

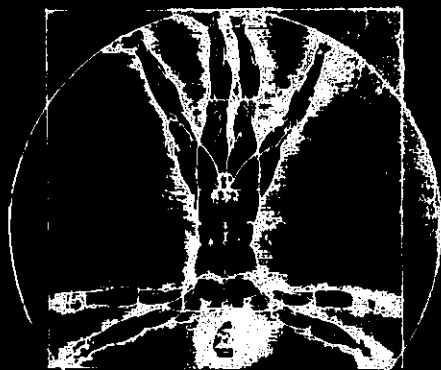
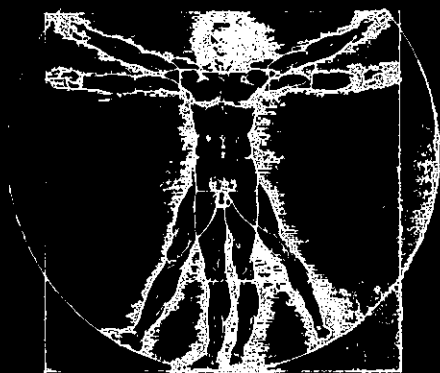
VOLUME

III

DOCUMENTATION

OF

IL GRANDE GRIDO



Ruggero Maria Santilli

DOCUMENTATION
OF
IL GRANDE GRIDO

Volume III

Ruggero Maria Santilli

— 1984 —
Alpha Associates
Rome, Italy

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**DOCUMENTATION
OF
IL GRANDE GRIDO
VOLUME III**

by

Ruggero Maria Santilli

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PART XXI:
REJECTIONS
BY THE
NATIONAL
SCIENCE
FOUNDATION
DURING THE
PERIOD 1972–1978

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

SEP 22 1972

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215

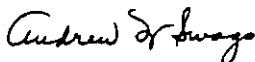
Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigations on a New Analytic Extension of the Scattering Amplitude."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,



Andrew W. Swago
Acting Division Director for
Mathematical and Physical Sciences

Copy to:
Dr. Robert F. Slechta
Associate Dean
Graduate School

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

JUL 16 1975

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215

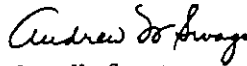
Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigation of Generalized Analytic, Algebraic and Statistical Formulations for Interacting Systems."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,



Andrew W. Swago
Acting Division Director for
Mathematical and Physical Sciences

Copy to:
Mr. Charles W. Smith
Vice President for Finance

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

JUN 28 1976

Dr. Ruggero M. Santilli
Department of Physics
Boston University
Boston, Massachusetts 02215


Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Investigations on the Origin of the Gravitational Field."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other proposals which you might wish to submit.

Sincerely yours,



William E. Wright
Division Director
for Physics

Copy to:
Mr. Henry T. Spiers
Comptroller

October 28, 1976

Dr. Boris J. Kayser
Division Director For Theoretical Physics
National Science Foundation
Washington, D.C. 20550

Dear Dr. Kayser:

I hereby submit for consideration by NSF my research grant proposal entitled "Necessary and sufficient conditions for the existence of a Lagrangian in Newtonian Mechanics and in Field Theories".

I also enclose a list of scientists who have been exposed to my current research interests hoping that it might be of some value in your referee selection.

Finally, I enclose samples of my papers on the subject of the proposal, which will appear in Annals of Physics to indicate the status of my research. These papers were done during my visit at the Center of Theoretical Physics of the Massachusetts Institute of Technology thanks to the kind hospitality by Professor F.E. Low.

Sincerely yours,

Ruggero Maria Santilli
Associate Professor

RMS/cc

cc: Dr. A. Isaacson
Encl.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

DEPARTMENT OF PHYSICS

CAMBRIDGE, MASSACHUSETTS 02139

Center for Theoretical Physics

Dr. Boris Kayser,
Program Director for Theoretical Physics
National Science Foundation
Washington, D.C. 20550

December 22, 1976

Dear Dr. Kayser,

I am contacting you to provide additional materials and informations in relation to my research grant proposal

"Necessary and sufficient conditions for the existence of a
Lagrangian in Newtonian Mechanics and in Field Theories"
NSF No. 7703963

Since the last several years, I have been involved in a long term and laborious study of certain methodological aspects of theoretical physics which I hope to bring in due time up to the level of practical applications, particularly in high energy physics.

In line with my proposal, my studies consist of the following three phases:

1: The Inverse Problem in Newtonian Mechanics.

This problem basically consists of: a) the identification of the necessary and sufficient conditions for the existence of a Lagrangian for the representation of systems with arbitrary Newtonian forces; b) the methods for the construction of a Lagrangian from the given equations of motion; and c) an analysis of the significance of the underlying methodology for other aspects of the theory, e.g. the transformation theory.

I have been deeply involved in writing a monograph on this subject. This project is at a rather advanced stage as a result of several redraftings following the advice of my referees (P. Dedecker, R. Hermann, P. Huddleston, A.C. Hurst, H. Rund, S. Shanmugadhasan, A. Shimony and two of my graduate students). A copy of the currently available version of the manuscript is enclosed for your inspection.

The MIT Press has expressed interest in publishing my manuscript upon its finalization. Due to the poor mastering of the English language I still have, the manuscript needs a severe editorial control. I am pleased to report to you that Dr. Denis Nordstrom, Acting Editor of the Physical Review (after Pasternack departure) has accepted and initiated such editorial control. This project seems therefore proceeding along promising lines.

I called Kayser on March 16, 1977, to visit him at his office during my next trip to Washington. I said to be too busy to receive me.

