attention a no-go theorem on conventional quantization of the (pre)symplectic geometry, outlined in Part 6. This theorem, rigorously proved by mathematicians in the field, establishes that the conventional Heisenberg equations are inconsistent for dissipative forces, that is, generalized Hamiltonian structures capable of recovering true, genuine, Newtonian, dissipative forces non-derivable from a potential under the correspondence limit.

It appears that the study of dissipative nuclear processes is increasing, but it does not appear that the nuclear physicists are aware of the existence of this no-go theorem of quantization, at least speaking at large. I have seen papers around that are flatly wrong. Perhaps, you should keep this theorem in mind in case you stumble into polynomial Hamiltonians of order higher than the second. Your in house experts on this theorem are GUILLEMIN and KOSTANT.

Sincerely

Ruggero Maria Santilli

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encls.

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October 10, 1979

Professor VICTOR F. WEISSKOPF
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Weisskopf,

I am taking the liberty of sending to you enclosed copy of the "Chart 4.9" of my volume II of FOUNDATIONS OF THEORETICAL MECHANICS with Springer-Verlag, now in press.

This chart presents an outline of the studies on the "legacies" by Einstein, Fermi, Jordan, and others (see part 9 for the recollection of these legacies, pages 343-349). Perhaps, you may prefer the conventional scientific language of this presentation (as compared to the informal language of my note to HANGAS HURST I recently mailed to you).

Again, if you have any critical remark or historical recollection that may assist me in the final editorial control of this rather delicate chart, I would be sincerely grateful.

Permit me to stress that the presentation of this chart is that for the intended level of audience of my monographs: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical presentation of these studies is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS we held here from August 1 to 7, 1979 with the participation of mathematicians and physics from the USA, France, Belgium, Switzerland, and Israel, and with corresponding participants from the USSR Academy of Science. In case you are interested in a complimentary copy of these proceedings (scheduled for distribution in early 1980), please let me know and I shall do my best (the request is quite large already).

I would like also to take the liberty of stressing that the notion of hadronic constituent used in the paper by Dr. JIANG CHUN-XUAN on my structure model of hadrons (I recently mailed to you) is based on the totality of the studies for the Lie-admissible treatment of forces more general than the simplistic one of current use, $f = -\partial V/\partial r$, at all levels. I am referring here to the contributions by mathematicians and physicists in the analytic, algebraic, geometrical, field theoretical, statistical, thermodynamical, and quantum mechanical aspects. Lacking a technical knowledge of all these contributions, any judgment is purely superficial.

The enclosed chart 4.9 can give you only a rudimentary, non-technical, characterization of this hadronic constituent (we call eleton).

Permit me also the liberty of clarifying my scientific position with you. I am fully aware that you are deeply involved and committed to quark lines of study on hadrons. I would like to say that I sincerely respect these studies. The fact that these studies produce excellent physics is proved by a large volume of evidence. The fact that I support the continuation of these studies is proved by a new yearly series of reprint volumes, specifically and entirely devoted to quark lines, I have lately organized as part of the HADRONIC JOURNAL initiatives, under the independent editorial control by Professors D. B. LICHTENBERG and S. P. ROSEN. You will see this series advertised soon in PHYSICS TODAY.

More specifically, I believe that unitary models and QCD have a FINAL physical character for the classification of hadrons, or, you can say, for their "chemistry" or, you can also say, for their "Mendeleev-type" treatment.

My doubts rely only on their joint interpretation as providing an actual structure model of hadrons. To understand my doubts you must keep into account that I have tried for years to reach a structure model via quarks that is mathematically and physically consistent according to my own standards, and beginning, most importantly for the lightest known hadrons (Bohr did not start with the uranium, but instead, with the hydrogen atom).

The presentation of the technical difficulties that forced me to abandon quarks would be impossible here. They were simply too many. As a conceptual indication, when I was working at the $\pi^0$ via quarks, my first requirement was to achieve a $q\bar{q}$ bound state with an identically null probability of tunnel effects (and NOT with an approximately null value). For me, this was a fundamental condition for physical consistency to prevent the decay

$$\pi^0 \rightarrow q\bar{q}$$

which simply does not exist in nature. When this strict form of confinement is truly implemented, then you can see real problems. Once pushed to its extreme consequences, this confinement was simply incompatible with the basic laws of quantum mechanics.

The transition to field theory essentially obfuscates these technical difficulties. But, in my humble view, they persist . Simply peoples do not look deep enough. But there are exceptions. Numbu clearly stated in the Einstein's celebrations that confinement under gauge invariance is still an open question.

page 3.

A point of sort of "irreconciliable disagreement" between quark-supporters and quark-dissidents (for the structure profile only) is the following. When faced with these technical difficulties of confinement, quark-supporters often assume the attitude that it is a minor problem, that it can be resolved with, say, infinite potentials, or that it will be resolved soon, or that it can be resolved via phenomenological (rather than dynamical) models"à la bag".

Concerned scolars take a more serious attitude in these matters. The idea is that a quark model without a STRICT form of confinement via DYNAMICAL means (equations of motion obeying physical laws) is FLATLY INCONSISTENT WITH PHYSICAL VERITAS. The quarks are simply not produced in the spontaneous decays nor in all high and low energy scatterings.

As a distinguished scolar put it to me in a recent letter:

> "The lack of achievement of a strict form of confinement is such a major inconsistency that should be reason for rejection of all papers on quarks, when referred to hadron structure. It is the same as stating, on grounds of scientific accountability, that a symplectic structure is not closed."

The origin of this intriguing situation, in my view, lies on the assumption by quark supporters of desiring to resolve the entire hadronic phenomenology with one single model. I am referring here to the tacit implementation in virtually all papers on quarks that the models provide a joint representation of the classification of hadrons as well as the structure of each individual element of a unitary multiplet. Such an approach can be proved to be inconsistent for the atoms. When passing to the much more complex hadronic world , scientific accountability demands caution, much caution.

This is the reason why:

(I) I firmly believe in the final physical char acter of unitary (and QCD) models for the classification of hadrons (only);

(II) I favor the continuation of studies on the hadronic structure based on quark conjectures; but

(III) I oppose as vigorously as I can the restriction of the studies on hadron structure along quark lines only, and I favor instead the joint conduction of studies of fundamentally different orientation, under the condition that they achieve compatibility with the established, Mendeleev-type, unitary classification of hadrons.

This is my position both as an individual researcher as well as editor in chief of the HADRONIC JOURNAL.

page 4.

You are familiar with the studies along lines I and II. The studies along line III have proliferated substantially and are expanding rapidly. We already have two volumes of reprints APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, Edited by Professors H.C.MYUNG, S. OKUBO and Myself. We are working at two additional volumes of reprints. Plus my monographs with Springer-Verlag and with the Hadronic Press. The reading of all this would take you too much time. This is the reason why I have enclosed the summary chart 4.9 of my volume II with Springer-Verlag.

The differences of line III with line II are primarily of research attitude. An epistemological outline of the former is the following.

(A) The teaching by the Founding Fathers of contemporary physics. When entering into an unknown and unresolved field, such as the structure of hadrons, our first attitude is that of studying the teaching by the founding fathers of contemporary physics.

You may see in the enclosed chart that Einstein's legacy on the expected lack of terminal character of the conventional uncertainty of quantum mechanics is still fully open. Bohr and Heisenberg's refuted Einstein's criticism at a time when the words "strong interactions" had yet to be invented.

Also, you may see that Fermi's legacy on the expected inapplicability of conventional geometries in the region of space occupied by a strongly interacting particle is more open than ever.

Similarly, you may see that Jordan's legacy on the expected need to enlarge the enveloping algebra of quantum mechanics is fundamentally open at this time.

The contemporary quark community has literally ignored these legacies. On the contrary, we have studied them seriously. We believe that until these legacies are resolved via experiments we can only conduct conjectural studies. To state it explicitly, we believe that, until the validity or invalidity of conventional quantum mechanical laws for the strong interactions has not been resolved via experiments, the studies on hadron structure will remain controversial, conjectural and of tentative character, whether of quark-orientation or not. Still in different terms, we believe that the problem of the structure of hadrons is purely secondary. The basic laws come first.

This is reason why I have proposed, via separate letters, to Philip MORRISON and other friends at MIT the initiation of experimental studies to test the validity or invalidity of Pauli's principle in nuclear physics (where at most, very small deviations are conceivable). This experiment is conceptually and technical constructed to test the historical, unaswered legacies I am recalling here.

You should not see here a potential invalidation of quark lines and of QCD. Not at all. Permit me to elaborate on this point.

The origin of our contemporary theoretical physics must again be seen in the originators of the basic ideas. Galilei's relativity is fundamentally dependent on the notion of massive point conceived by Galilei and Newton. This notion of point-like objects is preserved in its entirety in the special relativity, as clearly indicated by Einstein. In turn, this notion emerges in an often ignored, but fundamental physical role in the most advanced research, such as QCD. I am sure you will agree that, after all, QCD is based on local differential equations (point-like abstraction), and the conventional Lagrangian structure $L = L_{free} + L_{int}$. Apart extreme technical complications (e.g, for renormalization) the physical foundations conceived by Galilei and Newton are intact: QCD treats only point-like objects with only action at a distance forces. This is Newton's idea that the sun can be approximated to a point, only applied to hadrons.

We all know that such an approximation produces excellent physics. QCD has indeed produced, and will continue to produce excellent physics. The point is that, by no means, QCD should be considered as the final, terminal, physical description of hadrons. Indeed, the weakness of QCD rest in its physical foundations: the point-like approximation of hadrons, and of their constituents.

A possible departure from established physical laws, we are expecting when the particles are treated as they actually are (extended objects), would be no disaster at all. Suppose that the special relativity is experimentally proved as inapplicable for the dynamics of a hadronic constituent. This may stimulate a"scientific renaissance", but QCD will remain intact in its current physical value: a description of hadrons under their point-like approximation.

(B) The efforts in the construction of covering formulations. As editor of the HADRONIC JOURNAL I have sensed more particularly the following scientific situation. I have published a variety of papers in differentiated fields of science, such as functional analysis, geometry, nonassociative algebra, Newtonian mechanics, space mechanics, quantum mechanics, engineering,

The trends in all these studies is to stay away from the trivial $f = -\partial V/\partial r$. I am sure you realize that an engineer would be fired if he ignores internal losses when treating, say, an electric circuit. Similarly, I am sure you realize that a NASA officer would be fired on the spot if he intended to treat SKYLAB with $f = -\partial V/\partial r$. In biophysics the situation is even selfevident.

page 6.

In conclusion, the virtual entirety of contemporary science is moving toward the construction and application of methods for the effective treatment of forces generally nonderivable from a potential.

But there is one exception: quark lines and QCD. I am confident you will agree with me that the totality of papers along these lines is based entirely on $L = L_{free} + L_{int}$, that is, $f = -\partial V/\partial r$. What I wanted to bring to your attention here is that, in the view of an increasing number of observers, this situation will create a scientific gap between quark studies on hadronic structure and the rest of science, if excessively protracted. The rest of science is already working at methods substantially more advanced than those used in quark lines which, most importantly on physical grounds, are capable of accomodating genuinely more general forces. There is no doubt that $f = -\partial V/\partial r$ is effective for the atomic structure and for most of the nuclear structure. But there are doubts, serious doubts, that $f = -\partial V/\partial r$ will result to be truly effective for the hadronic structure.

The enclosed chart 4.9 may give you an idea of these broader methods.

(C) The efforts in the experimental resolution of the basic laws for the strong interactions.

The invalidity of conventional relativities in classical mechanics for forces not derivable from a potential (representatives of the motion of extended objects in a resistive medium), is an established physical reality. Example: SKYLAB. The forces of this system were polynomial expansions in the velicities for which conventional space-time symmetries are simply inapplicable, and must leave the way to broader geometrical views.

Fermi's legacy, as you can see in the enclosed chart 4.9, is that particles under conditions of overlapping of the wave packets possess forces nonderivable from a potential. If these forces break conventional relativity at the classical level, then, as a necessary condition for consistency with the corresponding principle, the breaking of conventional space-time symmetries must persist at the quantum mechanical level.

This is only one (out of several) reasons of doubts on the final character of conventional laws for the strong interactions. Indeed, the overlapping of the wave packets for these interactions is expected to be the rule.

This is also the reason why we are involved in a scientific effort to reach maturity of formulation of experiments for the resolution of these fundamental physical problems, as well as in the promotion of the initiation of these experimental studies.

page 7.

But, we have encountered a considerable inertia in the scientific community at large toward these studies. Billions of dollars of taxpayers money are preferred to be spent, instead, in a pletora of experiments, all scientifically valuable indeed, but of ,minute incremental character as compared to the resolution of the historical legacies indicated.

This situation can be best expressed via the words by WERNER HEISENBERG (Physics and Byond, p. 70):

> "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 hazardous leap forward."

To which he adds soon after:

> "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."

Dear Professor Weisskopf, I sincerely consider you among the Founding Fathers of contemporary physics. Thus, I believe that you have the vision of men such as Heisenberg, Pauli, Einstein, Jordan, etc.

I am appealing to you for support in my proposal to Philip Morrison and other friends at MIT to initiate studies at MIT in the experimental verification of Pauli's principle in nuclear physics.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.

P.S. You might be interested to know that my recent preprint "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" (which was distribited rather widely at MIT) has been accepted for publication in FOUNDATIONS OF PHYSICS.

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October 10, 1979

Professor PHILIP MORRISON,
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Philip,

I miss your stimulating presence at the lunch meetings at MIT during my visit in 1976-1978. Here, we simply do not have meetings of this type.

Permit me the liberty of outlining what we are doing. It appears to have a rather intriguing astrophysical implication. Any critical comment or historical recollection you might have, would be sincerely welcome. If you are interested, we sould perhaps see each other some time and enter into more detail.

Statement of the problem. We are involved in achieving maturity of formulation of experiments for the resolution of the problem of validity (according to some) or invalidity (according to others) for the strong interactions of the familiar relativities and quantum mechanical laws of the electromagnetic interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and Heisenberg's indeterminacy principle.

Conduction of research. After an introductory, orientational phase, the problem is now studied, either directly or indirectly, by a coordinated group of mathematicians and physicists. Some of the mathematicians of our group are

- Professor MYUNG (university of Northern Iowa); Professor TOMBER (Michigan State University);,Professor OEHMKE (University of Iowa); Professor WENE (University of Texas at San Antonio); and others.

Some of the physicists of our group are

- Professor OKUBO (University of Rochester); Professor KOBUSSEN (Institut für Theoretische Physik der Universität Zürich); Professor KTORIDES (University of Athens in Greece, currently spending his sabbatical here as my guest); Professor SARLET (Instituut voor Theoretische Mechanics of the Rijksunieersiteit Gent, Belgium, who spent his 1978-1979 sabbatical as my guest); Professor LÕHMUS and his associates (of the USSR Academy of Science in Tartu); Professor ELIEZER and his group (at La Trobe University in Australia); Professor FRONTEAU and his group(at the Universite d'Orléans in France) and others.

This group is expanding in a quite promising way.

Coordination of research. The studies are coordinated via:

- a yearly workshop, called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980);
- a CONFERENCE IN LIE-ADMISSIBLE ALGEBRAS under independent organization by the mathematicians;
- the coordination of efforts by independent researchers via the HADRONIC JOURNAL, of which, as you eventually know, I am the founder and editor in chief (the second editor in Howard Georgi, and the editorial council comprises distinguished scolars, including two Nobel laureates).
- the reprinting of all papers in the problem in the series APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, edited by Professors MYUNG, OKUBO and myself (the first two volumes were printed in 1978, and two additional volumes are under way; and
- two series of research monographs I am currently involved in, one with Springer-Verlag entitled FOUNDATIONS OF THEORETICAL MECHANICS (the first volume was printed in 1978, and the second is in print); and the second series with the Hadronic Press under the title LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE (Volume I was printed in 1978; volume II is in print and volume III is scheduled for 1980).

The historical, authoritative, voices of doubt. These studies have a precise historical origin, in the sense that, first of all, they are not new, and, second, they see their origin in a number of "legacies" by the founding fathers of contemporary physics which have been simply ignored by the contemporary scientific community at large.

Einstein made it quite clear that he did not believe in the terminal character of the conventional uncertainty of quantum mechanics. He kept this conviction, up to his death, as you know well. In Heisenberg's words (From"Physics and beyond"), Einstein could at most tolerate quantum mechanics as a "temporary expedient". He made numerous counterexamples, that were however knocked down by Bohr and Heisenberg. The point is that Bohr and Heisenberg's criticisms strictly apply only for the atomic case. The case of the structure of astrophysical bodies, that of the structure of hadrons, and to a certain extent (see below), that of the nuclear structure, were out. As a result, EINSTEIN'S LEGACY IS STILL OPEN.

Pauli made it also quite clear that his principle was conceived for the lack of overlapping of the wave packets (atomic structure). Indeed, when the wave packets overlap, he had "stronger" forces which would prohibit him from separating the wave function, let alone to establish its totally antisymmetric character. The point is that, by no means, the lack of overlapping of the wave packets is a universal property. On the contrary, the overlap is rule, and the lack of overlap is the exception. Thus, we believe that PAULI'S LEGACY IS ALSO STILL OPEN.

Fermi also made it quite clear that he did not believe in the applicability of conventional geometries in the area of space occupied by a strongly interacting particle. Notice that Fermi was fully aware that the lack of applicability of conventional geometries implies that of conventional relativities and quantum mechanical laws. To my reconstruction, Fermi

thought that, under the conditions of overlapping of the wave packets we have forces more general than the trivial forces $f = -\partial V/\partial r$ which is dominating contemporary physics (to my dismay). Under these forces, he then saw the lack of the existence of a Hamiltonian $H = H_{free} + H_{int}$ and, thus, the lack of applicability of conventional Schrödinger's and Heisenberg's equations, and consequently, the need of broader formulations. We firmly believe that FERMI'S LEGACY IS ALSO FUNDAMENTALLY OPEN.

Jordan also made it quite clear that he did not believe (for statistical reasons) in the central mathematical structure of quantum mechanics, the universal enveloping associative algebra. Indeed, he suggested a generalization to a nonassociative form. He then selected a nonassociative commutative form (the celebrated Jordan algebras) for statistical reasons. The point is that the conventional associative envelop can be proved to be inconsistent when forces more general than $f = -\partial V/\partial r$ are used. Thus, we believe that JORDAN'S LEGACY IS ALSO FUNDAMENTALLY OPEN.

Von Neumann and Wigner joined Jordan in their celebrated joint paper of 1934 to clearly express doubts on conventional quantum mechanical views for the nuclear structure.

My list of authoritative, historical, voices of doubts could continue.

The role of the Hadronic Journal. When I decided to organize the Hadronic Journal in early 1978, the situation was essentially the following. We had all these authoritative, historical, doubts by the founding fathers of contemporary physics, but their followers had completely ignored them.

Actually, the very reason why I decided to organize a new Journal (and enter into all the predictable problems, such as financing-all successfully solved), was precisely this complete ignorance of the teaching by these masters. The organization of a new Journal was clearly essential. Indeed, you will agree with me that the conduction of studies of this type, against a dormient orthodoxy, would have taken decades in conventional Journals.....

In particular, at the time of the organization of our Journal we had
- no organized conduction of an in deth study of these legacies;
- no organized efforts at the formulation of experiments for their resolution; and
- no initiation of the study for possible generalized formulations.

I am happy to report to you that, beginning from its first issue of April 1978, and thanks to the participation of numerous scientists with a genuine vision and interest in the pursuit of knowledge, the HADRONIC JOURNAL has made significant contributions along all these three aspects.

State of the theoretical studies. Dear Philip, the amount of the litarature accumulated by now in this problem is so large, to discourage an outline, and to prohibit it in a letter.

What I can say is that all the seemingly unrelated, independent, historical, voices of doubt by Einstein, Fermi, Pauli, Jordan, Wigner, von Neumann, and others, turned out to be deeply interrelated, self-consistent, and mutually compatible.

Fermi's vision on the existence of forces non-derivable from a potential under the conditions of overlapping of the wave packets turned out to be the fundamental physical point. It sets in motion an array of methods for the treatment of these forces which are a covering of those of conventional use. The physical implications are selfevident. In particular, mathematicians in the (pre)symplectic quantization have lately proved a no-go theorem of conventional (Heisenberg's) quantization . Once this theorem is applied to actual physical situations, without point like academic abstractions, the problem emerged as being precisely at the level of the geometry, the symplectic geometry in canonical realization. In substance, some thirty years later, FERMI'S VISION TURNED OUT TO BE CORRECT. Conventional ideas, such as Lie algebras, Heisenberg's equations, and the like, are simply out, because they can be proved to be inconsistent via the most effective and rigorous mathematical methods available at this time. This point was established at the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS.

Einstein's vision of a genuine, nonincremental, advancement turned out to be correct under Fermi's view. This has been indicated in the Literature of Lie-admissibility in a number of different ways leading to the same conclusion. One way you can see it is by first setting your mental attitude toward forces which are simply outside the arena of applicability of what you have read in the books. The time evolution law under forces nonderivable from a potential is noncanonical at the classical level, and nonunitary at the quantum mechanical level. Thus, assuming that, under the conditions of overlapping of the wave packets, Heisenberg's principle is valid at one given time, it is not valid at a later time. In conclusion, despite Bohr's and Heisenberg's counter-criticisms (all of atomic character), EINTEIN's VISION TURNED OUT TO BE CORRECT AND DEEPLY INTER-RELATED TO FERMI'S VISION.

Jordan's vision turned out to be crucial. A necessary condition to represent forces nonderivable from a potential is to alter the structure of the envelope. As a matter of fact, Jordan's vision is at the foundation of the notion of Lie-admissibility. This latter notion simply realizes nonassociative enveloping algebras. These algebras are selected to be Lie-admissible to provide a genuine algebraic covering of conventional Lie stuff, which is capable of representing (via the generalized time evolution law) forces more general than the simplistic $f = -\partial V/\partial r$. The point is that this algebra turns out to be also Jordan-admissible. Thus, Jordan's approach is fully contained in the Lie-admissible approach. In conclusion, JORDAN'S VISION TURNED OUT TO BE CORRECT AND DEEPLY INTERRELATED TO FERMI'S AND EINSTEIN'S VISION. I am confident you realize that, when the associative envelope of quantum mechanics is

generalized into a nonassociative form, the conventional uncertainty of quantum mechanics does not makes sense, mathematically and physically, and must leave the way to broader views, of course, all under the idea that we have generalized forces for the conditions of overlapping of the wave packets. When particles leave this condition, and move in vacuum under only action at a distance forces (derivable from a potential), conventional stuff is recovered identically, apart secondary effects, because the Lie-admissible algebras reduce themselves into the conventional algebras at the limit of null forces nonderivable from a potential, and therefore, the conventional laws are recovered identically.

Pauli's vision also turned out to be correct. In essence, under forces nonderivable from a potential we have a breaking of the SU(2)-spin symmetry, that is, we lack the technical ingredient to properly characterize the notion of fermion, which is obviously a prerequisite for Pauli's principle. This has been independently established in the literature of Lie-admissibility. To see it, again, you must set your mental attitude outside what you read in contemporary books of physics, because you are treating forces and physical conditions outside their arena of applicability. Actually, the occurrence can be seen at the Newtonian level. Consider the spinning top. Conventional books treat this system, from a group theoretical viewpoint, via the SO(3) symmetry. I leave this treatment to their authors. As a physicist I intend first to look at the physical reality, and only after identify the methods for the treatment. The exact SO(3) symmetry for the spinning top literally implies the perpetual motion. Physical reality is different than these academic abstractions. The angular momentum of the spinning top decays in time. This means that the SO(3) symmetry is meaningless for the treatment of the system. Indeed, it must be necessarily broken to comply with physical evidence. apart technical aspects (Lie-admissible quantization), the situation for Pauli's principle under overlapping of the wave packets is conceptually the same as that of the spinning top. In both cases, point-like abstractions are academic hand-waving. In actuality, we have extended bodies and our theoretical tools must represent this extended character. In both cases we have, furthermore, extended bodies moving in a resistive medium. The spinning top rotates in a viscous macroscopic medium. A wave packet, when in a state of penetration with other wave packets, is conceptually along the same lines. In both cases the rotational symmetry is broken. In both cases, the physical quantities characterized via the group of rotations are inapplicable.

Equivalently, you simply do the homework of constructing a wave equation capable of actually representing forces nonderivable from a potential (this can be done starting from generalized classical Hamiltonians, and then quantizing via Hamilton-Jacobi equations - nothing more). You then recover Pauli's troubles which forced him to exclude the condition of overlapping of the wave packets. Indeed, the generalized Schrödinger's equation cannot, in general, be separated. Its totally antisymmetric character is then only in the imagination of physicists desiring to preserve the status quo.

Please excuse the passionate language in this passage. I have experience in my past academic life real hardship because of the refusal by the ortho-

xy to even consider this legacy by Pauli, let alone to treat it decently.

But things are changing fast. I am happy to inform you that, nowday, the number of physicists who believe that, say, a proton in the core of a star is a fermion, is decreasing quite rapidly. The proton is an extended object. When under the extremely high pressures and densities of the core of a star, its wave packet is forced to penetrate within those of the surrounding particles. Under these conditions, the idea that it "spins" in exactly the same amount as that when spinning in vacuum under long range elm interactions (the hydrogen atom) is clearly questionable. For me, quite candidly, is it is an extremely crude approximation of a complexity beyond our imagination.

In conclusion, the literature of Lie-admissibility has indicated that, when a system of identical fermions penetrates hadronic matter, the particles are no longer exact fermions under forces nonderivable from a potential. In this sense, Pauli's principle is inapplicable. Thus, PAULI'S LEGACY TURNED OUT TO BE DEEPLY RELATED AND COMPATIBLE WITH THE LEGACIES BY FERMI, EINSTEIN, JORDAN, AND OTHERS.

A rather feverish research activity is now going on for the construction of coverings of conventional insights. You see, the Lie-admissible algebras are a bona fide algebraic covering of the Lie algebras with a fundamental physical origin: the time evolution law under forces nonderivable from a potential. "Universality theorems" for their existence under these conditions have been proved both classically and quantum mechanically. They verify the correspondence principle, in the sense that classical and quantum mechanical equations can be uniquely related. As a result, this settings allows the quantitative formulation of the covering notion under forces nonderivable from a potential (e.g., spin). This is the reason why an increasing number of mathematicians and physicists is joining our group. They see a clear possibility not only of doing physics, but new physics.

I have personally proposed a classical and quantum mechanical covering of Galilei's relativity for the conditions considered, which is under developement now by a number of independent researchers. The studies for the covering notion of spin, which is the most important part of my Lie-admissible relativity, is now studied:classically by Eliezer and his group in Australia; quantum mechanically by myself and others; field theoretically by Kobussen in Switzerland; statistically by Fronteau in France; algebraically by Myung in the USA; etc.

At this moment I am involved in extending this relativity to "relativistic" conditions of particles which are strictly outside the physical arena of Einstein's conception: motion of extended particles within a hadronic medium.

The expected astrophysical implications. We now come to the aspect for which I decided to write you this letter.

Permit me to clarify my scientific position. I am a firm believer of Einstein's special relativity (general relativity) for the motion of charged particles in vacuum under external elm interactions (the exterior problem of gravitation). Actually I am fascinated by the volume of experimental evidence for these settings and their credibility.

Nevertheless, I believe that the special relativity (the general relativity) are only a first, crude, approximation when referred to extended particles moving within hadronic matter (to the interior problem of gravitation).

The reason is that I have difficulties in establishing the conceptual foundations of these relativities under the latter conditions. For instance, when referring to the interior of a hadron, I have difficulty in establishing the conventional propagation of light, let alone the fact that its value is c and costant. This, of course, if you abandon point-like abstractions of the constituents and represent them as ordinary quantum mechanics tells us: with extended wave packets. Similarly, I believe in the elevator experiment. But this is strictly an exterior case. How do you do the elevator experiment in the interior case? do you do a cicylindrical hole in a star? Assuming that you do so, then the problem becomes automatically an exterior one. How you treat moving observers when referring to the interior of an astrophysical object? Can we let them move within a star? and if we keep them outside a star, how do we "measure" something occurring in its interior?

Irrespective of personal theoretical views, a rather visible "collapse" of the current models of gravitation for the interior case has been recently identified by NASA: One of the simplest possible interior systems is the motion of a satellite in earth atmosphere. In particular, SKYLAB, while it was falling on earth, had forces highly nonderivable from a potential (polynomial expansions in the velocities). At the Newtonian level, this system is simply not derivable from a Lagrangian action principle in the coordinates of its experimental detection (see my monographs with Springer-Verlag for equivalent formulations in new coordinates). This feature persists in its entirety after gravitational extension. The conventional theoretical views, whether Riemannian, of supergravity or of gauge type, simply failed to properly characterize these elemental physical aspects of SKYLAB. They yielded only a point-like approximation.

The situation was put to me in rather vivid terms during the SKYLAB episode. I am sure you realize that NASA people were under pressure those days. They were unable to predict the location of impact up to the very last moment. I understand that the entirety of our theoretical (and computer) knowledge was feverishly inspected in the hope to gain some clue. I was contacted by NASA because they learned of my monographs with Springer-Verlag (entirely devoted to forces more general than $f = -(\partial V/\partial r)$ as well as the concentration of papers in the HADRONIC JOURNAL for the treatment of these forces. Predictably, I was unable to make any contri-

bution, after all, with only a few days notice. Nevertheless, one thing was identified. Galilei's relativity, Einstein's special relativity and Einstein's general relativity all see their origin and arena of applicability in the notion by Galilei and Newton of a massive point. SKYLAB was a fundamentally different physical system. As a NASA men vividly put it to me on the phone: "if a theoretician comes here suggesting the treatment of SKYLAB with conventional relativities, he would be likely chased out of NASA premises."

Dear Philip, I believe that a scientifically effective attitude is that of simply facing the fact that the interior problem of a hadron or a star will likely call for forces outside the capability of current geometries; and their study, in any case, is recommendable. The second step is than that of looking at the authoritative voices of doubt by the founders of contemporary geometry.

As an example, Cartan made it quite clear that the Riemannian geometry is unable to recover Newtonian mechanics at large, but only that part with geometrizable forces.

Lagrange and Hamilton conceived their equations with external terms. Their analytic vision was therefore substantially broader than that of contemporary astrophysicists working on the interior problem. Indeed, irrespective of realizations in curved manifolds, these physicists simply use Lagrange's equations in their truncated form, that without the external terms Lagrange and Hamilton were so keen on (for good reason too - to avoid perpetual-type approximations). Under these conditions, the very terms "Lagrange's equations" are historically incorrect and misleading. Once, again, the teaching by the founders is taken seriously, the next step is the search of a geometry capable of accomodating broader forces, as well as the recognition that the capability by the Riemannian geometry of representing the forces of nature is truly limited.

You might be interested to know that a feverish activity is going on also along these lines by mathematicians and physicists. We are using, as a first step, a geometry under study called "symplectic-admissible" (you might call it also "Riemannian-admissible"). This geometry has been proved at the recent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS as being able to represent directly, in local charts (that is, without changing the coordinates of the experimental setting), the most general forces we now know : the variationally nonselfadjoint, integrodifferential forces (superpositions of local and nonlocal forces derivable and nonderivable from a potential). By comparison, the representational capability of the forces that are expected in the interior problem, for the case of the Riemannian geometry, are truly small.

The experimental profile. The studies indicated in this letter can be best expressed via the words by WERNER HEISENBERG (Physics and Beyond, p. 70).

> "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 hazardous leap forward."

To which, he adds immediately after:

> "However, when it comes to entering 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."

In other words, there is an inertia in the orthodoxy of physics against the experimental resolution of theoretical divergences, when they imply the possibility of departures from established doctrines. More specifically, we spend these days truly large amounts of taxpayers money in experiments which are certainly valuable, yet, in my view, of minute incremental character when compared to the experimental resolution of the legacies indicates here.

But there are exceptions to these general rules. These are the men that truly advance physics, that is, the foundations of physics.

I believe that you are one of these (few) men. After all, this is the reason why I am making this report to you.

After considering a number of alternatives (the possibilities are, in principle, many), we decide to concentrate our efforts in the achievement of maturity of formulation for

THE EXPERIMENTAL TEST OF PAULI'S PRINCIPLE IN NUCLEAR PHYSICS

according to my original proposal in the Hadronic J. 1, 574 (1978), subsequently elaborated in a number of articles and treated again at our recent workshop.

The idea is to ascertain whether Pauli's principle is valid in nuclear physics in the same quantitative amount as that of atomic physics, or very small departures exist, are experimentally detectable, and have escaped available inspections simply because not looked for, along much of the quantitatively similar, historical discovery of parity violation in weak interactions.

On more specific grounds, the proposal suggests the test via the use of low energy scatterings of hadrons in nuclei selected in such a way that their charge volume is below that predicted by the proportionality rule with the total number of nucleons. In these nuclei, the nucleons are in an experimentally established, statistically small state of overlapping of the wave packets. This activates the expected presence in the nuclear force of a small term nonderivable from a potential. In turn, this implies the expected breaking of the SU(2)-spin symmetry in a small form. Still in turn, this view implies that nucleons, under these conditions, are not exact fermions. Still in turn, this imply the expected, conceivable,

statistically small departure from the applicability of Pauli's principle. This latter aspect can be established via the experimental verification whether the wavefunction of identical nucleons of the nuclei selected is totally antisymmetric under particle permutations, or small deviations from this statistical character exist.

In the view of independent physicists, the test is feasible with current technology, under the expectation that it is indeed delicate, and that it will call for an additional, coordinated, joint effort by theoreticians and experimentalists.

I am confident you will see that this proposed experiment is conceived to test, directly, FERMI'S LEGACY. The test, can be also used for an indirect test of EINSTEIN'S LEGACY (because the mechnism of possible departure implies a corresponding departure from conventional uncertainty via nonunitary time evolutions), as well as JORDAN'S LEGACY (because the possible departures from an exact fermionic character call for a departure at the level of the enveloping associative algebra). But, perhaps most intriguing, this test can be seen as a first experiment to resolve the current belief in the interior problem of gravitation, according to which all admissible geometries are Lorentz in local character. Indeed, the possible breaking of the SU(2)-spin symmetry under variational nonselfadjoint forces directly tests this point. In the transition from the nuclear to the astrophysical conditions we only have a quantitative difference. But the theoretical foundations are the same.

Many of us see the Massachusetts Institute of Technology as one of the leading Istitutions in this Country in nuclear physics (and for very good reasons).

I would like to close this letter by appealing to you for the initiation at MIT of the experimental resolution of these historical legacies, beginning in nuclear physics.

Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.

Dear Philip,

I enclose copy of "Chart 4.9" of my volume II with Springer-Verlag. This chart presents an outline of the ideas discussed in the letter.

Please keep in mind that this outline is intended for graduate students and researchers without a technical knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

Thus, this outline is non-technical . The technical treatment is presented elsewhere and, in particular, in the proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS that will be available in early 1980.

Sincerely

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

October 1o, 1979

Dr. ROBERT BIRGENEAU
Department of Physics
M.I.T.
CAMBRIDGE, Massachusetts 02139

Dear Robert,

As you can see, I am still in the USA (rather than back to Europe). As a result, we purchased a new house. I am confident you are enjoing yours.

I do not know whether what we are doing here may have some relevance or connection with what you are involved with. In any case, I am taking the liberty of sending you some informative material.

You will find enclosed copy of "Chart 4.9" of my Volume II of FOUNDATIONS OF THEORETICAL MECHANICS with Springer-Verlag, now in press. This chart essentialy outlines the problem we are working on, that is, a technical study of the reasons of doubts on the validity of conventional quantum mechanical laws for particles under conditions of overlapping of the wave packets. The chart also recalls the historical, authoritative, voices of doubt by Fermi, Einstein, Jordan, and others (you may see part 9, pages 343-349).

Please take into account that the presentation of this chart is that for the intended audience of my monographs: graduate students and researchers without an in depth knowledge of the symplectic quantization and of the broader Lie-admissible quantization.

The technical content is elsewhere and, in particular, in the Proceedings of the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS we held here at Harvard from August 1 to 7, 1979, with the participation of mathematicians and physicists from the USA, France, Belgium, Switzerland, and Israel, and with corresponding participants from the USSR and the People's Republic of China. These proceedings are scheduled for distribution in Early 1980. In case you are interested, please let me know, and I shall attempt to let you have a complimentary copy.

You might also be interested to know that our group is involved in a promotional effort to resolve these historical "legacies" via experiments. In particular, we are establishing a contact with the community of

experimenters in nuclear physics in Europe and in the USSR (here in the States nuclear physicists seems interested in doing only conventional stuff).

One experiment we have formulated is to test the validity of Pauli's eclusion principle in nuclear physics. The idea is that of ascertaining whether this principle is valid in nuclear physics in the same quantitative amount as it is valid in atomic physics, or very small deviations exist, can be detected, and have escaped inspection until now simply because not looked for.

On more specific grounds, the proposal is via low energy scatterings of hadrons in nuclei selected in such a way that their volume is below the value predicted by the proportionality rule with the total number of nucleons. For these nuclei, we have an experimentally established, statistically small, condition of penetration of the wave packets of nucleons one within the others. Under these conditions, we expect the presence in the nuclear force of an additional term, this time nonderivable from a potential and with a small coefficient (Fermi's legacy). We have studied these forces to considerable extent. They essentially prohibit the consistent quantization via Heisenberg's equations because of a no-go theorem of the (pre)symplectic quantization (Part 6 of the enclosed chart). At a deeper analysis, these broader forces imply the breaking of the SU(2) spin symmetry, that is, identical nucleons, under the condition of overlapping of the wave packets, are not exact Fermions. The inapplicability of Pauli's principle is then consequential. Under the classical limit we have a conceptually similar situation: spinning tops with drag torques in a resistive medium, for which the SU(2) rotational symmetry simply does not make sense (it would imply the perpetual motion).

This proposal is in my article Hadronic J. 1, 574 (1978), and has been subsequently elaborated in a number of articles, as well as at the recent workshop.

Well, I do not want to take too much of your time. In case you are interested in these things, perhaps we should see each other, or have lunch sometime together. It would be a pleasure for me to see you again.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

November 14, 1979

Dr. R. M. Santilli
Science Center, Room 331
1 Oxford Street
Cambridge, MA 02138

Dear Ruggero,

Thank you for your note of Oct. 10 and the preprints. Your ideas and proposals all sound very interesting. Unfortunately my own interests in physics are probably at the opposite limits from yours - collective behavior and phase transitions in condensed matter with special attention paid to the role of symmetry and the dimensionality of space. Our recent work has been focussed on systems at their lower marginal dimensionality, that is, those for which algebraic decay of correlations is allowed but no true long-range order.

We are indeed enjoying 74 Baker's Hill Road. My wife is especially happy in the neighborhood. The people are both varied and surprisingly interesting. If you have ever driven by then you will undoubtedly have noticed that I have become a tree and shrub fanatic. So far I have planted over 100 (maples, rhododendrons etc.)! My only real complaint is with the heating bill.

I would, of course, enjoy seeing you again some time. As you suggest, perhaps some time when I am up at Harvard we can have lunch together. Please give our regards to your wife.

Yours sincerely,

Bob

Robert J. Birgeneau

RJB:mb

HARVARD UNIVERSITY

Area Code 617
495-3352

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

November 12, 1979

Professor F. E. LOW,
Director
Laboratory for Nuclear Physics
M.I.T.
Cambridge, Ma 02139

and

Professor A. KERMAN
Director
Center for Theoretical Physics
M.I.T.
Cambridge, Ma 02139

Dear Francis and Arthur,

You might be interested to know the expected energy-related implications of my recent proposal to test Pauli's principle in nuclear physics, and I enclose some informative material to this effect.

I remember you always with sincere pleasure.

Yours, Very Truly

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 9, 1980

Professor Gian-Carlo Rota
Department of Mathematics
M.I.T.
Cambridge, Ma 02139

Dear Gian Carlo,

Subject to administrative finalisation, my research grant from the Department of Energy has been renewed for 1980|1981 with full coverage of my salary and research expenses. I would be interested in visiting your Department at M.I.T. for part of the next academic year, if at all possible, as your personal guest or in some other form you consider appropriate.

The Proceedings of our second workshop on Lie-admissible algebras and their applications to strong interactions are now well under way, and I should have a complimentary copy for you within a few weeks. A preview of the contents is enclosed.

I believe that my visit at your Department might be mutually beneficial, in the sense that I might be exposed to your current research, while some of you might be intrigued by the mathematical and physical potential of the Lie-admissible generalization of Lie's theory.

Looking forward to hearing from you, I remain

Yours Sincerely

Ruggero Maria Santilli

RMS|ml
encls.

P.S. I am continuing to print the ad on your encyclopedia in each and every issue of the Hadronic Journal, as agreed. Also, I would like to encourage you again to let me publish one of your review or research papers in our Journal. You might also be interested to know that Alex Doohovskoy was one of my readers of the state of the art memoir on Lie-admissibility I have published in Part A of the Proceedings. (I understand he was one of your students).

In case I can visit you, I would appreciate the courtesy of a letter indicating this guest status, its duration, as well as the capability of using the MIT Library, without any financial support whatever from MIT. With your permission, I would like to include this letter in the application for renewal of my grant. The grant has been already approved. Please feel free to contact the DOE officer in charge of my grant, Dr. D.C.Peaslee , tel. 301 353 3624.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS

January 18, 1980

Professor Ruggero Maria Santilli
Department of Mathematics
Harvard University
One Oxford St.
Cambridge, Mass. 02138

Dear Professor Santilli:

I was very pleased to receive your letter and curriculum vitae. I would be delighted to have you join us at M.I.T. in the capacity of a Visiting Scholar (unsalaried). I have sent a copy of your letter to Professor Louis Howard who will act upon it and correspond directly with you.