Dr. B. Kayser

- 2 -

Dec. 22, 1976

On historical grounds, you might be interested to know that, to the best knowledge of several experts contacted by me as well of myself, there is no recent account of this problem in both the mathematical and physical literature. I therefore initiated a detailed and laborious search of the prior state of the art which I conducted in all the science libraries of the Boston area as well as in the Library of Congress, by moving backward in time up to the beginning of the past century. All my findings will be reported in the forthcoming monograph. Basically, I discovered that the problem had been rigorously formulated on fascinating intuitional grounds by H. Helmholtz in 1887 and subsequently virtually solved within the context of the calculus of variations in specialized mathematical journals of the first part of this century. Regrettably, however, since that time the problem had remained virtually ignored.

On pedagogical grounds, the monograph appears to be potentially significant for the intended audience of first or second year graduate students. This is so because the Inverse Problem constitutes one of the best arenas for an in-depth study of the fundamental analytic equations, namely, the Lagrange's and Hamilton's equations. As one referee put it "... after Santilli work, no student will be able to claim a knowledge of the Lagrange and Hamilton equations without a knowledge of the necessary and sufficient conditions for their existence". Additional referee's reports are enclosed for your consideration. For any additional information please feel free to contact Mr. A.B.Evans of The MIT Press.

On technical grounds, the monograph apparently solves one of the central and vexing problems of Newtonian Mechanics. The conventional Lagrangian representations of Newtonian systems are virtually restricted to only conservative systems due to lack of knowledge on how to construct the Lagrangian for the case of more general Newtonian forces. This, however, often represents only a crude approximation of the Newtonian reality. To put it quite candidly, while I was teaching a graduate course in Classical Mechanics according to the conventional patterns I felt like the inventor of the machine for the perpetual motion. As a matter of fact, one of the primary motivation to undertake this laborious task was precisely my uneasiness with currently available methods. By looking in retrospective, I am now satisfied of my efforts. Indeed, I am now in a position to compute the Lagrangian for Newtonian systems as they actually are in the physical reality and not only in their conservative approximation.

In line with my application, I must add that this analysis demands, for completeness, the study of its extension to the case of Newtonian systems with generally non-integrable subsidiary constraints. This is part of my contemplated subsequent research.

Dr. B. Kayser

- 3 -

Dec. 22, 1976

2: The Inverse Problem in Field Theory

My Newtonian studies attracted the attention of the MIT in 1975 where I was then invited to be a visiting scientist since Jan. 5, 1976 thanks to the interest by Professors H. Feshbach and F. E. Low.

During this calendar year here at MIT I have worked on a series of seven papers on the extension of the Inverse Problem to classical relativistic field theories. The first three papers will appear in Annals of physics; papers IV and V are currently under inspection by leading scientists prior to their submission to Annals of Physics; and papers VI and VII are under finalization.

Copies of these papers are enclosed for your consideration. There is a possibility that the MIT Press might be interested to reprint them in due time as a follow up to the first volume on the Newtonian aspect of the problem. If this project will materialize, I intend to dedicate the volume to my teacher and friend Professor Paul Roman. It is in this spirit that they are presented to you as a collection.

The papers are intended for a broader audience, rather than for few experts, in view of their potential technical as well as pedagogical significance. This is reflected in the adopted style of presentation. After all, the Inverse Problem is again the best arena for an in-depth study of the fundamental analytic equations of all our field theoretical models. As the referee of Annals of Physics put it in his official report, "Santilli has performed a real service in reviving beautiful old ideas and extending them to field theories. Such scholarly virtue is rare these days and is very important".

The technical content is here perhaps multifold. First of all there is the intrinsically significant Lagrangian representation of Lorentz covariant systems of field equations with arbitrary couplings. Secondly, as you can see from papers I and II, by matching my field theoretical and Newtonian analyses, the unified gauge theories of weak and electromagnetic interactions emerge with a new light because their Newtonian limit results to be precisely of non-conservative type as it is after all self-evident from the velocity dependence of their couplings. In my opinion this indicates that forces which are not derivable from a potential have a precise physical role at both a Newtonian as well as a field theoretical level. If one searches for further generalizations of the couplings aiming at the inclusion of the strong interactions, then the Inverse Problem emerges with a self-evident potential significance.

Some of the most intriguing implications of the Inverse Problem appear to be within the context of the transformation theory. See in this respect

Dr. B. Kayser

- 4 -

Dec. 22, 1976

paper V. I should indicate in this context that I purposely avoided any elaboration and application of these results in these introductory papers.

3: Applications to High Energy Physics

A part from the few technical points I indicated in my proposal, it would be simply presumptuous for me to ventilate at this time possible results prior to their appearance. This is ultimately the fickle nature of a research proposal where often the author, for professional attitude or unpredictable turns of events, either cannot fully disclose his ultimate objectives or cannot predict, more often, its outcome.

I think however that it is appropriate for me to indicate to you that the ultimate motivation for my undertaking this long term, time consuming and laborious program, is precisely my personal conviction of the possible significance of the Inverse Problem in high energy physics.

In any case, I am now very close to the completion of my "homeworks" 1 and 2 and I will soon dedicate myself entirely to this third phase of my studies.

In closing, I would like to recall to you that I have already applied to the NSF for research support in the past but unfortunately the NSF was not in a position to fund my proposals.

More specifically, I applied for the first time in 1972 with a proposal on the study of the analyticity properties of the scattering amplitude. These studies resulted in my paper in Phys. Rev. D10, 3396 (1974) as well as several others, in which I reached the generalization of the PCT theorem to all discrete space-time symmetries. I understand that this paper, which several colleagues consider rather highly, is currently used in various fields ranging from the discrete symmetry violations to the analyticity properties of the S matrix.

I then applied for the second time in 1974 with two proposals. The first one was related to a feasibility study to ascertain whether with the present technology it is possible to experimentally verify or disprove the central prediction of the Einstein-Maxwell theory according to which any distribution of electromagnetic fields generates a gravitational field. In subsequent correspondence with NSF I stressed the need of support for the continuation of these studies because, unlike my application of 1972, they required the set up of a team of experts in various disciplines. And indeed, various experts had agreed to rather enthusiastically participate. But on June 1976 I received a letter from Mr. W. E. Wright to the effect that the NSF was unable to fund my proposal. Regrettably, I had to abandon this project.

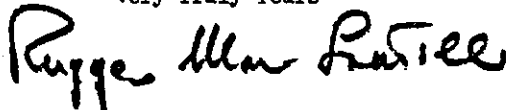
My second proposal of 1974 was closely related to my recently submitted proposal although at that time the presentation was predictably embryonic in nature.

Almost needless to say, I have recalled the above proposals for the sole intent of assisting you in your identification of my previous contacts with NSF.

Thrusting in your understanding, I would like to disclose to you that I am currently considered for a position here at MIT and at Berkeley. I understand that a faculty decision will be reached sometimes in February-March 1977. I would be sincerely grateful to you if any decision can be reached on my application by that time. If this is too early, I would appreciate the courtesy of an indication of the anticipated time of the decision.

Thanking you for your consideration and with my most sincere best wishes for the coming holidays, I remain

Very Truly Yours

A handwritten signature in dark ink, reading "Ruggero Maria Santilli". The signature is written in a cursive, flowing style. The first name "Ruggero" is written in a larger, more prominent script, while "Maria Santilli" follows in a similar but slightly smaller and more compact script.

Ruggero Maria Santilli

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139
Center for Theoretical Physics

Dr. B. Kayser,
Division of Theoretical Physics
NSF
Washington, D.C.

March 14, 1977

Dear Dr. Kayser,

following my phone call of March 7, 1977 and according to your suggestion, I am indicating in a letter the reasons for my request of a meeting to discuss my pending application No. NSF7703963.

By separate parcel post, I have mailed to you copies of my three monographs on the Inverse Problem, MIT-CTP publication Numbers 606, 607 and 608.

I would appreciate whether you can return to me the copies of the previous drafts, because now obsolete, which I mailed to you on December 22, 1976 (although I do not know whether you received them).

In my pending application I indicated the appearance of these monographs. As a matter of fact, the application was for financial support primarily in the writing of these monographs. I would appreciate the courtesy of your mailing a copy of these manuscripts to the referees of my application. Just let me know how many you need and I shall send them to you by return mail.

The reason for such a request is that, understandably, I have exposed myself to the physics community with such an announcement. Your mailing of the copies of my manuscripts would give the opportunity to the referees to inspect my results.

Secondly, I would appreciate your advice as to whether I should rewrite the proposal or leave it as it is. In essence, the basic research aspect of the pending proposal is by now completed, while the part of the proposal related to possible applications of the Inverse Problem to High Energy Physics remains in full effect. I personally prefer leaving the proposal as is, although I considered advisable to bring to your attention this new situation. Ultimately, I shall follow your advice "ad litteram".

I enclose on confidential grounds copy of the report to the CTP here at MIT on my trip to Washington of March 9 through 12. Hopefully, this report should provide you with an indication of the possibilities of the Inverse Problem.

Dr. B. Kayser, page 2 - March 14, 1977

Please let me know whether it would be appropriate for me to submit to your division the program (b), namely, that discussed with NASA, or some other of the contemplated applications of the Inverse Problem.

In closing, I would like to recall a phone conversation with Dr. R. Isaacson of some two years ago in which I stressed my full confidence on your capabilities, indicated my understanding of the difficult situation in which you operate and at the same time I indicated my reservation as to whether the current rules and regulations in which you are forced to operate are actually the best for the best interest of the Country.

In case your division will be involved sometime in the future in any revision of the current rules and regulations aiming at a more democratic dispersal of the available funds, please keep me in mind. You will have my unconditional support.

Sincerely Yours

Ruggero Maria Santilli

c.c. Dr. R. Isaacson

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

DEPARTMENT OF PHYSICS

CAMBRIDGE, MASSACHUSETTS 02139

Center for Theoretical Physics

March 14, 1977

Dr. E. Creutz
Division of Mathematical Science
NSF
Washington, D.C.

Dear Dr. Creutz,

I would like to express my appreciation for your kind reception during my visit at your Division on March 11, 1977.

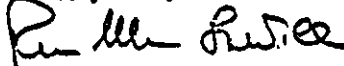
I am enclosing, on fully confidential grounds, copy of the report to the Center here at MIT on my trip to Washington hoping that it might indicate to you the range of applicability of the Inverse Problem.

In particular, I would like to bring to your attention the fact that possible practical utilizations of the Inverse Problem vitally depend on its application to the Optimal Control Theory, which was the topic of my informal presentation. And indeed, both the USAF and NASA have strongly recommended this line of study.

I contemplate to apply to your Division for studies along these lines in the near future. Let me however candidly confess that, unless properly supported, I will simply be unable to conduct the contemplated extension of the Inverse Problem to the Optimal Control Theory.

My applications for federal research support on the Inverse Problem of few years ago were not supported because, according to my best reconstruction, the so-called experts in analytic mechanics had considered the problem to be vacuous. I did the job on a completely unsupported basis resulting on three monographs for some 1500 pages, the third volume of which is a collection of papers appearing in Annals of Physics. This is the result of some five redraftings. The related expenses for typing, xeroxing and mailing to my referees have completely exhausted my personal financial resources. As a result, despite my best intention, unless properly supported, I simply am not in a position to conduct research to any significant dept.

Very Truly Yours



Ruggero Maria Santilli

c.c.: Dr. B. R. Agins

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

DEPARTMENT OF PHYSICS

CAMBRIDGE, MASSACHUSETTS 02139
Center for Theoretical Physics

March 14, 1977

Dr. B. R. AGINS,
Division of Mathematical Sciences
NSF
Washington, D.C.

Dear Dr. Agins,

I simply have no words to express my appreciation and gratitude for your kind reception during my recent visit and for your several suggestions.