I look forward to seeing you at M.I.T. some time during the 1980/81 academic year. Please keep me advised of your plans.

Sincerely,

Gian-Carlo Rota / lb

GCR:lb

Gian-Carlo Rota

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

26.

AREA CODE 617
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ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 28, 1980

Professor GIAN CARLO ROTA
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Rota,

Please accept the sentiments of my appreciation and gratitude for your kind letter of January 18, 1980 and for the possibility of being a guest (or visiting scolar) at your department during 1980/1981.

Again, I would appreciate the possibility of using conventional research facilities (e.g., library, parking, and address) so that I can actuate my research according to my grant with the Department of Energy. A desk would be welcome, but it is not essential. No expense of any nature by your Department is expected.

I contemplate to pay you and Professor LOUIS HOWARD a brief visit this spring (I am now leaving for Europe to deliver a few seminars on Lie-admissible algebras). In the meantime, I shall keep you informed of our progresses in this line of study.

Sincerely

Ruggero Maria Santilli

RMS/ml

c.c.: Prof. L. Howard

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

March 20, 1980

Dr. M. A. HORNE
Department of Physics
Division of Nuclear Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Mike,

We would like to express our appreciation for your kind reception during our visit yesterday at your laboratory.

We appreciated in particular your comments concerning our proposal to achieve a quantitative experimental knowledge on the validity of Pauli's exclusion principle under strong interactions. A list of references on the proposal is enclosed for your convenience.

As you know, we are theoretical physicists with limited capabilities (if any) to identify specific and concrete experimental settings. Therefore, we would like to rely on the judgment by you, Mr. Atwood and Mr. Arthur whether or not your equipment can do the proposed experiments.

We remain at your disposal for any further consultation and exchange of ideas.

Best Personal Regards

Ruggero M. Santilli

[redacted]

RMS-CNK/ml
encls-

c.c.: Prof. [redacted] University
Mr. D.K.ATWOOD and J. ARTHUR, M.I.T.

REFERENCES ON THE PROPOSAL TO TEST PAULI'S EXCLUSION PRINCIPLE UNDER STRONG INTERACTIONS AS PER MARCH 20, 1980.

The proposal was originally formulated in the paper

1. R.M.SANTILLI, Need of subjecting to an experimental verification the validity within a hadron of Einstein special relativity and Pauli's exclusion principle, Hadronic J. 1, 574-901 (1978)

with particular reference to pages 786-797, 807-818, and 880-882.

In this paper the following results were apparently achieved for the first time:

(a) the SU(2)-spin symmetry is broken under nonlocal interactions, including their local nonpotential approximation, as expected for strongly interacting particles under conditions of mutual penetration of their charge volumes and (to a smaller extent, their wave packets).

(b) the replacement of Lie's formulations with the covering Lie-admissible formulations, and, in particular, the replacement of the conventional associative envelope of quantum mechanics with a covering nonassociative envelope, implies a generalization of Heisenberg's equations of Lie-admissible algebraic character which is capable of representing forces structurally more general than $f = \partial V/\partial r$.

(c) the generalized dynamics as per approach (b) implies a covering notion of particle under conditions of mutual penetration with other particles whose spin is given by the generalized form for suitable operators C

$$J_i J_i = \hbar^2 j(j+1) \longrightarrow J_i C J_i = s(\hbar, j, r, \ldots)$$

This conceivable deviation from the value of conventional spin then implies a conceivable inapplicability of Pauli's principle .

It was stressed in ref. 1 that the quantitative study of a conceivable deviation was conducted to the effect of stimulating the currently lacking experimental verification of Pauli's principle, and that possible deviations from conventional values of spin can at most be internal effects in nuclear, hadron,and astrophysical structures.

The subsequent paper

2. C.N.KTORIDES, On a possible incompatibility of Lie-admissible local powers of a quantum field with the Wightman axioms and the spin-statistics theorem Hadronic J. 1, 1012-1020 (1978)

the implications of the discrete analysis of ref. 1 were extended to field theory. Ref. 2 essentially established that, under interactions structurally more general than the electromagnetic ones, realized via the generalization of the envelope to a nonassociative form, the spin-statistics theorem is not expected to be necessarily valid. This result provided the quantum field theoretical counterpart of the conceivable deviations from Pauli's principle in discrete mechanics. Again, the analysis was motivated by the intent to stimulate the achievement of a quantitative experimental knowledge on the basic laws of the strong interactions.

The subsequent paper

3. C.N.KTORIDES, H.C.MYUNG, and R.M. SANTILLI, Elaboration of the recently proposed test of Pauli's principle under strong interactions, Physical Review D, in press,

initiated the rigorous study of the quantitative theoretical profile. Thanks to the collaboration by Professor Myung (a leading mathematician in Lie-admissible algebras), the authors entered into a technical analysis of the replacement of the associative with a nonassociative envelope for the computation of possible spin mutations. Ref. 3 also presented the specific proposal to test Pauli's exlusion principle for nuclei whose volume is below the value predicted by the proportionality rule with the total number of nucleons. In fact, the conditions of mutual penetration of the charge volumes is an experimentally established fact for these nuclei, although of predictable small character.

The subsequent paper

4. H.C.MYUNG and R.M.SANTILLI, Further studies on the recently proposed experimental test of Pauli's exclusion principle for the strong interactions, Hadronic J. 3, 196-255 (1979)

continued the quantitative theoretical study of the proposal of ref. 1. In particular, the paper identified a hierarchy of generalizations of the SU(2)-Lie algebras to a Lie-admissible form. This hierarchy was applied to the study of a possible hierarchy of mutations of conventional spin (or SU(2)-spin breakings), depending on the conditions of mutual penetration of particles , such as in the transition from the nuclear structure to the structure of hadrons, and to the structure of astrophysical bodies undergoing gravitational collapse.

The more recent memoir

5. R.M.SANTILLI, Status of the mathematical and physical studies on the Lie-admissible formulations on July 1979 with particular reference to the strong interactions, Hadronic J. 2, 1460-2018 (1979)

presented an analysis of the implications of the experiment proposed in ref. 1 with particular reference to the historical profile (a possible deviation from Pauli's principle under strong interactions would apparently be in agreement with a number of authoritative voices of doubt), and the open problem of the structure of hadrons (where a possible deviation from Pauli's principle would apparently allow the identification of the hadronic constituents with physical particles).

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ONE OXFORD STREET
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May 16, 1980

Professor LOUIS HOWARD
Department of Mathematics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Howard,

I would like to thank you for your recent phone confirmation of my guest status at your Department from June 1, 1980 to June 1, 1981, following the kind invitation by GIAN CARLO ROTA (a copy of his letter of January 18, 1980 is enclosed).

Following this confirmation, I have instructed the staff at the HADRONIC JOURNAL to change my address in the second page of the Journal (copy of the current version is enclosed) to the new form

Ruggero Maria Santilli
Massachusetts Institute of Technology
Room 2-236
Cambridge, Massachusetts 02139

which will be used beginning from the June issue, 1980, currently in print. I have also communicated the change of address to other places (societies memberships, etc.).

As verbally indicated to you, I would prefer to avoid the indication "Department of Mathematics" in my editorial activity, as I have done with Harvard. The indication of the secretarial room number 2-236 is fully sufficient for mail purposes. As also indicated to you, the availability of an office on campus would be appreciated, but it is not essential. In fact, I can use my home office. In essence, I am interested in the capability for my conducting at MIT the current studies on the direct experimental verification of conventional laws for the strong interactions. For this purpose, I only need the possibility of using the parking-library-address facilities. Your assistance in obtaining the parking and library permits would be appreciated.

You might be interested to know that I was a guest of FRANCIS E. LOW at MIT from January 1, 1976 until August 31, 1977. I have always remembered this hospitality with pleasure and gratitude. I will be delighted to be at MIT again.

My research contract under DOE support expires here on May 31, 1980. I am therefore in the process of dismantling my office. Until I see you at MIT in June, you can reach me at home (temporary address: 367 Linwood Avenue, Newtonville, Ma 02160).

Very Truly Yours

Ruggero Maria Santilli

RMS/ml; c.c. Professors G.C.ROTA and F.E.LOW, M.I.T.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS
2-377

May 30, 1980

Dr. Ruggero M. Santilli
Science Center
Harvard University
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

When you called me on the telephone, you stated that you hoped to be here as a guest at the Mathematics Department at M.I.T. "with Gian-Carlo". I interpreted this to mean that you would be working with Professor Rota, and as I knew that he has several collaborators whom he hopes to have as guests next year, I assumed that you were one of these. Perhaps I was, in consequence, rather over encouraging about the prospects, though I recalled mentioning specifically that we are currently very tightly stretched for space and that I could not say anything definite until we have had an opportunity to look over the whole picture of visitors. The end of the term has brought this opportunity, and indeed it appears now that the space situation is even worse than anticipated, so that it seems that only in very exceptional circumstances shall we be able to provide any office space for visitors.

But now also, on consulting Professor Rota, I find that he will not be collaborating with you at all (and indeed scarcely knows you) nor does there seem to be any connection or common interest with other people in the applied mathematics group at MIT. Under these circumstances, the proposed guest status, in the Mathematics Department, does not really seem to be appropriate and I think we should not pursue this further. It is certainly inappropriate in any case to list the Mathematics Department's main office as an address for editorial correspondence of the Hadronic Journal, a Journal with which this Department has no connection, and I trust you will rectify this immediately.

Sincerely yours,

Louis N. Howard

Louis N. Howard, Chairman
Applied Mathematics Committee

/nt

cc: Ms. Gerane West
Prof. Gian-Carlo Rota

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE, MASS 02139

DEPARTMENT OF MATHEMATICS

2-377

June 13, 1980

Dr. R. M. Santilli

[redacted]

Dear Dr. Santilli:

Professor Low has advised me that the listing of the Mathematics Department Headquarters as your address in the Hadronic Journal cannot be amended until the next issue. Please be assured that there will be no difficulty about this, and that any correspondence which might arrive for you during the summer will be promptly forwarded.

If you wish some address other than that above to be used for this, kindly advise Mr. James Dalton in the Mathematics Department Headquarters, Room 2-236.

Sincerely yours,

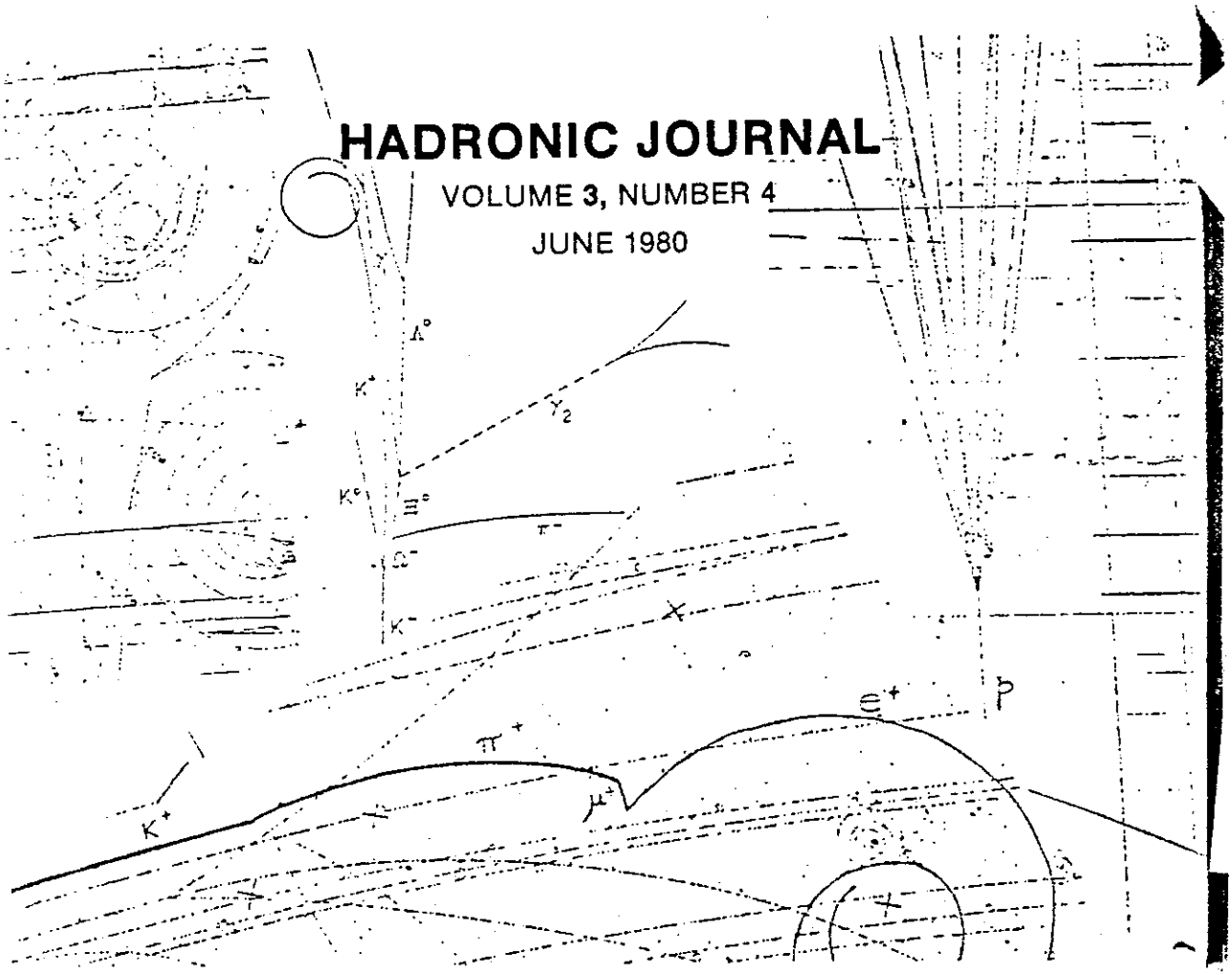
Louis N. Howard /gw.

Louis N. Howard, Chairman
Applied Mathematics Committee

LNH/gw

Dictated by Professor Howard
and signed in his absence.

cc: James Dalton, Administrative Officer
Francis E. Low, Professor of Physics


HADRONIC JOURNAL
VOLUME 3, NUMBER 4
JUNE 1980
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# HADRONIC JOURNAL


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The particle events printed in half tone in the front page of the Journal are derived from 80" bubble chamber photographs kindly provided by BROOKHAVEN NATIONAL LABORATORY

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

CAMBRIDGE, MASS. 02139

DEPARTMENT OF MATHEMATICS

August 27, 1980

Professor CLIFFORD G. SHULL
Department of Physics, Division of Nuclear Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Shull,

On March 19, 1980, during your leave, I visited your associates M.A.HORNE, D.K.ATWOOD, and J.ARTHUR for the purpose of indicating that your neutron interferometer equipment appears to be particularly suited for the experimental verification of the SU(2)-spin symmetry as well as of Pauli's exclusion principle under strong interactions. Copy of the correspondence with Mike Horne is enclosed.

I am referring, for instance, to suitable modifications and/or improvements of the initial tests on the $4\pi$ spinor symmetry already done by the European experimental group headed by Professor RAUCH (a copy of his last paper on the subject is enclosed).

On experimental grounds, the need for additional measurements are numerous. For instance, (1) the exact symmetry value of 720° barely makes it within experimental data (716.8 ± 3.8 deg); (2) the median angle in the latest as well as in the preceding experiments has a tendency to be below 720 deg; and (3) the best fit does not appear to be provided by a sinusoidal curve, as necessary for the exact symmetry (see the diagram of fig.3 of Rach's paper, p. 284).

On theoretical grounds, the need for additional measurements are equally numerous, and they have been discussed in detail in the specialized literature on the topic (see the enclosed list of references, copies of which were released to your associates). In its most rudimentary form a primary argument is as follows. For the case of the elm interactions the exact validity of the SU(2)-spin symmetry is incontrovertible, as established (for instance) by the property that the total angular momentum of a charged particle under an external elm field is conserved. For the case of the strong interactions the situation does not appear to be necessarily the same. As clearly indicated by available experimental data, strongly interacting particles are actually constituetd by wave packets in conditions of mutual penetration or overlapping (which is absent for the elm case, in general). This confirms the rather old expectation that one component of the strong interactions is constituted by a nonlocal, nonpotential (non-Hamiltonian) force. In turn, this is expected to imply the lack of applicability in an exact form of the entire Lie's theory, let alone that of the SU(2)-spin case. Irrespective from this aspect (or as a complement to it), the total angular momentum of a particle under strong interactions is not expected to be conserved (to avoid the perpetual-motion-type of approximation that, say, a proton orbits inside a star with a conserved angular momentum.....). In turn, this is expected to imply a form of breaking of the SU(2) symmetry. Needless to say, such a possible breaking can be only an internal effect of closed strong systems and, as such, not observable via external elm interactions. Also, for the case of the nuclear forces the effect can at most be quite small.

These ideas have been subjected to a quantitative study by a number of mathematicians and physicists via the so-called Lie-admissible generalization of Lie's theory. In essence, the approach studies the generalization of the Lie algebra/enveloping algebra/Lie group in such a way to permit the representation of nonpotential forces, whether local or not.

Also, the approach is applicable for the quantitative treatment of any broken Lie symmetry, and admits the conventional Lie theory as a particular case. The application of these new mathematical tools to the case of a strongly interacting particle under condition of penetration with other particles and expected nonlocal forces has provided: (A) the prediction of a conceivable deviation from the exact SU(2) symmetry of the order of at least $5 \times 10^{-4}$ for the case of low energy nuclear processes; (B) the apparent interpretation of the "slow down effect" of the median angle; and (C) the apparent improvement of the fit of the experimental data by Rauch and his collaborators.

In conclusion, and to our best understanding at this time, the current experimental data appear to be compatible with both the exact and the broken SU(2) spin symmetry. The fundamental character of the symmetry for theoretical as well as applied physics (e.g. the problem of the controlled fusion) then warrants, in my view, additional experiments.

Since the time of my visit to your laboratory several developments have occurred, such as
- a number of experimentalists have answered my call for the initiation of a feasibility study for more refined experiments;
- I have delivered an invited talk at the recent Conference in Differential Geometry and Applied Mathematics held from July 23 to 25 at Clausthal-Zellerfeld, with encouraging results; and
- We recently had our Third Workshop in Lie-admissible Formulations here in the Boston area from August 4 to 9 with the participation of some 30 scientists, including mathematicians, theoretical and experimental physicists. The workshop was virtually devoted to the study of the problem.

In case you are interested in more detailed information, I would be happy to visit you either for an informal meeting or for delivering a seminar on the subject (I could essentially repeat my presentation at Clausthal-Zellerfeld). I can be reached more readily at my home address given below.

Best Personal REgards

Ruggero Maria Santilli
Visitor

RMS/ml

c.c. Professor FRANCIS E. LOW, M.I.T.
encls.

Z. Physik B 29, 281–284 (1978)

Zeitschrift
für Physik B
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# Precise Determination of the $4\pi$-Periodicity Factor of a Spinor Wave Function*

H. Rauch and A. Wilfing
Atominstitut der Österreichischen Universitäten, Wien

W. Bauspiess
Institut für Physik, Universität, Dortmund and
Institut Laue-Langevin, Grenoble

U. Bonse
Institut für Physik, Universität, Dortmund

Received December 12, 1977

The perfect crystal neutron interferometer is used to perform a precise determination of the $4\pi$-symmetry of a spinor. The high precision is gained by the use of very well defined magnetic fields within two magnetized Mu-metal sheets, which are rotated in opposite directions within the coherent neutron beams. The periodicity of a spinor is determined to a value of $\alpha = 716.8 \pm 3.8$ deg., which is in agreement with the theoretical prediction.

## 1. Introduction

The $4\pi$-periodicity of a spinor wave function is a well known quantum mechanical property [1]. A $2\pi$-rotation produces a phase factor of $-1$, which is usually not an observable quantity because quantum mechanical expectation values are quadratic in the wave function. Aharanov and Susskind [2] and, independently, Bernstein [3], predicted that the $4\pi$-factor may be observed in certain experiments. Further suggestions for an experimental verification with neutron- and electron interferometers are given in literature [4–7]. In any case two coherent beams or states have to be created first and superposed afterwards.

Our group achieved the first verification of the $4\pi$ periodicity of the spinor wave function in 1975 [8] using the perfect crystal neutron interferometer developed previously [9]. Further measurements on this subject with another perfect crystal neutron interferometer [10], a Fresnel diffraction neutron interferometer [11], a molecular beam system [12] and for a NMR transition [13] have been reported. The basic theoretical treatment for a state where nuclear phase shifts and spinor rotations occur simultaneously within a neutron interferometer is given in [14, 15], and some of the related beat effects have also been verified experimentally [16].

In the first $4\pi$-rotation experiment we obtained for the periodicity a value of $\alpha = 704 \pm 38$ deg [8]. The main contribution to the error arises from the uncertainity of the magnetic field distribution along the paths of the coherent neutron beams due to the stray field of an air gap electromagnet (Fig. 1a). Therefore, we repeated this experiment with a much more well defined magnetic field distribution using magnetized Mu-metal sheets (Fig. 1b).

## 2. Theory

Within the neutron interferometer a coherent splitting of the incoming wave and a coherent superposition at the exit is achieved. The intensity of the beams behind the interferometer has contributions from the wave

* Work supported by Fonds zur Förderung der Wissenschaftlichen Forschung (Projekt 3185) and by Bundesminister für Forschung und Technologie 03-41A09

0340-224X/78/0029/0281 $01.00

**Atominstitut der Österreichischen Universitäten**
Prof.H.Rauch

Schüttelstraße 115
A-1020 Wien
Tel. (0222) 72 51 36
AUSTRIA

Prof.R.M.Santilli
28 Cross Street
West Newton
MA 02165
U.S.A.

Wien, July 23, 1981

Dear Professor Santilli:

Before going to vacation I would like to answer your letters from January 19, March 23, and April 20, 1981 and to send you some supporting documents for an application of funds from DOE. Please change the numbers according to the usual application form and rewrite the text in good English. The proposed measuring procedure should give high accuracy indeed. Please sign also the research proposal for ILL-Grenoble and send it back to me or directly to ILL and a copy to me. I hope you have a delightful conference now at Boston and we will meet at the Orléans-conference. Please inform me as soon as you know about the decision of DOE concerning our common project. In principle we can start the experiment at the beginning of the next year.
Best wishes also to your family,
Yours sincerely,

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

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 |

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Appendices

A. Information on The Institute for Basic Research

B. Addresses of investigators

C. Papers:

H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik B29, (1978), 281

H. Rauch and A. Zeilinger, Hadronic J. 4 (1981), 1280

R.M. Santilli, Hadronic J. 1 (1978), 574 (excerpts)

C.N. Ktoridss, H.C. Myung, and R.M. Santilli, Phys. Rev. D22 (1980), 892

G. Eder, "Physical implications of a Lie–admissible spin algebra", Hadronic J. 4 (1981), in press.

## ABSTRACT

As it has been known for some time, the magnetic moment of neutrons can change within and perhaps even near the region of the strong interactions. The possibility of a corresponding change of the spin of neutrons under strong interactions was pointed out by R.M. Santilli (Hadronic J. 1 (1978), 574), and subsequently studied by several authors. More recently, G. Eder (Hadronic J. 4 (1981), in press) has pointed out possible fluctuations of the spin of the neutrons due to the magnetic field in the neighborhood of the nuclei, which are of the measurable order of one percent. All these effects can be tested most accurately via neutron interferometers, where widely separated coherent neutron beams are available. The most direct and precise test of the SU(2)—spin symmetry for neutrons has been done by H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse (Z. Physik B29 (1978), 281) via the test of the $4\pi$ periodicity of the spinorial wave function, yielding the value $\alpha_0 = 716.8 \pm 3.8$ deg. Recent corrections due to up—dated physical constants yield the value $\alpha_0 = 715.87 \pm 3.8$ deg which does not include the 720 deg expected for the exact SU(2)—spin symmetry. This proposal recommends a joint AUSTRIA—FRANCE—USA collaboration for the repetition of the experiment in such a way to render it most sensitive to the addition of the strong interactions, as well as to the electromagnetic fields in the vicinity of atomic nuclei. This can be achieved via an additional (Bi or Pb) phase shift placed alternatively into the coherent beams of the interferometer at a position with and without magnetic precession fields, as suggested by H. Rauch and A. Zeilinger (Hadronic J. 4 (1981), 1280) and R.M. Santilli (Hadronic J. 4 (1981), 1166). It can be estimated that a relative accuracy of $\Delta\alpha/\alpha_0$ in the range of $10^{-4}$ can be achieved by this advanced technique. It should be noted that the measure of any deviation from the SU(2)—spin symmetry due to strong interactions and/or other interactions at short range would require a suitable generalization of quantum mechanics, perhaps of the type studied at the yearly *Workshops on Lie—Admissible Formulations.*

## PRIMARY BIBLIOGRAPHY ON THE PROBLEM OF THE EXACT OR APPROXIMATE VALIDITY OF THE SU(2)–SPIN SYMMETRY UNDER STRONG INTERACTIONS


*PART A: THEORETICAL PAPERS*

1 – R. M. Santilli,
*'Need of subjecting to an experimental verification the validity within a hadron of Einstein's special relativity and Pauli's exclusion principle'*, Hadronic J. 1, 574 (1978)
NOTE: Only excerpts of this memoir are included.

2 – H. C. Myung and R. M. Santilli,
*'Further studies on the recently proposed experimental test of Pauli's exclusion principle for the strong interactions'*
Hadronic J. 3, 196 (1979)

3 – C. N. Ktorides, H. C. Myung and R. M. Santilli,
*'Elaboration of the recently proposed test of Pauli's principle under strong interactions'*, Phys. Rev. D22, 892 (1980)

4 – R. M. Santilli,
*'Experimental, theoretical, and mathematical elements for a possible Lie-admissible generalization of the notion of particle under strong interactions'*, Hadronic J. 4, 1166 (1981)

5 – G. Eder,
*'Physical implications of a Lie-admissible spin algebra'*
Hadronic J. 4, 2018 (1981)

*PART B: EXPERIMENTAL PAPERS*

6 – H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse,
*'Precise determination of the $4\pi$ periodicity factor of a spinor wave function'*, Z. Physik B29, 281 (1978)

7 – A. Rauch and A. Zeilinger,
*'Demonstration of SU(2) - symmetry by Neutron Interferometers'*
Hadronic J. 4, 1280 (1981)


**Prepared by the staff of the Institute for Basic Research, Harvard Grounds, 96 Prescott Street, Cambridge, Massachusetts 02138**

**EDITORIAL NOTE**
**prepared by the staff of**
**The Institute for Basic Research**
**on October 19, 1981**

Professor RAUCH (President of the Atominstitut of Wien, Austria) and his associates have revised on July 1981 their best measure of the periodicity of the neutron wave function

716.8 ± 3.8 deg (1)

originally derived in Article 6 of this collection and discussed in Articles 4,5 and 7. The revision was suggested by up-dated physical constants and other reasons. The new value is given by

715.8 ± 3.8 deg (2)

Note that *the new measure does not include the value*

720 deg (3)

needed to establish via experiment the exact validity of the SU(2)-spin symmetry under strong interactions, which therefore constitutes an open physical problem at this moment, as pioneered in Article 1.

SOURCE:
Letter of Professor RAUCH to Professors SANTILLI (U.S.A.), FRONTEAU and TELLEZ-ARENAS (France) dated July 7, 1981
and
Invited talk of Professor EDER (Director for Theoretical Physics of the Atominstitute of Wien) at the
**Fourth Workshop on Lie-Admissible Formulations**
**August 3-7, 1981, Cambridge, U.S.A.**

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

Office of the President

October 29, 1981

Dr. A. ZEILINGER
Department of Nuclear Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Zeilinger,

This letter is to review the recent events related to the research grant application entitled
"EXPERIMENTAL VERIFICATION OF THE SU(2) SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION"
which, as indicated by the enclosed copy of the front page, has been signed by all (scientific and administrative) participants except you.

As you know, the application arrived at our Institute from Wien on October 16, 1981 following the signature of the Principal Investigator Professor H. Rauch, and you were immediately informed. At our meeting the day following arrival of the application, you indicated the need of time to secure the M.I.T. authorization for you to sign. To assist you in this process, I indicated to you the following points.

(1) The staff of our Institute had investigated the administrative profile of your signature to ascertain whether there was any potential conflict with your current support at M.I.T. The results of the investigation were that no administrative conflict exists, provided that your current Governmental support at M.I.T. is fully disclosed in the application and, in addition, it is specifically indicated in the application that no proceed from the possible funding would be used for your salary.

(2) Your signature therefore appeared eminently an internal issue at M.I.T. With this in mind, I indicated that, on our part, we would welcome your signature and participation, with the understanding, of course, that your superiors at M.I.T. favor the experiment suggested in the proposal. Professor Rauch's approval of your participation is self-evident because he had suggested it in the first place.

(3) Since at our Institute we have no intention of interfering in the internal affairs of your college, I indicated also that, in case of unforeseeable difficulties, we would welcome the filing of the application in Washington without your signature. The separate decision whether or not you should participate to the experiment could then be taken at a later time.

Finally, as indicated and stressed to you, our Institute was unable to reach any decision (whether to file the application in Washington or return it to Wien) until you and your superiors at M.I.T. had reached a decision on the matter. As of today (October 29, 1981), no decision has been reached on your part, to our knowledge.

I am therefore formally asking you here that a decision be reached as soon

as possible, in order not to delay due scientific process on an experiment which is clearly of fundamental nature.

Your reply by letter would be appreciated.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-pm

cc. C.G.Shull, Head, M.I.T. Nuclear Division and
H. Feshbach, Head, M.I.T. Physics Department

WEDNESDAY, NOVEMBER 18

Massachusetts Institute of Technology

11:00 - Seminar on Optics and Quantum Electronics, RLE Conference Room, 36-428
Picosecond Electrical Pulses: Generation and Amplification
G. Mourou, University of Rochester

Harvard University

2:30 - Mathematical Physics Seminar, Jefferson 356
On Witten's Proof of the Morse Inequalities
Prof. Raoul Bott, Harvard

Boston College

3:45 - Refreshments, Higgins Hall Room 354
4:15 - Physics and Biology Colloquium, Higgins Hall, Room 307
Membrane Structural Transitions Modulate Growth Limits of E. coli
Dr. Andrew S. Janoff, Harvard Medical School/Massachusetts General Hospital and Department of Biology, Boston College

Boston University

3:45 - Refreshments, College of Liberal Arts Building, 725 Commonwealth Ave.
4:15 - Physics Seminar, CLA Building, Room 510
Inhibited Spontaneous Emission
Prof. D. Kleppner, M.I.T.

Massachusetts Institute of Technology

4:00 - Coffee and Tea, CTP Seminar Room, 6-322
4:30 - Joint Theoretical Seminar, CTP Seminar Room, 6-322
Random Lattice Field Theory
Prof. T.D. Lee, Columbia University

THURSDAY, NOVEMBER 19

***SEE LAST PAGE OF CALENDAR FOR SPECIAL EVENT ANNOUNCEMENT AT M.I.T.***

Massachusetts Institute of Technology

3:30 - Refreshments, Room 26-110
4:00 - Physics Colloquium, Room 26-100
Neutron Interferometry: Experiments with Matter Waves
Prof. Anton Zeilinger, M.I.T.

FRIDAY, NOVEMBER 20

***SEE LAST PAGE OF CALENDAR FOR SPECIAL EVENT ANNOUNCEMENT AT M.I.T.***

Massachusetts Institute of Technology

12:15 - Joint Experimental and Theoretical Particle Physics Seminar, CTP Seminar Room
Strings and Other Vacuum Structures
Prof. Allen Everett, Tufts University
Bring lunch; coffee provided

# BOSTON AREA PHYSICS CALENDAR

The Boston Area Physics Calendar is published by the Physics Department, Tufts University. Items should be submitted to Celia Mees at 628-5000 x344. The deadline for submitting items is 1:00pm on the Monday preceding the week of the event.

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NOVEMBER 16 - NOVEMBER 20

MONDAY, NOVEMBER 16

Harvard University

4:00 - Tea, Jefferson, Room 461
4:30 - Physics Colloquium, Jefferson, Room 250
$10^{-35}$ Seconds After the Big Bang
Prof. Alan Guth, M.I.T.

TUESDAY, NOVEMBER 17

Massachusetts Institute of Technology

10:30 - Coffee, Marlar Lounge, Room 37-252
11:00 - Seminar on Modern Optics and Spectroscopy, Marlar Lounge
Macroscopic Coherence in Atomic Vapors
Thomas W. Mossberg, Harvard University

Brandeis University

2:00 - Theoretical Physics Seminar, Bass, Room 229
Confining Models of the Weak Interactions in Technicolor
Dr. E. Farhi, M.I.T.

Massachusetts Institute of Technology

3:45 - Coffee, Marlar Lounge, Room 37-252
4:15 - Astrophysics Colloquium, Marlar Lounge, Room 37-252
General Relativity and the Binary Pulsar
Dr. Saul Teukolsky, Center for Astrophysics (on leave from Cornell)

Massachusetts Institute of Technology

4:00 - Refreshments, Room 26-414
4:15 - Nuclear Physics Seminar, Room 26-414
The Measurement of Magnetic Moments of High-Spin Rotational States
Dr. L. Grodzins, M.I.T.

Brandeis University

4:00 - Coffee and cookies, Physics Building, Bass 333
4:30 - Martin Weiner Lecture Series Physics Colloquium, Nathan Goldstein Lecture Hall, Abelson 131
Molecular Orbitals in 1st Row Diatomics, Where is the $2p\sigma^*$ Orbital?
Prof. Russell A. Bonham, Indiana University

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

Professor Ruggero Maria Santilli, President

November 30, 1981

Professor L. GRODZINS,
Department of Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Grodzins,

Some of the members of our Institute did listen to your talk at MIT on November 17, 1981 entitled "The measurements of magnetic moments of high spin rotational states".

It appears that there is a considerable connection between your studies and those by Professor RAUCH (Director of the Atominstitut of Wien) on the test of the spinor symmetry of neutrons via neutron interferometers. In fact, in both cases we have an apparent breaking of the SU(2)-spin symmetry due to intense fields which alter the space geometry of charge distributions. In both cases the data can be apparently studied in a quantitative way via a Lie-admissible generalization of the SU(2) algebra (the simplest one is the replacement of the associative product AB with the isotopic product ATB, T = fixed). In both cases the SU(2)-spin symmetry is broken even when the value of the magnitute of the spin and of its third component are the conventional ones (recent results by G. EDER). In fact, the breaking-mutation of conventional charge distributions under intense fields can affect only the other components of the spin and the Casimirs of order higher than two.

In case you are interested, please come and see me for a friendly conversation on the topic (regrettably, I could not attend your seminar). Also, we are having an International Conference in France (January 1982) which will be much on the topic, with numerous mathematicians presenting the structure of the SU(2)-spin breaking and its Lie-admissible generalization for the quantitative treatment of the kroken context.

Sincerely

Ruggero Maria Santilli
RMS-vf

P.S. I feel obliged to enclose xerox-copy of a recent publication by our Institute, including the recent experimental data by Rauch which DO NOT reproduc the exact SU(2)-symmetry, even though, of course, are still inconclusive. This is dictated by the fact that, subsequent to your talk, Mr. ZEILINGER of your nuclear physics division delievered a seminar on Neutron interferometry, including the fundamental spin experiment by Rauch, but he FAILED to mention the recent numbers, as well as the crucial contribution by Eder. I can personally testify that Zeilinger was fully informed of these advances. His silence is therefore a rather preoccupying and self-qualifying occurrence.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

December 4, 1981

Professor R.M. Santilli
Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Mass. 02138

Dear Professor Santilli:

There seems to be considerable confusion as to the nature of the talk that I gave at M.I.T. on November 17, entitled, "The Measurements of Magnetic Moments of High Spin Rotational States." There is no connection between these studies and those by Professor Rauch on the test of the spinor symmetry of neutrons via neutron interferometers. I regret that members of your institute who heard my talk came away with the wrong impression. I obviously did not explain the material clearly enough for them.

Sincerely yours,

L. Grodzins

Professor Lee Grodzins
Physics Department

LG:bak

December 9, 1981

Dr. LEE GRODZINS
Physics Department
M.I.T.
Cambridge, Massachusetts 02139

Dear Dr. Grodzin,

I am in irreconciliable disagreement with your letter of December 4.

(1) Your experimental results are a clear indication of the breaking of the SU(2)-spin symmetry in nature. Period. Explicitly, the most direct possible interpretation of your results is that via the breaking of the SU(2)-spin symmetry (on which several thousands of pages of research have been written--all ignored at MIT). Of course, the interpretation is not final. The point that even a child can see (but not MIT people) is that the interpretation cannot be excluded. Only the well known mumbo-jambo dances of academic politics can exclude it.

(2) Your experiment is directly related to the fundamental experiment by Professor Rauch on the spinor symmetry of nucleon-nucli interactions, because you merely do nuclei-nuclei interactions. Period. Physics is one. The physical laws in your experiment and that by Professor Rauch are exactly the same. On financial-academic grounds the situation is different, inasmuch academicians may attempt dances to distinguish physical laws for certain academic objectives. But we then have no scientific content. Only a political one.

(3) Your firm rejection of clear point (2) in your letter of December 4, and your firm rejection of point (1) apparently made during your seminar may give the impression that you are acting under what is called "MIT politics". You should be fully aware of it. Of course, nobody can prove it. It is an allegation. Yet the doubt persists. Permit me the liberty to let you know, as an outsider, that the terms "MIT politics"are becoming more and more known as meaning "politics in favor of MIT interests, at times in disrespect of the true pursue of novel human knowledge, and, at the extreme, potentially against national-technological interests for advancements".

In the case at hand, there are known financial-academic interests at MIT, documented through the years, favoring the preservation of the SU(2)-spin symmetry, or that, at any rate, would be damaged by the possible theoretical-experimental establishing of the breaking of the SU(2)-spin symmetry. The problem is that this type of academic mumbo-jambo is done via a river of public funds.

I am concerned; I am very concerned indeed, because what I have been seen at MIT during the last years can only result into a crisis.

Very Truly Yours

Ruggero Maria Santilli

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

Professor Ruggero Maria Santilli, President

February 5, 1983

Professor W. F. WEISSKOPF
Center for Theoretical Physics
Massachusetts Institute of Technology
CAMBRIDGE, Massachusetts 02139

Dear Professor Weisskopf,

I would gratefully appreciate the courtesy of your review of the enclosed paper by Professors [redacted] and [redacted] [redacted] from the [redacted] which has been submitted for publication to the HADRONIC JOURNAL.

As you can see, the paper deals with certain aspects of the bag model of hadrons which were developed by you and your collaborators at MIT.

The paper needs a generous refereeing, and I thought that you were the most qualified person for the review.

Thank you for your consideration and time.

Very Truly Yours

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS-mlw

encl.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

February 23, 1983

Professor Ruggero Maria Santilli
Editor in Chief
Hadronic Journal
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Professor Weisskopf asked me to look at the manuscript you recently asked him to referee. It appears to me, from the cover letter accompanying the manuscript, that the authors have not submitted the paper for publication but merely sent your institute a copy of one of their preprints.

Sincerely,

Robert L. Jaffe

RLJ:jm

enclosure

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 4, 1983

Professor ROBERT L. JAFFE
Massachusetts Institute of Technology
Department of Physics
CAMBRIDGE, Massachusetts 02139

Dear Professor Jaffe,

We were suprised in reading your letter of February 23, 1983. The paper by Professors [redacted] and [redacted] has indeed been formally submitted to the HADRONIC JOURNAL as you can see from copy of the formal letter of submission.

Very truly yours,

[redacted]

Editorial Office
HADRONIC JOURNAL

cc: Professor Weisskopf

mlw

# PART V:

# NORTHEASTERN UNIVERSITY

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 5, 1980

Professor ROY WEINSTEIN
Chairman
Department of Physics
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Weinstein,

To confirm our phone conversation of this afternoon, I am interested in a visiting (or guest) position at your department for next academic year, and preferably beginning from June 1980. Besides carrying no salary, the position would merely allow me to use the library-parking-address facilities of your Campus. An office would be appreciated, but it is not essential, as we are organizing the editorial office of the Hadronic Journal here in Cambridge on a permanent basis (on facilities close to Harvard University). No salary is, of course, expected for this visiting (guest) status, and all logistic expenses (xeros, etc.) can be paid from my own research funds.

During this year of visiting (guest) status, I would like to have the opportunity of applying for a research grant from the Department of Energy under the administration of your Department. This grant should cover all my salary and research expenses as principal investigator. It would be a pleasure for me to include interested colleagues from your Department as co-investigators.

As indicated to you since the time of my first visit, Harvard University has kindly provided hospitality for my DOE grant for the past two years. It is appropriate for me to seek hospitality from some other Institution for the continuation of this grant. I understand that the year 1980-1981 is considered for administration of my grant by a non-academic Institution. My visiting status during this period should provide ample time to prepare the research grant application to the satisfaction of your administration.