As soon as my plans are finalized, I shall take the liberty of contacting you again.

Sincerely Yours

A handwritten signature in dark ink, appearing to read 'Ruggero Maria Santilli'.

Ruggero Maria Santilli

Encl.

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

March 28, 1977

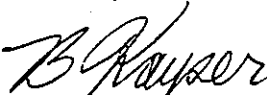
Dr. Ruggero Maria Santilli
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

In reply to your letter of March 14, 1977, I suggest you send us six copies of the material you wish the reviewers to see. I am afraid this material is needed immediately if the reviewers are to see it.

With regard to revising your proposal, I suggest you do not do so, since any revision at this time would make it impossible for the Foundation to consider possible funding before the fall.

Sincerely yours,

A handwritten signature in dark ink, appearing to read "B. Kayser", written in a cursive style.

Boris Kayser
Program Director for
Theoretical Physics

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

DEPARTMENT OF PHYSICS

CAMBRIDGE, MASSACHUSETTS 02139

Center for Theoretical Physics

Dr. B. Kayser, Director
Division of Theoretical Physics
NSF
Washington, D.C.

April 21, 1977

Dear Dr. Kayser,

I enclose for your attention the first four of a series of nine articles entitled "A hadronic model for the nonapplicability of Pauli principle". This series is the result of some twelve years of preparatory studies for a primary objective which I disclose only now. My studies on the Inverse Problem from 1970 until recently were part of this preparatory program. The series of monographs and articles in your possession on this topic were primarily conceived for this new hadronic model. The remaining half of the needed methodology falls in the so-called Lie-admissible problem which I worked out from 1964 until 1969 in a series of articles.

In essence, I construct a new model on the structure of the hadrons by using the old idea that the strong interactions are not derivable from a potential. This idea, however, is subjected to direct analysis rather than the customary approximation in terms of couplings derivable from a potential. You can now see the vital need of the Inverse Problem as a methodological tool.

The results of this series of articles are of grave physical, methodological and emotional nature. When the strong couplings are taken "ad litteram" as not derivable from a potential, they became so powerful to literally destroy our entire knowledge. Fundamental disciplines such as the special theory of relativity, quantum mechanics and quantum field theory simply became nonapplicable within the hadron, even though the analysis confirms their unequivocal validity for the arena in which they have been experimentally tested until now, electromagnetic interaction. In particular, the $SU(3)$ model on the structure of the hadron is invalidated at all levels, from its recent color implementation, to the same central idea of multiplet. In particular, the concept of quark as the elemental constituent of the hadrons becomes vacuous because the strong interactions, under said assumption, imply the nonconservation of the charge, spin and magnetic moment of the constituents even though the total characteristics are of course conserved and quantized according to conventional rules. According to the opinion of all colleagues I have consulted until now, there is simply no way conceivable at this moment that the $SU(3)$ can be even partially salvaged under the assumption that the strong interactions are not derivable from a potential. I should add that the results of my analysis indicate that the $SU(3)$ models do have a clear physical significance, but only when interpreted as describing the chemical external behaviour of the hadrons and as producing their classification of Mendeleev type. However, the moment the same models are

interpreted as characterizing the actual structure of the hadrons they result to be invalidated at all level. It is in essence the same situation which occurred in atomic physics. The Mendeleyev classification has a precise role in the theory. The Bohr model has an equally precise role but profoundly different than the former. The interpretation of the $SU(3)$ models as structure model would be the same as constructing a model on the structure of the hydrogen atom whereby the valence play a dominant role.

I am now deeply involved in completing this series of articles. My central duty is to indicate that the needed generalization of known disciplines to treat forces not derivable from a potential are not only conceivable, but actually possible by using my preparatory methodological studies on the Lie-admissible problem and the inverse problem. After studying this problem for over a decade, I can assure you that the emerging new methodology exhibits a unique beauty, simplicity and physical effectiveness. In much the same way as quantum mechanics was specifically conceived for the atomic structure, this emerging new methodology results to be specifically conceived for the hadronic structure, generalizes the known disciplines according to a physically clear pattern and recover these disciplines under a limit procedure of clear physical significance, the limit of null values of the couplings not derivable from a potential. In the final analysis, this limit appears to characterize the transition from the hadronic to the atomic structure.

Unfortunately, my research program must be truncated by June 1, 1977. The reason is that Boston University, despite the sincere support of my colleagues, is not financially capable of extending my contract without a federal research grant. I have applied to all U.S. Physics Departments with a graduate school during this academic year (without disclosing my research program on the hadronic structure) without one single offer until now. My contract at B.U. expires on June 1, 1977. I have a family of four to feed. I must therefore take a full time job in the industry or leave the U.S.A.

As indicated to you and Dr. Isaacson, I have no faith in the current referee system. I discourage you from submitting my enclosed studies to any expert in current hadronic physics for self-evident reasons: my results may invalidate the very motivation for their grants. As also indicate earlier, my entire faith is in your personal vision, professional qualifications and human integrity.

In your judgement you should also take into account that the potential impact of my studies goes considerable beyond hadronic physics. Again I am not in a position to disclose studies prior to their achievement, but I am sure you realize that my studies may have a profound impact on a problem of central social significance: the controlled fusion.

Please reach either a positive or a negative decision on my grant application no later than the last week of May 1977.

Sincerely

Ruggiero Maria Santilli
Ruggiero Maria Santilli
Visiting scientist

c.c.: Dr. R. Isaacson

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

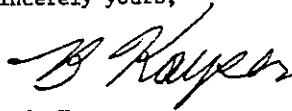
June 21, 1977

Dr. Ruggero Maria Santilli
Visiting Scientist
Center for Theoretical Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Santilli:

Thank you for your letter of June 1 advising us of your current situation. I hope that the Foundation will be able to advise you of the status of your proposal reasonably soon. I also hope that something satisfactory turns up for you in your career plans.