Thanking you for your courtesy and time, I remain

Yours Very Truly

Ruggero Maria Santilli

RMS/ml

NORTHEASTERN UNIVERSITY

BOSTON, MASSACHUSETTS 02115

ROY WEINSTEIN, CHAIRMAN
DEPARTMENT OF PHYSICS

June 26, 1980

Dr. Ruggero M. Santilli
Harvard University
Department of Mathematics
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli:

This letter is to state more formally what we have discussed by phone.

I invite you to visit our Department for the 1980-81 academic year. It will be a pleasure to have a researcher of your intellectual abilities to add stimulus to our particle theory groups.

The Department will arrange library access, parking, mail facilities, etc., and minor logistic support such as xeroxing. Major logistic support will have to be arranged as you suggest, via use of your research funds.

We will act as an umbrella for proposals you may wish to make which are within our Department constraints (which are minor).

I am fairly sure that we can arrange office space - virtually certain. However, I must review this with our Building Committee since we are a bit crowded, due to recent growth. The office space will probably be an office shared with another visitor.

When you want to start your visit, please tell our Administrative Assistant, Pat Kent, 437-2902, who will get things moving.

Sincerely,

Roy Weinstein

Roy Weinstein
Chairman
Physics Department

RW:pk

cc: Prof. E. von Goeler, Executive Officer

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

July 9, 1980

Professor ROY WEINSTEIN,
Chairman
Department of Physics
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Weinstein,

It is a pleasure for me to accept your invitation to visit your Department for the 1980-1981 academic year, as per your letter of June 26, 1980.

I would appreciate whether your Department can arrange for library access, parking, and mail facilities and let me know at my home address

[redacted]

Your kind offer of supporting xeroxing expenses has been particularly appreciated. All other logistic expenses, such as telephone, etc., will be paid out of my research funds.

In regard to a possible office, on my part I would be glad to bring to your Department the Editorial Office of the Hadronic Journal (of which I am, as you know, the editor in chief). Nevertheless, this would call for a small office anywhere in your building where I can fulfill my duty of confidentiality for consultation (direct or by phone) with referees and authors. In case this small office is not available, I would be glad to share an office with a colleague, but in this case I have to keep the editorial office of the Journal in another location. The communication of an early decision in this respect would be appreciated.

After the initiation of the academic year, I would be interested to deliver an informal seminar course for graduate students (whithout any charge to the Department), along the lines of the course I delivered at Lyman Laboratory in 1978-1979 (see the enclosure), as well as along my research monographs with Springer Verlag. The topic essentially deals with a variety of theoretical means to treat nonpotential forces as they occur in the real world. In principle, such an informal seminar course might be of interest to graduate students in mechanics, space mechanics, plasma physics, engineering, as well as high energy physics. Depending on the receptiveness of the students to this rapidly expanding new sector of research, I can deliver either a summary, one-semester presentation or a detailed two-semesters presentation. Due to the intended informality of the meetings, I am contemplating to contact you in this respect sometime in September for proper authorization and procedure. Nevertheless, in case you desire an outline of the course prior to September, please let me know and I would be glad to prepare it.

My whereabout for this summer is the following. On July 19 I shall leave for Chausthal, West Germany, to deliver an invited talk on the current experimental study for the test of Pauli's exclusion principle under strong interactions at the 1980 Conference on Differential Geometric Methods in Mathematical Physics. I should be back to the Boston area by July 27.

From August 4 to 9 we have the Third Workshop on Lie-admissible Formulations (also for nonpotential forces) which this year will be held at the Faculty Club of the New Harbor Campus of the University of Massachusetts (the meeting room has a beautiful view of the Boston Harbor). This yearly workshop is organized by the Hadronic Journal. Half of the participants are mathematicians and half are physicists. Needless to say, any colleague from your Department who is interested to attend the Workshop would be welcomed.

From August 15 to August 30 I will be back to Europe for working sessions with experimentalists interested in verifying the validity (or invalidity?) for the strong interactions of the conventional physical laws of the electromagnetic interactions.

Beginning from September 1 I should be able to initiate my regular attendance to your Department. I look forward to it with sincere pleasure.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

C.c. Professor E. VON GOELER, Executive Officer
Ms. PAT KENT, Administrative Assistant

NORTHEASTERN UNIVERSITY
360 HUNTINGTON AVENUE
BOSTON, MASSACHUSETTS 02115

COLLEGE OF ARTS AND SCIENCES
OFFICE OF THE DEAN

July 16, 1980

Dr. Ruggero M. Santilli
[redacted]

Dear Dr. Santilli:

Upon the recommendation of Professor Weinstein of the Department of Physics in the College of Arts and Sciences, I am pleased to extend to you our cordial invitation to spend the period of your leave from Harvard University at Northeastern University as Visiting Senior Research Associate of Physics.

Your appointment here will cover the period of August 1, 1980 through June 30, 1981, and carries with it a stipend of $1.00, which will enable you to use the University's library, laboratory, and parking facilities.

I believe that you can make a fine contribution to our program and gain valuable experience at the same time. I look forward to having you with us.

Sincerely,

Richard Astro
Dean

ng

cc: Department of Physics
Dean's Office File
Office of Personnel

HARVARD UNIVERSITY

Area Code 617
495-3352

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

July 18, 1980

Professor RICHARD ASTRO,
Dean
College of Arts and Sciences
Northeastern University
BOSTON, Massachusetts 02115

Dear Professor Astro,

I would like to express the sentiments of my sincere gratitude for your letter of July 16, 1980 and for your confirmation of my visit. It will be a pleasure for me to spend a year at Northeastern.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml

c.c.: Prof. R. Weinstein
Department of Physics

# PART VI:

# VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

HARVARD UNIVERSITY

Area Code 617
495-3352

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

August 30, 1979

Head Search Committee
Physics Department
VPI & SU
BLACKSBURG, Virginia 24061

Dear Committee Chairperson,

I would like to express my interest for the position of Chairman of your Department, as advertised in the August issue of PHYSICS TODAY.

If my qualifications are not suitable for the chairmanship opening, I would appreciate the consideration for a regular faculty opening.

I am currently the recipient of a DOE grant for research in high energy physics that expires on June 1980. To keep continuity on this project, I should apply for an extension sometime during the end of 1979. I would appreciate whether a decision on my case can be reached during this calendar year.

I enclose for your consideration my curriculum, a list of referees, and other informative material on my teaching, research and editorial activities, while I remain at your disposal for any assistance you might need.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

*Blacksburg, Virginia* 24061

DEPARTMENT OF PHYSICS

October 1, 1979

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

Dear Dr. Santilli:

Thank you for your recent application for our Department Head position. We certainly appreciate your interest and look forward to considering your application.

Please find enclosed a "faculty race/sex identification form"; we are required to send you this form and to ask your indulgence in filling it out and returning it to us at your earliest convenience.

With best regards,

CDWilliams

C. D. Williams
Chairman, Head Search Committee

CDW:hh

Enclosure

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

October 15, 1979

Professor C. D. WILLIAMS
Chairman, Head Search Committee
Virginia Polytechnical Institute and State University
Department of Physics
BLACKSBURG, Virginia 24061

Dear Professor Williams,

I would like to express my appreciation for your recent letter acknowledging my application, and for your consideration.

As per your request, I enclose a duly completed "faculty race/ sex identification form", while I remain at your disposal for any assistance you might need. I understand that you have solicited letters of recommendation on my behalf, and I shall abstein from soliciting them on my own. Nevertheless, in case you need any assistance in this respect (e.g., names of additional distinguished scolars), please let me know.

This morning I have separately mailed to Professor MARSHAK copies of my monographs
- "Foundations of Theoretical Mechanics" with Springer-Verlag, and
- "Lie-admissible approach to the hadronic structure" with the Hadronic Press.

Also, I have mailed copies of the reprint volumes
- "Applications of Lie-admissible algebras in Physics" Vols I and II edited by Professors H.C. MYUNG, S. OKUBO and myself, and published by the Hadronic Press.

Copy of my letter to Professor Marshak is enclosed.

During the consideration of my candidacy, please take into consi-detation that I am interested and I would be honored to assume the chairmanship of your Department. As you can see from my curriculum, I have been chairman of the board of director of a Massachusetts corporation for four years. Also, I have organized the HADRONIC JOURNAL, that comprises in its editorial organization truly distinguished scientists, including two Nobel Laureates. Finally, I have numerous years of experience in academic committees. I therefore feel confident that, in case appointed, I can effectively serve your Department, with priority to a genuine loyalty to each individual member, as well as in the continuation of a relaxed and humanly rewarding atmosphere.

Nevertheless, my primary interest is in joining your department as professor of theoretical physics. Therefore, I would appreciate the possibility of being also considered for a normal faculty position. I would like to release to the Search Committe the selection between the consideration for chairman or for a faculty position without a chairmanship appointment.

Finally, during the consideration of my application, I would appreciate whether you can take into account the following aspect. I am currently the recipient of a research grant from the U.S. DEPARTMENT OF ENERGY. The DOE is apparently pleased with the research output of this grant and it is seriously interested in continuing it. Please feel free to contact the DOE officer in charge of my case, Dr. DAVID C. PEASLEE, Tel. 301 353 3624.

This grant expires on June 1, 1980. To keep continuity of grant, I should reapply sometime before the end of this calendar year. It is advisable that this application is filed with my new Institution, in order to avoid the usually long procedure of grant relocation.

In case my application is accepted by your Department, I would then appreciated an appointment without salary for the period December 1979-May 1980; a regularly salaried appointment beginning from June 1, 1980 until September 1, 1980 (the summer period), but with salary entirely covered by my grant; and finally, a regular appointment with teaching functions beginning from September 1, 1980 on.

In particular, the initial appointment from December-1979 to June 1 1980 should be such to allow me to apply for a DOE grant as principal investigator. In case senior members of your Department are interested in joining me in this application, I would be pleased and honored. As a matter of fact, I would like to take this opportunity to stress that I am sincerely interested in establishing the best possible collaboration with any interested member of your Department.

Yours, Very Truly

Ruggero Maria Santilli

RMS/ml

c.c.: Professor MARSHAK

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

October 15, 1979

Professor R. E. MARSHAK, Department of Physics
Virginia Polytechnical Institute and State University
BLACKSBURG, Virginia 24061

Dear Professor Marshak,

As you eventually know, I have recently applied for a position at your Department, following a suggestion by Professor OKUBO. Copy of my recent letter to Professor WILLIAMS is enclosed. Please note in this letter that I would be honored to assume the chairmanship. Nevertheless, I am also interested in joining your department in a regular faculty position. I would like to release to your Department the selection of the position for the consideration of my candidacy which is most appropriate.

In this letter I would like to take the liberty of outlining a rather intriguing research program, which is rather feverishly pursued by a number of mathematicians and physicists. In particular, the mathematicians of our group are

- Professor H.C.MYUNG of the University of Northern Iowa (currently on leave in Korea); Professor TOMBER of the Michigan State University; Professor OEHMKE of the University of Iowa; Professor WENE of the University of Texas at San Antonio; and others.

The physicists actively involved in our line of study are

- Professor OKUBO of Rochester, Professor KTORIDES of the University of Athens (currently spending his sabbatical as my guest here at Harvard), Professor CONSTANTOPOULOS also of the University of Athens in Greece; Professor KOBUSSEN of the Institut für Theoretische Physik der Universität Zürich; Professors CANTRIJN and SARLET* (the latter spent his sabbatical of 1978-1979 as my guest here at Harvard); Professors FRONTEAU and TELLEZ-ARENAS, of the Université d'Orléans in France; Professor SALINGAROS of the University of Massachusetts in Boston; Professor ELIEZER and his group at the La Trove University in Australia; Professor LÕHMUS of the USSR Academy of Science; and Professor JIANG CHUN-XUAN of the People's Republic of China.

The number of mathematicians and physicists joining our research is expanding in a quite promising way. The efforts are coordinated via (A) the conduction of a yearly Workshop, Called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first was held on August 1978 at Harvard; the second was held in August 1979 also here at Harvard; and the third is scheduled for August 1980). (B) the organization of a mathematical conference on

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* of the Instituut voor Theoretische Mechanica of the Rijksuniversiteit Gent, Belgium.

Lie-admissibility (the first is scheduled for the spring 1981, upon return of Professor Myung from Korea); and (C) the coordination of the studies via the HADRONIC JOURNAL, of which, as you are eventually aware, I am the founder and editor in chief.

To keep a record of research, all contributions are reprinted in a yearly series entitled "APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS". edited by Professor MYUNG, OKUBO and myself. Volumes I and II were printed in 1978. We are currently working on two additional volumes, the first comprising all studies by mathematicians and physicists up to 1978 (including a historical paper by ALBERT in which he introduced the notion of Lie-admissibility), and the second comprising all contributions in 1979. The proceedings of the second workshop will be published in the December issue, 1979.

In addition to articles I have written and I am writing in collaboration with mathematicians and physicists, my personal contributions in this line of studies are complemented via the following research monographs:

- "Foundations of Theoretical Mechanics", Volume I (1978) and II (in press) published by Springer-Verlag, Heidelberg, under the series "Texts and monographs in physics" edited by Professor BEIGLBÖCK, and
- "Lie-admissible approach to the hadronic structure", Volume I (1978), II( in press) and III ( scheduled for 1980), published by the Hadronic Press, Nonantum, Massachusetts in the series "Monographs in theoretical physics".

I have taken the liberty of mailing you, by separate parcel, copies of all printed volumes. Copies of the typescripts of the volumes now in print are at your disposal.

I have also mailed to you, separately, a "chart" of my Volume II with Springer-Verlag, which essentially outlines, in a way understandable by graduate students, the technical treatments of our Second Workshop. (the proceedings of this workshop will be available in 1980 and I shall mail you a copy soon thereafter). Also I have mailed to you copy of a paper entitled "An intriguing legacy by Albert Einstein: the possible invalidation of quark conjectures" which was intended to stimulate a moment of reflection on current trends in hadron physics (this paper was distributed world wide in some 15,000 copies). The paper has been accepted for publication by FOUNDATIONS OF PHYSICS, and I am working to finilize it, with the inclusion of numerous comments and generous help I received, including by quark committed physicists, as well as with the editorial assistance of Professor BIEDENHARN.

As you can see, the material which has already accumulated on these studies is considerable and expanding considerably. Hoping that I do not take too much of your time, in this letter I would like to provide an outline of the essential aspects. Please consider my presentation as informal as possible (I simply did not have the time to rewrite this letter up to all the necessary maturity- so please kindly excuse any

insufficiency and deficiency). I hope in this way you may have an idea of what we are doing, without spending too much of your time in filtering it through the existing literature. Also, please consider this presentation as my personal view. Our colleagues and friends working on this line may indeed have a different perspective.

THE HISTORICAL VOICES OF DOUBT ON THE TERMINAL CHARACTER OF THE CURRENTLY USED BASIC LAWS FOR THE STRONG INTERACTIONS. When I decided to initiate active studies on the strong interactions during my graduate studies at the University of Torino, Italy, I decided to give priority to the identification of the teaching by the Founding Fathers of contemporary physics. I identified in this way a number of authoritative, historical, voices of doubt, that are certainly known to you.

FERMI made it quite clear in his teaching and historical notes "Nuclear Physics" that he did not believe in the validity of conventional geometries (the symplectic and the Riemannian, in our current language) in the region of space "occupied by a strongly interacting particle". To my reconstruction via correspondence with his colleagues, he thought that the strong forces are local and non-derivable from a potential, as an approximation of expected non-local settings. In this way, a conventional Hamiltonian description did not exist, in his view, for an effective representation of the strong interactions. This, in turn, would imply the lack of existence of conventional Lie algebras at the level of the time evolution law, both classically and quantum mechanically. Still in turn, this would call for the inapplicability of conventional relativity and quantum mechanical laws, thus callig for the courageous construction of broader methods, relativities and laws, specifically conceived for the physical arena considered.

EINSTEIN is well famous for his lack of belief in the terminal character of the undeterminacy of quantum mechanics, which he kept up to his death. As you certainly know, this historical occurrence ( "Einstein legacy" in my language) is touchingly reported in Heisenberg's memoires "Physics and Beyond", pp. 79-81). In Heisenberg's words, Einstein could at most tolerate conventional quantum mechanics as a "temporary expedient".

JORDAN is equally famous for his doubt on a central mathematical structure of quantum mechanics, in our current language, the universal enveloping associative algebra of a Lie algebra. He suggested a broadening of this structure which resulted in the now celebrated Jordan algebras.

PAULI made it quite clear in his historical papers and lectures that his principle had been conceived under the conditions of lack of overlap of the wave packets, that is, the atomic structure. When the wave packets did indeed overlap, he was simply unable to establish the totally antisymmetric character of the wavefunction because of the emergence of "stronger" forces which would prohibit even the separability of the wave function, in general, let alone the identification of its totally antisymmetric character (for fermions).

WIGNER also made it quite clear in his memoir "Symmetry and Reflections" that there are fundamental open problems on the notions of interations in general, let alone the strong.

VON NEUMANN joined Jordan and Wigner in these doubts, in their celebrated paper of 1934.

My list of historical, autoritative, voices of doubt could continue.

THE ROLE OF THE HADRONIC JOURNAL. When I decided to organize the HADRONIC JOURNAL in January 1978, the situation was essentially the following. We had all these numerous, authoritative, historical voices of doubt on the applicability to the strong interactions of tools, laws, and insights conceived specifically for the electromagnetic interactions. Nevertheless, we had:

- no coordinated effort aiming at an in depth study of these legacies;
- no initiation of the studies for the possible construction of more general mathematical tools and physical laws for the strong interactions; and, most importantly,
- no initiation of studies aiming at the formulation of experiments on the resolution of these legacies.

I am happy to report to you that, thanks to the participation by numerous mathematicians and physicists, the HADRONIC JOURNAL has made valuable contrinutions in each of these aspects, as I shall outline below.

Also, at the time of the founding of the HADRONIC JOURNAL we had the known of proliferation of papers along quark lines. Nevertheless, as admitted by a number of qualified quark supporters, these models are faced with rather fundamental, open problems, such as (1) the lack of identification of the quarks with physical particles (despite Fairbanks' indication of fractional charges); (2) the yearly multiplication of different, unidentified quarks, as forced by new experimental data; (3) the lack of achievement of a genuine form of confinement which prohibits, according to clearly proved and explicitly stated calculations, decays of the type $\pi^{\circ} \longrightarrow q\,\bar{q}$ (NAMBU has clearly stated this deficiency at his speech in Israel in connection with Einstein's celebration); etc.

The priority which I have given to the HADRONIC JOURNAL vis-a-vis with this situation is that of considering specific models of structure of hadrons as of purely secondary physical value. Instead, utmost priority is given to the experimental and theoretical finalization of the basic physical laws. The problem, in my view, is that only after achieving a finalization of these laws, studies on hadron structure can acquire a solid ground of scientific credibility, whether these studies are of quark orientation or not.

In conclusion, the HADRONIC JOURNAL has initiated a process of critical examination of the basic laws currently used in hadron studies, which

takes in full account the teaching by the Founding Fathers of contemporary physics, and which is primarily devoted to the achievement, in due time, of their experimental resolution, either in favor or against.

THE CONCEPTUAL OUTLINE OF THE STUDIES CONDUCTED AT THE HADRONIC JOURNAL.

Our starting point is Fermi's teaching. All hadrons have approximately the same "size" which coincides with the "range" of the strong interactions. Thus, as a necessary condition to even activate the strong interactions, hadrons must enter into a state of penetration of their wave packets. By using similarities with the atomic context, the same situation is then expected for the hadronic constituents, that is, they are in a state of mutual penetration of the wave packets during the life of the system. This produces a view of the structure of hadrons which is fundamentally different than that of the atomic structure (in which no appreciable overlapping of the wave packets occurs). According to an established physical line, fundamentally different physical arenas generally imply differences in the dynamical behaviour and in the physical laws.

Most of the technical efforts for Lie-admissibility are centered in the identification of the implications of such a condition of overlapping of the wave packets. That is, we have taked Fermi teaching seriously, and worked out, as much as possible, its implications.

Predictably, Fermi teaching turned out to be fully correct. A recently proved no-go theorem on (pre)symplectic quantization essentially establishes that the symplectic (and Riemannian) geometry do not produce a consistent quantization under forces more general than $f = -\partial V/\partial r$, as expected by the overlapping of the wave packets. This theorem has been studied at the recent second workshop, and it will be considered in detail in the proceedings. It essentially turns out that Heisenberg's equations are inconsistent for the arena considered.

The inconsistencies are apparently removed via the replacement of the conventional Lie algebras with their algebraic coverings called Lie-admissible algebras. The enlargment of the product then allows the direct representation of broader forces (variationally non-self-adjoint), thus directly allowing the treatment of the conditions of overlapping of the wave packets.

Additional theorems have established the "universality" of the Lie-admissible character of the time evolution law, classically and quantum mechanically, for the representation of the broader systems considered. Apart technical aspects, what we have is a return to the analytic equations originally conceived by Hamilton and Lagrange, those with external terms, (representative precisely of these broader forces). Permit me to stress, as I did it in my monographs with Springer-Verlag, that the "Lagrange's equations" and "Hamilton's equations" currently referred to in the con-

temporary physical and mathematical literature are not those conceived by the Founding Fathers of Analytic Mechanics; instead, they are in the "truncated" version without the external terms. In different terms, Lagrange's and Hamilton had an analytic vision substantial broader then that of their followers in high energy physics, who restrict the basic equations in their "truncated" form, and attempt the representation of reality via the simplistic structure $L = L_{free} + L_{int}$. Notice that this situation occurs at all levels of contemporary condution of conventional studies, up to the most advanced technical developments, such as non-abelian gauge theories and QCD. We simply have $L = L_{free} + L_{int}$. Nothing more. I am confident you will see that Fermi's vision on the complexities of the strong interactions is fully in line with Lagrange's and Hamilton's vision of Analytic Mechanics. Within this context, current trends emerge as a a first approximation.

Einstein's legacy has also been taken seriously and worked out to the best possible details. It essentially turns out that, when the wave packets do not overlap, the forces are variationally self-adjoint (action at a distance forces only). Then, the conventional uncertainty of quantum mechanics applies in full. Thus, Bohr and Heisenberg were indeed right in refuting Einstein's counterexamples (all based on this type of force). Nevertheless, when Fermi's point is taken in full consideration, Einstein's vision does indeed emerge as being true, to our best understanding. In simple words, the time evolution under nonselfadjoint forces is necessarily nonunitary. This implies that, assuming Heisenberg's indeterminacy principle is valid at a given time, it is not valid at a later time. In actuality, the no-go theorem of symplectic quantization prohibits the consistent formulations of all the algebraic ingredients for the formulation of the conventional Heisenberg's principle, let alone to establish it on physical grounds. In conclusion, Fermi's and Einstein's visions turned out to be deeply interrelated.

Jordan's legacy has also been taken seriously. As a matter of fact, his teaching is at the foundation of the physical and mathematical use of the Lie-admissible algebras. Indeed, in order to treat forces more general than $f = -\partial V/\partial r$, we are forced to an enlargment of the basic envelope of quantum mechanics, from an associative Lie-admissible form (the conventional one) to a nonassociative, but still Lie-admissible form. In particular, this form turns out to be Jordan-admkssible, thus including Jordan's view in its entirety. But, once the basic envelope is enlarged, conventional relativities and principles of quantum mechanics are forced to leave the way to broader views of Lie-admissible (rather than Lie) character. Thus, Fermi's, Einstein's and Jordan's visions turned out to be deeply interrelated.

Pauli's teaching has also been taken seriously by us. It turns out that a system of identical fermions, when it penetrates hadronic matter (e.g., a star) is subjected to broader forces whose time evolution law is nonunitary. Thus, assuming that they are fermions at a given value of time, they do not preserve this statistical character in time. The lack

of preservation of this statistical character then implies the inapplicability of Pauli's principle under the conditions considered.
Again, the teaching of all Founding Fathers of contemporary physics, which were seemingly independent and unsubstantiated in early 1978, turned out to be deeply related and fully substantiated, provided that one abandons the point-like abstractions of hadrons, and confronts their experimentally established, extended character.

Upon identification of this situation, a number of sequential studies was then implemented. Here I am forces to be sketcy, to avoid a prohibitive length.

In essence, I believe that a prerequisite for the proper formulation of the problem of structure of hadrons (let alone its consistent treatment) is the achievement of a mathematically and physically consistent notion of particle under joint electromagnetic and strong interactions. The physical consistency is inferred from the expected necessary condition of overlapping of the wave packet with that of other particles.

Once the problem of particles (or constituents) is approached from this profile, a rather fundamental differentiation with the conventional notion of particle under elm interactions (the <u>only</u> experimentally destablished today) emerges. Dirac's equation for the peripheral electron of the hydrogen atom characterizes a non-conservative system (or an open system), trivially, because the elm field is external (that is, Dirac's equation does not describe the closed, two-body, system composed by the electron and the proton, but only the electron in the external field of the proton). The force in this case is variationally selfadjoint. Most crucial is the property that, in this case, the SU(2)-spin part of the Poincaré symmetry is an exact symmetry for the electron. This allows the identification and technical treatment of the fact that the electron, under these circumstances, is a fermion.

In the transition to a particle under elm and strong, with the condition of overlapping of the wave packets, the physical profile is profoundly altered. We still have the characterization of an open system , in exactly the same measure as that of Dirac's equation . But the condition of overlapping of the wave packets implies the additional presence of the broader non-self-adjoint forces. Studies have indicated that these forces imply the <u>breaking of the SU(2)-spin symmetry</u>. As a result, the conventional notion of intrinsic quantities, as currently known for the elem interactions, become inapplicable under the conditions considered. Instead, we see the direct applicability of the covering Lie-admissible algebras, and, in particular, of the covering SU(2)-admissible algebra. In turn, this provides the technical characterization of a covering notion of the intrinsic quantities which are expected under the conditions considered (which is feverishly under study).

<u>All</u> our studies possess a direct Newtonian counterpart under the correspondence principle. Thus, you can visualize this occurrence in the

physical spinning top, that is, not the conventionally treated top with an exact SU(2) symmetry and the consequential realization of the perpetual motion. No. Instead, the terms" physical spinning top" are referred to the system actually occurring in our environment, with drag torques responsible of the decay in time of the angular momentum. The breaking of the SU(2) symmetry is then necessary to comply with this Newtonian evidence. Upon Lie-admissible quantization, the situation persist conceptually, apart a number of technical implementations. What is important is that a covering SU(2) admissible treatment of the broken SU(2) symmetry exists at the Newtonian level of the physical spinning top, and persists in its entirety for the description of an extended hadron (say, a proton) within dense hadronic matter (say, the core of a neutron star).

In conclusion, we are feverishly working at a covering notion of particle under conditions of overlapping of the wave packets, and broader forces. This activates the applicability of the Lie-admissible formulations, thus allowing a technical treatment of the problem. The emerging notion is a covering of the conventional one because the Lie-admissible algebras are capable of reducing themselves to the conventional Lie algebras. This algebraic property is physically interpreted as the condition of exiting a hadronic medium, with the consequential null value of the strong forces. In other words, consider a scattering process of a hadron within hadrons e.g., a hadron "passing through" a nucleus . Then, three stages are identifiable according to this view: (A) the approching motion to the nucleus- the motion is in vacuum; no overlapping of the wave packets occurs; and conventional relativities and quantum mechanical laws of Lie algebraic character hold; (B) motion through the hadronic matter- state of overlapping of the wave packets; emergence of the nonselfadjoint forces and consequential treatment via the covering, Lie-admissible, relativities, and quantum mechanical laws; (C) exiting of the hadronic matter- return to the motion in vacuum without overlapping of the wave packets- the Lie-admissible algebras collapse into their Lie particularization and all conventional relativities and quantum mechanical laws are recovered, apart secondary processes (emission of neutrinos, and others).

This broader setting was then applied to the construction of the rudiments of a new model of structure of hadrons. The key idea is to achieve a model in which the constituents are physical particles. The covering Lie-admissible formulations essentially allow the possibility that the hadronic constituents are physical particles simply produced <u>free</u> in the spontaneous decays. They simply perform the transition from generalized forces, laws and formulations, to the conventional ones (apart, again, secondary effects). Notice that this simple view is strictly prohibited by conventional, Lie, relativities, and laws.

I have no words to stress the fact that these studies are at the very beginning and a truly long way remains to be covered. What we have done is merely identify the technical problems and conduct a preliminary study for their solution. One thing has transpared clearly: if one takes seriously the legacies by Fermi, Einstein, Jordan, Wigner,

von Neumann, Pauli and others, the possibility of genuine, non-incremental advancements of the foundations of our current theoretical knowledge is possible. This is the vision of the "scientific renaissance stimulated by the strong interactions" I dared to indicate in my paper "An intriguing legacy by Albert Einstein...".

The following words by Heisenberg here come to my mind:

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

To which he adds soon thereafter

> " However, when it comes to 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."

THE EXPERIMENTAL PROFILE. Again speaking on personal grounds, all these efforts are devoted to the experimental resolution of the validity (according to some ) or the invalidity (according to others) of the conventional relativities and laws for the strong interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and Heisenberg's indeterminacy principle .

I believe that the fundamental ethical rule of our profession is the separation of facts from beliefs, and the resolution via experiments of diverging theoretical views. When referring to fundamental issues, such as the validity or invalidity of basic laws for the strong interactions, the implementation of this ethical rule becomes mandatory.

In particular, we are living in a period with a cloudy economic future of our society. Basic, open, issues of this type have a subtle, but potentially crucial role for energy-related profiles. Despite opposing views by a number of colleagues, I see the controlled fusion as nothing more than the laboratory construction of a bound state of hadrons.
The problem of the basic laws has indeed a crucial character. When considering the high temperatures and pressures of the controlled fusion, the condition of overlapping of the wave packets appears, to me, unavoidable. In conclusion, apart scientific profiles, I believe that we simply cannot affort the luxury of overlooking issues of this type.

If nothing else, I see the need of these experimental resolutions as a tribute to the Founding Fathers of contemporary physics, that is, as a concrete expression of the fact that their teaching of broader scientific visions has not been replaced by the easy acceptance of familiar doctrines.

The studies I have outlined here are all based on the conception of the invalidity of conventional settings for the strong interactions. I would like to stress that this is solely motivated by the intent of stimulating their experimental resolution. The rationale is quite simple. I believe that, until somebody indicates the plausibility of the violation,

the scientific community has full reason to continue its dormient attitude and the easy preservation of the status quo.

In conclusion, all efforts (at least mine) are solely intended for these experimental resolutions. We are currently working at a number of experimental tests and it is understood that much work remains to be done. The experimental proposals currently available are essentially the following.

- I have proposed (Hadronic J. 1, 574 (1978)) the experimental verification of Pauli's principle in nuclear physics to the effect of ascertaining whether the principle is valid in nuclear physics in the same quantitative amount as that in atomic physics, or small deviations exists, can be experimentally established, and have escaped inspections until now. The nulei recommended for this test are those whose volume is below the proportionality rule with the total number of nucleons. For these nuclei, nucleons must be in a statistically small state of penetration of their wave packets, thus activating the breaking of the SU(2)-spin symmetry in a very small measure, the emergence of forces non-derivable from a potential added to conventional nuclear forces with very small coefficients, and the Lie-admissible covering formulations of spin and of the time evolution law of the individual nucleons. The experiment is suggested via low energy scatterings of hadrons in the nuclei selected, and it is essentially aimed at the establishing of the totally antisymmetric character of the wave function either in an exact measure, or only as an approximation , owing to these conceivable departures. They, incidentaly, are quantitatively similar to the historical case of parity violation in the weak interactions. We simply have the transition from the breaking of the discrete part, to that of the space-time part of the Poincaré symmetry. According to the view of experimental nuclear physicists, the experiment is feasible with current technology.

- Kim (Hadronic J. 1, 1343 (1978)) has independently suggested more accurate measurements of the mean life of unstable hadrons in flight. If the structure forces are non-local and approximated via nonselfadjoint forces, deviations from the values predicted by the special relativity are expected. These deviations are significant (of the order of 12 % at certain energies). Thus they are detectable with current technology, according to the view of a number of physicists.

These two proposals, even though seemingly non-related, have in actuality exactly the same dynamical basis. In both cases the mechanics of breaking of conventional settings is due to broader forces. Thus, these two proposals simply deal with complementary views of the same physical arena.

A GUIDE TO THE LITERATURE ON LIE-ADMISSIBILITY. As indicated earlier, the literature on these studies is considerable and expanding rapidly. We have until now material for some nine volumes of research contributions (four printed, two in press and three forthcoming). I am fully aware that you do not have the time to inspect this material. I am therefore taking the liberty of suggesting a reading program, as an indicative guide, with

a progressive increase of time committment.

Suggested first reading. For a general outline of the quantitative aspects treated in this letter, I suggest the reading of "Chart 4.9" of my volume II with Springer-Verlag, which has been enclosed in the material mailed separately to you.

Suggested second readings. After the introductory first reading, the second depends on the line you desire to inspect in more detail.

- For the notion of "nonselfadjoint interactions" the reading of my two volumes with Springer may be advisable;
- for the classical universality of Lie-admissible algebras, I suggest the reading of my articles Hadronic J. 1 223 (1978), and 1, 1279 (1978).
- for the application to a new model of hadron structure, I suggest the reading of my article Hadronic J. 1, 574 (1978) and the paper by Jiang Chun-Xuan (also mailed to you separately).

Suggested third readings. The notion of hadronic constituent we are working on demands the knowledge of the totality of the studies, most importantly, of the mathematical contributions by mathematicians. My suggestions for an in depth study of this notion calls for the reading of the entire literature, that is, steps one and two, plus

- the outstanding articles by Okubo, Myung and others on Lie-admissible algebras (reprinted in the volumes mailed to you);
- the study of the mathematical and physical literature prior to 1978 (currently under reprint); and, most importantly,
- the study of the proceedings of the Second Workshop on Lie-admissibility (which is now in press).

In case I can be of any assistance, please do not hesitate to call me (Office no. (617) 495 3352; home no. (617) 969 3465).

Hoping that I did not abuse of your time and courtesy with this long (passionate) letter, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml

c.c. Professor WILLIAMS, VPI &SU

THIS LETTER WAS NEVER ACKNOWLEDGED BY MARSHAK.

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

*Blacksburg, Virginia* 24061

DEPARTMENT OF PHYSICS (703) 961-6544/6545

May 5, 1980

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

Dear Dr. Santilli:

At long last our search for a department head has ended, and it gives me great pleasure to announce that Dr. Alexander Abashian will be our new Head. Dr. Abashian is currently Program Director for Elementary Particle Physics at the N.S.F., and he will join us in September 1980.

The pool of applicants for our head position was one of exceptional quality by any standard, and you have certainly honored us by your interest and willingness to apply. The selection process unfortunately consumed a long span of time, about twice as long as we initially expected, and we both apologize for it and extend to you our appreciation for your patience in awaiting its development.

With best regards,

CDWilliams

C. D. Williams
Chairman, Head Search Committee

CDW:hh

COLLEGE OF ARTS AND SCIENCES

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

*Blacksburg, Virginia* 24061

DEPARTMENT OF PHYSICS (703) 961-6544/6545

May 26, 1980

Dr. R. M. Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santili:

I am writing in response to your recent telephone call concerning the possibility of some kind of faculty position for you in the Physics Department at Virginia Tech. I am sorry to report that, after a fairly extensive assessment of the situation, I find that there appears to be no possibility of such a position for you here, at least in the near future, despite the likelihood of your salary being provided by your current grant. The problem is two-fold: there seems to be only tangential interest at most among our faculty concerning your indicated research interests, and faculty openings for the next several years here in physics are likely to occur only by retirement or other modes of displacement, if then (i.e. we are given to understand that our number of physics faculty positions will not only probably not grow but may in fact decrease in the 1980's).

In any case though we appreciate your interest and wish you all good fortune in your future associations.

Yours sincerely,

CDWilliams

C. D. Williams
Associate Head

CDW:let

# PART VII :

# INSTITUTE FOR THEORETICAL PHYSICS, UNIVERSITY OF CALIFORNIA AT SANTA BARBARA

**Institute for Theoretical Physics**
**University of California**
**Santa Barbara, CA 93106**

July 27, 1979

Professor Ruggero Maria Santilli
Department of Mathematics
Harvard University
Cambridge, MA 02138

Dear Professor Santilli,

Your letter applying for a position at Santa Barbara associated with the Institute for Theoretical Physics has been received and given to the search committee of our advisory board for their consideration.

We appreciate your interest in the Institute for Theoretical Physics and in Santa Barbara.

Sincerely yours,

Douglas J. Scalapino
Acting Director

pm

HARVARD UNIVERSITY

Area Code 617
495-3352

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

August 14, 1979

Professor DOUGLAS J. SCALAPINO,
Acting Director,
Institute for Theoretical Physics
University of California
SANTA BARBARA, California 93106

Dear Professor Scalapino,

I would like to express my appreciation for your letter of July 27, indicating reception of my application to join your Institute.

We have just complited the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, held here at Harvard from August 1 to 9.

I believe that the search committee might be interested to know the outcome of this meeting. I am therefore taking the liberty of sending you copy of my informal report to the DEPARTMENT OF ENERGY. As you can see, it is informative, although it is written in a friendly, non-formal language.

I would appreciate whether the search committee can consider this disclosure as confidential.

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encl.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

July 21, 1979

Professor Walter Kohn, Director,
Institute for Theoretical Physics
University of California
Santa Barbara, California 93106

Dear Professor Kohn,

following your announcement in the July issue of PHYSICS TODAY, I am here applying for a permanent position at the NSF Institute of Theoretical Physics. I enclose my resume, as well as informative material on my recent research, teaching and editorial functions. I am currently the recipient of a grant from the Department of Energy that covers my full salary until June 1, 1980. I am available for relocation beginning from September 1979. Subject to approval by the Department of Energy, my possible appointment would require no salary during the first year. The following elements may have some value for the consideration of my candidacy.

Editorial. I am the founder and editor in chief of the HADRONIC JOURNAL, and I would like to continue this editorial function at my new Institution. Under the guidance of an Editorial Council of distinguished scolars, we are attempting a dialysis between mathematics and physics, with priority on the presentation of advanced mathematical tools of direct or potential relevance for hadron physics. Jointly, we are also attempting a contact with the engineering community, because methods conceived for the treatment of strong interactions as nonderivable from a potential have already seen their application in engineering problems (trajectory problems with drag forces, electric circuits with internal losses, etc.).

The Journal has completed Volume 1, 1978 for a total of 1,603 pages, and Volume 2, 1979 is at an advanced stage. The Journal is published bimonthly. We have a record of publishing each and every issue on schedule. Our Journal has also a good record of rapidity of publication. The current average between reception of an article and its distribution (including refereeing, printing, and mailing) is of 48 days. Finally, our journal is conceived to promote scientific values with the minimal possible burden to the authors. Indeed, we have no publication charges, no restriction on length, and no editorial restrictions on footnotes, figures, etc.

My personal attitude in the conduction of this Journal is to attempt the presentation of a well balanced research on hadrons, in which all valuable lines of study are equally considered, whether of quark or non-quark inspiration. To be specific, I respect studies on quarks; I favor their continuation; and I often invite papers on quarks. Nevertheless, I oppose the restriction of studies on the fundamental problem of hadron structure

along quark lines only. This editorial attitude appears recommendable because of the known, rather numerous, problematic aspects of quark models.

In case of my appointment at your Institute, you can count on my best possible efforts to review my editorial conduction of the HADRONIC JOURNAL in such a way to achieve compatibility with your Institute, if at all needed. In any case, we are continuously expanding our editorial organization, and we would be happy to add interested members of your Institute.

Conferences. I am the organizer of a yearly workshop on the so-called Lie-admissible formulations. The first meeting was held here at Harvard on August 1978, and the second meeting will be held here from August 1 through 4, 1979. In case of my appointment at your Institute, I would like to have the opportunity of continuing this workshop.

The primary lines of studies essentially consist of the attempts by mathematicians and physicists of generalizing Lie's theory via the covering Lie-admissible algebras. At the mathematical level we have identified an intriguing array of open mathematical problems ranging from the generalization of Cartan's classification, Lie's theorems, representation theory, to the treatment of the geometry underlying the covering Lie-admissible algebras (which is not of symplectic nor of Riemannian type). These problems are attracting an increasing number of mathematicians in the field. At the physical level, the analytic equations originally conceived by Hamilton, those with external terms, have resulted to possess a Lie-admissible (rather than Lie) structure. As a result, Lie-admissible algebras have resulted to be "universal" in Newtonian Mechanics, that is, capable of characterizing the brackets of the time evolution law under arbitrary forces (of class $C^1$). These forces generally imply the breaking of conventional symmetries. Thus, Lie-admissible algebras have emerged as valuable for the treatment of broken Lie symmetries, that is, via the replacement (technically realized via embedding at the level of the enveloping algebras) of the Lie algebra with the covering Lie-admissible algebra. The departure from the Lie product is then directly representative of the symmetry breaking forces. These ideas have been applied to a number of cases, such as the breaking of SU(3) under strong interactions and the construction of the Gell-Mann-Okubo mass formula, or the breaking of the SO(3) symmetry of the spinning top (to avoid perpetual-motion-type of abstractions) due to drag torques.

At the quantum mechanical level, the Lie-admissible formulations have identified an additional array of intriguing, open problems of both mathematical and physical nature. In essence, we have identified the existence of a mapping from functions to operators which preserves the Lie-admissible (rather than Lie) algebras. As a result, the Lie-admissible algebras appear to be intriguing for the problem of quantization of variationally nonselfadjoint forces (nonderivable from a potential). At the mathematical level, this has stimulated a number of issues, ranging from geometric quantization to spectral decomposition, to the use of nonunitary transformation, etc. At the physical level, this situation is stimulating a number

page 3.

of intriguing problems, ranging from dissipative nuclear processes, to quark confinement, to the problem of the structure of the strong hadronic forces.

As you can see from the wording of the enclosed announcement, this workshop is inspired by the desire to avoid monopolistic conductions of research, by soliciting the partecipation of researchers of different beliefs or orientations. The workshop is also intended to attempt the conduction of research without prejudices as much as humanly possible. Finally, the workshop is strictly devoted to the study of fundamental, nonincremental, open problems (research of minute incremental character is gently referred to other meetings).

Research. I am involved in a long term research project concerning the experimental verification of the validity (according to some) or invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The issue appears to be related to the problem whether the strongly interacting particles can be consistently subjected to the conventional point-like abstractions, or a genuine representation of their actual, extended, size is needed. In turn, this is related to the issue whether the strong forces are of the simplistic structure of current use, $f = - \partial V / \partial r$ (variationally selfadjoint), or structurally more general forces (variationally nonselfadjoint) are needed. In the former case conventional laws and principle are expected to be valid. In the latter case, suitable coverings of these laws are expected to apply.

The search for achieving maturity of formulation of these problems is conducted via: (1) a series of monographs I am publishing with Springer-Varlag and the Hadronic Press; (2) research papers in collaboration with mathematicians and physicists; (3) the coordination of efforts by independent researchers via the HADRONIC JOURNAL; (4) contacts with qualified colleagues (experimentalists, in particular); and (5) the yearly workshop on Lie-admissibility. All valuable contributions are now reprinted in a yearly series of volumes by the Hadronic Press (the first two volumes have been printed in 1978, and we are now working on two additional volumes). Copies of the first two volumes are at your disposal.

This project has now completed the initial (predictably nebulous) orientational phase, and it is now entered into the second phase of technical treatments of specific issues by mathematicians and physicists. It is understood that a long way remains to be yet covered. Irrespective of the future outcome, permit me to confess that I am personally fascinated by the some many and so intriguing open problems which are created by the mere attempt to formulate the experimental verification of basic laws for the strong interactions. For instance, it has been for me rewarding to see that methods conceived for strong, variationally nonselfadjoint

page 4.

interactions have already seen their application to a number of problems in engineering (you may consult my monographs with Springer-Verlag in this respect, copies of which are at your disposal upon request).

I believe that the backing of your Institute to the pursue of this fundamental physical problem would be scientifically invaluable. It is in this spirit that I submit my candidacy.

A list of references is enclosed. Please feel free to contact directly the colleagues indicated, as well as, in addition, any member of the Editorial Organization of the HADRONIC JOURNAL.

In case you desire that I solicite letters of recommendation, please let me know. I remain at your disposal for any additional assistance you might desire.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

# PART VIII:

# UNIVERSITY

# OF

# CALIFORNIA

# AT

# BERKELEY

HARVARD UNIVERSITY

Area Code 617
495-3352

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

July 21, 1979

Professor GEOFFREY F. CHEW,
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

I am here applying for a faculty position at your Department commensurate to my qualifications. I enclose for your consideration my resume, as well as additional, informative, material on my recent teaching, research and editorial activities.

I am currently the recipient of a grant from the Department of Energy that covers my full salary until June 1, 1980. I would like to have the opportunity of applying for the renewal of this grant as principal investigator, either alone or jointly with interested colleagues. As you know, I am also the editor in chief of the HADRONIC JOURNAL, and I would like to continue this editorial function at my new Institution. Finally, I am the organizer of a yealy workshop on the studies by mathematicians and physicists of the Lie-admissible algebras, and I would like to have the opportunity of continuing this workshop.

As you eventually know, I am involved in a long range research project concerning the experimental verification of the validity (according to some) or the invalidity (according to others) of conventional physical laws for the strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.

The search for achieving maturity of formulation of these problems is conducted via (1) a series of monographs I am publishing with Springer-Verlag and the Hadronic Press; (2) research papers in collaboration with mathematicians and physicists; (3) the coordination of the efforts by independent researchers via the Hadronic Journal; (4) contacts and correspondence with qualified colleagues interested, but not yet formally committed to the problems; and (5) the yearly workshop on Lie-admissibility. All valuable contributions on the problems considered are now reprinted in yearly volumes published by the Hadronic Press (the first two volumes appeared in 1978 and we are now preparing two additional volumes).

The ultimate issue appears to be related to the problem whether strongly interacting particles can be consistently subjected to point-like abtractions, or representations of their extended size are necessary. In turn, this appears to be related to the problem whether the strong interactions can be well approximated with the current simplistic forces $f = -\partial V/\partial r$,

page 2.

or structurally more general forces are needed. A rather feverish research activity is now going on on these issues, and ranging from the methods for the treatment of local nonselfadjoint forces (i.e., nonderivable from a potential), to conceivable coverings of the relativity and quantum mechanical laws of the electromagnetic interactions and related point-like approximations. In particular, a new line of research has been lately initiated. It consists of the identification of the scattering theory and related tools of experimental relevance for forces more general than $f = - \partial V/\partial r$. It is understood that these studies are at the very beginning, and a long way remains to be yet covered.

I am interested in joining your Department because of its close connection with the Lawrence Berkeley Laboratory. In essence, the initial, predictably nebolous phase of identification of the existence of the problem appears now to be completed, and we have entered into the second phase of technical treatments of the individual issues by mathematicians and physicists. In order to effectively continue my studies, both as a researcher and as an editor, I need an exposure also to the experimental community. Such an exposure appears to be essential to focus the research into a form suitable for the future, actual, conduction of experiments.

Permit me the liberty of touching the delicate issue of quarks vis-a-vis with these studies. You might be aware that I have launched a moment of reflection on quarks, via recent articles and forthcoming papers, when interpreted as actual structure models (and not when used for Mendeleev-type classifications). I would like to stress here that these initiatives are solely motivated in the hope of performing a scientific function. I respect studies on quarks; I favor their continuation; I routinely accept them for publication as an editor; and I often invite papers on quarks. Nevertheless I oppose the restriction of research on the fundamental problem of hadron structure to quark lines only. Instead, I favor the conduction of research on the sector in which all valuable lines are pursued, whether of quarks or non-quark inspiration. I believe that this more balanced conduction of research is recommendable, owing to the complexity of the problems to be yet solved.

The point I would like to indicate is that, in case I join your Department, my studies should not be interpreted as opposing quark oriented research. On the contrary, they should be interpreted as complementing such study. In the final analysis, my primary objectives are the future experimental resolutions of the basic laws for the strong interactions. I am confident that all colleagues at Berkeley, whether of quark or non-quark orientation, will favor these experimental resolutions.

In closing, you might be interested to know that I am a nontenured member of the Department of Mathematics here at Harvard. Nevertheless, I have now completed my desired exposure to advanced mathematics. Also, I am a theoretical physicist, and I am now seeking a faculty appointment in a Department of Physics close to experimental facilities.

page 3.

I have enclosed a list of references in case you desire to contact them directly. On my part, I would appreciate the courtesy of an indication whether my candidacy is actively considered in order to solicite letters of recommendation.

Thanking you for your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

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UNIVERSITY OF CALIFORNIA, BERKELEY

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DEPARTMENT OF PHYSICS BERKELEY, CALIFORNIA 94720

July 31, 1979

Dr. Ruggero Maria Santilli
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138

Dear Dr. Santilli,

Professor Chew is away from Berkeley until September, as is our current Chairman, Professor Jackson, so I am acknowledging your application for a faculty position in our department. Assuming a position is made available to the department, the process of assessment and selection requires 6 to 8 months.

Thank you for your interest in Berkeley.

Sincerely yours,

Howard A. Shugart
Acting Chairman

HAS:fs

HARVARD UNIVERSITY

Area Code 617
495-3352

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

September 18, 1979

Professor G. F. CHEW
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

As you are eventually aware, I have applied for a position at your department with a letter originally addressed to you of July 21, 1979, and later kindly acknowledged by Dr. Shugart as acting chairman.

I would like to take the liberty of indicating that the Department of Energy is sincerely interested in continuing my grant, including the complete support of my salary, for a limited number of years. The officer in charge of my case is Dr. DAVID C. PEASLEE at the DOE, Tel. 301 353 3624. Please feel free to contact him if you so desire.

I am seeking a three year nontenured appointment with a minimum of stability during this period to justify the relocation of my family, as well as to have a minimum peace of mind to work (I have been living since my arrival in the States in 1967 with the known "one-year-terminal-nontenured-contract" and, with the passing of the years, it is beginning to take its toll).

I am also interested in the possible consideration for tenure at the end of this three year period, or a longer period if necessary. This possibility is quite important for my relocation.

I have taken the liberty of applying to you because you have been indicated to me by a number of colleagues as a scientist with a genuine vision toward fundamental studies (which is a quite rare occurrence in our community). I believe that my type of studies call for such a vision to remain alive.

You might be interested to know that we have achieved considerable progress since the time of my letter of July 21. Hoping that I do not abuse of your time and courtesy, permit me the liberty of outlining them.

Informal statement of the problem . As you eventually know, my group, by now composed of a number of mathematicians and physicists from the USA and adroad (France, Switzerland, Grece, USSR and People's Republic of China-see the enclosed paper for this latter Country) is working on the problem of validity or invalidity of conventional laws for the strong

page 2.

interactions, with particular reference to Einstein's special relativity, Pauli's exclusion principle and other conventional quantum mechanical laws. Our primary objective is to reach maturity of formulation of experiments. Our belief is that fundamental issues of this type simply cannot remain to be lingering in our community for ever, and should be resolved either way via experiments.

Historical doubts. As you certainly know, we are not the first to have doubts on the validity of conventional laws for the strong interactions and the hadronic structure in particular.

Most famous are Einstein's doubts on the final character of the Undeterminacy principle and other quantum mechanical laws (so vividly depicted in Heisenberg's memoires).

Equally valuable is Pauli's teaching that His principle was conceived and should be considered as applicable under the lack of overlap of the wave packets (atomic structure). Simple calculations show that, as a necessary condition, the hadronic constituents must be in a state of overlap of their wave packets. At the extreme, the current easy application of Pauli's principle, the spin-statistics theorem and all that, by hadron physicists, without a genuine critical inspection, could be qualified as being contrary to Pauli's teaching.

Also known are Fermi's reservations on the validity of conventional geometries and relativities within the region of space occupied by a strongly interacting particle (which he explicitly expressed in his lectures in Nuclear Physics). To my reconstruction, Fermi's argument is that Einstein's special relativity is strictly conceived for point-like particles under the action of forces derivable from a potential. This must be the case because this relativity is simply a relativistic extension of Galilei's relativity. In turn, this latter relativity is crucially dependent on the point-like notion of particle by Newton and Galilei (think at the point approximation of the sun). The point is that when the strong interactions are concerned, the Point-like approximation of hadrons is directly in conflict with the necessary need of overlap of the charge volumes-wave packets (as you know, the "size" of all hadrons coincides with the "range" of the strong interactions). Fermi therefore argued, to my reconstruction, that these conditions of overlapping are bound to result in forces more general than those of the still current simplistic use, $f = -\partial V/\partial r$, resulting in the expected breaking of conventional geometries (and, thus, relativities). The current easy use of the special relativity by hadron physicists without a genuine critical inspection, can be conceived, in the final analysis, as contrary to the teaching of these Masters.

Jordan's arguments on the need to generalize conventional quantum mechanic are equally known, and they lead to the foundations of the Jordan algebras even though their physical applications have not been as expected.

page 3.

The list of historical reasons of doubts by authoritative physicists could continue, but it would be here inessential. You are certainly aware of them.

The contributions in the Hadronic Journal. When I organized the Journal in April 1978 this was essentially the situation. There were many authoritative doubts. Nevertheless, we lacked the quantitative study of the problem and, most important, we lacked the initiation of the study to formulate experimental tests.

I believe that a considerable progress has been achieved in the Hadronic Journal in this crucial issue. We are now working at the fourth volume of reprinting of articles in the series devoted to this problem "Applications of Lie-admissible algebras in Physics", Edited by Professors H.C.MYUNG, S. OKUBO and myself. It is therefore impossible for me to be technical in a letter (or even in one single seminar).

A conceptual and purely indicative outline of the thought is the following.

(1) We have confronted the problem of the conditions of overlapping of the charge volume (or wave packets, if you prefer) of particles, as necessary to activate the strong interactions. This has resulted in the identification of nonlocal forced nonderivable from a potential (integro-differential equations).

(2) We have identified the implications of these broader forces vis a vis with current theoretical knowledge. Whether believed or not by the orthodoxy, these forces imply the collapse of most of current knowledge. There is the impossibility of introducing all Lie algebras via the brackets of the time evolution law, let alone the SU(2)-spin algebra, the SL(2.c) algebra, the Poincaré algebra, the conformal algebra, the gauge algebra, etc. More deeply, there is the breaking, under the forces considered, of the theoretical tools for the definition of the intrinsic quantities of particles, such as the conventional notion of spin under electromagnetic interactions. The breaking of the SU(2)-spin is much along the breaking of the SO(3) symmetry of the spinning top in Newtonian mechanics as necessary to avoid the perpetual motion. Even though rejected by the orthodoxy at this time (to the risk, quite candidly, of remaining behind), this occurrence is expected on physical grounds. Think at a proton created in the core of a star. We have an extended objected within superdense hadronic matter. Clearly, the physical conditions are different than those of the same proton when it is the nucleus of a hydrogen atom. In particular, such a proton is not expected to "spin" as when in vacuum. Interferences of the extended character of the proton with the hadronic matter are expected to result in a different notion of spin (which we technically realize via the Lie-admissible mutation). This is, conceptually, much along the breaking of SO(3) for the spinning top within Earth's atmosphere. At the hadronic level the situation is similar, as far as the constituents are concerned (also in a state of overlapping of respective wave packets), even though the hadronic densities are smaller. Thus, we expect a weaker

form of SU(2)-spin symmetry breaking for each individual hadronic constituents. At the nuclear level the situation is also similar, although the conditions of overlapping of nucleons is statistically quite small. We then expect an infinitesimal-type of SU(2)-symmetry breaking for the nuclear structure (see below for the experimental profile). With respect to quarks, the question of fractional charges is purely secondary. More deeply, the doubts rest on the conventional notion of spin in the quark model which has been conceived for the electromagnetic interactions and too easily extended to the strong without a critical inspection. In different terms, a fundamental question prior to any meaningful treatment of the hadronic structure is: what is the quantitative formulation of the notion of particle under strong interactions, that is, under the condition of overlapping of the wave packets?

(3) To confront this latter, crucial, issue, we have started a laborious homework beginning necessarily at the Newtonian level. I enclose copy of my Vol. I with Springer-Verlag on "Foundations of Theoretical Mechanics". Vol. II is now in press and copy of the typescript is at your disposal on request. As you can see, these volumes are devoted to the identification of methods for the treatment of forces nonderivable from a potential, as an approximation of nonlocal forces which is customary in Newtonian Mechanics. Think at the motion of Skylab while falling on Earth. Its forces were polynomial expansions in the velocities. Its dynamics was in rather brutal violation of Galilei's and Einstein's relativies because not only the equations of motion are necessarily form noninvariant under conventional space-time symmetries, but the conservation laws are lost. Thus, Skylab was a rather visible proof of the limited validity in physics of conventional relativities. If you go deeper into this situation you can identify the collapse of Galilei's and Einstein's relativity for Skylab as due to the extended size of the object, while these relativities are crucially dependent on point-like abstractions. So, you are back at Fermi's supposed argument.... Even more, Skylab's equations, as well as Newtonian Mechanics in general, are in direct incompatibility with Riemannian geometry, as stressed by Cartan (who advocated for Newtonian Mechanics the affine geometry), trivially, because a necessary condition for physical consistency is that the equations of motion are not derivable from an action principle and, thus, they cannot be all "geometrized". Thus, Skylab was also, at least for me, a clear example of the insufficiency of conventional gravitational models for the interior problem (which are all derivable from an action...). You are then back again to the formally written doubts by Fermi on the validity of conventional geometries within hadronic matter....

(4) The studies of my monographs with Springer-Verlag is only a rather small part of our efforts. A complementary line of studies has been initiated via the Lie-admissible approach to classical mechanics. You should have received complementary copies of the first two volumes of reprints "Applications of Lie-admissible algebras in Physics" I mailed you some time ago. Perhaps, you are familiar with the fact that the

page 5.

Lie-admissible algebras are a covering of the Lie algebras, and that they emerge for the time evolution law of Hamilton's equations in their original form with external terms (and not the "truncated" form of current use). This line of studies is now in full development. We have published numerous articles by several independent authors along the classical applications of Lie-admissible algebras, and a rather regular flow of papers arrives at my editorial desk.

(5) Finally, we have initiated a vigorous effort aiming at the quantization of local forces nonderivable from a potential, that is, strong interactions in our views, again, as an approximation of nonlocal settings. A crucial point to avoid scientific "hand wavings"(that is, illusions of treating quantum mechanically genuine forces nonderivable from a potential) is that the formulations should verify uniquely a correspondence limit into a bona fide nonconservative Newtonian system. This condition eliminated most of the current efforts in dissipative nuclear physics, by and large, via additive terms in Schrödinger's equations. These terms result in additive terms of the Hamilonian of the Hamilton-Jacobi equation under the correspondence principle. Thus, the forces are derivable from a potential and the system is far from being dissipative.

This line of studies is also in full motion and it is difficult for me to technically outline it here. In essence we have moved along two lines.

- Quantization via generalized Schrödinger's equations. The use of the techniques of the Inverse Problem (my monographs with Springer) allow the computation of generalized Hamiltonian structures. Quantization via the second Beltrami procedure then yields generalized Schrödinger's equations which uniquely satisfy the correspondence principle into the original, genuine, nonconservative equations. The implications for conventional knowledge are conspicuous, when forces nonderivable from a potential are genuinely confronted. For instance, the phase of wave packets must necessarily violate conventional space-time symmetries in order to admit a Schrödinger's equation which, under the correspondence principle, yields genuine Newton's equations with forces nonderivable from a potential. This occurrence is desired to achieve compatibility with classical formulations. After all, at the Newtonian level the equations of motion must necessarily violate Galilei's symmetry in order to admit the forces considered (e.g., polynomial expansions in the velocities). What I am attempting to say here is that the very notion of "wave packet" needs a renspection when considered within hadronic matter. You are perhaps familiar with other breakdowns presented in my papers (i.e., the breakdown of all the technical ingrediants to formulate Pauli's principle, in full agreement, I would like to add, with His teaching).
- Quantization via generalized Heisenberg's equations. We start with Hamilton equations with external term, thus admitting a Lie-admissible (and not Lie) structure. We have identified a mapping to operators which preserves this Lie-admissible structure. This, again, ensures us that, under the correspondence limit, we recover Newton's equations with genuine forces nonderivable from a potential.

page 6.

In particular, the quantum mechanical time evolution is necessarily nonunitary (Prigogine essentially received his Nobel price in chemistry much along this point). You can easily see it by noting that the time evolution of Hamilton's equation with external terms is necessarily noncanonical. We have then identified covering transformations which we call unitary-admissible and they yield precisely a Lie-admissible algebra for the time evolution law.

By using these two, new, generalized tools, a rather feverish activity is now going on. The ultimate objective is that of achieving a quantitative formulations of the notion of a massive and charged particle under the condition of overlapping of its (Lie-admissible) "wave packet" with hadronic matter (our idea of hadronic constituent).

My personal efforts have been verted in the identification of a possible generalization of Galilei's relativity at both the classical and quantum mechanical level with a Lie-admissible structure. I then use this generalized context to attempt the definition of covering of conventional notions of intrinsic quantities of particles.

More recent developments. Since the time of my letter to you, a genuine progress has been made via the SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS held here at Harvard from August 1 to 7. Half of the participants were outstanding mathematicians in nonassociative algebras in general, and Lie-admissible algebras in particular.Half of the participants were qualified physicists (e.g., of the caliber of OKUBO).

In this workshop we first reinspected, particularly in the several days of informal sessions following the formal ones, most of the doubts on the validity of conventional laws for the strong interactions. I am happy to report that these doubts were not only confirmed, but actually enhanced.

In addition, we made genuine progress toward these studies. I am taking the liberty of including, on an informal and confidential basis, copy of my report to the DOE on the outcome of the meeting.

The experimental profile. At this moment we possess two proposals of experimental tests in the Hadronic Journal, although they are at a predictably initial stage, and much remains to be done.

The first is my proposal to test Pauli's principle in nuclear physics for nuclei with volume below that predicted by the proportionality rule with the number of nucleons, and via low energy scattering of hadrons (no leptons!) on nuclei. The prediction of the Lie-admissible formulations is that, under a small state of overlapping of nucleons, there are very small deviations from Pauli's principle which have escaped current inspections, much along the historical (and quantitatively similar) case of the violation of discrete symmetries in particle physics.

page 7.

These small deviations are predicted from small nonlocal terms in the nuclear force originating from the small state of overlapping of nucleons. Indeed, according to our formulations, these forces imply that nucleons are not exact fermions, thus rendering Pauli's principle statistically inapplicable on a small scale (this is the very small breaking of the SU(2)-spin symmetry indicated earlier).

The second proposal is that by KIM and consists of an experimental measurement of the mean life of unstable hadrons in flight. If the constituents are under nonlocal forces, there are deviations from the value predicted by the special theory. In turn, this test can be conceived as an experimental verification of Einstein's special relativity for the strong interactions.

Most importantly from an experimental profile, we are working at a "nonpotential scattering theory", that is, at the identification of the basic tools for data elaboration (cross section, etc.) for forces non-derivable from a potential. Once we have achieved this objective at least in a preliminary, but solid form, we can formulate a variety of experiments for each and every major aspect. Then, the confrontation of the historical reasons of doubts on conventional laws for the strong interactions will not be procastinated any longer by the orthodoxy in physics, at least this is my hope....

As you can see, the continuation of this line of study demands a suitable Institution and a suitable, stimulating,environment, close to both the mathematical and the experimental circles. Lacking these qualifications, the entire program will be likely delayed years, perhaps decades in time. My application for a position at your Department has been submitted in this spirit.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY

AREA CODE 617
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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

October 10, 1979

Professor G.F.CHEW
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Chew,

Hoping that I do not abuse of your courtesy and time, I would like to inform you that I have applied to the Lwarence Berkeley Laboratory, the nuclear physics division, as per enclosed letter. The reason for selecting the nuclear division, after due consideration, is that I am primarily interested in collaborating with experimentalists for the future experimental resolution of Pauli's principle, Heisenberg's principle, and other laws under strong interactions beginning at the nuclear level. In particular, it is at this level where we are closer to maturity, and the experiments are less constly, on a comparative basis with high energy physics.

I also enclose copy of the "Chart 4.9" of my volume II with Springer-Verlag of "Foundations of Theoretical Mechanics", now in press. This chart presents an account, understandable to graduate students, of the intriguing situation of the basic laws for the strong interactions.

I am fully aware that it will be difficult for you to find the time to read it. Nevertheless, you might pass it to graduate students. It is understood that the technical treatment is conducted elsewhere, and it is re-elaborated in the Proceedings of the Second Workshop on Lie-admissible Formulations which will be distributed in Early 1980.

Tnak you for your time.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

UNIVERSITY OF CALIFORNIA, BERKELEY

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DEPARTMENT OF PHYSICS BERKELEY, CALIFORNIA 94720

Nov. 5th 1979

Dr. R.M. Santilli
Science Center, Room 331
Harvard University
Cambridge MA

Dear Dr. Santilli:

In reply to your letter of October 10th. I heard repeatedly Fermi expressing doubts about the Copenhagen interpretation of Quantum Mechanics, but he never was very explicit. He rather smiled and joked. My impression is that he had feelings, but not cogent arguments.

I do not know enough to read your mathematical papers. I would like however to remark that I thought that practically all natural forces ON A MICROSCOPIC SCALE are conservative and that non conservative forces are a suitable schematization on a macroscopic scale of the total effect of the microscopic forces. The bridge is furnished by statistical mechanics. Maybe I am wrong, but I do not know of any not conservative forse on a microscopic scale.

Sincerely yours

Emilio Segrè

Emilio Segrè

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

November 9, 1979

Professor EMILIO SEGRE
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Segre,

Please accept the sentiments of my sincere appreciation for your kind letter of November 5.

I feel a sense of relief in reading that Fermi had not detailed arguments for his doubts, because I had spent considerable time in searching through his papers and books, yet I was afraid that some important passage to this effect had escaped my attention. Nevertheless, Fermi's doubts, even though only embrionically expressed, have been invaluable for me.

I also appreciate the courtesy of expressing your view on the problem of the forces in the microscopic scale. There is little doubt that your view is the soundest at this moment, on grounds on established theoretical and experimental knowledge.

Nevertheless you might be intrigued to know that a rather feverish research activity is now going on, by mathematicians and physicists, on the study of forces nonderivable from a potential in the microscopic scale. I am personally involved in these studies, and, as editor in chief of the HADRONIC JOURNAL, I am particularly exposed to this recent trend with articles submitted from virtually all developed Countries.

To my reconstruction, this trend was initiated by nuclear physicists. After working with $f = -\partial V/\partial r$ for over half a century, nuclear physicists came to realize the existence of phenomena that simply cannot be effectivel treated with these forces. Nowaday, the study of dissipative nuclear phenomena is under a considerable expansion, even though these studies cannot yet be classified as belonging to a discipline, because the problem of the basic quantum mechanical laws and principles under forces structurally more general than $f = -\partial V/\partial r$ is theoretically and experimentally open at this time.

Once the "ice was broken", so to say, in nuclear physics, the transition to hadron physics and astrophysics, again at the microscopic (or local) scale, was expected. Judjing from the flow of papers arriving at my desk, it appears that the trend is that of studying the possible existence of

page 2.

forces structurally more general than $f = -\partial V/\partial r$ (technically called variationally nonselfadjoint forces) at all microscopic levels of the strong interactions (nuclear, hadronic and astrophysical), and then searching for direct compatibility with corresponding macroscopic settings. On more specific grounds, the trend is that of studying the forces considered in the miscroscopic scale, by following exactly the same pattern of the classical, mascroscopic transition from Newtonian mechanics to statistical mechanics, that is, the broader forces considered are studied first for the behaviour of one particle (under condition of overlapping of its wave packet with that of others), and then searching for a statistically compatible, quantum mechanical formulation, say, à la Prigogine.

If you accept the following data:
(1) all (massive) particles have a wave packet which is extended in size;
(2) all strongly interacting particles have approximately the same size which is of the order of 1F; and
(3) the dimension of a strongly interacting particle coincides with the range of the strong interactions;
a rather intriguing dynamical difference between the electromagnetic and the strong interactions emerge: the former generally occurs without appreciable overlapping of the wave packets (atomic structure-Copenhagen interpretation of quantum mechanics); while the latter demand, as a necessar condition to activate the strong interactions, a state of overlapping of the wave packets - or charge volumes - (new mechanics?).

The trend I am referring to essentially studies the question whether, under the conditions of overlapping of the wave packets, the forces $f = -\partial V/\partial r$ are truly capable of representing the dynamical behaviour or not.

Once this issue is subjected to a detailed, quantitative, treatment, a number of intriguing aspects emerge, all suggesting the presence of forces more general than those of conventional use. The literature in the subject is now quite vast, and expanding rapidly (Professors H.C.MYUNG, S. OKUBO and myself are editing a reprint series in the topic which is now approaching its fourth volume...). As an isolated case, I can mention the expectation that, under the conditions of mutual penetration, the spin-spin couplings are expectedly stronger than those of the atomic mechanics, that is, attainable via $f = -\partial V/\partial r$. If more general forces are admitted, an arbitrary strength of these spin-spin coulings becomes quantitatively possible.

On epistemological grounds, the following possibility is emerging. If all particles are approximated as being point like, then only forces derivable from a potential are possible, classically and quantum mechanically, that is, mascroscopically and microscopically. Indeed, a point can only have action at a distance forces, and these forces can be proved to be variationally selfadjoint (derivable from a potential).

In fact, if you approximate a satellite in earth atmosphere as being a point, it can only have selfadjoint forces and no inelastic collisions. According to established knowledge in space mechanics, a satellite has nonconservative forces while within earth atmosphere because it is an extended object. A technical study of the issue (I have also conducted in my monograph "Foundations of Theoretical Mechanics" with Springer-Verlag) indicates that the actual forces acting on the satellite are nonlocal (volume integrals), and that they can be well approximated as being local via polynomial expansions in the velocity (and thus, highly non-derivable from a potential).

The important fact is that, whenever the satellite exits the earth atmosphere, the actual shape and extension of the satellite do not affect the dynamics. Under these conditions, the satellite can be well approximated as being a point, and all forces are only derivable from a potential. This is the notion that the sun can be approximated as a massive point by Newton and Galilei.

The intriguing interplay electromagnetic/strong interactions seems to indicate the possibility that we may well have exactly the same situation at the miscoscopic level, of course, on conceptual grounds. Under elm interactions at large mutual distances the actual dimension of the wave packet does not affect the dynamics, and all forces are derivable from a potential (satellite moving in vacuum). Under strong interactions, and the apparent necessary condition of overlapping of the wave packets, the actual dimension of the particle seems to be needed to identify the dynamics, and, in any case, point-like abstractions are known to produce only a crude approximation (satellite in earth atmosphere). The broader forces nonderivable from a potential appears to be effective to represent, of course as an approximation of extected nonlocal settings, the dynamical implications of the extended size, e.g., the condition of one extended particle while spinning within another.

This is the reason why the current trend is studying the possible existence of variationally nonselfadjoint forces at the level of one single particle, and then searching for a compatible statistical setting, in such a way that:

(A) the one-particle quantum mechanical description recovers, under the correspondence principle, the one-particle Newtonian description according to Lagrange's and Hamilton's equations in their original conception, that with external terms (rather than the truncated form of these equations of current use); and

(B) the many-particles, quantum mechanical, statistical description recovers under the correspondence principle, the classical statistical description according to the original Liouville's conception, that for arbitrary forces (rather than the truncated form of this conception of current use, that for forces derivable from a potential only).

Independently from these considerations, the studies have indicated that, if the strong interactions are realized via forces that are analytically equivalent to the electromagnetic interactions, this condition necessarily

implies a form of dynamical equivalence between these interactions, which appears to be contrary to the profound physical differences between these interactions as manifested in nature. To put it explicitly, the positronium admits a physically effective, nonrelativistic, quantum mechanical description with $f = -\partial V/\partial r$. If the $\pi^0$ is represented as a bound state of two charged particles under forces $f = -\partial V'/\partial r$, the mere change of the potential $V \rightarrow V'$ in the transition from the positronium to the $\pi^0$ does not appear to be effective to properly represent the differences between these two structure. Besides the use of $f = -\partial V'/\partial r$ implies a host of problematic aspects for the $\pi^0$. As an indication, such a (nonrelativistic) model of the $\pi^0$ would imply, within quark models, a nonnull probability of tunnel effects of the constituents (which is rather well established recently), and, thus, the existence of the decay

$$\pi^0 \rightarrow q + \bar{q}$$

which is contrary to experimental evidence.

On the contrary, if one sees the possibility that, unlike the case of the positronium, the $\pi^0$ is made of a bound state of two particles under conditions of mutual overlapping of the wave packets, broader forces, and a consequential broder dynamics, a number of options become possible that would be otherwise precluded.

This is the reason why there is such a feverish activity going on. The use of nonconservative forces in the microscopic scale is stimulated by rather specific, technical, elements, and allows quite intriguing possibilities.

Of course, this crucial alternative ( $f = -\partial V/\partial r$ or not) for the strong interactions must be resolved in due time via experiments.

You might be interested to know that I have recently applied to the nuclear physics division of the Lawrence Berkeley Laboratory, according to the enclosed letters to Drs. B. G. HARVEY and R. W. BIRGE. I have also applied some time ago to Dr. G. F. CHEW for a position (or a joint position) at your department, and I am taking the liberty of enclosing copy of my letter in a confidential way.

As you can see, it appears that there is a realistic possibility of resolving the issue of the existence or nonexistence of nonselfadjoint forces in the miscoscopic, one-particle level under strong interaction by conducting more accurate measurements of something taken for granted: the validity of Pauli's exclusion principle in nuclear physics.

Hoping that I did not abuse of your time and courtesy, I remains

Yours, Sincerely

Ruggero Maria Santilli

encls.
RMS/ml

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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CAMBRIDGE, MASSACHUSETTS 02138

January 9, 1980

Professor E. Segre
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Segre,

I enclose an outline of the Proceedings of our Workshop on the problem of the basic laws of the strong interactions. As you can see, it consists of a considerable effort for over 1,500 pages, by distinguished mathematicians and physicists from the USA, USSR, China, and several other Countries. Part A (the review part) review some 500 theorems, lemmas, etc. of either direct or indirect or potential relevance to the problem of the strong interactions conceived as extended particles under conditions of overlapping of their wave packets. Particularly intriguing are two theorems, one lemma and several propositions invalidating Heisenberg's equations for all Hamiltonians of polynomial order in r and p higher than two (that is, for nonlinear systems). One theorem was proved by Abraham and Marsden, and the other by several physicists. These theorems bring , rather forcefully in my view, into focus the need to subject to an experimental verification the validity of conventional laws for the strong interactions, no matter how sound they may appear at the theoretical level.

The Proceedings focus on the need to subject to an experimental verification Pauli's principle in nuclear physics, as well as on the quantitative study of possible deviations. It essentially emerges that possible very small deviations at the nuclear level open truly intriguing possibilities at the hadronic and the astrophysical level. The possible breakdown of conventional laws for the strong interactions, rather than being a scientific disaster, appears to be truly intriguing for nonincremental advances.

You might also be interested to know that a first contact with experiments is not far in the future In fact, we have now rather accurate experiments being done using a beam of neutrons through selected cristals. The statistics of the neutron (and, thus, Pauli's principle) can be tested via comparison of data under two rotations. Current data, apparently, are affected by an error of 1/2 degree over two rotations. This is certainly not necessarily a deviation from Pauli's principle (although it is quantitatively of the order of that predicted). The point is that, perhaps, the accuracy of the experiments can be estanlished or improved via separate experiments. In this case, we would be finally in a position to asnwer at least the question: what is the quantitative meaning of the currently accepted validity of Pauli's principle in nuclear physics on a comparative basis with the exact validity experimentally established in atomic physics ? If a deviation could be experimentally detected, the momentum for nonincremental advances would be invaluable, in my view.

In case you are interested to be kept informed of these studies, please let le know. Also, please let me know whether you desire a complimentary copy of the two volumes of the Proceedings, and I shall do my best to secure it from the publisher.

I would appreciate the courtesy of your assistance, if at all possible, on my application to join your Department. In essence, I would appreciate the courtesy of the indication of the status of the application, as well as of the projected time for a decision.

page 2.

I do not know whether you are aware of my application. The major lines are essentially the following.

I applied for a position with a letter to Professor Chew of July 21, 1979. I selected Professor Chew because he was somewhat informed of the progress of our studies. The application was passed to Professor Shugart as acting chairman who kindly acknowledged the application on July 31, 1979 and indicated that it was under consideration.

On financial grounds, I do not know whether I have sufficiently conveyed the fact that my possible position at your department does not require funds. In fact, I am fully covered by my grant from the Department of Energy. This grant is at its second year, and it will be likely continued for a number of years. The renewal for two additional years has been confirmed. Please feel free to contact Professor D.C.Peaslee at the DOE, tel 301 353 3624 for confirmation on the availability of the renewal.

In short, I am primarily interested in your Department administering my grant. The funds are confirmed. I am interested in applying as principal investigator jointly with any interested colleague. Nevertheless, in case a senior colleague is interested, I would be happy to apply as a coinvestigator.

I understand that several letters of recommendation have arrived and my file is complete.

I would appreciate the indication of the status of the application. Incidentally, I am not interested in suggesting a rapid decision on my case. No. I believe that your Department should take all the necessary time to reach a decision. In fact, any delay will be in my favor, in the sense that the studies we are doing are attracting an increasing interest.

Hoping that I did not abuse of your courtesy and time, I remain

Yours, Sincerely

Ruggero

Ruggero Maria Santilli

RMS/ml
encls.

UNIVERSITY OF CALIFORNIA, BERKELEY

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DEPARTMENT OF PHYSICS BERKELEY, CALIFORNIA 94720

January 23, 1980

Dr. Ruggero Maria Santilli
Harvard University
Department of Mathematics
Cambridge, MA 02138

Dear Dr. Santilli,

Professor Segrè has passed on your recent letter concerning the status of your application for a faculty position at Berkeley. As Professor Shugart wrote to you at the end of July last year, your application had been handed on by Professor Chew to Shugart in my absence and from Shugart's hands to the appropriate faculty search subcommittee.

We have an ongoing search for outstanding young faculty. This search has many components and covers essentially all fields of physics. After a period of advertising, the various subcommittees sift through the applications and select one or more outstanding candidates for a faculty position in their area. The subcommittees submit reports to the departmental Committee on Policy and New Appointments, which folds in considerations of the Department's needs and the relative merits of different candidates from the different fields. Recommendations from this committee and from the search subcommittees then are taken to the full faculty for its consideration.

In this winnowing process one candidate will be approved to be offered the single position which we are allocated for July 1, 1980. As you can imagine, all of this takes a considerable time. I am hopeful that all decisions for the coming year will have been taken by April 15th at the latest.

I note that you have a DOE grant for the present year and anticipate its renewal. This does not, however, affect the considerations of you as a candidate for a faculty position.

Thank you for your interest in Berkeley.

Yours sincerely,

J. D. Jackson
Chairman

JDJ:fs

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

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January 29, 1980

Professor J. D. JACKSON,
Chairman
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Jackson,

Please accept the sentiments of my sincere appreciation for the courtesy of your letter of January 23, 1980.

Please take all the necessary time to reach a decision on my case. Upon consultation with the DOE, I have filed an application for continuation of my grant with a local administration, here in the Boston Area. Jointly, I am taking all the necessary precautions to ensure that I can move the grant to a different Institution, once supported.

In case I can be of any assistance during the consideration of my candidacy, please do not hesitate to contact me.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml

P.S. Independently of my application, you might be intrigued by the enclosed letter I am passing to editors and selected colleagues. It may give you more information on the intriguing (for some) or distressing (for others) state of hadron physics at the editorial level. The understanding is that, to reach a broader audience, I have avoided the use of the technical language of the symplectic quantization and of the broader Lie-admissible quantization. This situation might interest some of your colleagues at Berkeley, and I would be happy to report it in an informal, noncommittal manner, in case you consider it appropriate, at any time of mutual convenience (from mid February to mid-March I will be in Europe to deliver invited talks on the subject).

HARVARD UNIVERSITY
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February 11, 1980

Professor J. D. Jackson, Chairman
Department of Physics
University of California
BERKELEY, California 94720

Dear Professor Jackson,

Hoping that I do not abuse of your courtesy and time, I am taking the liberty of enclosing copy of the outline of a course in undergraduate, general physics I taught a few years ago, in case of possible value for the consideration of my candidacy. Please note the amount of time I dedicate in tutoring personally undergraduate students.

In regard to my current contacts with colleagues-editors on the puzzle of papers activating quantum mechanical inconsistency theorems (see my recent letter to you), you will pleased to know that several leading scientists have answered the call for help. For instance, Professors JERROLD MARSDEN and PAUL CHERNOFF of the Department of Mathematics at Berkeley (two leading experts in the problems)have expressed interest in helping us in reaching a mature editorial decision.

The situation is quite intriguing. Indeed, a number of editors (including myself) simply do not know how to handle papers activating the inconsistency theorems because of insufficient technical information either in favor or against. We are therefore grateful for the positive response by mathematicians.

These aspects will be discussed at the THIRD WORKSHOP ON LIE-ADMISSIBILITY I am currently organizing for this coming August. The meeting looks rather promising. In case you desire to be kept informed of the decisions reached at this meeting (irrespective of the consideration and outcome of my application), please let me know. It would be a pleasure for me to provide you with the most relevant data.

Sincerely

Ruggero Maria Santilli

NO COMMUNICATION OF A FINAL DECISION WAS EVER RECEIVED FROM BERKELEY.

RMS/ml
ecnsl

# PART IX:

# LAWRENCE

# BERKELEY

# LABORATORY

HARVARD UNIVERSITY

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

October 10, 1979

Dr. BERNARD G. HARVEY,
Associate Director, Nuclear Science Division
Lawrence Kerley Laboratory
BERKELEY, California 94720

Dear Dr. Harvey,

I am hereby applying for a position in the nuclear science division of the Lawrence Berkeley Laboratory commensurate to my qualification and experience.

My application is specifically referred to and intended for the possible collaboration with interested experimentalists and theoreticians on the experimental test of Pauli's exclusion principle under strong interactions, beginning at the nuclear level, and following my original proposal published in the Hadronic J. 1, 574, 1978, and subsequently elaborated in a number of articles, either alone of in conjunction with mathematicians and physicists.

The main idea is to test the validity of Pauli's principle in nuclear physics to the effect of ascertaining whether this principle is valid in the arena considered in the same quantitative amount as it is valid in atomic physics, or very small deviations are experimentally detectable.