Sincerely yours,

A handwritten signature in dark ink, appearing to read "B Kayser", written in a cursive style.

Boris Kayser
Program Director for
Theoretical Physics

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

JUN 30 1977

Dr. Ruggero Maria Santilli
Department of Physics
Boston University
111 Cummington Street
Boston, Massachusetts 02215

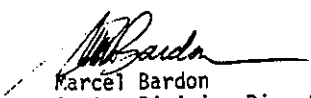
Dear Dr. Santilli:

We regret to inform you that the National Science Foundation is unable to support your proposal for "Necessary and Sufficient Conditions for the Existence of a Lagrangian in Newtonian Mechanics and in Field Theory," PHY77-03963.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,


Marcel Bardon
Acting Division Director
for Physics

Copy to:

Dr. Charles W. Smith, Vice President
Financial and Business Affairs

BORIS KAYSER
Program Director
for Theoretical
Physics

July 21, 1977

Dr. Ruggero Maria Santilli
International Center for
Theoretical Physics
Post Office Box 586
34100 Trieste, ITALY

Dear Dr. Santilli:

In response to your request of July 13 to Dr. Marcel Bardon, I
enclose the attached verbatim reviews of your proposal. I hope
that they will be of use to you in your future plans.

Sincerely yours,

Boris Kayser
Program Director for
Theoretical Physics

Enclosures

FORMAL REFEREE REPORT ON SANTILLI'S
MONOGRAPHS "FOUNDATIONS OF THEORETICAL
MECHANICS", VOLS I, AND II, SPRINGER-
VERLAG, IN PRESS, ACCEPTED AND
RELEASED BY NSF OFFICERS.

I have examined the proposal by Dr. Ruggero M. Santilli PHY7703963 (returned under separate cover). My reaction to it is rather negative. I also thought that Santilli was on the borderline between being a third rate scientist and a crack pot and I do not think that the monumental work can change substantially my opinion. The idea of reading it thoroughly produces in me an incoercible revulsion and if you insist on it I am going to resign as a reviewer. The book is written in a pompous, immodest, self-glorifying style which I detest given also the absolute lack of physical content. In view of this criticism I find the total figure asked for the project quite extraordinary.

OVERALL RATING

- ☐ EXCELLENT
- ☐ VERY GOOD
- ☐ GOOD
- ☐ FAIR
- ☒ POOR

Ruggero Maria Santilli

Foundations of Theoretical Mechanics I:

**The Inverse Problem in
Newtonian Mechanics**

Texts and
Monographs
in Physics

W. Beiglböck
L. H. Lieb
Series Editors



Springer-Verlag
New York Heidelberg Berlin

It is doubtful that this proposal should be given a high priority. The problem that the author has decided to devote so much work on is worthwhile, but I have a strong impression that not much will be accomplished. This impression is based on a careful reading of the proposal and 100 pages of Volume 1 of the voluminous treatise included with it. Here are some specific points that, though not individually of great importance, have led me to think that the author may not be up to the task.

Proposal, p 6. The discussion of $O(2)$ turning into $O(1,1)$. This is probably not so interesting as the author thinks it is - probably just a case of many-to-one mapping. The discussion is naive.

Vol 1, p 33. The definition stinks: "... Lagrangian or the Hamiltonian ...", does he really mean "or"? How can the rest of the definition be applied to the Hamiltonian? Kinetic energy is undefined and so is the concept of a "nontrivial additive interaction term". The footnote introduces additional undefined concepts.

Vol 1, p 46. The third paragraph: Very bad; "arbitrary functional dependence" is meaningless and is not a property.

Vol 1, p 51. That definition of interaction again! What does "nontrivial" mean?

Vol 1, p 57. The discussion on pp. 47 to 56 is very unclear and reaches absurdity in the summary 1.2.3 on p. 56 (spilling over into p. 57). What in the world is the meaning of (3) (top, p. 57). The rest of p. 57 is also unclear.

Vol 1, p 98 Nota Bene. This is an appropriately headlined remark. Here it becomes very clear that the author does not understand the meaning of the problem that he is working on. How can one investigate existence of a Lagrangian when everything is regarded as an approximation? Approximate existence? Certainly it is possible to develop physical ideas without mathematical rigor, but not existence theorems.

OVERALL RATING

- ☐ EXCELLENT
- ☐ VERY GOOD
- ☐ GOOD
- ☐ FAIR
- ☒ POOR

The author proposes essentially a scholarly study on classical mechanics and continuum physics; a main objection being the completion of a series of books. Although he should be encouraged if he wants to pursue this kind of work, I would rate the proposal in the category of research work only as "good", or below, for the following reasons. The foundations of mechanics and field theory is a very old subject and much has already been written about exhaustively. Many applied mathematicians and rational mechanists, civil and mechanical engineers, have developed considerable traditions and a new discipline. The author does not mention for example many people around "Arch. Rational Mechanics & Analysis", the Russian and European schools. It does not seem that the specific problem posed, namely, "the necessary and sufficient conditions for the existence of a Lagrangian", is either new, or exciting, or could lead to major advances in knowledge, or a difficult undertaking even if it were not completely solved.

OVERALL RATING

- ☐ EXCELLENT
- ☐ VERY GOOD
- ☒ GOOD
- ☐ FAIR
- ☐ POOR

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

November 28, 1977

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of September 4, 1977. By now you will have received the verbatim copies of the reviews of your proposal, and you will have seen that they contained strongly negative comments. These reviews resulted in the Foundation's inability to support your proposal. If, considering your reviews, you feel that you would like to appeal the Foundation's decision, you may follow the appeals procedure described in Important Notice #61.

We hope that it will be possible for you to continue with your work even in the absence of NSF help.