The proposal refers to the use of low energy scatterings of hadrons in the peripheral nucleons of nuclei selected in such a way that their volume is below the value predicted by the proportionality rule of the nuclear volume with the total number of nucleons. The objective, on experimental grounds, is that of ascertaining whether the wave function of identical peripheral nucleons of these nuclei is totally antisymmetric, or very small deviations exist and have escaped current inspections simply because not looked for. Preliminary assessements by nuclear physics indicate that this experimental test is feasible with current technology, with the understanding that it is predictably delicate and will predictably call for additional theoretical studies.

On theoretical grounds, the nucleons of the selected nuclei are in an experimentally established, statistically small, state of penetration of their wave packets. Recent studies have indicated that, under these conditions, we expect the emergence of forces more general than the trivial $f = -\partial V/\partial r$

(the so-called variationally non-self-adjoint forces), according, after all, an old idea since Fermi's time, which has been lately ignored to a considerable extent. In essence, this results in the expectation of an additive term in the (conventional) terms of the nuclear force, this time non-derivable from a potential and with a very small (statistical) coefficient. The structure of this nuclear force prohibits the possibility of representing the nuclear structure with conventional Hamiltonians $H = H_{free} + H_{int}$, by therefore invalidating the applicability of Heisenberg's equations also to a very small measure. Still in turn, this renders applicable the recently identified, broader, quantum mechanical equations of Lie-admissible (rather than Lie) algebraic character. Finally, this settings predicts very small deviations from the applicability of Pauli's principle for the nuclei indicated, that are quantitatively similar to the historical case of the violation of parity under weak interactions.

I would like to inform you that these studies are now conducted by an organized group of researchers including mathematicians and physicists. Some of the mathematicians are:
- Professor MYUNG, of the University of Norther Iowa; Professor TOMBER of the Michigan State University; Professor OEHMKE of the University of Iowa; Professor WENE of the University of Texas at San Antonio; and others.
Some of the physicists actively involved in these studies are:
- Professor OKUBO of the University of Rochester; Professor KOBUSSEN of the Institut fur Theoretische Physik der Universitat Zurich; Professor KTORIDES of the University of Athens in Greece (currently spending his sabbatical as my guest here at Harvard); Professor CANTRIJN and SARLET of the Instituut voor Theoretische Mechanics of the Rijksuniversiteit in Gent Begium (the latter spent the academic year 1978-1979 as my guest here at Harvard); Professor FRONTEAU and TELLEZ-ARENAS of the Université d'Orléans in France; Professor LÕHMUS of the USSR Academy of Science in Tartu; Professor JANG CHUN-XUAN of the People's Republic of China; and others.
Owing to the intriguing character of our studies, the number of mathematicians and physicists joining our efforts is increasing in a promising way. For instance, I just received notice that Professor ELIEZER and his group in Australia have initiated active studies on the so-called SU(2)-admissible generalization of the conventional SU(2)-spin symmetry (the latter being a prerequisite for Pauli principle, and the former a prerequisite for its expected deviations).

The coordination of these efforts is conducted via
- the HADRONIC JOURNAL, of which I am the founder and editor in chief;
- a yearly workshop on Lie-admissible formulations (the first was held here at Harvard in August 1978; the second was held also here in August 1979; and the third is scheduled for August 1980). In addition, a conference is independently under organization by mathematicians;
- two series of research monographs I am currently publishing, one with springer-Verlag under the title "Foundations of Theoretical Mechanics" (Volume I was published in 1978, and volume II is in press); and another with the Hadronic Press under the title "Lie-admissible approach to the Hadronic structure" (Volume I was published in 1978, volume II is in press; and volume III is scheduled for 1980).

The technical profile is essentially set by the so-called no-go theorem of Heisenberg-type quantization of the (pre)symplectic geometry, which is rigorously proved in the recent edition of "Foundations of Mechanics" by Abraham and Marsden. This theorem literally establishes that Heisenberg's equations are inconsistent for all polynomials Hamiltonians of order higher than the second. This exludes trivial, linear, selfadjoint models (in the variational sense, rather than the operator sense), but includes nonlinear conventional models, as well as generalized Hamiltonian structures for forces non-derivable from a potential, as computable via the so-called Inverse Problem of Mechanics (my monographs with Springer-Verlag). Regrettably, this theorem does not appear to be fully known in physics circles, inclusive nuclear physics circles involved in the recent intensification of quantum mechanical dissipative studies. Nevertheless, the theorem exists, it has been independently established by mathematicians experts in quantization, and it is stirring up a feverish activity.

These technical aspects have been considered in detail at our recent SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, attended by mathematicians and physicists from the U.S.A., France, Switzerland, Israel, and with corresponding participants from the USSR and the People's Republic of China. The proceedings will be published in the December issue of the HADRONIC JOURNAL, for distribution in early 1980.

For a preview of these proceedings, I enclose "Chart 4.9" from my Volume II with Springer-Verlag (now in press). Essentially, this is an account of the problem considered in a form understandable to graduate students, as well as researchers without a technical knowledge of symplectic quantization, and of the broader Lie-admissible quantization.

I would like to draw your attention on part 9 of this chart, pages 343-349 on the quotation of the rather numerous, historical, authoritative, voices of doubt on the practically implemented (these days), terminal character of conventional quantum mechanics. In particular, I would like to draw your attention on what we call a LEGACY BY EINSTEIN: the fact that he refused to believe up to his death in the terminal character of conventional quantum mechanics, as touchingly depicted by Heisenberg in his memoires "Physics and Beyond" (in Heisenberg's words, Einstein could at most tolerate quantum mechanics as a "temporary expedient").

I would like to express that this legacy by Einstein is closely related to my application and, in particular, to the fact that I have applied to the nuclear division of your laboratory. In essence, the studies indicated earlier have taken this legacy by Einstein seriously and worked out the implications to all possible details. In a few non-technical terms, it turns out that the conventional uncertainty of quantum mechanics is invalid under the conditions of overlapping of the wave packets and broader forces more general than those $f = -\partial V/\partial r$ of current use. The number of studies conducted on this legacy are numerous, independently based, and all leading to the same conclusion. For instance, the time evolution under variationally

non-self-adjoint forces is non-unitary . This implies (according to established theorems) that, assuming the conventional uncertainty is valid at a given time, it is not preserved in time. The mechanisms of departure from conventional setting is exactly the same as that of the departure from Pauli's principle which are expected under nonselfadjoint forces, thus confirming Einstein's vision for a genuine advancement. In particular, the departures from these conventional settings are differentiated into:

- the level of the nuclear structure, where at most very small departures are admitted, owing to the very small overlapping of the wave packets;
- the level of the structure of astrophysical bodies, where large deviations are expected, owing to the large state of penetration of the wave packets of the constituents one within the other due to the extremely high pressures and densities; and
- the level of the structure of the hadrons, where intermediate deviations are conceivable, as a result of these studies.

Nevertheless, in my view, the fundamental starting point is that at the nuclear level, where theoretical views by individuals or groups of researchers can be resolved at the experimental level in an expectedly clearer form and in an expected shorter period of time. It is understood that the experimental identification of very small deviations at the nuclear level would promote nonincremental studies at the hadronic level (beginning with a mathematically and physically consistent definition of constituent, that is, of a particle under joint elm and strong interactions and the state of mutual mpenetration of the wave packets) and at the astrophysical level (where a fundamental assumption would be invalidated, that the geometries for the interior problem are Lorentz in local character). This is the reason for my desire to be associated with the nuclear physics devision, although I would like to release to your laboratory any final decision, in case my candidacy is actively considered.

For your amusement, permit me to quote the following passage by Heisenberg:

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

to which he adds immediately after:

> "However, when it comes to 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."

I have taken the liberty of quoting these passages by Heisenberg as one way to indicate that I am fully waware of the inertia inherent in our community toward studies of this type. Nevertheless, one of the duties of our profession is that of not taking for granted general beliefs, even when they are fully sound, and establish our knowledge via experiments. When referring to the problem of the basic physical laws for the strong interactions, I think that the implementation of this duty is essential.

On administrative grounds, you might be interested to know that my studies are conducted with support from the DEPARTMENT OF ENERGY, under grant no. AS02-78ER04742. It appears that the DOE is pleased with the output of my research and it is interested in continuing this support under the administration of a qualified Institution. In case my application is seriously considered, please feel free to contact the High Energy Physics Division of the DOE, e.g., Dr. DAVID C. PEASLEE, tel. 301 353 3624.

In relation to the letters of recommendations, I would like to bring to your attention that I have applied for a position at the Department of Physics of the University of California at Berkeley. Professor HOWARD A. SHUGART, Acting Chairman, has in his file a number of letters of recommendation by distinguished scolars. In case acceptable by your Institutions, I would appreciate the courtesy of the use of these letters. But, if this procedure is not acceptable, please let me know, and I shall solicite letters of recommendation addresses to the person indicated by you . Almost needless to say, on my part I would be simply honored for the consideration of a joint appointment.

I enclose for your consideration my curriculum, a list of references, and some informative material on my recent research-teaching-editorial activities, while I remain at your disposal for any assistance you might need.

Sincerely

Ruggero Maria Santilli

RMS/ml
encls.

c.c.: Professor H.A.SHUGART, Department of Physics
University of California at Berkeley

HARVARD UNIVERSITY
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October 22, 1979

Dr. ROBERT W. BIRGE, Director,
Physics Division
Lawrence Berkeley Laboratory
BERKELEY, California 94720

Dear Dr. Birge,

I am taking the liberty of informing you that I have recently applied to the nuclear physics division of your laboratory for a position commensurate to my qualification and experience. In essence, I am interested either to a tenured appointment, or to an appointment with a serious consideration for tenure after the needed number of years of service. Also, I am the recipient of a DOE grant (now at its second year), and I am particularly interested in being able to apply, as principal investigator, for the continuation of this grant, either alone, or jointly with interested colleagues. I am also the editor in chief of the HADRONIC JOURNAL (now at its second year of bimonthly publications, all on full schedule), and I would like to continue this function at my new Institution. Finally, I am the organizer of a yearly meeting called WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (the first two were held here at Harvard in August 1978 and 1979), and I would like the opportunity to continue this meeting at my new Institution (although no expenses from your laboratory would be expected).

I enclose copy of my original application to Dr. BERNARD G. HARVEY of your Division of Nuclear Physics, as well as my curriculum and illustrative material on my recent research-teaching-editorial activities. In relation to the letters of recommendations, I have asked the kindness to Dr. SHUGART at the Physics Department to provide copy of the letters in his file, to expedite procedures. Nevertheless, I remain at the disposal of your laboratory to solicite additional letters from distinguished scolars upon request. A list of my references is enclosed.

I hope you will be amused and intrigued to see that my application is specifically intended to test something taken for granted by the physics community at large: the validity of Pauli's exclusion principle in nuclear structures.

In essence, a number of mathematicians and physicists, including myself, have initiated a long term, in depth, study, via the use of the most advanced possible mathematical techniques, of a number of "legacies" by the Founding Fathers of contemporary physics, such as Fermi, Einstein, Jordan, von Neumann, Wigner, Pauli, etc., that are, in our view, basically

open at this time. These legacies essentially deal with historical, authoritative, voices of doubt on the validity for the strong interactions of the basic physical laws of the electromagnetic interactions.

I am a theoretical high energy physicist. I have experienced, personally, the elements of crisis in the current state of the art in strong interactions. In particular, I have tried for years to reach a structure model of hadrons via quark conjectures that was mathematically and physically consistent according to my personal standards, without publishing any paper at all. What I was after, and I failed completely, was an apparently "simple" task: a structure model of the lightest known hadrons-the octet of light mesons- that (1) is of quantitative character, that is, it is realized by equations of motion obeying physical laws, and, as such, it can be confronted with experiments (I was not interested in phenomenological models); (2) reproduces all the known physical data of the hadrons considered-the intrinsic characteristics and the decays, as well as their related fractions; and, last but not least, (3) achieves a strict form of confinement, that is, the equations of the bound state of a quark and an antiquark imply an identically null (and not an approximately null) probability of tunnel effects, to prevent decays such as

$$\pi^0 \longrightarrow q\bar{q}$$

which simply do not exist in nature.

This "simple" task was essentially inspired by the desire to follow Bohr. He considered the lighest known atom (and not, say, the palladium); he searched for a quantitative model of structure via equations of motion (Schrödinger's equation); and he believed in his finding because the model was able to account for all data known at the time, including all spectral lines (and not part of the spectral lines).

As sayd earlier, I failed completely in this task, despite years of trials. The inconsistencies I found in the conduction of this study were multiplying in time, rather than decreasing. I finally reached the conclusion that a quantitative structure model of the light mesons with a strict confinement of the assumed quark constituents is virtually unfeasible in the context of nonrelativistic and relativistic quantum mechanics.

I then discovered that I was not alone in this crisis. Several mathematicians and physicists have joined me in the attempt to reach a new, nonorthodox, critical, inspection of the situation.

My position both as an individual researcher and as editor in chief of the HADRONIC JOURNAL is the following.

(1) I believe in the final character of unitary models and QCD for the classifications of hadrons only (or, you may say, for the "exterior" or "chemical" or "Mendeleev-type" classification of hadrons);
(2) I support openly and sincerely the continuation of studies along these models when they are interpreted as jointly providing a structure model of each individual element of a unitary multiplet (for example,

page 3.

I have recently organized, as part of the functions of the Hadronic Journal, a new series of reprint of papers along quark lines, and I have appointed Professors D.B. LICHTENBERG and S.P.ROSEN as independent Editors of this yearly series); but,

(3) I favor the conduction of studies also of fundamentally different orientation on the structure problem, under the condition that they are capable of achieving compatibility with the established unitary models of classification.

What I am trying to convey is that, according to our studies, it is possible that the historical dichotomy classification/structure that was necessary for the atoms, may result to be also necessary for the hadrons.

After having identified the established part (of current trends in hadron physics) in their classification content, that is, after having separated facts from beliefs, we have passed to a critical inspection of the structure problem.

However, unlike most of our colleagues, we have initiated this study by conducting an in depth analysis of the teaching by the Founding Fathers of contemporary physics. We have discovered in this way a number of crucial "legacies" that, in our view, are directly related to the problem of hadron structure. I am referring here to

Fermi's legacy: he clearly espressed doubts on the validity of conventional geometries (and, thus, conventional laws) for the region of space occupied by a strongly interacting particle; in particular, he thought that, under the conditions of overlapping of wave packets (as apparently necessary to even activate the strong interactions), particles experience forces more general than the simplictic $f = -\partial V/\partial r$ of current use (we call these forces these days variationally nonselfadjoint integrodifferential forces, that is, superposition of local and nonlocal forces derivable and nonderivable from a potential). This implies the lack of capability to represent the interactions via a Hamiltonian; the consequential lack of existence of Lie algebras at the level of the time evolution laws; the consequential inapplicability of conventional relativity and physical laws (the very Lie algebra of the Poincaré or Galilei group cannot be even defined); and the consequential need to do a serious homework, that is, to reinspect the foundations of contemporary physics. We believe that Fermi's legacy is fundamentally open at this time..

Einstein's legacy: he became famous for not believing in the terminal character of quantum mechanics in general, and of the conventional indeterminacy of quantum mechanics in particular. In Heisenberg's words (see "Physics and Beyond", page 81), Einstein could at most tolerate quantum mechanics as a "temporary expedient". As also reported by Heisenberg, Einstein tried up to his death to identify counterexample to the conventional uncertainty. We believe that Einstein's legacy is also open

page 4.

at this time, and it is particularly related to Fermi's legacy.

Jordan's legacy:Jordan became famous for not believing, this time, in the fundamental algebraic structure of quantum mechanics, the universal enveloping associative algebra of the Lie algebra of operators, as characterized in Heisenberg's representation (in our contemporary language). He then recommended a generalization of this associative algebra into a nonassociative form he selected of commutative type for statistical considerations (the celebrated Jordan algebras).We believe that Jordan's legacy is also open at this time, and deeply linked to Fermi's and Einstein's legacies.

Pauli's legacy: he made it quite clear in his historical papers and lectures that his exclusion principle was conceived under the lack of overlapping of the wave packets (the atomic structure). Indeed, when the wave packets overlap, he expected much "stronger" forces that generally prohibit the separability of the wave function, let alone the identification of the possible antisymmetric character. We believe that Pauli's legacy is also fundamentally open at this time, and deeply linked to Fermi's, Einstein's and Jordan's legacies.

Cartan's legacy: he made it quite clear that the Riemannian geometry can incorporate only part of Newtonian mechanics, that is, only the part compatible with Galilei's relativity. In particular, Newtonian mechanics includes systems with forces that are simply not geometrizable "a la Riemann". This legacy directly touches the current beliefs in the interior problem of gravitation (only). We believe that Cartan's legacy is still open at this time and deeply related to the other legacies.

What we have done is to conduct an in depth, technical, study of these legacies. Even though much work remains to be done, one think has transpared clearly from these studies. Apart from historical reasons, these legacies appear to have a sound, strong, clear, physical foundation for the interior problem of hadronic matter, whether a hadron or a star. If you accept the experimental data that all hadrons have approximately the same charge volume, that the dimension of this volume coincides with the range of the strong interactions, and that the wave packets of these particles is exactly of that range of dimension, these legacies are simply anavoidable. Indeed, a necesaary condition to even activate the strong interactions is that particles enter into a state of mutual penetration or overlapping of the wave packets.

We have then initiated studies of forces structurally more general than the trivial $f = -\partial V/\partial r$ of current use, within the context of

- advanced mathematics (including the use of the theory of nonassociative algebras, functional analysis, and differential geometry);
- individual branches of physics (including: Newtonian Mechanics, quantum mechanics, classical and quantum field theory, classical and quantum statistical mechanics; etc.).

page 5.

The literature already accumulated on the technical studies considered here is so large to discourage an outline, and to simply prohibit it in a letter. To give you an idea,

(a) I have written two research monographs with SPRINGER-VERLAG under the title FOUNDATIONS OF THEORETICAL MECHANICS (Volume I-1978-volume II-in press), entirely devoted to the identification of rigorous methods for the treatment of the forces considered);

(b) I have written an additional series of monographs with the HADRONIC PRESS entitled LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE (Volume I was published in 1978, Volume II is in press, and volume III is scheduled for 1980); plus numerous articles on the problem; and

(c) we have a series of yearly reprint volumes of all articles in the field entitled APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS, edited by Professors H.C.MYUNG, S. OKUBO and myself (the first two volumes were printed in 1978, and we are working at two additional volumes of reprints of articles by mathematicians and physicists).

In addition we have the PROCEEDINGS OF THE WORKSHOP ON LIE-ADMISSIBILITY, plus the proceedings of a conference under independent organization by mathematicians.

I am fully aware that the reading of all this material is a task simply beyond your time, as well as the time of my colleagues at your Laboratory. To assist you, I have therefore enclosed copy of the "Chart 4.9" of my Volume II with Springer-Verlag. It presents an outline of the problem in a language readable by graduate students and researchers without a technical knowledge of the simplectic quantization and of the broader Lie-admissible quantization.

I would like to stress, however, that the technical presentation is elsewhere.

I hope you will have the time of reading this "chart" (in its nautical sense...) and have the elements, in this way, to reach an independent assessement of these legacies (they are outlined in the final part, Part 9, pages 343-349). I hope you will have in this way a chance to share our views, that is, the need to resolve experimentally the problem of the basic physical laws for the strong interactions, as an item of first research priority, with intriguing historical implications, whether the test are positive or negative for conventional laws. The problem of the structure of hadrons is, in our view, of only secondary physical relevance.

After due consultation with the members of our group, I have selected my application to your Laboratory for the test of Pauli's principle (amoung numerous tests under consideration) for several reasons. The first is that this test is the most close to maturity of formulation. The second is that it will cost expectedly less than a comparative test in high energy physics, and will call for expectedly less time. The third is that the dynamics of the test has been conceived and patterned along

page 6.

the historical legacies indicated earlier. Indeed, we have selected for the test nuclei whose charge volume is below the value predicted by the proportionality rule with the total number of nucleons. For these nuclei, nucleons are in a statistically small state of penetration of their wave packets. In turn, this

- implies expected, small, additional, terms in the nuclear of force that are not derivable from a potential (Fermi's legacy);
- implies a nonunitary character of the time evolution law, by therefore invalidating, under these conditions, the preservation of the conventional uncertainty in time, assuming that it holds at one given value of time (Einstein's legacy);
- demands a generalization of the associative character of the envelope to technically represent the broader forces admitted (Jordan's legacy);
- predicts a very small departure from the Fermionic character of identical nucleons of the selected nuclei, via forces that imply a small departure from the exact separability (Pauli's legacy)
- and implies forces that are not geometrizable "a la Riemannian", thus implying a direct test of a central assumption of current trends in the interior problem of hadronic matter; that the geometry is locally Lorentz in character (Cartan's legacy).

In conclusion, we believe that the historical, authoritative, voices of doubts on the use of conventional physical laws, algebraic structures, and geometries for the strong interactions, are such to deserve a serious consideration by our community of experimentalists, beginning most importantly with our community of nuclear experimentalists. After all, we are currently spending truly large amounts of taxpayer's money in experiments in strong interactions. What we are interested in is, whether I join your Laboratory or not, that, jointly with the conduction of these valuable experiments, we also begin the experimental verification of the basic laws for the strong interactions, irrespective of how sound they may appear to the theoreticians.

In closing, permit me to candidly express my view for the implications of these experiments "vis-a-vis" with current quark conjectures for the hadronic structure.

Apart isolated exceptions,physicists academically and financially committed to quarks generally dismiss these experiments as without sufficient motivation or, in some extreme cases (not so unusual), as without physical values.

This is due to a number of circumstances. The first is that physicists, these days, are accustomed to reach judgments on a paper by spending 60, at most 90 seconds on it. I therefore believe that quark committed physicists simply have not the technical knowledge of the studies by mathematicians and physicists on these historical legacies. I also believe that they are in good faith. The fact remains, in my view, that the 60-to-90 seconds rule simply cannot be applied to our studies to

page 7.

reach a mature, technical, professional judgment (only the methods of my monographs with Springer-Verlag call for a two-semester seminar course for a detailed presentation, and this excludes the most important tool used in our studies: the Lie-admissible algebras).

Also, I believe that quark committed physicists see, in experiments that might even remotely invalidate conventional laws for the strong interactions a "threat" to the quark conjecture.

The reason why I am candidly touching this argument is to indicate that this is not the case and that, again, this conceivable negative attitude (I have personally experienced several times) is only the result of the lack of adequate technical knowledge.

Physical results of the type of the prediction and discovery of the $\Omega^-$ and, more recently, of the $J/\psi$ particles are now part of the history of physics and will remain part of it. No further advancement in knowledge can potentially invalidate them.

It is true that, in case the legacies by Fermi, Einstein, Jordan, Pauli, Cartan and others will eventually result to be true, the notion of quark as a hadronic constituent, cannot be even defined, let alone technically treated. But this is no disaster for the quark models, in my view. The true essence of physics is a sequence of improvements of our approximation of nature. Einstein's relativity did not "invalidate" Galilei's relativity. It merely identified the physical arena in which it provides a good approximation. These relativities are crucially dependent, conceptually and technically, on <u>point-like approximations</u> of particles and action at a distance only ($f = -\partial V/\partial r$). Apart extreme technical complications (e.g., renormalization), this notion by Galilei and Newton persists in its entirety in the most advanced contemporary treatments, such as QCD. Par contre, we are trying to do the expected next step, a first, but realistic, representation of particles as <u>extended</u> objects, when in interactions at distances smaller then their dimension. <u>Assuming</u> that our studies are true, the physical validity, effectiveness and value of the quark models will remain unaffected. We would have simply identified their arena of applicability as a first approximation: the point like interpretation of hadrons and of their constituents.

In conclusion, what I am trying to convey for the quark community of your Laboratory is that the conduction of the experiment I have proposed, under no circumstance should be interpreted as intended to test the credibility of their views. After all, in Bohr's words, we are all participants and spectators of a continuing scientific process.

In this spirit, I would like to express my appreciation for your time and consideration.

RMS/ml, encls.
c.c.: Drs.B.G.Harvey and
H.P.Stapp.

Sincerely

Ruggero Maria Santilli

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 9, 1980

Dr. B.G. Harvey
Associate Director
Nuclear Science Division
Lawrence Berkeley Laboratory
Berkeley, California 94720

Dear Dr. Harvey,

Hoping that I do not abuse of your time and courtesy, I would like to add additional data to my application of October 10, 1979 for a position at your Laboratory in regard to theoretical and, in due time, experimental studies for the experimental verification of the validity or invalidity of Pauli's exclusion principle in nuclear physics.

Theoretical aspect. The studies are progressing on schedule. I enclose copy of the Table of Contents of the Proceedings of our Second Workshop on the problem. As you can see, it is a considerable effort for over 1,500 pages by distinguished mathematicians and physicists from the USA, USSR, China, and other Countries. Part A on the review of the state of the art presents, without proof, some 500 theorems, lemmas, propositions, etc. all of either direct or indirect relevance to the problem considered.

Particularly intriguing are two theorems of invalidation of Heisenberg's equations for all Hamiltonians of polynomial order in r and p higher than the second (that is, for nonlinear equations of motion). One of them has been proven by mathematicians (Abraham and Marsden) and the other by physicists (Hood, Hellman, Kobussen, and myself). By keeping in mind that nonlinear effects are rather natural in strong interactions, according to a growing consensus, these theorems are suggesting, rather forcefully, the experimental verification of the basic laws. The theorems in question are reviewed in details in my state of the art memoir of Part A of the proceedings, jointly with their technical and historical implications.

In case you desire a complimentary copy of these proceedings, please let me know, and I shall do my best to secure it from the publisher.

Experimental aspect. I am not an experimentalist and, thus, I cannot provide you with a technical assessement of this profile. Nevertheless, permit me to express on an informal basis the remarks I have received by a number of colleagues.

You are eventually aware of the experiments currently under way by Dr. C.G.Shull of MIT and a number of other participants, via neutron beams on selected cristals. Pauli principle can be (apparently) tested via these experiments via a double rotation of the cristals, either magnetically or mechanically, and then comparison of the data. If neutrons, when passing through the cristal, are exact fermions, the data should coincide after a double rotation, apart experimental errors. It appears that current initial readings give the same data, up to 1/2 degree. This, of course, does not establish a deviation from Pauli's principle (even though that predicted is of this order of magnitude). In fact, it may be due to the error for the experimental set up available.

Nevertheless, the view expressed to me by a number of colleagues is that experiments of this types can be, in principle, improved considerably in their approximation via auxiliary means.

page 2.

I do not know whether this is indeed the case .The point is that, in case the approximation of the experiment can be independently established, the deviation from the exact $720^0$ rotation may be subjected to a quantitative experimental study, rather than to guessing.

At any rate, what I would like to convey is that the experimental contact of my studies may well occur considerably earlier than what we currently believe (on my part, I was expecting no contact of this type for years, and I have been surprised when informed by independent collegues of the experiments indicated above and their potential).

Administrative aspect. As you know, I am the recipient of a grant from the Department of Energy that covers completely my salary and all my research expenses. This grant is now at its second year. I have received verbal communication of the renewal of the grant. DOE is rather pleased of the outcome of the studies (particularly of the increasing number of qualified scientists all over the World they are attracting). According to all indications available, this grant will be not only continued for a number of years, but likely increased whenever the actual contact with experiments is achieved.

My application to join your Department is therefore primarily for the administration of my grant, if at all compatible with your administrative practices. I hope in this way to achieve some stability which appears to be essential for the type of study under consideration, not only because of inherent long term character, but also because potentially open to interferences from vested interests of different orientation.

In closing, permit me to suggest that you take all the necessary time to reach your decision on my application. I am renewing my grant via local administration, so that I am not in need of a rush decision. On the contrary, I would like to rely on your judgment in regard to the best time for reaching a decision.

Hoping that I can visit you some time* and have the pleasure of meeting you, I remain

Yours, Sincerely

Ruggero Maria Santilli

RMS|ml
encls.
c.c.: Drs. Birge and Trippe

* Perhaps the theorems of invalidation of Heisenberg's equations might interest your division. I understand that a number of Editors are considering a sort of moratorium on all papers activating these theorems.

HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170

SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

January 30, 1980

Dr. B. G. HARVEY
Associate Director
Nuclear Science Division
Lawrence Berkeley Laboratory
BERKELEY, California 04720

Dear Dr. Harvey,

You might be intrigued and amused by the enclosed letter I am passing to editors and selected colleagues. It may give you information on the status of the strong interactions at the editorial level, because of the lack of, not only sufficient theoretical insight, but also of experimental information on the basic laws.

The understanding is that, to reach a broader audience, I did not use the language of the symplectic quantization and of the broader Lie-admissible quantization.

Best Personal Regards

Ruggero Maria Santilli

RMS/ml
encls.

c.c: Drs. R.W.BIRGE and T.G.TRIPPE

ABSOLUTELY NO ACKNOWLEDMENT WHATSOEVER WAS RECEIVED FROM THE LAW. BERK. LAB FOR ALL THE ABOVE CORRESPONDENCE (SECT. X) AND SCIENTIFIC ENCLOSURES.

# PART X

# U.S. NATIONAL LABORATORIES

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

February 7, 1978

Dr. R.M. Santilli
Massachusetts Institute of Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

We here in the theory group at Fermilab wish to thank you very much for your application for a position with us. At this time it is not possible for us to offer you such a position, though the competition was strong and your name was always under careful consideration.

Sincerely yours,

Henry D.I. Abarbanel

Henry D.I. Abarbanel
Theoretical Physics Department

HDIA/em

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 22, 1978

Dr. ROBERT WILSON,
Director,
FERMI NATIONAL ACCELERATOR LABORATORIES
P.O. Box 500
BATAVIA, Illinois 60510

Dear Dr. Wilson,

Just a few words to inform you that the first issue of the HADRONIC JOURNAL has been printed in April 1978, on schedule, and distributed to subscribers.

A copy of the cover of the issue is enclosed for your consideration. I hope you will be pleased with the results of our efforts.

To my knowledge and understanding, it appears that potentially new approaches to hadron structure are now surfacing, with intriguing experimental and theoretical, potential implications. I shall take the liberty of keeping you personally informed in case they materialize.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS|is
encl.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

May 22, 1978

Professor WOLFGANG K. H. PANOFSKY,
Director,
STANFORD LINEAR ACCELERATOR CENTER
Stanford University
STANFORD, California 94305

Dear Professor Panofsky,

Just a few words to inform you that the first issue of the HADRONIC JOURNAL has been printed in April 1978 and distributed to subscribers on schedule.

A complimentary copy was mailed to Professor Joseph BALLAM and a regular subscriber's copy has been mailed to your library. A copy of the cover of the issue is enclosed for your consideration. I hope you will be pleased with the results of our efforts.

Again, I would like to thank you for the courtesy of your assistance.

To my knowledge and understanding, it appears that potentially new approaches to hadron structure are now surfacing, with intriguing, experimental and theoretical, potential implications. I shall take the liberty of personally informing you, in case they materialize.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS/is
encl.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address*
SLAC, P. O. Box 4349
Stanford, California 94305

February 3, 1978

Dr. Ruggero Maris Santilli
Editor-in-Chief
Hadronic Journal
Department of Physics
Harvard University
Cambridge, Mass. 02138

Dear Dr. Santilli:

Thank you for your letter of January 29 in which you request me to identify an experimental high-energy physicist to serve on the Editorial Council of the "Hadronic Journal." I would like to suggest Professor Joseph Ballam. Dr. Ballam is the Associate Director of the Research Division of SLAC and is well qualified to serve in such a position.

Sincerely,

W. K. H. Panofsky
Director

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 6, 1978

Dr. W.K.H.PANOFSKY,
Director
Stanford Linear Accelerator Center
Stanford, California 94305

Dear Dr. Panofsky,

I would like to express the sentiments of our appreciation for the courtesy of your letter of February 3, 1978.

I enclose copy of our invitation to Dr. Joseph Ballam for membership in the Editorial Council of the Hadronic Journal.

Best Personal Regards

Ruggero Maria Santilli

RMS|is

enc.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 6, 1978

Professor JOSEPH BALLAM
Associate Director of the Research Division
Stanford Linear Accelerator Center
Stanford, California 94305

Dear Professor Ballam,

we are taking the liberty of informing you of the recent organization of a new journal in high energy physics, called HADRONIC JOURNAL, according to the enclosed announcement.

It is our firm determination to come out with a quality journal which, in due time, could represent a valuable medium of publication of original contributions in fundamental issues of high energy physics. But, in order to achieve this objective, we need qualified advice.

We are therefore setting up the EDITORIAL COUNCIL of the journal. As customary, members of the Council are expected to provide confidential advice to the editors on papers and matters which go beyond the normal referee routine. Regrettably, we do not have a budget for a honorarium in 1978, although it appears that we will likely have a budget for logistic expenses. The projected yearly time per member will be quite minimal, but quite valuable for the journal.

Following a kind indication by Dr. W.K.H. Panofsky, we are here inviting you to became a member of this Editorial Council, jointly with a number of collegues from selected U.S. and foreign Institutions.

Thanking you for your consideration, we remain at your disposal for any additional information you might need.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli, for the
HADRONIC JOURNAL

c.c.: Dr. W.K.H.Panofsky, Director
SLAC

encl.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address*
SLAC, P. O. Box 4349
Stanford, California 94305

February 15, 1978

Professor Ruggero Maria Santilli
Lyman Laboratory of Physics
Harvard University
Cambridge, Ma. 92138

Dear Professor Santilli:

I am happy to accept your invitation to become a member of the Editorial Council of the Hadronic Journal.

Sincerely yours,

J. Ballam
Professor and
Associate Director
Research

JB:hm

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

February 23, 1978

Professor Joseph Ballam,
Associate Director of Research
Stanford Linear Accelerator Center
P.O.Box 4349
Stanford, California 94305

Dear Professor Ballam,

It is a pleasure to express the sentiments of our appreciation for your acceptance of the membership in the Editorial Council of the Hadronic Journal.

We are currently studying the best way to keep you fully informed of the initiatives and activities of the Journal while taking the least possible of your time.

We contemplate to consult with you in this respect in the near future.

Best Personal Regards

Ruggero Santilli

Howard Georgi

Ruggero Maria Santilli

HG-RMS/cgg

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 3, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
SLAC
Stanford, California 94305

Dear Professor Ballam,

You will be pleased to know that, in addition to your partecipation, we have the following members of the EDITORIAL COUNCIL of the HADRONIC JOURNAL
- Nobel Laureate CHEN NING YANG,
- Professor ROBERT MERTS, Director of the Instituut voor Theoretische Mechanica (an old European school in theoretical Mechanics) and
- Professor HYO CHUL MYUNG of the Univ. of Northern Iowa, Department of Mathematics (a leading expert in abstract algebras).

Additional memberships are expected and I shall inform you as soon as they are finalized.

The first issue of the Journal is also approaching finalization. Some of the confirmed papers are the following:
- S. STERNBERG, Harvard, Dept. of Math, on a geometric approach to Yang Mills,
- T. KATO, Berkeley, Dept. of Math., on certain aspects of scattering theory (imprimitivity theorems),
- S. OKUBO, Rochester, on certain restrictions for semisimple gauge groups,
- C.N.KTORIDES, Univ. of Athens, Greece, on a stimulating conjecture for Lie-admissible quantization in field theory (for couplings not derivable from a potential),
- H.C.MYUNG, on a review of the state of the art in mathematical literature on Lie-admissible algebras,

as well as possible additional papers from Lyman Laboratory.

I should add that these papers have been confirmed by their authors but I have not received them as of now, with few exceptions. If you want to have a preview of any of these papers, please let me know, I would be happy to mail them to you. In any case you will receive a complimentary subscription from the publisher with prompt mailing of the issues for your file.

As for the subsequent issues, I would appreciate the courtesy of a confidential indication of collegues at Stanford who are about to complete valuable papers for my independent invitation. You can trust in my utmost discretion.

I am also contacting you because I would greatly appreciate your confidential advice on the following delicate project.

We have all followed with interest the valuable studies by Fairbank at Stanford. I am, however, under the impression that our community is somewhat misinformed because of the dilution of the issue in specialized journal as well as nonspecialized magazines (see the MORPURGO-FAIRBANK letters in the Dec. 1977 issue of PHYSICS TODAY).

I believe that the presentation in the HADRONIC JOURNAL of an accurate state of the art in the search for quarks, whether possible or otherwise recommendable and, particularly, if contain an identification of the controversial points, would be a service to our community. In particular it could stimulate subsequent studies for a possible future resolution.

I would therefore greatly appreciate your advice on the following issues:
- do you think that it is advisable at this moment to attempt this identification of the state of the art in the search for quarks?
- If so, do you think we should invite contributions from different currents for joint publication (that is, in vite Fairbank as well as Morpurgo and other qualified contributors)?

My instinct tells me that the publication of only one view would imply partisanship by the HADRONIC JOURNAL in the issue and therefore could be in the short or long run counterproductive. To be more specific, I believe that we should either publish jointly representatives of different currents for the independent assessement by the interested readers, or skip the issue at this time. But, quite frankly, I do not know whether I can achieve a mature judgement alone. Your confidential advice would be therefore greatly appreciated.

In conclusion, if you think that it is advisable at this time, I could personally in vite FAIRBANK as well as MORPURGO and any other researcher you might suggest to write a review paper on their studies under the proviso that
- each researcher is made aware of the invitation for the other for joint publication, and that
- each researcher has full opportunity to inspect the paper by the other prior to the joint publication.

If you think that such an attempt is advisable at this time, I could draft a letter of invitation for your confidential inspection. In essence, I believe that MORPURGO could be interested in such a project, but I have no indication with respect to FAIRBANK.

Almost needless to say, particularly in view of the delicate nature of the issue, you can trust in my utmost confidentiality. To be more specific, I intend to disclose any possible advice from you to no third party, unless specifically authorized by you.

Please take all the necessary time to consider this issue because it is not of utmost priority. Rather than anwering by letter, please feel free to call me at any time of your convenience. My office phones are (617) 495 3212 (Lyman) and (617) 495 2170 (Dept. of Math.-most of the time) although it is sometime difficult to reach me during day time. You can always reach me at home (617) 969 3465 from 7 p.m. on, Boston time.

Again, I have no words to express my appreciation for your partecipation.

Sincerely

Ruggero Maria Santilli

P.S. You will be pleased to know that a research grant application I submitted with Shlomo Sternberg to the Department of Energy (formerly ERDA) on the old idea that the strong hadronic forces are not derivable from a potential, has been recently funded. As an incidental note you might be interested to know that the Journal was launched after an informal clearing with DE (David Peaslee).

HARVARD UNIVERSITY

Area Code 617
495-3352

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

July 19, 1978

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

I am contacting you for a respectful , but open and passionate appeal on the current situation of hadron physics, as well as for your advice and assistance on a promising but delicate step I have implemented in the HADRONIC JOURNAL. The situation of hadron physics is, in my view, so grave, to demand the use of a candid, nonacademic language as a prerequisite for the identification of the current problems in a way as clear as possible.

As we all know, gigantic financial investments have been devoted during the last decade to theoretical and experimental studies on the problem of hadron structure. The net outcome of these investments is essentially given by the current quark models. Permit me the liberty of indicating that, in my view, these models are fundamentally removed from the traditional values of physical research, that is, the manifestation of a scientific truth. Instead, they are the expression of mere opinions by individual or groups of researchers, such as the opinion that the quarks are the physical constituents of hadrons, complemented by the opinion that they confine, complemented by the still further opinion of rather complex decay processes sometimes mediated by additional yet unidentified particles, etc.

But, perhaps, these are opinions with only secondary implications. What I consider more fundamental is the tacitly implemented opinion that the relativity and quantum mechanical laws which have proved so effective for the atomic (as well as nuclear) constituents necessarily apply also to the hadronic constituents in their currently known form, without even considering the problem of their direct, explicit and unequivocal experimental verification.

I am confident of your agreement that we simply cannot continue indefinitely to conduct basic research on the basis of mere beliefs by individual physicists on fundamental issues. It is time to subject the current epistemological, theoretical and experimental attitudes to severe scrutinies and profound revisions.

The promising, but delicate step implemented in the first issues of the HADRONIC JOURNAL by a number of collegues (Professors Engels, George, Henin, Ktorides, Mayné, Myung and Nobel Laureate Prigogine) and myself consists of a tentative identification of the revisions considered which appear advisable. Additional papers by other authors (e.g., Professor Kim) along the same lines are likely to appear in the subsequent issues. I outline below my personal impressions on the outcome of these studies.

THE EPISTEMOLOGICAL PROFILE. As we all know, high energy physics is currently plagued by a sea of minute incremental contributions which has lately reached alarming proportions. Jointly, we have experienced the complete lack in recent times of any fundamental progress which is even partially comparable to the great achievements of the first part of this century.

Hoping in a benevolent impunity, I intend to be on record by indicating that the current grave situation of high energy physics is a direct consequence of a widespread resistance at a number of decisional levels against the consideration of truly fundamental issues, essentially motivated by the strictly antiscientific belief that we have reached the terminal laws of the physical universe.

I have experienced numerous occurrences of this nature during my past academic life. For instance,

my attempts in the past of establishing a dialogue on the need for an experimental verification of Einstein's relativity and Pauli's principle for the hadronic constituents have met with a categorical refusal to even consider the issue. Similarly, I know of a number of collegues with young brilliant minds who have been forced to truncate their promising studies on fundamental issues to academically survive.

In the interest of the pursuit of human knowledge, it is vital that we reestablish the traditional priorities of basic research which have produced such fundamental contributions up to the earlier part of this century and lately abandoned, via the traditional way of their conduction, that is, via criticisms of experimentally unverified basic laws.

According to this view, utmost priority should be given to the studies of the problem of the basic physical laws which are applicable to strong interactions in general and to the hadronic constituents in particular. The studies of the construction of specific models of hadronic structure is of purely secondary physical relevance. I am confident in your scientific vision to see that, after all, established laws may well result to be inapplicable in their currently known form in the arena considered according to much of the historical occurrence at the atomic level. In the final analysis, there exist profound physical differences between the electromagnetic and strong interactions which may well result in the need of the courageous construction of covering laws for the latter interactions. In turn, if this resulsto be the case, it will inevitably imply profound differences in the quantitative characterization of "constituents under electromagnetic interactions", that is, atomic constituents, and "constituents under strong and electromagnetic interactions", that is, hadronic constituents under covering laws. I am sure you realize that, if the established laws of the atomic phenomenology will result to be inapplicable to the hadronic structure, the quark conjecture is ruled out in a final form. The need for a revision of current trends is then simply imperative.

The HADRONIC JOURNAL was born for the specific objective of initiating the restoration of traditional priorities of basic research. Indeed, I give utmost priority to studies on fundamental issues, while papers of minute incremental nature on established trends are generally rerouted to other journals. I sincerely hope that independent and more effective restorations of these crucial priorities is initiated also in other Institutions as soon as possible.

THE THEORETICAL PROFILE. As we also know, the quark conjecture is literally dominating the current hadron physics. This situation, in my view, has also reached alarming proportions, to the point that the current technical and review literature either explicitly or implicitly exclude the possible study of fundamentally different models. There is no doubt that the quark models are scientifically valuable, deserve due attention and their study must be continued. However, the current virtual restriction of studies on the fundamental problem of comporary physics along only the quark conjecture is, in my view, strictly antiscientific.

Also hoping in a benevolent impunity, I intend to be on record by indicating that this grave situation is a direct consequence of a widespread resistance at a number of decisional levels against the support, promotion and guidance of any research on hadron structure other than quark oriented. I have personally experienced numerous occurrences of this type in my past academic life and I know of other occurrences experienced by other researchers.

To avoid a monopolistic condition and conduction of research on the fundamental problem of contemporary physics, ultimately based on mere opinions by individual researchers, it is vital to implement a well balanced community of basic studies in which, jointly with the continuation of efforts along the quark conjecture, fundamentally different approaches are attempted, solicited and guided for a comparative confrontation with physical reality. To be specific in this other crucial point, it is not sufficient that responsible officers at the decisional levels express a benevolent form of reception toward the construction of truly new models of structure, as conventionally stated, when they have reached maturity (which is the equivalent of the truncation of such studies). Instead, it is essential that they give proof of promoting, guiding and supporting them in the same measure as that for any other plausible approach.

The second objective of the HADRONIC JOURNAL is precisely that of initiating a nonmonopolistic presentation of research on hadron structure in which, jointly with papers on the quark models, you can see basically different approaches. Again, this is not the result of a benevolent, passive attitude on my part. Instead, it is the result of a laborious effort of promotion and support for valuable studies irrespectively of whether quark or non-quark oriented, because the problem of hadron structure is fundamentally open at this time and the "authority of a thousands" (in GALILEI's words) of quark believers is not sufficient to establish a scientific truth. I sincerely hope that independent and more effective achievements of a well balanced condition and conduction of research on hadron structure is implemented also in other Institutions as soon as possible.

THE EXPERIMENTAL PROFILE. As we also know, the experimental efforts of the last decade have produced clear contributions to human knowledge, such as the discoveries of the $\Omega^-$ and $J/\psi$ particles. However, these efforts have failed to produce the final solution of the problem of hadron structure, they have failed to provide clear means of selection on clear physical grounds among an ever increasing number of quark models, and they have failed to give even a minimal but effective guidance for the final orientation of theoretical studies on structure.

It is at this point where my appeal for benevolent impunity reaches its climax. I intend to be on record by indicating that, unless a profound revision of the current experimental trends is implemented as soon as possible, the responsible officers at the decisional levels will acquire a historical responsibility of delaying the advancement of fundamental human knowledge.

As we all know, the current experimental trends are essentially restricted to the identification of new particles and their data in symbiotic condition with unitary models. I believe that these unitary models with their impressive experimental backing have produced a Mendeleev-type classification of hadrons of clear physical relevance and of virtually conclusive character. However, I believe that the problem of structure demands fundamentally different (although compatible) approaches and, thus, fundamentally different experiments according to exactly the same dichotomy classification-structure which resulted as necessary at the atomic level. I simply do not see how the discovery of new particles can provide the final solution of the problem of hadron structure (besides a further proliferation of different unknown quarks and, thus, a further abandonment of traditional scientific values), when so many already discovered particles have failed to do so.

The third and most delicate objective of the HADRONIC JOURNAL is to attempt the identification of the future orientation of experimental high energy physics which is needed to achieve a truly effective conduction of research on the problem of hadron structure.

Copies of the first issues of the HADRONIC JOURNAL are enclosed for your consideration. Additional copies of representative papers have been separately mailed to you, in case you intend to submit them to some of your associates, in addition to the regular subscriber 's copy which should be in your library. Additional reprints are at the disposal of interested collegues within the limitation of my budget.

The revision of the future orientation of experimental high energy physics I have proposed (HADRONIC JOURNAL 1, 223-423 (1978) and 1, 574-901 (1978)) is that utmost priority be given to the experimental tests of the validity or invalidity of established relativity and quantum mechanical laws for the strong interactions in general and the hadronic constituents in particular. According to this proposal, the discovery of new particles and the problem of the right model of structure are of purely secondary priority.

Permit me to indicate that my rudimentary and speculative studies on the quantitative representation of the possible invalidity of Einstein's relativity and Pauli's principle for the hadronic constituents (A) are primarily intended to stress the need of their verification; (B) are deficient and incomplete on technical grounds, being the results of an isolated researcher; and (C) are not intended to express the sole approach to hadron structure. Nevertheless, you should be aware that these papers have been specifically written in a provocative language as a result of a laborious search to stress the grave situation of hadron physics and to emphasize the need of the revisions here outlined.

You might be also interested to know that Nobel Laureate Prigogine and his collaborators have presented an independent analysis (HADRONIC JOURNAL 1, 520-574 (1978)) based on a different technical profile (thermodynamical), but I believe inspired along similar lines: it is time to subject the basic physical laws used for hadron structure to a severe scrutiny. The problem of the right model of structure is of purely secondary priority.

Permit me to confess that, as Editor in Chief of the HADRONIC JOURNAL, I am facing a responsibility (I have, after all, created), which is growing considerably beyond my knowledge and possibilities. Indeed, to achieve the well balanced condition and presentation of research stressed earlier, I have solicited contributions along two opposite approaches to the same fundamental issue: validity and invalidity for the strong interactions of the experimentally established knowledge for the electromagnetic interactions. As a result of this action, intriguing, but quite delicate papers are likely to arrive in the near future.

Owing to this situation, I would be grateful for your advice and assistance on the following points.

I - GUIDANCE ON EDITORIAL ASPECTS. I would be grateful whether you can let me know your viewpoint on the following issues.

I-A. Do we possess at this time direct, explicit and unequivocal experimental evidence that the basic physical laws of the electromagnetic interactions (Einstein's special relativity and Pauli's exclusion principle, in particular) are verified for the hadronic constituents?

I-B. If not, do you think that a proper presentation of opposite viewpoints on this fundamental issue is scientifically valuable and effective? and

I-C. If yes, do you have specific guidelines in which the debate should be contained?

II - ASSISTANCE TO ACHIEVE A WELL BALANCED PRESENTATION OF RESEARCH. In essence, my speculative, quantitative studies on a possible inapplicability of established laws for the hadronic constituents have apparently stirred up an interest beyond my cautious expectation. The net result is that a number of authors are apparently working on contributions along the idea of the inapplicability of established laws. In turn, I have reason to expect that the next few issues of the HADRONIC JOURNAL will present contributions along only this profile. This is contrary to the principle of a well balanced conduction and presentation of research to which I have been educated.

I would therefore be grateful whether you can assist me in the identification of theoretical high energy physicists interested in balancing this situation with articles in the HADRONIC JOURNAL or in any other journal of their preference specifically devoted to the presentation of the quantitative reasons why, according to the quark models, the basic laws of the electromagnetic interactions also apply to the hadronic constituents.

In essence, I am asking that quark believers sit down, organize their thoughts, eventually reinspect available experimental data (e.g., from deep inelastic scatterings), and provide the scientific community with the quantitative reasons why they expect the validity of established relativity and quantum mechanical laws within a hadron.

Am I asking too much?

III - ASSISTANCE ON THE TRULY IMPORTANT ASPECT, THE EXPERIMENTAL TESTS OF ESTABLISHED RELATIVITY AND QUANTUM MECHANICAL LAWS FOR THE HADRONIC CONSTITUENTS.

As the recipient of a research grant from the Department Of Energy, it is my duty to identify the problem considered and provide my contribution for its future resolution either in favor or against established laws. Permit me to confess that, after having spent a number of years on this issue, I have reached the conclusion that the issue considered cannot be resolved either way at the theoretical level only, beyond the level of personal opinions, or viewpoints, or conjectures by individual physicists which in any case remain far from a scientific truth. This is due to the fact that, as we know, a complex topology of assumptions (e.g., in the current so-called experimental tests of QCD), in my view, does not establish an unequivocal scientific truth. This is here not intended

to diminish the scientific values of theoretical studies, which are and will remain essential. But, being a theoretician, I must stress that the final resolution of the fundamental issue considered must be achieved via clear, direct and unequivocal experiments.

The outcome of my tentative studies on this issue is essentially the following.

It appears that the first experimental tests on the problem of the physical laws for the strong interactions can be conducted by restarting at the nuclear level. In nontechnical terms, in my second paper (Section 4-21 and page 882) I have proposed the experimental test whether Pauli's exclusion principle is exactly valid for the nuclear constituents (that is, it is valid in the same measure as that for the atomic constituents), or very small deviations can be experimentally established (e.g., of the same order of magnitude as those of some of the violations of discrete symmetries in particle physics). The indication of some experimental nuclear physicist interested in considering such a test would be appreciated.

Owing to a number of technical reasons, possible very small deviations from Pauli's principle at the nuclear level offer the intriguing perspective of large deviations at the hadronic level. The technological aspect, however, now becomes considerably more involved. After all, I am here referring to the transition from the experimental detection of a hadron as a whole, to that of the behaviour of its constituents. For this reason I have absteined from suggesting specific tests in my papers at this time, a part from generic proposals presented in Section 5.5 of my second paper. Specific proposals will appear in the subsequent issues of the HADRONIC JOURNAL by other authors, and others are available in the existing literature, although they have been ignored until know, to my knowledge.

I would therefore appreciate the indication of experimental high energy physicists interested in

III-A: conducting a preliminary, orientational study for a more adequate identification of the problem;

III-B: studying the possibility whether available experimental data can be effectively used as tests; and

III-C: conducting a feasibility study whether new experiments can be specifically conceived and realized within the context of the currently available technology on the fundamental issues, that is, validity or invalidity of Einstein's relativity and Pauli's principle for the hadronic constituents.

Being supported by DOE, as indicated earlier, I am taking the liberty of sending copy of this letter to Drs. DEUTCH, HILDEBRAND, KANE, PEASLEE and WALLENMEYER of such Governmental Agency. I am also taking the liberty of sending a courtesy copy of this letter to Drs. KRUMHANSL and BARDON of the National Science Foundation.

In case your polyhedric duties and responsibilities will allow you to answer at some future time of your convenience, I would appreciate whether you can send a courtesy copy of your letter also to the indicated officers.

In closing, permit me to express the sentiments of my most sincere esteem for your person and of gratitude for the consideration, courtesy and time you have already provided for me during other occasions. Under no circumstances you should identify in the open language of this letter my intention of being offensive. If I failed to realize this point, please accept my most sincere apologies.

As a personal note, permit me also to express my unlimited faith that the U.S. community of basic studies will indeed achieve the common objectives either as outlined in this letter, or in a more mature form resulting from the possible contribution by other collegues. After all, despite my past difficulties recalled earlier, I am happy to testify that Harvard University and the U.S. Department of Energy have given clear proof of scientific vision by allowing the conduction of my unconventional, conjectural and speculative studies at the frontiers of knowledge.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

RMS|cgg

NOTE OF JUNE 1, 1984: ESSENTIALLY THE SAME LETTER WAS MAILED TO:

# Dr. R. R. WILSON, DIRECTOR OF FERMILAB, and

# Dr. G. H. VINEYARD, DIRECTOR OF BROOKHAVEN NATIONAL LABORATORIES

July 1978

## RUGGERO MARIA SANTILLI

### Biographical notes

Santilli, a member of the American Physical Society, the Italian Physical Society and the Society of the Sigma Xi, received his Ph.D. in physics at the Theoretical Physics Institute of the University of Torino, Italy, in 1966. He then was visiting scientist at a number of international institutions, including the International Centre for Theoretical Physics of the IAEA of Trieste, Italy, the Centre Europeenne pur la Reserche Nucleare of Geneva, Switzerland, the Institut Henri Poincaré of Paris, France, and the Center for Theoretical Studies of the University of Miami, Florida. From 1970 until 1976 he was in the faculty of the Department of Physics of Boston University, Boston, Massachusetts, where he left after promotion to associate professor of physics (without tenure). From January 1976 until August 1977 Santilli was visiting scientist at the Center for Theoretical Physics of the Massachusetts Institute of Technology. Since September 1977, Santilli is a research scientist at the Science Center of Harvard University and corecipient of the grant from the Department of Energy No. ER-78-S-02-4742.A000. It should be here indicated that Santilli's position at Harvard is of non-teaching, non-tenured and terminal nature.

Santilli is the author of some fifty papers published in various journals of theoretical physics and in differentiated topics. In axiomatic field theory Santilli performed the extension of the PCT theorem to all discrete space-time symmetries (Phys. Rev. D10, 3396 (1974)) a generalization of the Haag theorem to the first eight vacuum expectation values of scalar fields (Phys. Rev. D7, 2447 (1973)) and a U(3.1) analytic extension of vacuum expectation values (Nuovo Cimento 2A, 965 (1971)). In mathematical methods in theoretical physics, Santilli identified a covering of the Lie algebras called Lie-admissible (Nuovo Cimento 51A, 74 (1967) and Supplemento al Nuovo Cimento 6, 1225 (1968)) and pointed out their direct physical application in Newtonian mechanics via the brackets of the time evolution law for Hamilton's equations with external forces nonderivable from a potential (Meccanica 1, 3 (1969)) as well as in field theory for couplings nonderivable from a Hamiltonian density (contributed paper in "Analytic Methods in Mathematical Physics", Gilbert-Newton Editors, Gordon and Breach (1970), proceedings of the Indiana conference of June 1968). In elementary particle symmetries, Santilli presented a derivation of Poincaré covariance from causality requirements in field theory (Int. J. Theor. Phys. 3, 233 (1970)), causality restrictions on relativistic extensions of internal symmetries (Int. J. of Theor.Phys. 2, 201 (1969)) and other contributions. In plasma physics Santilli identified the phase-space symmetries of a relativistic plasma ( Nuovo Cimento 56A, 323 (1968)), worked out a Lie-admissible model wereby instabilities are linked to dissipative effects (Lett. Nuovo Cimento 2, 449 (1969)) and other contributions. In general theory of gravitation, Santilli presented an attempt of removing the problem of unification, consisting of the quantitative construction of the exterior gravitational field of hadrons with the sole use of the structure fields and proposed the experimental test of the prediction of current theories according to which any electromagnetic field is the source of the gravitational field via the use of available giant magnets (Annals of Physics, 83, 108 (1974)). In Classical Field Theory Santilli proved a theorem on the necessary and sufficient conditions for a system of second-order partial differential equations to admit a Lagrangian representation, via the implementation of the conditions of variational selfadjointness within the context of the calculus of exterior forms in general and the converse of the Poincaré lemma in particular; identified a computerazable method for the construction of a Lagrangian for the representation of systems with arbitrary local couplings, when its existence is guaranteed by the integrability conditions; and pointed out a number of applications of the underlying methodology to engineering via the optimal control theory, to space mechanics and missile control, nonconservative nonlinear plasma physics, Newtonian Mechanics, and high energy physics. This resulted in a methodology now known under the name of

the Inverse Problem of Classical Mechanics (Annals of Physics, 103, 354 (1977), 103, 409 (1977), 105, 227 (1977), MIT-CTP preprints Nos. 606, 607, 608, 609 and 610 (1977), also see below in the monograph section). In Newtonian Mechanics, Santilli made his first comprehensive contribution as a result of a project conducted since the times of his graduate studies, the construction of a generalization of the Galilei relativity for Newtonian systems which are nonconservative and Galilei form-noninvariant. The emerging covering of the Galilei relativity is nonrelativistic and classical and, as such, it is independent from existing relativistic and quantum mechanical extensions. This work therefore indicates the possible existence of generalizations of established relativity ideas for the case of forces nonderivable from a potential. The proposed covering relativity was the result of the previous identification of two complementary methodologies for the representation of the broader systems considered, that of the Inverse Problem and that via the use of the Lie-admissible algebras, the latter having the dominant constructive role (Hadronic Journal 1, 223-423 (1978), also, see below in the monograph section). In hadron physics Santilli made his most speculative contributions. He identified a possible physical origin of strong hadronic forces as being local but more general than the Lorentz force, that is, nonderivable from a potential, and then pointed out in details that for these broader structure forces the established relativity and quantum mechanical formulations are inapplicable. Therefore, he proposed the experimental test of established laws for the hadronic constituents as the crucial prerequisite for any meaningful orientation of effective studies on the structure. He then worked out the rudiments of a Lie-admissible quantization of strong hadronic forces nonderivable from a potential and of his covering of the Galilei relativity. Finally, he applied these broader formulation for the explicit construction of a structure model of mesons and the confrontation of the predictions with experimental data. It essentially emerged that if conventional laws are assumed as valid for the hadronic constituents as in current unitary trends, the known problematic aspects on the identification of the constituents with physical particles (or for their confinement) appear to be unavoidable. If instead, broader acting forces and covering Lie-admissible formulations are assumed, the hadronic constituents can be identified with physical particles directly produced in the spontaneous decays (Hadronic Journal 1, 574-901 (1978)).

Santilli is also the author of a number of research monographsand lecture notes. Springer-Verlag, Heidelberg, is currently printing in the series "monographs in physics" the works Foundations of Theoretical Mechanics, Volumes I and II. Hadronic Press Inc., Nonantum, Ma, is printing the series Lie-admissible approach to the hadronic structure, Volumes I, II and III. Santilli's lecture notes were printed in Italian by the University of Torino (on Lie algebras) and by the Institute A. Avogadro of Torino (in nuclear physics).

Santilli has a number of years of teaching experience in prep courses (in Italy), undergraduate courses (in Italy and at Boston University), graduate courses (at Boston University on Classical Mechanics, Quantum Mechanics, mathematical methods in theoretical physics and Calculus of Variations) and seminar courses (at Boston University on Hadron Physics, Regge pole theory, and Interacting systems with nonintegrable subsidiary constraints). His last seminar course has been delivered at Harvard University in the fall term of 1977 on the methodology of the Inverse Problem and its applications.

Santilli has conducted referee work for a number of journals, including Phys. Rev., J. Math. Phys., Annals of Physics, J. of Physics, Nuovo Cimento and others. He is the founder and the Editor in Chief of the Hadronic Journal.

Santilli is married with two cildren age 9 and 11. He is a permanent resident of the States since 1967. Santilli's extracurriculum interests are also varied. Among other activities, he has been chairman of the Board of Directors of a Massachusetts Corporation for four years (from 1970 until 1974).

Ruggero Maria Santilli

# Foundations of Theoretical Mechanics I:

## The Inverse Problem in Newtonian Mechanics

Texts and
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Hadronic Press Monographs in Theoretical Physics
Number 1

**RUGGERO MARIA SANTILLI**
Harvard University
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# LIE-ADMISSIBLE APPROACH TO THE HADRONIC STRUCTURE

Volume 1

## NONAPPLICABILITY OF THE GALILEI AND EINSTEIN RELATIVITIES?

**Hadronic Press, Inc.**
Nonantum, Massachusetts 02195, U.S.A.

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RUGGERO MARIA SANTILLI
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July 19, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Ballam,

I enclose a complimentary copy of the second issue of June of the HADRONIC JOURNAL. You will be pleased to know that the journal is picking up momentum as a result of its formula. As you know, we provide the fastest possible distribution of important papers without regards to length (our current average between reception and distribution of articles is 45 days) as well as without publication charges. Also, the journal might well become, in due time, that with the highest percentage of rejection in the trade owing to a number of factors, such as (a) our lack of interest in minute incremental contributions in established trends which are rerouted to other journals; (b) our emphasis in thought provoking, potentially innovative papers in relevant issues; as well as (c) a limitation on the number of papers per issue we have to comply with.

In this latter issue we have implemented a promising, but delicate step, as presented in details in the enclosed letter of July 19, 1978 to Professor W. K. H. PANOWSKY.

Your consideration of this material would be very much appreciated. Your advice and guidance in the future orientation of the journal would be gratefully acknowledged. In particular, please let me know whether you want to personally inspect possible future papers, prior to their publication, on the intriguing debate under way: validity or invalidity for the strong interactions of the experimentally established knowledge for the electromagnetic interactions.

Please keep notice of my new address for the forthcoming academic year, as given above.

Sincerely

Ruggero Maria Santilli

RMS|is

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July 19, 1978

Dr. H. D. I. ABARBANEL,
Theoretical Division
FERMILAB
BATAVIA, Illinois 60510

Dear Dr. Abarbanel,

I acknowledge receipt of your recent communication to the effect that my application for a research position at the Theoretical Physics Division of Fermilab for the study of the problem of the experimental verification of Pauli's exclusion principle and Eisntein's special relativity for the hadronic constituents, had been declined.

I sincerely hope that this refusal was the result of insufficient funding or other reasons not related to the topic of my studies. In any case, you can rest assured that I understand and respect in full such decision.

I do feel obliged, however, to clearly and openly express my utmost concern on the current conduction, operation and policy of the Theoretical Division of FERMILAB. I believe that this division is:

- monopolistic, in the sense that it has only conducted research based on the conjecture that quarks are the constituents of hadrons;
- unbalanced, because of the literal lack of diversification of studies on the fundamental problem of contemporary physics; and
- of marginal effectiveness, in the sense that the virtual entire theoretical production on the problem of hadron structure conducted in this division in recent times is devoted to minute aspects along mere opinions by groups of physicists, without any direct consideration of truly fundamental physical problems.

For more details on my view, you may consult my recent letter to Professor WILSON, copy of which is enclosed.

I would like to stress that this candid expression of my concern is not related to the negative decision on my application. To understand this, you must realize that I have written similar letters also to other Institutions in which I have been invited with full support. Also, just while receiving your letter, I was in the process of writing you to ignore my application. I hope you understand that I simply do not have time to visit your division now, owing to my research committments with a grant from a U.S. governmental agency, my contractual committment with Springer-Verlag and Hadronic Press to release for printing a number of research monographs, my duties as Editor in Chief of the fast growing HADRONIC JOURNAL, my accademic involvement at Harvard, etc.

Very Truly Yours

Ruggero Maria Santilli

c.c.: Professor R. WILSON, Fermilab.

RMS|cgg

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July 19, 1978

Professor SIDNEY D. DRELL, Deputy Director
Stanford Linear Accelerator
STANFORD, California 94305

Dear Professor Drell,

Permit me the liberty of respectfully, but candidly expressing my irreconciliable disagreement with the content and inspiration of your article in the June issue of PHYSICS TODAY entitled "When is a particle?".

In essence, I would have accepted your article in its entirety if inspired by the clearly stated objective of presenting only one plausible view on the problem of hadron structure. On the contrary, in reading the article I reveiced the perhaps erroneous feeling of expression of your personal viewpoint that the quark conjecture is the only possible approach to hadron structure and of your personal epistemological efforts to justify it, which I consider unproductive as far as basic research is concerned, particularly in relation to young readers.

In case you are interested to my viewpoint, I enclose copy of a letter of July 19, 1978 to Professor W. K. H. PANOFSKY, which is precisely intended as a candid expression of criticisms toward the current monopolistic attitude of quark supporters. In case you are interested in technical profiles, I enclose copies of my papers in this topic.

Almost needless to say, any criticism would be received with noting but sincere gratitude.

In closing, permit me to confess that the writing of this letter has been for me reason of considerable regret. Via your invaluable prior writings, you have been one of my theachers of theoretical physics. I have noting but sentiments of sincere esteem in your person. Therefore, your should not identify, under any circumstance, my intention of being offensive. I simply felt a moral obligation of candidly expressing my viewpoint to you on this controversial issues of the ever increasing plurality of unidentified, different quarks, in the hope of only stimulating a moment of reflection.

Very Truly Yours

Ruggero Maria Santilli

Ruggero Maria Santilli

RMS|cgg

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address*
SLAC, P. O. Box 4349
Stanford, California 94305

July 27, 1978

Dr. Ruggero Maria Santilli
Science Center, Room 331
1 Oxford Street
Cambridge, Mass. 02138

Dear Professor Santilli:

Thank you very much for your letter of July 19, 1978 which I read with great interest. Let me say first that I do not view the situation in hadron physics as being as grave as you indicate. On the contrary speaking as an experimentalist, I am impressed by the recent, almost explosive evolution of new experimental information and it comes as no surprise to me that the theoretical fraternity is not united in its approach to dealing with this new flood of data.

You are criticizing the fact that the majority of theoretical physicists are focusing their calculations bearing on these recent results on variants of a quark model, with particular emphasis being put on the question of confinement. You point out correctly that this assault by the majority is going on, notwithstanding the fact that the validity of some of the fundamental laws, in particular quantum mechanics and some facets of relativity, have not been unambiguously established on the hadron scale. I don't see why this should be a surprise to you; after all, quantum mechanics has been and is being applied constructively on an atomic scale, notwithstanding the fact that controversy persists and people continue to write and publish papers on the axiomatic basis of quantum mechanics and diverse interpretations of that basis.

You complain that the majority effort of theorists is going in a direction not meeting your sense of priorities. Scientific interest, in particular by theorists, cannot be directed but should be triggered by the challenge of the results themselves. This is a task for you to undertake through your work in research and publication, and not by appeal to government agencies, editors or laboratory directors.

I am not a theorist. Speaking as an experimentalist I feel that you profoundly misinterpret both the experimental status of elementary particle physics and the methods of conducting experimental investigation. You criticize that "current experimental trends are essentially restricted to the identification of new particles and their data in symbiotic conditions with unitary models." Most experimentalists would be highly astonished in seeing their work described in this manner. Experimental work is largely

conditioned by technological opportunity and is not as programmatic or directed as your description implies. The striking discoveries of new particles, states, and cross sections have indeed been interpreted by the majority of theorists along the lines you indicate but this does not mean that this has been the directed motivation of experimentalists. Moreover, there have been extensive experiments not meeting your description at all, such as the thrust to explore the limits of quantum electrodynamics, the exploration of violation of parity in electromagnetic interaction, the examination of jet structure in fundamental annihilation processes, etc., etc. The fact that it is your desire to have unambiguous proof of the validity of quantum mechanics on the scale of individual hadrons does not mandate that it shall be possible for an experimentalist to design an experiment which will unambiguously answer that question. What specific experiment(s) do you propose?

You are contrasting the current situation in physics as being unfavorable relative to that pertaining in the early part of the century. I disagree. I venture to suggest that you have read few, if any, of the experimental papers which were published in the scientific journals prior to the discovery of quantum mechanics. The Ruggero Maria Santilli of the first part of the century would have accused the experimentalists of sponsoring experimental fashions restricted to the identification of new spectral lines, or the investigation of uninteresting atomic phenomena, in disregard of fundamental considerations. Yet it was the accummulation of large masses of spectroscopic data and other atomic physics experiments of those days which led to the formulation of quantum mechanics in three alternate forms, which were then later shown to be equivalent. However, as I mentioned before, even to this day complete understanding on the most profound level of the basis of quantum mechanics remains a subject of some controversy even though it has formed a highly successful calculational basis of all the physics as uncovered to date.

To summarize, I see little merit in your arguments. You are clearly entitled to your opinion that the majority of your theoretical colleagues are pursuing goals less important than those represented by your own scientific interests. On the other hand, I feel your letter reflects a complete misunderstanding of the role of the experimenter in pursuing his work and I believe that most of your theoretical colleagues are ahead of you in appreciating the enormously fundamental importance of the recent experimental discoveries in hadron physics.

With best wishes,

Wolfgang K. H. Panofsky

W. K. H. Panofsky

cc: Dr. J. Kane
Dr. B. Hildebrand
Dr. D. Peaslee
Dr. W. Wallenmeyer
Dr. J. Deutch
Dr. J. Krumhansl
Dr. M. Bardon

HARVARD UNIVERSITY

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

July 31, 1978

Professor W. K. H. PANOFSKY
Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

I would like to express my sincere appreciation for the courtesy of your letter of July 27, 1978. I am also grateful for the "benevolent impunity" I am under the impression you have granted me for candidly expressing my concern.

I am in agreement with most of your letter and, in particular, with your remarks related to the experimental profile. There is no doubt among theoreticians that the situation of experimental hadron physics is in no grave situation at all. As a matter of fact, I am an admirer of current experimental results, to the point that I consider experimental hadron physicists considerably ahead of theoreticians. You have misinterpreted my letter to you of July 19 if you believe that I do not appreciate the fundamental relevance of recent experimental discoveries. The experiments you indicate on the limits of quantum electrodynamics, violation of parity in electromagnetic interactions and jet structure in annihilation processes, were completely extraneous to the content of my letter, which was solely devoted to experiments of direct relevance for the problem of hadron structure. It is understood that such a notion is debatable and that any experimental data on hadron is useful for the structure problem. I simply attempted to present in my letter the experimental profile which, in my view, can really produce an unequivocal orientation of theoretical studies on hadron structure from a fundamental profile, that of the basic physical laws.

Being a theoretician, the primary area of my concern is restricted to the current status of theoretical studies on the problem of hadron structure. It is understood that this is also a debatable notion. Indeed, it demands a differentiation between the problem of classification and structure. It appears that the consciousness of this possible differentiation is simply lacking at this time, to the best of my knowledge. For the sake of clarity, permit me to restress that, in my view, the unitary models have achieved a Mendeleev-type classification of hadrons of virtually conclusive character. The situation in regards to the quark models of hadron structure is, also in my view, fundamentally different. But your letter is silent on the problematic aspects of current theoretical studies.

Permit me, however, to disagree with certain parts of your letter and, in particular, with your objection of my appeal to laboratory directors and governmental agencies. The central point of my letter was to attempt to create an awareness on the need to subject to a critical scrutiny, theoretical study and experimental finalization the fundamental physical laws used in current hadron physics. You are eventually aware that these laws are simply assumed as valid in the virtual totality of the rather vast literature on quark models in a fully tacit form. A problem of this nature cannot be resolved by individual researchers and demands, in my view, the participation of the physics community at large. I strongly oppose the idea that

laboratory directors and governmental agencies should be excluded by scientific debates of orientational nature on fundamental issues.

I believe you have answered negatively the crucial question I have posed to you:

> "Do we possess at this time direct, explicit and unequivocal experimental evidence that the basic physical laws of the electromagnetic interactions (Einstein's special relativity and Pauli's exclusion principle, in particular) are verified for the hadronic constituents?

Permit me to confirm the answer as suggested to me by a number of experimentalists: NO! This literally implies that the rather massive theoretical efforts during the last decade on the problem of hadron structure are based on experimentally unverified, fundamental physical laws. You add to this my exposure, as editor, to an apparently mounting study on the invalidity of these laws within the arena considered (which I felt obliged to report to you). I believe that there is sufficient reason for concern. But you see little merit in the consideration of the issue.

Permit me to confess that my personal concern is increased, rather than decreased, by your letter. Indeed, I am under the perhaps erroneous impression that you do not see the need of reorienting the current experimental trends for the primary objective of achieving the direct, explicit and unequivocal experimental resolution of the validity or invalidity of the basic physical laws for the hadron structure. There is no doubt in my mind that, until this problem is confronted and, in due time, resolved, the problem of hadron structure will remain fundamentally unresolved and all theoretical efforts will remain in limbo. But this this is a problem which cannot be effectively resolved at the level of the individual, theoretical or experimental researcher only, and demands the joint consciousness at the decisional level. My expectation was that the participation, council and wisdom of laboratory directors and officers of governmental agencies in the decisional process would be invaluable.

In relation to the specific tests you refer to, I have proposed only one test at this time: the experimental verification whether Pauli's exclusion principle is exactly valid for $_7N^{16}$ and other nuclei, or very small deviations at this <u>nuclear</u> level are experimentally detectable. There exists a number of proposals in the available literature in relation to specific tests of Einstein's special relativity for the <u>hadronic</u> structure by other physicists (see, for instance, ref. 1, page 883 of the HJ and related literature which I see no point in recalling here in details). Additional specific tests are apparently forthcoming in future issues of the HADRONIC JOURNAL, as well as in other Journals, to provide even a wider selection for the interested experimenters.

As a matter of fact, permit me to disclose that the primary reason of my concern (as well as of the candid language of my letter to you) is precisely due to the fact that these proposals for the experimental verification of fundamental physical laws within a hadron have been made a number of years ago, but, despite their fundamental character, they have remained ignored until now, to my knowledge. From your letter I am under the impression that they may remain ignored for a number of years to come.

Very Truly Yours

Ruggero Maria Santilli

RMS|cgg

c.c.: Drs. J.DEUTCH, J.KANE, B.HI LDEBRAND, D.PEASLEE, W. WALLENMEYER of DOE

Drs. J. KRUMHANSL AND M.BARDON of NSF.

HARVARD UNIVERSITY

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

August 8, 1978

Professor JOSEPH BALLAM,
Associate Director of Research
Stanford Linear Accelerator Centre
STANFORD, California 94305

Dear Professor Ballam,

it is a pleasure to invite you for an informal workshop on the problem of the physical laws for the strong interactions which will be held here on August 24 and 25.

I am sorry for the insufficient notice, but I received only today the acceptance (and encouragment) by Ktorides (from Greece) and Myung (from IOWA). It is also with regret that I must acknowledge my inability to provide support for any of the participants. My grant with Shlomo Sternberg is virtually restricted to my salary and extremely few items, while the situation for university funds is predictably equivalent.

I enclose an outline of the meeting with additional information. From the indications available, it appears that it could be intriguing indeed. As you can see, we did and we will carefully avoid matter of minute incremental nature, while we are interested in though provoking speculations on fundamental issues. Also, we have avoided topics of direct structural character for hadrons. Thus, topics such as quarks, partons (and eletons) are not expected to play a fundamental role. This is also in line with our view, by now familiar to you, that we should first look at the laws for strong interactions, and then at the problem of structure.

I would like to add that the workshop is a truly working-research session and not a conference. H.C.Myung has some new, quite valuable results in Lie-admissible algebras (the August issue of the HADRONIC JOURNAL will publish his second, quite long paper-monograph on Lie-admissible algebras of nonflexible type -- the first written by a mathematician on this topic). C.N.Ktorides (who, as you eventually know, is an axiomatician) has some recent arguments on the possible inapplicability of the spin-statistics theorem for the strong interactions which I consider intriguing. Stephen Adler has a quite intriguing approach to classical chromodynamics and I hope he can attend. I keep myself busy with my interest in attempting a Lie-admissible covering of Einstein's special relativity (but it will take some time before I can publish anything). D.Y.Kim has some specific proposals for experimental tests (partly his and partly review of proposals by others). Regrettably, he cannot attend because of lack of funds, since he is now in Cambridge, England, on sabbatical. But, perhaps, we can expect some communication by him.

I believe that your participation would be invaluable, not only for the scientific aspect per se and for the possibility of knowing each other, but also for the future of our Journal.

Please kindly extend this invitation to Professor Panofsky and Drell.

Sincerely

Ruggero Maria Santilli

RMS|cgg

HARVARD UNIVERSITY

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

August 2, 1978

Professor W.K.H. PANOFSKY,
Director
Stanford Linear Accelerator Center
STANFORD, Ca 94305

Dear Professor Panofsky,

As a follow up to our recent correspondence, permit me to express my apologies in case I have been of any inconvenience to you.

Also, I would like to indicate that I have no intention of doing any follow up and, from now on I shall behave, shall I say, like "a good boy". After due consideration, I simply felt the need to express my sincere concern on theoretical hadron physics for whatever its value is, via my letter to you and my papers. From now on I would like to abstein from any direct partecipation in possible debates outsides regular publications on the rather controversial quark conjecture. If my seeds have any value, they will grow in due time via an orderly and conventional scientific process.

Permit me to close with a humouristic note. I enclose two "vignette" depicting certain crucial aspects of current theoretical physics. They have amused (although, I believe, left skeptical) my Lyman collegues. I present them to you in the hope that you will smile.

With my warmest regards, I remain

Sincerely Yours

Ruggero Maria Santilli

RMS|cgg

C.C. Professor S. Drell and J. Ballam (only).

INTERNATIONAL ATOMIC ENERGY AGENCY
UNITED NATIONS EDUCATIONAL, SCIENTIFIC AND CULTURAL ORGANIZATION

INTERNATIONAL CENTRE FOR THEORETICAL PHYSICS
34100 TRIESTE (ITALY) - P.O.B. 586 - MIRAMARE - STRADA COSTIERA 11 - TELEPHONES: 224281/2/3/4/5/6
CABLE: CENTRATOM - TELEX 46392 ICTP

DIRECTOR
ABDUS SALAM

10 August 1978

Dear Santilli,

I am deeply grateful for your kind letter and the thought-provoking enclosures. They deserve a very careful study and a careful answer. Regretfully, I am just now writing up the Tokyo talk and will then go to Tokyo, China, Korea - and even the US (Ohio 27,28 and 29 September) - most of the journey is in aid of keeping the Centre here alive and well. (Incidentally I appreciate your kind remarks about the Centre.)

Is there any chance of our meeting in the US or here and talking about these matters? If not, I shall write in detail later.

With my appreciation, again, for your thoughtful and stimulating letter.

Yours sincerely,

Abdus Salam

Abdus Salam

Professor R.M. Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge, MA 02138
USA

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August 18, 1978

Professor ABDUS SALAM, Director
International Centre for Theoretical Physics
34100 - TRIESTE, MIRAMARE, Italy

CONFIDENTIAL

Dear Abdus,

I would like to express my sincere appreciation for your understanding attitude in relation to my letter of July.

In actuality, the occurrences which preceded my letter to Professor Panofsky were considerably more alarming of what disclosed in my preceding letter to you. In essence, it was my understanding that some collegue had reached such a level of disappointment, to hire attorneys for legal actions on research-grant related issues. An action was needed to prevent events of unpredictable outcome.

I was in a rather unique situation because I am the recipient, jointly with a distinguished mathematician of Harvard, of what appears to be the first research grant from the Department of Energy which is specifically intended to be of non-quark orientation, that is, an attempt at hadron structure other than of unitary character.

It appears that I did succeed with my letter to reach the objective intended. Indeed, I did succeed in full to quite down excessive disappointments and delay unnecessary action. My only disappointment is that I do not know whether I was able to preserve my good relationship with Professor Panofsky. Indeed, all this background is entirely unknown to him, to my knowledge. At the same time, the writing of the letter in question was for me reason of considerable regret (although it was absolutely necessary in my view), because I have a sincere esteem for Professor Panofsky as well as a sincere gratitude for the assistance I received from him during the organization of the HADRONIC JOURNAL.

In any case, it appears that the wheels for achieving a more balanced conduction of research in hadron structure are in motion. As you know, the proposals originate from quite responsible physicists. They desire that primary funding along unitary trends must continue and that, jointly, a minor funding of possible alternatives should be initiated. Recent events were due to the truly pre occupaying level of monopoly in fundings of the unitary trends. In any case, I am happy to see that, after my letter (which, as you know, was mailed also to Governmental Officers), I did receive for refereeing research grant proposals of non-quark inspiration, with indication of a serious intent of consideration. This was very welcome indeed. It gave me additional essential elements to quite down spirits, by indicating that the wheels for a sincere desire to improve the dispersal of research funds were in motion. In the final analysis, these responsible collegues wanted noting more.

In conclusion, I am happy to report to you that, to my knowledge and understanding, things are back to normal. Almost needless to say, I do not intend in the future to enter into disputes related to fundings of quark-oriented research. I did it once and I think I should not do it again.

I am grateful for your kind consideration of my studies. As I have formally presented them, they are "an exercise of scientific curiosity" intended for the primary purpose of calling contributions from collegues on the problem of the basic physical laws for the strong interactions.

Again, I am warmly encouraging you to an active participation in this issue, by presenting your viewpoint on the issue, possibly, either in favor or against established laws. I believe that your knowledge and wisdom would be invaluable to achieve a resolution of the issue. Almost needless to say, at the HADRONIC JOURNAL we are solely interested to the pursuit of the scientific truth and not to that of specific insights. As a result, I beg you to feel completely free to criticize my papers (as well as those of the other collegues) in the form and substance you desire.

Thank you for your interest in a verbal discussion of these matters. Regrettably, I am unable to come to Europe for numerous committments. Neverthless, since you will be in the States in September, It would be very nice indeed, if you could spend a brief visit at Harvard, either before the Ohio trip or after (you could return to Italy via Boston, Logan Airport-direct flight to Milano).