Sincerely yours,



Marcel Bardon
Director, Division of Physics

Enclosure
Important Notice #61

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

Mr. MARCEL BARDON, Director,
Division of Physics
National Science Foundations
Washington, D.C. 20550

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138
November 28, 1977

CERTIFIED

Dear Mr. Bardon,

I acknowledge receipt of your letter dated November 28, 1977, received on the same date. I must express a profound dissatisfaction for my many years of totally unrewarding relationship with your division and, in particular, for the following occurrences.

1) My last (of a series of) application No. PHY77-03963 was for the study of the inverse problem in Newtonian Mechanics and field theory. Specifically, the proposal was for the study of : (a) the integrability conditions for the existence of a Lagrangian, or, independently, of a Hamiltonian for the representation of systems of ordinary or partial differential equations with arbitrary couplings, (b) the methods for the computation of these functions from the given equations of motion when their existence is guaranteed by the integrability conditions and (c) the application of the underlying methodology to other aspects of analytic mechanics (such as the transformation theory, symmetry and first integrals of systems with arbitrary couplings) as well as the identification of its significance for applied physics problems, such as nonlinear nonconservative plasma equations, missile trajectory problems and engineering problems (e.g. circuit design inclusive of internal losses) treatable with the optimal control theory. The research was expected to result, as stated in my application of the fall 1976, in three monographs (suggested by the total silence of contemporary theoretical physics on the inverse problem) as well as in a series of papers.

2) On May 1977 I organized a trip to Washington to discuss the intriguing possibilities of the inverse problem with federal agencies. I was cordially received by several governmental agencies (such as ERDA, USAFOSR, as well as another division of NSF). The case with your division of NSF was different. My phone request from MIT for an appointment to present my latest results in relation with the then pending application PHY77-03963 met with Mr. Boris Kayser's answer: "we do not have time to receive all our applicants".

3) At the specific request by Mr. Kayser, I then did a follow up by letters on my way back from Washington according to the letters which should be in your file. This resulted in the official enclosure to my proposal of the three monographs (MIT-CTP publication numbers 606, 607 and 608) which, since the time of my application of one year earlier, were then ready in a preliminary form. Six copies of these monographs (for some 7200 pages all at my personal expenses) were mailed to your office upon formal assurance by Mr. Kayser that they would, in turn, be mailed to the referees selected by NSF for the finalization of their personal opinions on my proposal.

Mr. Bardon, page 2, Nov. 28, 1977

3) Your letter of Nov. 30 communicated to me that my proposal PHY77-03963, as it had been the case for all my preceding proposals beginning from 1972, was unfunded because, as you put it in your recent letter, of "strongly negative comments". The questionable nature of these comments, as well as the NSF responsibility in the selection of their authors, is easily established by the following facts: (A) my studies on the inverse problem for partial differential equations had resulted in a series of articles in Annals of Physics which, in turn, resulted in over 500 requests of preprints from all over the world to the Center of Theoretical Physics of the Massachusetts Institute of Technology (most of which in my possession and none of which evaded due to lack of funds). Most impressive was in particular the differentiated nature of the unsolicited intended applications. (B) My three monographs were subsequently accepted for publication by one of the most selective publishers, Springer-Verlag of Heidelberg(WG), in their series "Monographs in Physics", under the title "Foundation of Theoretical Physics". This was the results of enthusiastic referees reports, as officially acknowledged by Springer-Verlag, on the novelty and significance of my studies for theoretical and applied physics by numerous experts in Europe, USSR and USA. (C) The significance of my studies is such to have motivated the preparation of independent previews of the contents of my monographs by other authors which will eventually appear in the specialized press for broad physical audience. The NSF responsibility in the selected referees can be best expressed with a comment I was told this summer during my trip of invited lectures in Europe: "the fact that you, with your scientific achievements, are unsupported is a disgrace for the USA".

4) Jointly with the finalization of my studies on the methodology of the inverse problem I also worked on what is, in my opinion, its most significant applications: the study of the old idea (e.g., Enrico Fermi) that strong interactions are due to local couplings not derivable from a potential with particular reference to the problem of the hadronic structure. A tentative and highly confidential (at that time) series of papers in their first version was rushed to your division as an informal collateral element of my application PHY77-03963. The most visible implication is the need of subjecting the validity of established relativity and quantum mechanical laws within a hadron to an experimental verification (rather than the tacit acceptance of currently supported research). This disclosure, in my opinion, provides additional indications on the questionable nature of the reports by the NSF selected referees, as indicated by the following facts: (A) after predictable numerous revisions, my studies have been approved for publication by a US publisher as a monograph under the title "Lie-admissible approach to the hadronic structure". I believe that the referees reports (in my possession) are the clearest illustration of the highly questionable selection of referees by your division. (B) I have written a series of summary papers which will appear in print, again, with additional referees backing totally contrary to your statement of "strongly negative comments". And, last but not least, (C) I have delivered a series of invited lectures and collected a number of written opinions by leading scientists on the need of conducting my line of studies on the problem of the hadronic structure (jointly with the currently supported trends) to render any different opinion solely motivated by financial interests of established groups of scientific power.

Mr. Bardon, page 3
Nov. 28, 1977

5) My request of July 10, 1977 made directly to you to disclose copies of the negative referee reports had not been honored, contrary to your statement in your letter of November 28 and contrary to the current rules and regulations of NSF. And indeed I simply have not received these reports at this or at any of my previous addresses.

6) My request of disclosure of the rules to file an appeal has clearly not been honored in time. And indeed, following my written request of July 11, 1977, it took you the months of July, August, September, October and November to answer with your latest letter finally disclosing the "Important Notice No. 61". The point is that, as this notice clearly states, your disclosure occurred after the deadline of 180 days to be counted from your letter of June 30, 1977 of lack of support. According to the opinion of all contacted people, this has also been in violation of the NSF rules.

7) My request to reconsider application PHY77-03963 for a reduced amount of \$20K to \$25K to be granted to me as an individual, has again not been honored and it is ignored by your letter of November 28, 1977.