In the event that this is indeed possible, I have enclosed a formal invitation from the Department of Mathematics (I am a member of this Dept). I am sure that the Lyman collegues would welcome you too.

There is a feverish activity going on in relation to the study of a possible generalized nature of the strong hadronic forces. Next week we will have here at the Dept. of Math. an informal workshop on Lie-admissibility (Ktorides from Greece, Myung from Iowa and few other will attend). Intriguing, but delicate papers are also forthcoming on the Hadronic Journal. I shall provide my best efforts to keep you informed of relevant events.

I have no words to express my esteem and admiration for your person and your accomplishments.

Sincerely

Ruggero Maria Santilli

RMS|cgg
encl.

THIS LETTER
REMAINED UNACKNOWLEDGED

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

Directors Office

September 27, 1978

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

Dear Dr. Santilli:

This is an apology for not having answered your thoughtful letter of last July. Somehow in the confusion of my stepping down as Director of Fermilab, the letter, because of its format, ended up in my non-personal file.

The kind of questions you have raised are not answered easily, and since I am no longer the Director, there seems little point in trying. You do make some pretty harsh charges regarding our Theory Department. Generally speaking, we have tried to hire the best people available based on the advice of the best theorists in the country. A broad range of theorists come to visit Fermilab for various periods to supplement the efforts of the Fermilab theorists. Having done that, as Director, it would never occur to me to try to influence or restrict their work. Although the tragic death of Ben Lee set us back, I have been satisfied with and proud of our theoretical department.

The theorists do not determine the experimental program at Fermilab except as they are able to influence on a logical basis the experimenters themselves. We have a Physics Advisory Committee with a broad membership from all parts of the country and which represents many specialists including theorists. Again those theorists and some of our own join in the debate that leads to the eventual decisions. The constant flux in the membership of the PAC means that many points of view have been represented.

It is by influencing the physics community directly with your arguments, as you are appropriately trying to do by your letters and with your new journal, that you should expect your views to have an effect.

With regard to your various specific suggestions and questions, it seems more appropriate that I turn your letter over to my successors, so that they may consider them.

Again, may I apologize for not having answered your letter more promptly.

Sincerely,

R. R. Wilson

HARVARD UNIVERSITY

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

STRICTLY CONFIDENTIAL

October 2, 1978

Dear Professor Wilson,

I simply have no words to express my appreciation for the courtesy of your letter of September 27, as well as for the "benevolent impunity" you have apparently granted to me in relation to the content of my letter of July 19, 1978.

Almost needless to say, I am entirely in agreement with the content of your letter, to the point that I have no further or counter-comment.

What I would like to indicate to you on a confidential basis is the background which lead to my letter to you. I am sure you have sensed that my letter was not the result of a one day decision. In actualy, it was the climax of a series of events which left me no other alternative to serve our community in the form which appeared to me as more appropriate under the circumstances.

I am sure you are aware of the delicate situation of our community owing to insufficient funding, insufficient openings, lack of tenure, etc. This situation has apparently exasperated spirits. A number of physicists have lost tenure, jobs etc. allegedly because of lack of funding of their research proposals. Out of this tense moment a specific accusation was reported to me, namely, that of a scientific and (most insidiously) financial monopoly in the sector of theoretical hadron physics by quark oriented studies.

Additional events urged me to attempt a PREVENTIVE action, that is, the prevention of gestures which could be detrimental for our community because could create shadows on the ethical profile of funding in the sector. My letter to you was conceived and used as such preventive tool. I am happy to report to you that I did apparently succeed in calming down excessive malcontemt. The reason is that our malcontent collegues are, in my view, quite responsible physicists. It is the irresponsible (also in my view) behaviour of other collegues currently "under the money tree" which has created this situation. In essence, my letter has stimulated an orderly scientific process of consideration of the issue, thanks also to the invaluable partecipation by Dr. Krumhansl. This was fully sufficient for the objective at hand.

I would appreciate the courtesy of your conveying this background to the new Director of FERMILAB (I do not know at this moment his name). Please also feel free to show him this letter with the understanding that it should remain strictly confidential. There is no need to answer to my letter. If the new director feels that an asnwer is due, I would appreciate the indication of the main point, namely that the points raised are under an orderly consideration.

What I am more interested is in the possible, direct involvement of FERMILAB in the rather exiting things going on as a result of this "vigorous call for a moment of reflection on quarks". In short, it appears that a number of experimental physicists are seriously considering the undertaking of an initial feasibility study for the experimental test of Pauli's principle in nuclear physics (according to my proposal of the H.J. 1, 574 (1978)) as well as of the Special Relativity in hadron physics (see, for instance, ref. 1 and quoted paper of same reference).

Almost needless to say, these are noting but initial steps of a rather formidable experimental problem. Some of the initial results or proposals will be (proudly !) presented in our Journal.

Also, these studies are due to courageous physicists who have been capable of overcoming what I call the "QUARK SINDROME", that is, the extreme repugnance at the mere idea that QCD, in the final analysis, could be experimentally proved to be invalid via a possible experimental verification of the inapplicability of the Pauli's principle and the spin-statistics theorem under strong interactions.

Permit me to close with a candid confession in relation to the theoretical division of FERMILAB. It is that, being an Italian, I am emotionally attached to the Laboratory bearing the name of ENRICO FERMI. As such, more than for other Laboratories, I am sincerely interested in yours being at the frontiers of the pursuit of physical knowledge, both theoretically as well as experimental because, as you know, this was the rather unique dual quality of Fermi.

In relation to the fundamental problem of current theoretical and experimental physics, hadron structure, permit me to acknowledge my emotional state in sing the studies only restricted to the quark conjecture and numerous (if not all) experiments essentially interpreted only via such conjecture.

The appeal of my letter to you which was genuinely felt is to consider the implementation of a more balanced conduction of research consisting of the necessary continuation of studies along quark lines, jointly with the beginning of the inclusion of fundamentally different approaches.

My peculiar position of Editor of a Journal already known as devoted to the nonmonopolistic presentation of research on hadrons, as well as being the recipient of what appears to be one of the first research grants of non-quark inspiration, has exposed me to a number of situations which were perhaps unknown to you. I am here referring, for instance, to a strong increase in very recent times of highly qualified physicists who, after so many years of yet inconclusive efforts, simply do not believe in quarks. All these indications suggested to me the proposed implementation of research into a well balanced form of quark and NON-quark inspiration. Quite candidly, I do see this implementation (or at least its orderly study) as a necessary prerequisi to avoid a substantial, potential deterioration of the current situation. This was, after all, the intend of my action.

In closing, I would like to beg you not to consider my letter as offensive for your conduction of FERMILAB. You have been Director of this laboratory in one of the most difficult moment of its history until now. You have my unconditional esteem for the sole courage you proved to possess simply in holding this post. Also, you have acquired the respect of our entire community via appropriate warnings on the current situation. Under no circumstances you should consider my letter as a form of criticism for the past, especially in view of the fact that certain informatbon was still unknown to you. As a matter of fact, I have selected you for my call for a moment of reflection precisely because fully aware of the fact that my letter would leave your scientific, ethical and human stature entirely unaffected.

Hoping to have the pleasure of meeting you some time, I remain

SINCERAMENTE TUO

~~Sincerely Yours~~

Ruggero Santilli

Ruggero Maria Santilli

State University of New York
at Stony Brook
Stony Brook, New York 11794

Institute for Theoretical Physics
telephone: (516) 246- 6701

**StonyBrook**

September 26, 1978

Professor Ruggero Maria Santilli
Harvard UniversityScience Center, Room 331
One Oxford Street
Cambridge, Massachusetts 02138

Dear Prof. Santilli:

Could you please send me a copy of your letter to Panofsky of July 1978?

Yours sincerely,

C. N. Yang

Chen Ning Yang

CNY:ct

c.c. Prof. Gerzi

HARVARD UNIVERSITY

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

September 18, 1978

Professor CHEN NING YANG
State University of New York
Stony Brook, N.Y.

and

Professor JOSEPH BALLAM
Stanford Linear Accelerator Center
Stanford, Ca

Dear Professors Yang and Ballam,

Following a meeting with HOWARD GEORGI, I would like to take the liberty of providing you with additional comments in regards to my letter to WOLFGANG PANOFSKY of July 1978.

I am sure you have realized the great emotional state which brought me to this letter. It was due to the current condition of our community of basic research which appeared to me as quite delicate and, as such, demanding a call for a moment of reflection. I do not know whether my means have been proper or improper, but I can assure you that my intentions were solely devoted to the interest of our community. Also, permit me to reassure you on my unconditional loyalty to Governmental Agencies (after all, I am completely supported by research funds).

In any case, it appears that I have achieved my objective of quieting down excessive malcontent. To the best of my knowledge, it appears that an orderly scientific process has been implemented (e.g., a consideration of the situation by the NSF Advisory Committee). As you also know, I have made it clear to all parties that I do not intend to provide another intermediary action, as far as the future is concerned. As a matter of fact, I believe that once the situation has been properly studied, it will be improved and no such action will be needed.

I hope that my letter, written as an individual physicist, will not affect the HADRONIC JOURNAL. In any case, it was not intended to express the viewpoint of other independent members of the Editorial Organization of our Journal.

As you know, the Journal is devoted to the presentation of a primary line of mature papers on quark models. This line is under the complete, independent and final editorial control by Howard Georgi. The Journal then presents a secondary line of speculative papers on possible alternative approaches to hadron structure under my supervision. Finally, the Journal presents a third line of non-conjectural, rigorous papers by mathematicians on techniques of potential interest to hadrons.

I believe that this structure fullfils a precise function in relation to the delicate moment of our community. Indeed, as indicated in my preceding communications to you, our community is under an apparent accusation of an alleged scientific and financial monopoly by quark-oriented studies in the sector of theoretical hadron physics. I believe that the HADRONIC JOURNAL is the best evidence of the lack of existence of such an alleged monopoly, at least for its scientific part. I sincerely hope that the Journal can continue to benefit of your council and advice, particularly in this delicate moment of our community, so that it can continue its function.

In case I can be of any assistance for additional information, please do not hesitate to contact me (Office 617 495 33 52, home 617 969 3465).

Very Truly Yours

Ruggero Maria Santilli

c.c.: Prof. H. GEORGI

RMS|cgg

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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CAMBRIDGE, MASSACHUSETTS 02138

November 15, 1978

Professor CLIFFORD WILL
Department of Physics
Stanford University
STANFORD, California 94305

Dear Professor Will,

As a follow up to my recent letter to you, I enclose copy of a review-comment by Professor D.Y.KIM (now at Cambridge-England) on the problem of the experimental verification of Einstein's relativity at small distances.

I would appreciate the courtesy of an indication whether you can participate in this scientific effort by presenting your assessement of the state of the art.

Sincerely Yours

Ruggero Maria Santilli
HADRONIC JOURNAL

RMS/cgg
encl.

P.S. Please read first, for your amusement, the very last sentence of Kim's paper.

HARVARD UNIVERSITY

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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
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February 28, 1979

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

Dear Professor Panofsky,

You might be amused to know that the enclosed flier has been printed and distributed in 60,000 copies to the mathematics and physics communities.

I always remember you with sincere pleasure and gratitude.

Very Truly Yours

Ruggero Maria Santilli

RMS/se

**HADRONIC PRESS** presents the reprint series

## APPLICATIONS OF LIE-ADMISSIBLE ALGEBRAS IN PHYSICS

edited by

**Hyo Chul Myung**
University of Northern Iowa
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Cedar Falls, Iowa 50613

**Susumu Okubo** and
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Cambridge, Massachusetts 02138

**Volume I** (1979), 400 pages, $45.00
**Volume II** (1979), 595 pages, $55.00

**Excerpts from the prefaces of these volumes**

The Lie-admissible algebras constitute a generalization of the Lie algebras and, as such, have a far reaching mathematical potential. Their study was initiated by A. A. ALBERT in 1948, . . . but it has been only sporadically pursued by mathematicians until now.

The recent surge of interest in the Lie-admissible algebras appears to be due to the intriguing possibilities of physical applications. It was first pointed out by R. M. SANTILLI in 1967 that the Lie-admissible algebras arise in a natural way in Newtonian Mechanics via a generalization of Hamilton's equations for the representation of forces nonderivable from a potential. Since that time, a number of physicists have made contributions which have brought to light other applications. These include: the treatment of open systems in continuum mechanics; the quantum mechanical descriptions of forces nonderivable from a potential; the characterization of broken Lie symmetries and supersymmetries, etc. . . . Strong interactions at both the nuclear and hadronic levels have long been suspected of being nonderivable from a potential, as an approximation of nonlocal settings. Thus, the Lie-admissible algebras have recently emerged as being potentially significant for nuclear and hadron physics, in general, and the problem of the structure of hadrons, in particular.

Owing to these possibilities, a considerable effort to promote the study of the Lie-admissible algebras in both mathematics and physics has been implemented via the HADRONIC JOURNAL since its first issue of April 1978.

**A primary objective is to appeal to the most advanced possible mathematical tools to study, formulate and promote the experimental verification of the validity or invalidity for the strong interactions of fundamental physical laws, such as Einstein's special relativity, Pauli's exclusion principle and the spin-statistics theorem, which are experimentally established until now only for the electromagnetic interactions. It is understood that the problem of the structure of hadrons can be confronted in a truly effective way after the achievement of these basic experimental resolutions.**

In summary, the papers presented in the first two volumes of this series are sufficient to indicate the existence of a hierarchy of Lie-admissible algebras of increasing mathematical complexity, such as the Lie algebras (which are trivially Lie-admissible), the flexible Lie-admissible algebras and the general Lie-admissible algebras. Intriguingly, this hierarchy emerges as a conceivable algebraic counterpart of a hierarchy of forces in the physical universe of increasing structural complexity and methodological needs.

Almost needless to say, a long way remains to be covered to reach a sufficient mathematical and physical maturity in this new scientific horizon, . . . But this is in line with the primary objective of this reprint series: to stimulate the participation of a broader number of mathematicians, physicists and engineers in the study of issues of fundamental character for basic research.

**HADRONIC PRESS, INC., NONANTUM, MASSACHUSETTS 02195, U.S.A.**

(Tables of Contents of Volumes I and II and sale terms are listed on the back of this leaflet).

HARVARD UNIVERSITY

Area Code 617
495-3352

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

May 7, 1979

Professor WOLFGANG K. H. PANOFSKY, Director
Stanford Linear Accelerator Center
STANFORD, California 94305

CONFIDENTIAL

Dear Professor Panofsky,

Apparently, the situation in hadron physics has deteriorared considerably since the time of my warm appeal to you of July 19, 1978. The enclosed paper is a manifestation of this situation. 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 on a world wide scale, jointly with the study of fundamentally different lines. If they have technical arguments to disprove these criticisms, they must publish them in scientific papers. The sometime used corridor-type talks on quark-non-oriented studies by quark-committed physicists are nowaday ineffective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at SLAC of studies for the experimental verification of the basic physical laws currently used in strong interactions, with particular refererence to Einstein's special relativity and Pauli's exclusion principle. Even the activation of an initial feasibility study at SLAC would be invaluable, provided that its conduction is not restricted to quark supporters only.

I am confident that you will see that the protraction of the current situation in hadron physics may invite a crisis. I am referring here to the current investments of truly large amount on money on strong interactions, all based on the mere belief of the validity of the basic laws, without jointly conducting their experimental verification. Quite frankly, I am seriously concerned that the protraction of such a situation may imply a process to our scientific accountability.

I think that we still have time to prevent further deteriorations. But we simply cannot continue to effectively conduct studies in hadron physics on the basis of mere beliefs by individual physicists on fundamental issues. The return to the traditional conduction of physics, that via experiments, is, in my humble view, much needed and needed soon.
Again, with my sincere esteem, I remain

Very Truly Yours

Ruggero Maria Santilli

* The paper should be soon distributed to SLAC

RMS/ml

HARVARD UNIVERSITY

AREA CODE 617
495-3352

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

May 7, 1979

CONFIDENTIAL

Professor GEORGE H. VINEYARD, Director
Brookhaven National Laboratory
UPTON, Long Island, N.Y. 11973

Dear Professor Vineyard,

Apparently, the situation in hadron physics has deteriorated considerably since the time of my warm appeal to you of July 19, 1979. The paper enclosed is a manifestation of this situation. 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 on a world wide basis, jointly with the study of fundamentally different lines. If quark-committed physicists have technical arguments to disprove these criticisms, they must publish them in scientific papers. The often used corridor-type of talks is no longer effective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at BROOKHAVEN NATIONAL LABORATORY of studies for the experimental verification of the expected invalidity (according to some) or possible validity (according to others) for the strong interactions of the currently used basic physical laws, with particular reference to Einstein's special relativity and Pauli's exclusion principle. Even the initiation of a feasibility study for these experiments would be invaluable, provided that its conduction is not limited to quark believers only.

I am confident you will see that the protraction of this situation indefinitely may invite a crisis. I am referring here to the current investments of truly large amounts of money on strong interactions, all based on the mere belief of the validity of the basic laws, without jointly conducting their experimental verification. Quite frankly, I am seriously concerned that such a propraction may invite a process to our scientific accountability.

I think that we are still in time to prevent further deteriorations. But, the return to the traditional conduction of physics on fundamental issues, that via experiments, rather than beliefs, is much needed and needed soon.

Again, with my most sincere esteem and gratitude for your past courtesy, I remain

Very Truly Yours,

Ruggero Maria Santilli

* The paper should be soon distributed to Brookhaven

RMS/ml

HARVARD UNIVERSITY

AREA CODE 617
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RUGGERO MARIA SANTILLI
SCIENCE CENTER, ROOM 331
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

May 7, 1979

Professor JOSEPH BALLAM
Stanford Linear Accelerator Center
STANFORD, Ca 94305

Dear Professor Ballam,

A number of elements has suggested that I anticipate my plans for promoting a moment of reflection on quarks. They also originate in Washington, and are related to an understandable need to revitalize the conduction of research in hadron physics.

I have therefore prepared the enclosed article of review on the criticisms on quarks. I have provided my sincere efforts to reach a balanced presentation (by even avoiding the presentation of additional, technical criticisms). Nevertheless, I do not know whether I did succeed in this difficult task.

I would appreciate your advice whether the publication of this article in the HADRONIC JOURNAL is appropriate or not. I do not have personal preferences.

Almost needless to say, any critical comment for bringing this paper to the necessary maturity, would be gratefully appreciated.

If, for any reason, you do not have the time to look at this paper, simply ignore this letter.

Very Truly Yours

Ruggero Maria Santilli

RMS/ml
encls.

HARVARD UNIVERSITY

Area Code 617
495-3352

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

May 7, 1979

Professor ROBERT WILSON, Office of Directors
Fermi National Accelerator Laboratory
BATAVIA, Illinois 60510

CONFIDENTIAL

Dear Professor Wilson,

Apparently, the situation in hadron physics has deteriorated considerably since the time of my warm appeal to you of July 19, 1978. The paper enclosed is a manifestation of this situation. 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 on a world wide basis, jointly with the study of fundamentally different lines. If quark believers have technical arguments to disprove these criticisms, they must publish them in scientific papers. The often used corridor-type of talk on quark-non-oriented studies by quark-committed physicists is no longer effective.

The only way to defuse this deteriorating situation is the experimental way.

I would like to take the liberty of warmly encouraging again the initiation at FERMILAB of studies on the experimental verification of the expected invalidity (according to some) or possible validity (according to others) of the basic physical laws currently used in strong interactions, with particular reference to Einstein's special relativity and Pauli's exclusion principle.Even the activation of an initial feasibility study would be invaluable, provided that its conduction is not restricted to quark believers only.

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I think that we still have time to prevent further deteriorations. But the return to the traditional conduction of physics, that via experiments rather beliefs on fundamental issues, is much needed and needed soon.

Again, with my most sincere esteem, I remain

Yours, Very Truly

Ruggero Maria Santilli

* The paper should be soon distributed to Fermilab

RMS/ml

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address*
SLAC, P. O. Box 4349
Stanford, California 94305

19 March 1980

Dr. R. M. Santilli
Harvard University
Science Center, Room 331
One Oxford Street
Cambridge MA 02138

Dear Dr. Santilli:

Since I will be away from the U. S. for a year beginning this summer, I would like to resign from the Editorial Council of the Hadronic Journal.

I believe my resignation is appropriate at this time, particularly since the articles in the Journal have been theoretical in content while my own expertise is in experimental particle physics.

Sincerely yours,

J. Ballam
Professor

JB:alb

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

Professor MELVIN B. GOTTLIEB, Director
Plasma Physics Laboratory,
PRINCETON, New Jersey 08544

May 31, 1981

Dear Professor Gottlieb,

I would appreciate the consideration of selecting or otherwise recommending a representative from your Laboratory to our forthcoming
- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS to be held here in Cambridge from August 3 to 7, 1981 under partial support by the Department of Energy.

In these yearly Workshops we gather experimentalists, theoreticians, and mathematicians to study the problem whether the intrinsic characteristics of hadrons (magnetic moments, spin, space parity, etc.) are preserved or altered in the transition from the conditions they have been measured until now (long range electromagnetic interactions), to the different physical conditions of the strong interactions. To put it in different terms, we are interested in achieving direct or otherwise clear experimental, theoretical, and mathematical information on the magnetic moment, spin, parity and other intrinsic characteristics of nucleons under the high pressures, densities, and temperatures of the controlled fusion.

In a few nontechnical words, the alternatives are essentially the following.
Alternative I. The conventional point-like approximation of hadrons (and their constituents) is effective for the strong interactions; the views currently prevailing in high energy physics are correct; and the underlying mathematical structure (that of local differential character) is truly valid. Under these assumptions, there is no reason to suspect any fundamentally new or otherwise different occurrence, besides well known effects (such as the transition from spin 1/2 to 5/2 due to resonances).
Alternative II. Hadrons under strong interactions are truly represented according to experimental evidence, that is, as wave packets in necessary conditions of mutual penetration and overlapping (recall that the range of the strong interactions coincide with the size of hadrons). In this case there is the need of fundamentally new mathematical formulations, e.g., of nonlocal type because of the need to represent the interactions at all points of the volume of mutual wave penetration. Also, there is the need of new physical approaches. In fact, the interactions are (partially) of nonpotential type because the notion of potential has no physical foundation for contact interactions, whether Newtonian or quantum mechanical, and discrete or continuous. But nonpotential forces imply nonunitary time evolutions. In turn, nonunitary time evolutions imply rather profound departures from the electromagnetic characteristics of particles, and new situations become conceivable as internal dynamical effects for strong bound systems which are not detectable from the outside. For instance, the spin and magnetic moment of the proton while the nucleus of an hydrogen atom have the well known perennial values. However, if the proton exits the atom , and enters into conditions of mutual wave overlapping with other nucleons, the "spin" becomes dependent on time and local conditions, the magnetic moment can vary (up to 50 % and more), the discrete symmetries are broken, etc.
A copy of my review presentation at the Clausthal Conference of 1980, as well as at our Third Workshop of the past summer is enclosed.

The greatest majority of experiments currently available in strong interactions have been conducted via the assumption of conventional electromagnetic characteristics in their data elaboration. As a result, these experiments, even though valuable for other objectives, are not suitable for our purposes. Our problem simply calls for direct measurements, that is, measurements under strong interactions. A rather in-

tensive effort is under way to formulate experiments of this type, for the future resolution of the problem considered either in favor of the electromagnetic characteristics, or in favor of new ideas. Intriguingly, as you can see from the enclosed paper, the available direct information in nuclear physics either favors variations of the electromagnetic characteristics, or the variations cannot be excluded.

A number of members of our group are experts in nonpotential, classical and quantum mechanical, statistical mechanics. However, none is an expert in the plasma physics which is needed for the controlled fusion.We would therefore appreciate the participation by one or more members of your Laboratory, either of theoretical or experimental orientation (or both). The problems we would like to study are the following.

1. Identification of the state of the art of our experimental and theoretical knowledge on the intrinsic characteristics of nucleons and underlying time evolution for the conditions of the controlled fusion;

2. Identification of experimental and theoretical implications for the controlled fusion of the possibility that nucleons evolve according to a nonunitary time evolution because of wave overlappings, with consequential local variations from intrinsic characteristics of electromagnetic-Lie type; and

3. Identification of the orientation of the research conducted by our group which is most effective for energy related profiles.

For your information, our Workshops are not conferences, but actual workshops, that is, we gather to conduct actual research in a friendly relaxed atmosphere. Each participant presents a partially or fundamentally open problem and benefits from the presence of a combined group of mathematicians, theoreticians, and experimentalists.

A registration form is enclosed for your convenience, while I remain at your disposal for any needed additional information.

A formal presentation of the results of the Workshop of this year (as well as of the preceding three Workshops) will be conducted at the
- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, to be held at the University of Orléans, France, from January 5 to 7, 1982, under financial support by the French Government.

Thanking for your consideration and time, I remain

Yours Very Truly

Ruggero Maria Santilli
Organization Committees of the Fourth Workshop and First International Conference on Nonpotential Interactions

Tel (617) 964 1684

cc.Drs. D.J.GROVE, H.P.FURTH, P.J.REARDON, R.A.ROSSI, P.H.RUTHERFORD, T.H.STIX, E.C.TANNER, M. SHOAF, G.V. SHEFFIELD, J. JOYCE, R.G.MILLS,K.E.WAKEFIELD,E.B. SIMON, J.A.SCHMIDT, R.LITTLE,D.M.MEADE, F.W.PERKINS, A.R. DE MEO,R.J.GOLDSTONE, D.L.JASSBY, M. OKABAYASHI, D.E.POST, J.C.SINNIS, G.M.BROWN, C.NUSHNELL,F.K. BENNETT, R.B.FLEMING,G.H.RAPPE, at the Plasma Physics Laboratory.

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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

tel (617) 964 1684

July 2, 1981

Professor WOLFGAND K. H. PANOFSKI, Director
Stanford Linear Accelerator Center
Stanford University
SANFORD, California, 94305

Dear Professor Panofski,

During the past years I have contacted you at the rate of less than once per year to solicite the initiation at SLAC of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

This is my letter of solicitation for 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of existing data, and, of course, evaluating the available experimental information. More specific recommendation on the composition and function of this committee are at your disposal on request.

For your information, we have made some progress on the problem in experimental nuclear physics. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists actively working at the problem. In particular, we have identified experimental information such as:

- the apparent, quite large, deviation of the magnetic moments of hadrons under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the predictions of the conventional laws in the optical activity of neutron beams under strong nuclear interactions, according to the experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al;
- the apparent, also considerable, deviations of experimental points from the behaviour of the exact SU(2)-spin symmetry, according to the experiment by Rauch et al; and other data.

Admittedly, the experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish the fact that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions, which, if confirmed by future measurements, would imply the irreconciliable invalidations of the entire Poincaré symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capabilities.

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. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by experimentalists in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which will be held at the Université d'Orléans, France, from January 5 to 7, 1982, under financial support by the French Government via local Institutions.

Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

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

cc. Professors SIDNEY D. DRELL, JOSEPH BALLAM, RICHARD BARR NEAL, and JOHN R. REES.

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

tel. (617) 964 1684

July 2, 1981

Professor LEON LEDERMAN, Director
Fermi National Accelerator Laboratory
P.O.Box 500
BATAVIA, Illinois 60510

Dear Professor Lederman,

During the past years I have contacted your Laboratory (with letters addressed to ROBERT WILSON and other executives) at the rate of less than once per year, to solicite the initiation at FERMILAB of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with paricular reference to Einstein's special relativity, Pauli's exclusion principle, and other laws.

This is my letter of solicitation for the year 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians, with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating the available experimental information. More specific recommendations on the composition and function of this committee are at your disposal upon request.

For your information, we have made some progress on the problem 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 experimental information such as:

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits;
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Admittedly, this experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

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.

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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.

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I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

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

cc.: Professors P.V.LIVDAHL, J.R.ORR, F.R.HUSON, C.N.BROWN, C.QUIGG, B. CHRISMAN, R.P.JOHNSON, D.D.JOVANOVIC, T.B.KIRK, R.A.LUNDY, E.I.MALAMUD, P. MC INTYRE, J. PEOPLES, R. CARRIGAN, W.B.FOWLER, K.STANFIELD, L.C. TENG, D.E.YOUNG, S.I.BAKER, M.F.GORMLEY, Q.A.KERNS, M.E.JOHNSON, E.T. NASH, F.A.NEZRICK, A. TOLLESTRUP, D.A.BAUER, D.A.EDWARDS, H.T.EDWARDS, L.V.COULSON, A.E.BRENNER, T.YAMANOUCHI, P.LIMON,

Courtesy copy to R. WILSON and H. ABARBANEL

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
tel. (617) 964 1684

July 2, 1981

Professor GEORGE H. VINEYARD, Director
Brookhaven National Laboratories
UPTON, Long Island, New York 11973

Dear Professor Vineyard,

During the past years, I have contacted you at the rate of less that once per year to solicit the initiation at BROKHAVEN NATIONAL LABORATORIES of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other conventional laws.

This is my letter of solicitation for the year 1981. Again, permit me to recall that I do not recommend specific tests. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians, with the task of ascertaining the feasibility of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating the available experimental information. More specific recommendations on the composition and function of this committee are at your disposal upon request.

For your information, we have made some progress on the problem 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 experimental information such as :

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large, deviation from the predictions of the conventional laws in the optical activity of neutron beams under strong nuclear interactions, according to the experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions according to the experiment by Conzett et al;
- the apparent, also considerable, deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data could be manipulated to force compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see below), and which, if confirmed by future expeiments, would imply the irreconciliable invalidation of the entire Poincare symmetry. The understanding is that the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

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. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

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

cc.: Professors RONALD R. RAU, JAMES R. SANFORD, NICHOLAS P. SAMIOS, LYLE W. SMITH, ARTHUR Z. SCHWARZSCHILD, RONALD T. PIERLS, and M. GOLDHABER

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Harvard Grounds
96 Prescott Street
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Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees

tel (617) 964 1684

July 2, 1981

Professor HERMAN POSTMA, Director
Oak Ridge National Laboratories
Union Carbide Corporation
OAK RIDGE, Tennessee 37830

Dear Professor Postma,

I am contacting you to solicite the initiation at OAK RIDGE NATIONAL LABORATORIES of experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

Permit me to indicate from the outset that, despite the availability of several proposals of specific tests, I do not recommend a specific test. Instead, I recommend the setting up of a committee of experimentalists, theoreticians, and, possibly, mathematicians (see below why), with the task of ascertaining the feasibility at your laboratories of new experiments, assessing the possible re-elaboration of old data, and, of course, evaluating available experimental information. The understanding is that the tests, to be accepted by the scientific community at large, should be as direct as possible, and should avoid unverified conjectures in their data elaboration, such as those of quark type. Additional suggestions on the composition and function of the committee are at your disposal.

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 experimental information such as:

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits;
- the apparent, also quite large deviation from the predictions of conventional laws in the optical activity of neutrons under strong nuclear interactions, according to experiments by M. Forte et al;
- the apparent, also substantial violation of the T-symmetry under strong nuclear interactions, according to experiments by Conzett et al;
- the apparent, also considerable deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data can be manipulated to force compatibility with orthodox laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. However, the experimental information is sufficient to establish that the exact validity of conventional laws under strong interactions is a mere belief by individual groups of researchers at this time. In fact, the information points toward the alteration of the intrinsic characteristics of particles under strong interactions which is theoretically quite plausible (see also below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry, and the thrust toward a fundamental advancement. Understandably, the symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

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. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

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Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

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

cc. Professors A.ZUCKER, P.H.STELSON, H.N.HILL, F.C.MAIENSCHEIN, H.E.TRAMMEL, R.S.LIVINGSTON, D.B.TRAUGER, M.K.WILKINSON,

Courtesy copy to Professors P.D.MILLER and W.B.DRESS

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96 Prescott Street
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Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
tel. (617) 964 1684

July 2, 1981

Professor DAVID A. SHIRLEY, Director
LAWRENCE BERKELEY LABORATORIES
KERKELEY, California 94720

Dear Professor Shirley,

During the past years I have been contacting your Laboratory at the rate of less than once per year, to solicit the initiation of suitable experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

This is my letter of solicitation for the year 1981. Permit me to recall that, despite the availability of a number of proposals as well as of initial tests, I do not recommend any specific test. Instead, I recommend the setting up of a committee composed of experimentalists, theoreticians, and mathematicians, with the task of ascertaining the feasibility of new experiments at the LAWRENCE BERKELEY LABORATORIES, assessing the possible re-elaboration of old data, and, of course, evaluating available experiments. Predictably, for the experiments to be accepted by the scientific community at large, the data should not be elaborated via unverified conjectures, such as those of quark and other type. Additional suggestions for the structure and function of the committee are at your disposal on request.

You might be interested to know that we have made some progress in experimental nuclear physics. In fact, we now have in the field a coordinated group of mathematicians, theoreticians, and experimentalists actively involved. In particular, we have identified experimental information such as:

- the apparent, quite large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits and other data;
- the apparent, also substantial, deviation from the prediction of orthodox laws in the optical activity of neutrons beams under strong nuclear interactions, according to the experiments by Forte et al;
- the apparent, also quite large, violation of the T-symmetry under strong nuclear interactions, according to the experiments by Conzett (at your Laboratories) et al;
- the apparent, also considerable, deviation of experimental points from the predictions of the exact SU(2)-spin symmetry, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data might be re-interpreted to reach a form of compatibility with conventional laws; and all experiments may, in the final analysis, be disproved by future more accurate measures. However, the experimental information, when taken globally, points toward the alteration of the space-time characteristics of particles under strong interactions, which is quite plausible theoretically (see below), and which, if confirmed by future experiments, would imply the irreconciliable invalidation of the entire Poincaré symmetry, as well as the thrust toward fundamental advancements (despite conceivable opposition to achieve new fundamental knowledge). Evidently, the Poincaré symmetry would preserve physical value, but only as a crude approximation of a physical reality beyond its technical capability.

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.

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I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

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Very Truly Yours

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

cc.: Professors E.K.HYDE, R.K.WAKERLING, R.W.BIRGE, E.L.ALPEN, A.W.SEARCHY, G.C.PIMENTEL, W.D.HARTSOUGH, H.A.GRUNDER, W.A.LESTER, E.J.CAIRNS, J. HOLLANDER, J.CERNY, L.W.ALVAREZ, G.T.SEABORG

Courtesy copy to Professor H.E.CONZETT, as well as to R.L.KELLY, A.RITTENBERG, and T.G.TRIPPE.

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

tel. (617) 964 1684

July 2, 1981

Professor DONALD M. KERR, Jr., Director
Los Alamos Scientific Laboratories
Los Alamos, New Mexico 87545

Dear Professor Kerr,

I am contacting you to solicite the initiation at LOS ALAMOS SCIENTIFIC LABORATORIES of suitable experimental studies on the validity or invalidity for the strong interactions of the basic physical laws of the electromagnetic ones, with particular reference to Einstein's special relativity, Pauli's exclusion principle, and other basic laws.

Permit me to indicate from the outset that, despite the availability of several old and new proposals as well as initial tests, I do not recommend any particular test. Instead, I suggest the setting up of a committee composed of experimentalists, theoreticians, and mathematicians with the task of ascertaining the feasibility at your laboratories of new tests, assessing the possible re-elaboration of old data, and, of course, evaluating available experimental information. Predictably, for the tests to be accepted by the scientific community at large, the data should avoid unverified conjectures in the their elaboration , such as those of quark and other type. Additional suggestions on the composition and function of the committee are at your disposal.

For your information, we have recently made some progress in <u>experimental nuclear physics</u>. In fact, we have today a coordinated group of mathematicians, theoreticians, and experimentalists working at the problem. In particular, we have identified experimental information such as:

- the apparent, rather large, deviation of the magnetic moment of hadrons from conventional values under strong nuclear interactions, as identifyable via the Schmidt limits and other data;
- the apparent, also quite substantial, deviation from conventional predictions in the optical activity of neutrons under strong nuclear interactions, according to the experiments 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 considerable, deviation from the predictions of the exact SU(2)-spin symmetry under strong nuclear interactions, according to the experiments by Rauch et al; and other data.

Admittedly, this experimental information is still preliminary; all data can be re-interpreted to achieve a sort of compatibility with conventional laws; and all experiments can, in the final analysis, be disproved by future, more accurate measures. Nevertheless, the experiments are such to establish that the exact validity of conventional laws under strong interactions is a mere belief at this time by individual groups of researchers. In fact, the information, once seen globally, points toward the alteration of the space-time characteristics of particles under strong interactions which is quite plausible theoretically (see below), and which, if confirmed, would imply the irreconciliable invalidation of the entire Poincaré symmetry, as well as the thrust toward a fundamental advancement (despite conceivable opposition to achieve new knowledge). Needless to say, the Poincaré symmetry would preserve physical value, but only as a crude approximation of a reality beyond its technical capability.

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. Yet, the constituents (e.g., a satellite during re-entry) evolves according to a time evolution which must be noncanonical to accomodate the nonpotential (non-Hamiltonian) contact interactions. In the final analysis, our Earth can be a Newtonian image of the structure of hadrons, in exactly the same way as our planetary system is a Newtonian image of the structure of atoms.

I have recalled these known points to stress the complexity of the problem underlying my proposal to you. In fact, my proposal ultimately calls for direct measures under strong interactions, which is not an easy task. Yet, the need to initiate at least feasibility studies is much pressing, and increasing in time. Following several international conferences on the subject, and countless articles, the open character of the basic laws under strong interactions is too well know to be continued to be ignored by researchers in high energy physics; the human and financial resources we currently spend in the development of the theory of the strong interactions are too huge to justify ignorance of the fundamental aspects without risking dangerous administrative unbalances; and the implications of the knowledge advocated (e.g., for the controlled fusion) are too serious to prevent the accumulation of a need of potentially crushing and definitely unpredictable consequences.

In case at our Institute we can be of any assistance for the initiation of the proposed research, you can count on my best possible collaboration. Also, our group will meet at the forthcoming

- FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which will be held here from August 3 to 7, 1981 (see enclosed announcement).

A representative from your laboratory as an observer would be sincerely welcome. I would like to encourage the participation by your Laboratory, in particular, to our forthcoming

- FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which will be held at the Université d'Orléans, France, from January 5 to 7, 1982, under financial support by the French Government via local Institutions.

Perhaps, you should consider attending this conference personally, to have a direct knowledge from the various scientists from several Countries working at the problem, as well as to ascertain the status of possible studies in the field in foreign Countries, such as the U.S.S.R. In case, however, your understandable duties prevent you from participating, you can count on my best assistance to arrange the participation by representatives from your laboratory.

Very Truly Yours

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

cc. F. ZACHARIASEN, Associate Director
(names of divisional directors not available, but copies of this letter are available on request)
Courtesy copy to R.A.HARDEKOPF, P.W.KEATON, P.W.LISOWSKI, and L.R.VEESER

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

4-th of July, 1981

Professor SYDNEY MESHKOV
National Beaureau of Standards
WASHINGTON, D.C.

Dear Professor Meshkov,

I am here inviting you to become an EDITOR FOR THEORETICAL PHYSICS of the HADRONIC JOURNAL, in the specific field of quark models, QCD, and gauge theories, to continue the editorial function by HOWARD GEORGI, who served as an editor from the initiation of the Journal in 1978 to his promotion to full professor of physics at Harvard in the past fall. This invitation originated via a kind, informal indication by GINO SEGRE at the University of Pennsylvania, and was subsequently approved by representative members of the current editorial organization of the Journal, copy of which is enclosed. The following information may assist you in your decision.

General information on the Journal. It was initiated in 1978, and since then, it has been published exactly on schedule. We are now at the fourth volume (which is just about to be completed). Originally, the Journal published only articles. More recently, the Journal initiated the publication of a selected number of Proceedings of Workshops, Conferences, and Summer Schools. This service has been well accepted by the community and it is becoming a success due to the virtual impossibility, lately, to locate articles in Proceedings of this type (to have an idea of the numbers, there are some 125 graduate schools in physics in the U.S.A., more than half of which are under serious financial restrictions—therefore,the sale of 45-to-50 Proceedings is outstanding these days...). By having the Proceedings appear in a regular Journal, the problem of the location of articles is considerably alleviated. Copy of the Table of Contents of some of the Proceedings we have published is enclosed. More recently, the Journal has initiated the publication of a considerable series of reprint volumes in hadron physics. They are:

- "Developments in the Quark Theory of Hadrons"; Edited by DON B. LICHTENBERG of the University of Indiana, and PETER S. ROSEN of Purdue University; and
- "Applications of Lie-admissible Algebras in Physics"; Edited by Professor HYO C. MYUNG ofthe University of Norther Iowa, Professor SUSUMU OKUBO of the University of Rochester, and myself.

You have perhaps seen the first volume of the first series (if not, please let me know, and I shall let you have a complimentary copy). Don and Peter are now working at the second wolume, and we, at the Journal, are ready to publish it whenever they have finished their (rather coplex) work of selecting good articles. The second series is a sort of negative of the first, and viceversa, in the sense that it publishes articles in hadron physics which are not of quark inspiration. The idea is that, via the two combined series, we may have a record of valuable research in this last part of our century which may acquire value in time.

Thanks to these various scientific initiative, as well as, more importantly, to the invaluable contribution by all members of the Editorial Organization, the Journal has now reached financial selfsufficiency, as well as world wide distribution (for example, all research libraries of China subscribe to our Journal). It should be said that the primary strength of the Journal is abroad, owing to the disastrous financial condition here at home. Particularly rewarding is the request of reprints from virtually all over the world that our Authors receive.

General information on the Editors. Each Editor has complete, final, and independent scientific authority. In particular, the Publisher is obliged to publish all articles accepted by each editor. The Editors often consult each other, but decisions are taken individually. Also, the Journal is known to avoid the criptic anonimity so conducive

to academic mumbo-jumbo. The Editors select valuable articles for study, and return those considered not suitable with a nice letter of recommendation for other Journals. The few articles selected are personally studied by the Editors almost all of the cases, occasionally, with outside consultation when needed. The Editors therefore write directly to the Authors, by communicating their comments. In this way, we have established a nice record of excellent relationship Editor-Author, which is friendly, cooperative and most effective. The Editorial Load per author is truly minimal. In fact, we study an average of three articles per month in our field. Those are articles which we would study anyhow, whether Editors or not. The position of Editor does not carry a salary at this time, although the Journal reimburses all direct expenses, such as secretarial, postage, and phone. If the subscriptions will increase to the point of rendering the Journal profitable, each Editor will have a salary/honorarium. Each Editor has a complimentary subscription to the JOurnal, as well as a free copy of all publications of the Hadronic Press.

Journal's scientific attitude. We have a record of publishing articles for their scientific contents, and of being totally insensitive to ethnic, religious, political, or other aspects. A nice episode is that of a refusenik from the U.S.S.R., Dr. Y.A.GOLFAND. FRITZ ROHRLICH called me one day indicating the search by this author of a western Journal to publish one of his articles, and his difficulties with another qualified Journal. I stressed to Fritz that our Journal is keenly open to Authors for technical assistance, and that is what happened. Dr. Golfand received scientific assistance by a number of us to reach the utmost possible maturity, and his article was subsequently published in Hadronic J. 2, 261 (1979). More receintly, Dr. Golfand was reinstated in his post, and I received very nice and rewarding letters of thanks by Jewish organizations in the U.S.A. for the little our Journal had done. We would like to keep our function of a medium open to all, and I believe that you would be particularly qualified for the continuation of this function.

Also, we are particularly interested in avoiding a monopolistic restriction of articles along quark lines only. I have personally invited several authors for papers along quark lines. Yet, I have made an effort in publishing, jointly, papers along different lines, so that the Journal is open to all potentially valuable lines of studies on the fundamental problem of hadron structure (the terminal character of unitary models for hadron classification is out of the question for us, and the dubious aspect is only for the joint structure). This attitude is received with criticism by physicists financially committed to quarks. Yet, the scientific community at large seems to be particularly receiptive to the uncommitted character of the Journal. As one distinguished scientist put it to me, the publication of articles based on quark assumptions for the hadronic structure, but in their current status of lacking a true, strictly rigorous confinement, is literally equivalent to the publication of articles in mathematical journals stating that 2 + 2 = 138792.345. At our Journal we therefore make a point in acknowledging the scientific value of papers along quark lines, but, at the same time, we equally make a point in focusing their yet incomplete and nonterminal character.

I believe you would be an excellent editor for such a scientific function. You have a proved record of ethical standard. I therefore believe you would see with grace the publication of articles accepted by you, while subsequent issues could publish articles expressing different or oppositive viewpoints. At any rate, this is the only way for the true pursuance of physical knowledge.

Journal's primary objective. In reaching your decision, you should know that the primary reason why I initiated the laborious process of setting up a new Journal is to promote the experimental verification of the validity or invalidity (exact or only approximate validity, if you prefer) for the strong interactions of the basic physical laws of the electromagnetic interactions, with particular reference to the entire (connected and discrete) Poincaré symmetry, the gauge symmetry, and Pauli's exclusion principle.

Thanks to our yearly WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS, this problem is now studied by a coordinated group of mathematicians, theoreticians, and experimentalists. We have made considerable progress in experimental nuclear physics via the identification of experimental information indicating the invalidity, such as

- the apparent quite large deviation of magnetic moments under strong nuclear interactions according to the data of the Schmidt limits;
- the apparent, also quite large, deviation from conventional predictions (including those of the Weinberg-Salam theory) for the angle of precession of the optical activity of neutron beams under strong nuclear interactions, according to an experiment by Forte et al;
- the apparent, also substantial, breaking of the T-symmetry under strong nuclear interactions, according to the experiment by Conzett et al; and other.

We shall soon initiate studies in experimental high energy physics. The objective here is to formulate direct experimental tests( say, of intrinsic quantities of hadrons under strong interactions) without experimentally unverified theoretical assumptions in the data elaboration. Specifically, to reach the needed final character, the elaboration should not be based on quark conjectures (it would be, in this case, only a vague argument of plausibility under a serious and strict scientific scrutiny). It is a long way to go, but we are determined to follow it to its end, that is, to the end of this truly unique episode of the history of physics whereby huge amounts of human (and financial)resources are spend in the development of the theory of the strong interactions, by virtually ignoring the verification of the basic physical laws. As an historian put it to me, unless this situation is corrected soon, and direct experiments are at least started to be considered, there is little doubt that future historians will have a severe judgment of the ethical standards of contemporary high energy physicists. After all, the open character of the basic laws is known from open legacies of the founders of contemporary physics, let alone a large series of (completely ignored) articles.

My recommendation. In case you are seriously interested in considering the position of Editor of the Hadronic Journal, I would like to recommend that you participate as an auditor to our FOURTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, which we will have here in Cambridge from August 3 to 7, 1981. At our Institute we have funds from the DEPARTMENT OF ENERGY to support (most of) your expenses. This would give you an opportunity to listen and meet several members of our group.

You could then initiate your function of EDITOR FOR THEORETICAL PHYSICS beginning from the first issue of Volume 5, that of December 1981.

In case you need any additional information, please feel free to call me (tel(617) 964 1684) or HOWARD GEORGI (tel. (617) 495 3908), or any other member of our Editorial Organization.

Sincerely

Ruggero Maria Santilli
Editor in Chief
HADRONIC JOURNAL

RMS-ml
encls.

cc. Professors HOWARD GEORGI, Harvard Univ. and GINO SEGRE, University of Pennsylvania

**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 15, 1981*

*Professor S. MESHKOV*
*National Beaureau of Syandards*
*WASHINGTON, D.C.*

*Dear Professor Meshkov,*

*In the afternoon of July 29, 1981, the buildings on Harvard Grounds known as the PRESCOTT HOUSE were acquired to provide permanent facilities for the new INSTITUTE FOR BASIC RESEARCH.*

*Ruggero and Carla Santilli cordially invite you to attend*

*THE INAUGURATION CEREMONY*
*Monday, August 3, 1981*
*from 9 a.m. to 9.30 a.m.,*

*and*

*THE AFTER-DINNER PARTY*
*Thursday, August 6, 1981*
*from 8 p.m. to 10 p.m.*

*R. S. V. P.*
*(617) 964 1684*

MESHKOV NEVER ACKNOWLEDGED ANY OF THESE INVITATIONS.
R.M.S.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address*
SLAC, P. O. Box 4349
Stanford, California 94305

July 13, 1981

Professor Ruggero Maria Santilli
Professor of Theoretical Physics, and
Chairman of the Board of Trustees
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Mass. 02138

Dear Professor Santilli:

Thank you very much for your letter of July 2 which you describe as the annual letter "to solicit the initiation at SLAC of experimental studies on the validity or invalidity for the strong interactions of . . ."

You correctly refer to the fact that the experimental information is still preliminary; in fact all experimental information is preliminary in the sense that it can and will be superceded by newer results. You also say "All data could be manipulated to force compatibility with conventional laws." Your principal proposal is that I should convene a meeting of leaders of our laboratory and in the field to consider experiments to specifically test your hypotheses.

Experiments are not conceived or designed in committee; rather, individual initiative arises from the scientific community and from that initiative results a proposal for a specific undertaking which appears technically feasible to the laboratory. The laboratory directors have little and should have little influence over this process. Therefore the <u>only</u> recourse you have is to disseminate your theoretical deliberations to as wide an audience of experimentalists as possible in a manner such that they can extricate easily the experimental implications of the theory.

With best personal regards,

Wolfgang K. H. Panofsky

Wolfgang K. H. Panofsky
Director

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

Professor WOLFGANG K.H. PANOFSKY, Director
Stanford Linear Accelerator Laboratories
Post Office Box 4349
STANFORD, California 94305

July 20, 1981

Dear Professor Panofsky,

I would like to express my appreciation for the courtesy of your recent letter, as well as for elucidating a number of points.

I agree that the decision to undertake an experiment is taken by one or more individual experimentalists, rather than a committee. Please keep in mind that a number of specific proposals by independent authors are available for experimentalists with the necessary courage and determination. I have recommended a committee of study because the selection of the best and most effective (technically and administratively) proposal appears to be rather complex, and demanding a variety of skills (from experimental aspects to mathematical profiles, all mostly new), which go beyond the capability of one single person these days. The study of the committee should therefore only pave the way to the actual, subsequent selection by experimentalists.

I am also in agreement, of course, with the fact that Laboratory Directors should have little influence, if any, in the selection of experiments.Since you have a better knowledge of the members of your laboratory, you could perhaps pass the information to potentially receptive experimentalists in a way much more effective of that we might do from far away. At the same time, I am sure you will agree that Laboratory Directors should be kept fully informed of primary aspects, particularly those with potentially delicate administrative implications.

I am flattered by your nice attitude to give me the paternity of the ideas. However, please keep in mind that the mathematicians and physicists of our group are simply trying to study as seriously as possible the teaching of our scientific fathers such as FERMI (strong interactions should contain a nonpotential component due to contact effects which are absent in electromagnetism), EINSTEIN (the uncertainty of the Copenhagen School is only a temporary episode in physics), JORDAN (the envelope of quantum mechanics should be generalized form the current associative form AB to a non-associative one $A \cdot B$), etc.

Please let me know in case you visit the Boston area (or simply pass by). It would be a sincere pleasure to meet you, show you the facilities of our new Institute, and, depending on your availability of time, have you as our guest for dinner. Several people tell me that I am much nicer and respectfull in person than in letters (which still suffer of my classic studies at a European lyceum).

Sincerely,

Ruggero

Ruggero Maria Santilli,
Director
RMS-ml

P.S. H. RAUCH, Director of the Atominstitut in Wien has just informed me that, following our work, his peaople in Wien have reviewed and updated their data elaboration regarding the results of their $4\pi$ experiment and, to his surprise, the new experimental number is $\alpha = 715.87 \pm 3.8^{\circ}$ (the old number was $\alpha = 716.8 \pm 3.8^{\circ}$ as reproduced in the paper I mailed you). As a result, there exist no experiment currently available which is capable of reproducing the $720^{\circ}$ which are needed to establish scientifically (that is, outside academic politics) the SU(2)-spin symmetry under strong nuclear interactions.

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 · Batavia, Illinois · 60510
312-840-3211 FTS 370-3211

Directors Office

July 28, 1981

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Your letter of 2 July has raised procedural problems we have no way of addressing. This Laboratory provides facilities for carrying out experiments in High Energy Physics - orthodox or not - as long as the Physics Advisory Committee deems the proposal of sufficient scientific merit.

The main point is that this Laboratory does not do experiments. These are proposed to us by users groups at Harvard, Caltech, and some 100 institutions in the U.S. and abroad. We would be happy to receive unorthodox proposals for research to which we can react. We do not have any mechanism to set up committees to address the kind of tasks you outline. This would have to be done at your initiative outside of the activities of Fermilab.

Sincerely,

Leon M. Lederman

Leon M. Lederman

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
tels. (617) 964 9859
(617) 964 1684

August 12, 1981

Dr. LEON M. LEDERMAN, Director
Fermi National Acceleration Laboratory
P.O.Box 500
BATAVIA, Illinois 60510

CONFIDENTIAL

Dear Dr. Lederman,

I would like to express my appreciation for your kind letter of July 28, 1981. However, permit me the liberty of expressing concern for its content.

Truly large financial and human resources have been spend through the years and are currently spent at FERMILAB in strong interactions, all under the assumption of the validity of conventional laws, and despite the knowledge, repeated through the years, that possible modifications of the basic laws imply such technical consequences to result in different numbers for the same experiments. The seriousness of the problem is then self-evident.

On my part I have simply accomplished the scientific duty of bringing to the attention of Fermilab (to Dr. Wilson first, and now to you) the existence of a rapidly growing community of scientists and observers calling for the experimental verification of the basic laws, irrespective of its result (whether in favor or against), as well as, perhaps equally importantly, the achievement of a more balanced use of public funds.

My concern for FERMILAB has been increased considerably by your letter because Harvard, Caltech, and all the other academic Institutions you mention <u>are not</u> responsible for the situation. In fact, these institutions have good reasons to resist any intrusion in their own internal decisional processes. As a result, the entirety of the responsibility of the situation is viewed to rest on you, as well as all the other executives at FERMILAB and other national laboratories. The fact that, according to your letter, FERMILAB does not have mechanisms to set up committees of study, can aggravate the situation, but cannot eliminate your responsibility. To be specific, if fifty colleges propose independently exactly the same experiment, they infringe no rule. It is the responsibility of bodies such as FERMILAB to prevent that public funds are wasted by unnecessarily repeating the same experiment fifty times. If all the colleges affiliated with FERMILAB abstein from proposing a needed experiment, they also violate no rule. In fact, if the experiment is needed to provide credibility to others, or for any other scientific reason, its promotion is expected from laboratories such as FERMILAB.

It is usually difficult to predict the future, and it is more so in this case. This means that everything may continue to function smoothly and orderly for years, or a serious crisis may be triggered a few months from now by malcontent or other unforeseable reasons, particularly in this delicate moment of considerable scrutiny on the use of public funds.

What appears recommendable in view of the circumstances, is to initiate all possible preventive measures to ensure the orderly continuation and function of our community. But, despite my best personal intentions, as well as the proved best intention of all colleagues I am in touch with, this objective simply cannot be accomplished in a way completely outside

FERMILAB, as your letter appears to suggest. For instance, no physicist will expectedly spent his time to conceive an experiment for a machine at FERMILAB without the collaboration of an experimentalist fully familiar with that machine.

In view of these and other aspects (which we should eventually discuss verbally), permit me the liberty of suggesting the following course of action.

1. Select at least one theoretician employed by FERMILAB with the task of studying the literature. This person would be welcome at any time and for any length of time as a guest at our Institute following your personal recommendation (I am told that most of the literature is not available at Fermilab).

2. Upon achievement of a minimum but sufficient knowledge of the literature, have this person attend the forthcoming International Conference at Orléans, France (January 1982), where he or she can speak directly with the various originators of the studies. Subsequent to that, have this person prepare a report to you on the issue. The understanding is that a copy of the report will be released to our Institute as a condition for participation.

3. In case of confirmation of the need to conduct direct experimental tests of the basic laws under strong interactions by this independent person, as expected and following full scientific documentation, FERMILAB will either make available, or recommend informally, by next Spring one or more experimentalist, whether employed by Fermilab or not, who are familiar with the machines at FERMILAB. The committee of study indicated in my letter of July 11, 1981 will then be set up by our Institute, and will include this (or these) experimentalists, as well as any other scientist recommended (formally or informally) by FERMILAB.

Whatever your final decisions are, you can count on my best possible understanding. However, prior to reaching such a final decision, permit me to recommend that you meditate a moment on the implications for a lack of any action on the problem.

Sincerely

Ruggero M. Santilli

RMS-ml

P.S. You might be interested in the following results which have occurred since my letter of July 11. This may give you an idea of the proliferation of studies in the sector in only one month.

1. Professor H. RAUCH (Director of the Atominstitut of Wien, Austria) and his collaborators have revised their measures on the spinor symmetry under strong nuclear interactions via neutron interferometers (Z. Physik B29, 281 (1978)). The old measure was 716.8 ± 3.8 deg, and, thus, it inluded the 720° needed for the exact SU(2)-spin symmetry (as reported in p.76 of my paper enclosed with my letter of July 11). The new measure is 715.87 ± 3.8 deg and, as such, it does not contain the 720°. This is the ONLY direct measurement of spin under strong interactions. It then follows that there exist no experiment at this moment which is capable of establishing

the exact character of the SU(2)-spin symmetry under strong interactions on true scientific grounds (that is, by excluding academic politics). The need to conduct new experiments is self-evident for all physicists in the field.

2. Professor G. EDER (author of a celebrated texbook in nuclear physics by The MIT Press) has completed extensive theoretical studies on the test of the SU(2)-spin symmetry via neutron interferometers.His results are numerous and each of them is substantial. First, he has proved that, even assuming the preservation of the conventional eigenvalues of the magnitude of the spin and of the third component, the SU(2)-spin symmetry can still be broken by non-Hamiltonian (as well as Hamiltonian) forces capable of distorting (mutating in our language) the charge distribution of the neutron. The breaking is quantitatively identifyable in the other components of spin, as well as in higher powers of the Casimir, and it is realized via the proposal of 1978 (replacement of the conventional associative envelope with a nonassociative, Lie-admissible, mutational algebra). After applying this generalized/broken SU(2)-spin structure to several aspects of nuclear physics, Prof. Eder concludes by pointing out that, when neutrons propagates through matter, the electromagnetic field produced by the electron clouds inside an atom is so strong that might produce a distorsion of the spin of the neutron of the measurable order of one per-cent (which is exactly in lines with the experimental results of point 1). For your information, at the time of my letter to you of July 11, 1981, we had doubts on the measurability of the mutation of spin via neutron interferometers because we considered <u>only</u> the nucleon-nuclei interactions. In short, not only the available direct experiments do no reproduce the conventional theory of spin, but the deviations are in full agreement with the theory.

3. Professor R. MIGNANI (of the Theoretical Physics Institute of the University of Rome, Italy, and well known for his studies on unitary symmetries for hadrons) has recently identified the foundations for a nonpotential scattering theory, including the basic lines for the computation of the cross section under contact effects due to mutual wave overlapping. Predictably, the construction appears to be a theoretical version of the experiment by Conzett et al on the violation of the T-symmetry. The theory appears to confirm, this time from a scattering profile, the expectation that under mutual penetration of wavepackets, the entire Poincaré symmetry (connected AND discrete part) is broken. The orthodox physicist will predictably have a difficult time in reaching credible dismissals. In fact, the theory is of direct interest to experimenters, and embodies basic physical notions which are so natural in all branches of physics except high energy physics. In fact, we must violate the T-symmetry in Newtonian mechanics to prevent perpetual-motion-approximations. The symmetry must be violated in classical and quantum statistical mechanics for a number of reasons which are well known. Orthodox physicists claim that the T-symmetry is misteriously restored in certain segments of particle physics. We respect this view. However, to prevent delicate administrative implications, the use of scientific authority should be prevented as much as possible, extensive investments on experiments based on the assumption of the exact symmetry should be avoided for the time being, and the issue should be resolved <u>experimentally</u> one way or the other in due time.

Several additional developments have occurred during this single month by mathematicians and physicists from the U.S.A., France, Switzerland, Israel, Italy, Sweden, West Germany, and other Countries. They are simply too numerous and too complex, to be indicated in a letter.

UNIVERSITÉ DE LAUSANNE
INSTITUT DE PHYSIQUE NUCLÉAIRE
Bâtiment des sciences physiques
Dorigny
CH 1015 LAUSANNE
Tél. (021) 24 00 46

Lausanne, le July 15th 1981

Professor R. M. SANTILLI
The Institute for Basic Research
Harvard Grounds
96, Prescott Street

CAMBRIDGE - Massachusetts 02138

U S A

Dear Professor Santilli,

Thank you very much for sending me the poster for the Orleans Conference and your preprint TP-DE-81-3. Here after are the addresses of the physicists which you requested :

Prof. E. Leader, Westfield College, Kidderpore Av.
London NW3 7ST, UK.

Prof. J. Bienlein, DESY, Notkestr. 85, 2000 Hamburg 52, Germany

Prof. L. Madansky, John Hopkins University, Baltimore MD 21218

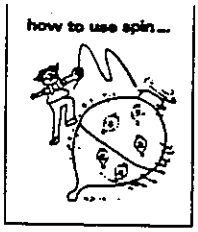
Prof. B. Montagne, CERN, 1211 Geneva, Switzerland

Prof. Ch. Prescott, SLAC, P.O. Box 4349, Stanford CA 94305

I am entirely at your disposal for further information on the 1980 Lausanne Conference on spin physics and remain

Sincerely yours

Trân Minh-Tâm




1980 International Symposium on High-Energy Physics
with Polarized Beams and Polarized Targets

Lausanne (Switzerland)
25 September – 1 October 1980

# IS SPIN PHYSICS WORTHWHILE ?

Round table discussion
held during the

1980 INTERNATIONAL SYMPOSIUM ON HIGH-ENERGY PHYSICS
WITH POLARIZED BEAMS AND POLARIZED TARGETS

Lausanne 25 September - 1 October

Summary
prepared by

M. Jacob, CERN, Geneva,
Moderator of the discussion
Scientific secretary: M. T. Tran, Lausanne

Organizing committee:
C. Joseph, Lausanne, R. Hess, Genève, J. Soffer, CERN, E. Heer, Genève, L. Dick, CERN, B. Montague, CERN, D. Möhl, CERN, T. Niinikoski, CERN, S. Mango, SIN
Organized jointly by the Universities of Geneva and Lausanne
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R. Hess Département de Physique Nucléaire et Corpusculaire 32, Bd d'Yvoy CH–1211 Genève 4 022/21 93 55

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

October 21, 1981

Office of the President

Professor Ch. PRESCOTT,
Stanford Linear Accelerator Center
STANFORD, California 94305

with copy of this letter to:

Professor L. Madansky, John Hopkins University, Baltimore, Md
Professor E. Leader, Westfield College, London, England
Professor J. Bienlein, DESY, Hamburg, West Germany
Professor G. Montagna, CERN, Geneva, Switzerland

Dear Professor Prescott,

I have read with interest your paper "IS SPIN PHYSICS WORTHWILE?", from the Round Table Discussion held during the 1980 International Symposium on High Energy Physics with Polarized Beams and Polarized Targets (Lausanne, Sept. 25-Oct.1, 1980). Copy of the report, jointly with your addresses were kindly mailed to me by Dr. TRAN MINH-TAM of the Institut de Physique Théorique de l'Université de Lausanne, who is here gratefully acknowledged.

I have found the various sessions of your Round Table intriguing and sound. Nevertheless, permit me to bring to your attention a rather important aspect of spin physics of which you are, apparently, not aware of. It consists of the problem whether the conventional atomic notion of spin is exact or only cr dely approximative in the transition from the electromagnetic interactions (for which the notion was conceived and it still is experimentally testes today), to the different physical arena of the strong interactions. The problem is posed by the following layers of physical knowledge.

(1) For macroscopic bodies, it is well established since the past century that charge distributions and their magnetic moments exterience an alteraction under intense fields which generally implies a change of the intrinsic angular momentum, if any.

(2) In the transition to quantum mechanics, it has been equally well established since the early part of this century, that the charge distribution of atoms experiences an alteration under strong fields, which is essentially a form of "quantized version" of the classical one.

(3) In the transition to the deeper layer of particle physics, the situation is not equally well established at this moment. To my best knowledge, it appears that the Schmidt limits in nuclear physics suggest in a rather forceful way that, say, a proton experiences an alteraction of its magnetic moment in the transition from the condition of being the nucleus of an Hydrogen atom, to the different conditions of being a member of a nuclear structure, say, Pa. This possibility is self-evident also from chain (1)-(2) of physical laws, and it is indicated in well written treateses in nuclear physics. Permit me to stress, again, that the knowledge is not experimentally established in a final form. Nevertheless, we can say with confidence that the alteration of the magnetic moment is definitely the most probable and plausible, not only on intuitional grounds, but also on grounds of available data. If orthodox physicists prefer the preservation of the magnetic moment, to reach credibility by the scientific

community at large, they should provide convincing experimental evidence as well as theoretical argumentations proving that the chain of laws (1)-(2) suddenly breaks down at level (3).

The open character of the notion of spin under strong interactions is a direct consequence of the situation above of the magnetic moment. In particular, the following two schools of thought can be identified in the contemporary community of basic research.

SCHOOL 1: This first school assumes the exact validity of the conventional SU(2)-spin Lie symmetry under strong interactions, and constructs thereafter the theory in such a way to comply with the assumption. In particular, this implies the assumption that the spin stays the same even under alterations of the magnetic moment. More fundamentally, the assumption enters in the data elaboration of contemporary experiments under strong interactions in a general tacit way. The experiments, therefore, simply cannot test the assumption.

SCHOOL 2: This second school (of which I am a member) makes no aprioristic assumptions of experimentally unverified knoweldge, particularly of fundamental character such as this one. The research attitude instead is that of identifying all most probable possibilities and letting the experiments solve the issue. When hadrons are accepted as they actually are (extended charge distributions), an alteration of their magnetic moment is expected to imply, most likely, that of spin with a possible, consequential breaking of the SU(2)-spin symmetry under strong interactions. At any rate, the same conclusion can be reached via a number of equivalent arguments. First, the construction of a theoretical model whereby the magnetic moment of the hadrons changes under strong interactions, but the spin stays the same is extremely difficult to realize owing to a host of consistency problems (the assumption is, again, that the hadron is not conceived as a point, and, also, that the issue is not left at the level of words, but actual calculations are conducted). Second, spin was conceived by the Founders of atomic mechanics for isolated particles in vacuum under long range electromagnetic interactions, while we have here a fundamentally different physical arena (wave packets in conditions of mutual penetration and overlapping). The most probable case is therefore that deep wave overlappings create interferences in the intrinsic angular momentum, resulting in the need of a more general theory. Third, it has been recently shown in the literature that, if the strong interactions have a longly claimed nonpotential nonlocal term, the SU(2)-spin symmetry breaks down at its central part, the enveloping algebra; similarly, it has been proved that, even when the third component and the magnitude of a spin are the conventional ones, the SU(2)-spin symmetry can be grossly broken because of deformations/distorsions resulting in the other components as well as in the higher-order Casimirs; etc.

But, perhaps, the most intriguing information is that of experimental nature. Apparently, the only available experiment capable of directly measuring spin-related quantities under joint strong and electromagnetic interactions is the fundamental experiment by Rauch and his associates of the Atominstitut of Wien on the spinor symmetry of neutron wave functions via neutron interferometers. The expected measurement for the $4\pi$ symmetry (two complete spin flips) was, of course, 720 deg. The best measure (conducted in 1978) was 716 ± 3.8 deg, while recent updatings due to new physical data give the value of 715.8 ± 3.8 deg. As you can see, the value 720 deg <u>IS NOT</u> part of the best available measures. The problem of the spin under strong interactions is therefore open at this time.

Needless to say, a problem of this nature must be solved with the participation of the scientific community at large. In the hope of contributing toward this goal, we have organized the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS which will be held at the Université d'Orléans, France, from January 5 to 9, 1981, as per enclosed announcement. A number of experimentalists, theoreticians, and mathematicians will present aspects directly or indirectly related to the problem, beginning with talks at the Newtonian level, then passing to the statistical level, and finally entering into the realm of particle physics.

I am here formally inviting you, and/or your colleagues Professors MADANSKY, LEADER, BIENLEIN and MONTAGNA to participate to this Conference, by presenting your view, whether in favor or against the preservation of the atomic notion of spin, and whether of theoretical or experimental orientation.

In particular, you can participate by either
- in person, in which case please contact the organizational office at Orléans as soon as possible, as the closing date for registration is near. In this case, please also let us know whether you are interested in delivering a talk either on the subject, or in a related topic.
  Or, in case you cannot attend,
- by mailing a contributed paper for the Proceedings of the Conference as a corresponding participant.

A registration form is enclosed for your convenience. To provide assistance in reaching your personal view on the topic, I have separately mailed to each of you one copy of a collection of seven articles prepared by our Institute entitled
"Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions".
while I remain at your disposal for any additional assistance you might need.

Needless to say, whenever any of you will be in Cambridge, you would be sincerely welcome to visit our new Institute.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics and
President

RMS-pm
encls.
cc.: Professors J. FRONTEAU and A. TELLEZ-ARENAS, Univ. of Orléans, France and Dr.TRAN MINH-TAM, Univ. of Lausanne, France.

HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

October 28, 1982

Professor CHRIS QUIGG
Head, Theoretical Physics
FERMILAB
P. O. Box 500
BATAVIA, Illinois 60510

Dear Professor Quigg,

FERMILAB is renowed for the completeness of its libraries, with particular reference to its subscriptions to technical Journals in physics and mathematics. Yet, your laboratories do 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 the general libraries at your laboratories, information about our Journal. As you know, our Journal is the forerunner in the promotion of experimental, theoretical, and mathematical knowledge 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 the conduction of research and the publication of articles with the tacit 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 your laboratories have not subscribed to the HADRONIC JOURNAL until now apparently because of the opposition by individual members of 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:

[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 FERMILAB 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 studies on 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,

C. G. Gandiglio
President
HADRONIC PRESS, INC.

cc: Professor R. M. SANTILLI, Editor in Chief, Hadronic Journal, I.B.R., Cambridge, Massachusetts

Professor L. LEDERMAN, Director, FERMILAB

CGG/mlw

Enclosures

# PART XI:

# TACUP

# COMMITTEE

UNIVERSITY OF CALIFORNIA, LOS ANGELES

UCLA

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO

SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 8, 1982

Dear Colleagues:

As you probably know, the High Energy Physics Division of DOE has appointed a Technical Assessment Committee for University Programs (TACUP) and requested that TACUP undertake a review of DOE's University-based high energy physics program.

The charge to TACUP has two major parts. The first of these is an evaluation of the individual Tasks and Contracts. This review is being carried by eight Technical Panels, three in theory and five for experimental work, whose activities have already commenced. A list of the Panel members of TACUP is attached.

The second part of the TACUP charge is a broad analysis of the University-based high energy physics program with emphasis on questions of balance between various research components, on current problems and on future opportunities. The Panels will also participate in this second task after the review of individual Contracts is completed. The charge to TACUP dealing with the overall evaluation of the program reads as follows:

(2) Review the DOE university research program in the context of the entire HEP program including the following:
   (a) The appropriate role and scope of the program of university research.
   (b) The appropriate balance of research efforts such as: experiment and theory, accelerator and non-accelerator experiments; experiments using U. S. accelerators and foreign accelerators; large and small efforts; speculative and precision experiments; etc.
   (c) Present and foreseen university research problems and opportunities.
   (d) Outlook for the future.

The response of TACUP to this part of the charge should reflect not only the views of the TACUP members but, preferably, of the high energy physics community at large. Accordingly, I am writing to you as a member of DPF to solicit your views and comments on any topic relevant to this charge. Principal Investigators and Task leaders have already been invited to comment in the context of the Questionnaires which they were asked to complete. This letter is intended to elicit a broader range of views.

TACUP will greatly appreciate your suggestions and comments. If you care to respond please send your letter to me before December 30, 1982.

Best wishes,

Harold K. Ticho
TACUP Chair

HKT:chs
Enclosure

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 19, 1982

Professor HAROLD K. TICHO
Department of Physics
University of California
LOS ANGELES, California 90024

URGENT — FEDERAL EXPRESS MAIL

Dear Professor Ticho,

We would like to prepare for your TACUP Committee a comprehensive report on certain basic aspects of theoretical physics (gravitation, particle, and nuclear physics), and of experimental physics (high energy and nuclear physics). In particular, we would like to indicate to your Committee for consideration, certain beautiful, potential, fundamental advances that, in our view, are within reach of the DOE with current financial and human resources. But, for us to do so, we need the following information.

[1] Is TACUP only an advisory committee? or is it invested with decision-making authority?

[2] Which are the guidelines for evaluation? Did you devise an evaluation instrument? and, if so, is the instrument available to us?

[3] Will TACUP report only to DOE? or will TACUP report also to other governmental bodies?

[4] Which are the deadlines for the completion of [4a] existing contracts and [4b] of the entire HEP program?

[5] Will the final TACUP report be available to the public? and, if so, when, where, and how can it be obtained?

[6] Is the review limited to currently executed contracts? or does it include proposals under consideration?

[7] To achieve sufficient maturity, including reviews prior to your submission, we have difficulties to complete our report prior to the second half of January 1983 (I believe that several other colleagues will have similar difficulties owing to shortness of the notice and its occurrence within the holyday period). Kindly indicate whether the delivery of our report on general HEP aspects in the second half of January 1983 is acceptable.

A written replay to the above questions would be gratefully appreciated, possibly be Federal Express (to avoid its possible arrival after your deadline because of the current state of mail).

Wishing you the best Season Greetings, I remain

Yours, Sincerely

Ruggero M. Santilli
President

RMS-mlw cc: Drs. WALLENMEYER, HILDEBRAND, and THEWS, DOE

P.S. I am the principal investigator of DOE Contract No. DE-AC02-80ER10651.A002. However, I did not receive the questionnaire indicated in your letter. Kindly let me have a copy, in case I am entitled to. Thank you.

I enclose as a gesture of courtesy general information on our recently organized institute.

UNIVERSITY OF CALIFORNIA, LOS ANGELES

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SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 27, 1982

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

Dear Dr. Santilli:

Thank you for your letter of December 19, 1982, and the attachments.

When TACUP was set up, DOE provided us with a list of programs to be reviewed. Your Contract was not among those so flagged and for this reason it was not included in our reviewing plans. I would suggest that you contact the DOE program monitors for clarification.

Sincerely,

Harold K. Ticho, Chair
TACUP

HKT:chs

cc: Dr. B. Hildebrand

DOE F 1325.8
(7-79)

U.S. DEPARTMENT OF ENERGY

# memorandum

DATE January 17, 1983

REPLY TO
ATTN OF

SUBJECT Next Meeting of HEPAP, February 7-8, 1983

TO Distribution

The next meeting of the High Energy Physics Advisory Panel is scheduled for February 7-8, 1983, at DOE Headquarters in Germantown, MD, Room A-410. The meeting will run from 9am to 6pm on February 7, and from 9am to 4pm on February 8. Tentative agenda items include: Discussion of the FY 1983 DOE/HEP Continuing Resolution and the NSF/EPP Budget; Discussion of the FY 1984 Presidential Request Budgets (if available) for DOE/HEP and NSF/EPP; Discussion of the planned 1983 Subpanel on Future HEP Facilities; Discussion of progress of the technical assessment panels for university programs and non-accelerator experiments; and Status of international bilateral agreements in HEP.

John Wollenmeyer

for P.K. Williams
Executive Secretary
High Energy Physics Advisory Panel

1/14/83

Tentative Agenda
High Energy Physics Advisory Panel
U.S. Department of Energy
Germantown, MD
February 7-8, 1983

Monday, February 7, 1983

| | |
|---|---|
| 9:00am | Administrative |
| 9:15am | Discussion of NSF/EPP FY 1983 Budget and FY 1984 Presidential Request Budget*, D. Berley |
| 10:00am | Discussion of DOE/HEP FY 1983 Continuing Resolution and FY 1984 Presidential Request Budget*, W. Wallenmeyer |
| 11:00am | Break |
| 11:15am | Brief Discussion of U.S. Program Abroad, B. Hildebrand |
| 11:45am | Discussion of Budget Matters Relating to FY 1983 and FY 1984 |
| 1:15pm | Lunch |
| 2:00pm | Discussion of the 1983 Subpanel on Future HEP Facilities |
| 4:00pm | Break |
| 4:15pm | Further Discussion of FY 1983 and FY 1984 Budget Matters |
| 6:00pm | Adjourn |

* Subject to availability of the FY 1984 Presidential Request Budget

Tuesday, February 8, 1983

| | |
|---|---|
| 9:00am | Administrative |
| 9:05am | Status of International Bilateral Agreements in High Energy Physics, B. Hildebrand, E. Fowler |
| 9:45am | Small Business Innovation Research Program, W. Wallenmeyer |
| 10:00am | Report of the Experimental Technical Assessment Panel (Proton Decay Experiments), R. Adair |
| 11:00am | Break |
| 11:15am | Status of the Technical Assessment Panel on University Programs, H. Ticho |
| 12:15pm | Further Discussion on 1983 Facilities Subpanel |
| 1:15pm | Lunch |
| 2:00pm | Further Discussion on Budget Matters and 1983 Facilities Subpanel |
| 4:00pm | Adjourn |

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

January 22, 1983

Dear Professor Ticho,

I am contacting you on a personal and confidential basis in the hope that you can provide us with advice and council on a problem of contemporary high energy physics we believe of particular fundamental character:

The experimental resolution at National Laboratories of the exact or only approximate validity of Einstein's special relativity for the interior of systems with strong forces

I enclose a general presentation which, upon suitable modification, was intended for your TACUP committee. Since our contract has not been flagged, and following consultation with Bernie Hildebrand, we have absteined from submitting any formal report.

Nevertheless, we believe that the problem considered persists. I hope that the enclosed presentation, even though non-technical, possesses sufficiently diversified information to show that the problem, besides being manifestly intriguing, is indeed within experimental, theoretical, and mathematical reach.

Our old idea on how to proceed is as collegial as possible, and consists of the setting up of a Review Panel with the tasks, among others, of:

[1] Identifying the pitfalls of the current arguments intended in the hope of nullifying the need of the tests (see Section 5.3 of the enclosed report for an outline);

[2] Identifying all available proposals of direct tests of the special relativity under strong interactions, and assessing their feasibility (such as Kim's proposal to measure the meanlife of unstable hadrons -not leptons- in flight at different energies, as briefly touched in Part 3);

[3] Identifying the equipments available at National Laboratories which appear to be most suited for the tests, by keeping in mind that we are referring here to the new experimental challange of reaching actual measures under external strong interactions;

[4] Identifying additional equipments that might be suitable for recommendation particularly for low energy, high sensitivity experiments as apparently needed for some of the tests;

[5] Pointing out some of the theoretical aspects that deserve further developments as a pre-requisite for true advances in the tests (such as Mignani's studies on the nonpotential generalization of the potential scattering theory currently used in the data elaboration at National Laboratories).

I have attempted to recommend the setting up of such a Review Panel to Panofski, Lederman, and Vineyard (when director of Brookhaven) for a number of years without any success. Their formal position is that they have no internal rules for the setting up of panels of study (sic!).

The true reason, of course, is that the experimental verification of the special relativity under strong interactions is vigorously (at times histerically) opposed by well known, vested, academic interests.

With the passing of time, however, I believe that the situation is getting more and more serious for evident reasons of scientific accountability vis -a-vis the taxpayer. In fact, we are all spending large amounts of public funds in strong interactions. Most of these funds are spent on the mere belief of the exact validity for the strong interactions of basic physical laws clearly and directly established only for the electromagnetic inte-

Outline of the

THEORETICAL, EXPERIMENTAL, AND MATHEMATICAL STUDIES
CONDUCTED AT

THE INSTITUTE FOR BASIC RESEARCH
Cambridge, Massachusetts

# TOWARD A GENERALIZATION OF GALILEI'S AND EINSTEIN'S RELATIVITIES IN CLASSICAL AND QUANTUM MECHANICS

January 1983

ractions. Scientific accountability demands that we de-emphasize all our personal theoretical views, whether in favor of old laws or in favor of suitable generalizations, and resolve the issue in the only scientifically possible way: via direct experiments. The suppression of such resolution which has been successfully achieved by vested interests until now has grossly aggravated the situation and rendered . the initiation of preliminary collegial studies simply mandatory, in my view.

My problem is to promote such scientific process in a way as smooth as possible, and by avoiding public confrontations as much as possible (without such a committment to an orderly process you would have likely read the issue in national newsmedia). The understanding is that vested interests are capable of moderation.

To succeed in such orderly scientific objective I need council and advice. This is the reason I am contacting you. Some of the points in which any suggestion would be gratefully appreciated (even by phone) are the following.

A. Do you foresee possibilities of an <u>informal</u> and <u>confidential</u> consideration of the possibility of setting up the above Review Panel (or some other alternative) at the forthcoming H.E.P. Advisory Panel meeting in Washington on Feb. 7-8?

B. Do you believe that my presence at such a meeting might be recommendable?

C. Willy Wallenmeyer and Bernie Hildebrand at DOE are fully informed of the studies. In fact, they have been conducted under their support since the initiation at Harvard back in 1978. Nevertheless, I have never disturbed Willy and Bernie for assistance in the initiation of studies at National Laboratories, owing to the mostly human (at times questionably human) aspect of the issue (I thought that they have already sufficient administrative duties and responsibilities to keep our community together in difficult financial times to warrant my additional burden). Nevertheless, do you think that the proposal of a Review Panel or other alternatives should be submitted to Willy and Bernie?

D. Do you know any physicist at National Laboratories sufficiently independent from existing vested interests and with sufficient committment to fundamental issues to consider some help in a collegial approach to the problem?

E. Do you think that a formal presentation of the need for the verification of the special relativity under strong interactions because of manifest need of scientific accountability vis-a-vis our society, is recommendable for submission to the current H.E.P. Review Panel organized by the White House and/or other receptive governmental members and/or committees?

Thanking for your courtesy and time, and hoping to have the pleasure of meeting you in the near future, I remain

Sincerely Yours

Ruggero Santilli

R.M. Santilli

THIS LETTER WAS NOT ACKNOWLEDGED

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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 1, 1983

Professor W.K.H.PANOFSKY
SLAC
Stanford, California

Dear Professor Panofsky,

It would be a pleasure to see you during your forthcoming visit at Harvard next week.

In fact, a friendly meeting (possibly outside Harvard) would be the ideal oppurtunity to exchange ideas on the orderly approach to the problem of the experimental test at national laboratory of the Lorentz symmetry under strong interactions.

In case you are interested in the issue and have the time, simply call me at any time.

Sincerely,

Ruggero Santilli