I am under the impression that you do not realize the fact that all these years of completely unsuccessful research grant applications to your division have resulted in an enormous moral, scientific and academic damage to me up to the point that I cannot any more take them lightly. In particular, this lack of research support resulted in the impossibility by Boston University of considering me for tenure at the seventh year of my service.

I am also under the impression that you do not realize the extreme unrest in the U.S. community of basic studies toward the current criteria of dispersal of federal funds by your division. To have an idea I suggest you to secure copy of the recently circulated report

R. Hermann, "ELEMENTARY PARTICLE PHYSICS: THE SCIENTIFIC FRAUD OF THE CENTURY"

Finally, I am also under the impression that you do not realize the legal implications of the occurrences 1) through 6). I therefore suggest you to consult an NSF attorney, e.g., on the legal implications whether a fully tenured, fully salaried and fully supported (by NSF) physicist conducts active research in any of the several applications of my methods, while their initiator, despite a fully documented application (inclusive of monographs for 1200 pages) has been unable to receive any support whatsoever.

There is still a residual possibility that I can initiate a scientifically productive association with your division, but, for several reasons which is inappropriate here to disclose, the time for you to reach a positive decision is very small.

Through my several years of applications you have all the necessarily elements of decision and, therefore, I do not consider necessary any further mailing of material, such as the monographs and papers of my studies on the hadronic structure.

Mr. Bardon, page 4, Nov. 28, 1977

In the event that your division is positively inclined toward the support of my studies, I would recommend you to consult additional referees, such as

[REDACTED] y,
[REDACTED]
3 [REDACTED] ITT,
[REDACTED],
[REDACTED]

[REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]

These scientists are aware of my studies and there is no need for you of mailing material. The selection of additional scientists only motivated by a genuine interest in basic studies is left to your capabilities.

I must strongly recommend you not to suggest the resubmission of another proposal because, quite frankly, I would consider it offensive.

The only possibility which I foresee for NSF supporting my research is to honor my request of July 11, 1977, namely, to reconsider my application PHY77-03963 under the reduced budget of \$ 20K to \$ 25K to be granted to me as an individual.

In case of lack of action on this request, you should not expect any further communication on my behalf.

Very Truly Yours

Rugges Max Lucile

Ruggero Maria Santilli
Honorary Research Fellow Without Stipend

P.S. In the extremely remote possibility that you are truly serious in supporting my proposal (which, in the opinion of many, clearly surpasses by far most of the other proposals you have jointly considered with mine and, unlike mine, funded) you should keep into account that I am not in a position to accept support unless it initiates from December 1977 or, at the very latest, January 1978.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

December 22, 1977

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

CERTIFIED MAIL

Dear Dr. Santilli:

It is unfortunate that you did not receive copies of the reviewers' comments. They were sent to you on July 21. Another set and a copy of Dr. Kayser's letter of July 21 are enclosed.

In view of the regrettable delays induced by this and other problems, the NSF is taking the position that the deadline discussed in the appeals procedure will be counted from my November 28, 1977 letter, and not from June 30. You therefore have plenty of time to appeal the declination of your proposal if that is what you wish to do.

Your request for reconsideration of application PHY77-03963 for a reduced amount can not be accepted since that application was already declined. You may, if you wish, submit a revised proposal at the lower level, but it would be best also to take into account the reviewers' comments. I am not suggesting the resubmission of another proposal. You have made clear you would find this offensive.

It is indeed regrettable that you have had several years of unrewarding efforts in attempting to obtain NSF funding. We are very limited in what we are able to support. Many worthwhile projects are in the same situation as yours. We and our review process are surely not perfect, and we must constantly be alert to possible errors, but I must conclude that your proposal has been appropriately reviewed and fairly handled. Of course, the appeals procedure is available to you if you find that appropriate.

Sincerely yours,


Marcel Bardon
Director, Division of Physics

Enclosures

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HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138
December 29, 1977

Mr. M. BARDON,
Director, Division of Physics
National Science Foundation
Washington, D.C. 20550

CERTIFIED MAIL

Dear Mr. Bardon,

I acknowledge receipt of your letter of December 22, 1977 and (finally!) of the copies of the verbatim reports by the NSF referees on my proposal.

Upon inspection of these reports I hereby formally request the reconsideration of my proposal PHY77-03963 according to section 5 of the NSF Notice No. 61, January 27, 1976. In accordance with the same section, I expect that "within 30 days following the date of the request, the Assistance Director (or other official designated by the Assistant Director) shall furnish to the PI in writing the results of the reconsideration."

The reasons for requesting this reconsideration are the following.

- The referees have been erroneously selected. In my opinion, their reports clearly indicate that none of them is an expert in the Inverse Problem of the Calculus of Variations (the central topic of my proposal). Therefore, none of them was in a position to objectively evaluate the technical aspects of my proposal, as well as the physical and mathematical relevance of my research.
- NSF should have returned these reports to their authors because of lack of technical qualifications. For instance, the second referee (in the order of your mailing) states "How can one investigate existence of a Lagrangian when everything is regarded as an approximation?" The fact is that the word "approximation" is absent throughout the entire content of my three volumes on the Inverse Problem (MIT-CTP Nos. 606, 607 and 608). The analysis is devoted to the integrability conditions for the existence of a Lagrangian (or, independently, of a Hamiltonian) within the context of the calculus of differential forms and the converse of the Poincaré lemma in particular. The mere mention of the word "approximation" in relation to these techniques indicates the complete lack of technical qualifications or the pursuit of nonscientific objectives through a referee process. The technical content of the first report is simply entirely absent. The third report merely expresses some vague personal feelings which are completely unsubstantiated. For instance, after having spent some three years of laborious library search, having consulted virtually all experts on the Inverse Problem in Europe, USSR and (the few) in USA, and after having visited and lectured at several of the best institutions in analytic mechanics, this third referee has the courage to state "the author does not mention for example many people around "Arch. "ational Mechanics & Analysis", the Russian and European schools."

Mr. Bardon, NSF, December 29, 1977, page 2.

- You and Mr. Kayser should have rejected these reports on etical grounds. An incontrovertible aspect of these reports is their language. Such a language is justifiable, say, for a frustrated mine worker. For a referee procedure involving the delicate financial issue of the allocation of tax payers money for research programs, languages of the type of the reports you have accepted and mailed to me can only have a dubious interpretation. It is common practice among reputable journals to return to their authors either nontechnical reviews of technical material or reviews containing questionable language. For a referee procedure involving the indicated delicate financial issues, the rejection of reviews of the type you have mailed to me should be simply mandatory.

To give you an idea of the difference between the reports of the referees selected and accepted by NSF and independent scientists, I enclosed a number of reviews on my studies. Additional reviews, perhaps more enthusiastic, have not been included. The material which I want to be included in the reconsideration is the following.

- (A) My proposal PHY77-03963 as is. It is by now largely obsolete because most of the indicated research objectives have already been achieved. Nevertheless, it is my opinion that the proposal, as is, is sufficient for a review process which is qualified on both technical as well as ethical grounds. The objection by one referee that its language is naive simply reminds me of the objection by a physicist on Yang-Mills paper soon after its appearance that the presentation was naive.
- (B) My three monographs on the Inverse Problem in Newtonian Mechanics and Field Theory (MIT-CTP publ. nos. 606, 607 and 608), because they were officially attached to my proposal PHY77-03963. These monographs are totally obsolete at this time. In essence, they were a draft rushed to your division to provide more evaluational material. I do not intent to release the new versions which have been accepted for publication by one of the most selective publishers, Springer-Verlag of Heidelberg, WG. The reason is that I have found simply preposterous the pretention by one of your referees that these manuscripts should be perfect. If these manuscripts had reached full maturity, I simply do not see the reasons why to apply for a research grant. I am here formally asking that the subsequent highly tentative series of papers on the application of my studies for the construction of a new model of the hadronic structure (which I mailed to your division in April 1977) should not be included in the reconsideration because they were intended to be a confidential disclosure. In any case, after many redraftings, implementations and expansions, these papers have resulted in a series of preprints of the Lyman Laboratory of Physics of Harvard University and in a series of monographs which have been accepted for publication by Hadronic Press, Inc., as you can see from the enclosed reviews. I am, however, formally asking that the reconsideration

- 783 -
Mr. Bardon, NSF, December 29, 1977, page 3.

must take into account the central reason why I entered into such a laborious research program on the Inverse Problem: these new techniques are centered on the study of systems with couplings not derivable from a potential; as such, they are significant for the study of the old idea that the strong hadronic forces are precisely of this type. It is my opinion that this remark alone is sufficient to complement the material of my MIT-CTP monographs and of my proposal. I do not see the necessity of mailing to you my Harvard preprints and my monographs on the study of this physical application. In any case, my totally unsupported studies have by now resulted in over 5,000 research pages. I simply do not see how your division can effectively and objectively review all this material in 30 days.

(C) This letter and its enclosure of independent reviewers.

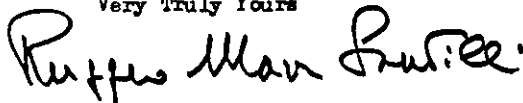
If I can be of any assistance in the reconsideration, please do not hesitate to contact me. I am formally asking that any additional review by NSF referees be promptly mailed to me.

In closing, I must stress my complete disagreement with your statement, in the letter of December 22, 1977, that

"Your proposal has been appropriately reviewed and fairly handled".

You should be also informed of my intent of disclosing our correspondence, at any time I consider appropriate, to a number of observers currently monitoring the operations of the Theoretical Physics Division of the U.S. National Science Foundations.

Very Truly Yours



Ruggero Maria Santilli

encl.: Verbatim review by independent
scientists.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

January 9, 1978

Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

I have your letter of December 29, 1977, in which you request reconsideration of your proposal, PHY77-03963. You will note from NSF Important Notice #61 that such a request must be addressed to the appropriate Assistant Director. Accordingly I have forwarded your letter to Dr. James Krumhansl for his consideration.

Sincerely yours,


Marcel Bardon
Director, Division of Physics

MEMORANDUM

DATE: January 9, 1978

To : Dr. J. Krumhansl, Assistant Director, MPE
Via : Dr. Ronald E. Kagarise, Deputy Assistant Director, MPE
From : Director, Division of Physics
Subject: Correspondence from Ruggero Maria Santilli

I have received the attached letter from R. Santilli, requesting reconsideration of his proposal to the National Science Foundation, which was declined by the Physics Division. I am forwarding it to you for appropriate action. Also attached is the file for his proposal, PHY77-03963.

Marcel Bardon

Attachments

Copy to:
Dr. Ruggero Maria Santilli
Department of Physics
Harvard University
Cambridge, Massachusetts 02138

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HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138
January 31, 1978

Dr. Wayne GRUNER,
National Science Foundation
Room 305
1800 G Street N.W.
Washington, D.C. 20550

Dear Dr. Gruner,

I would like to express my appreciation for the courtesy of your phone call of this afternoon and take the liberty of providing you with some additional information in relation to the reconsideration of my grant application PHY77-03963.

As you know from my application of reconsideration mailed to the National Science Foundation on December 29, 1977, the material which I have asked to be reconsidered is the application itself, plus the preliminary versions of my three monographs on the Inverse Problem, MIT-CTP publication numbers 606, 607 and 608. I have also asked that my letter of December 29 and its enclosures be considered as part of the proceedings.

I am fully aware of the difficulties of your job, essentially due to the limited period of time allowed by NSF Important Notice No. 61 of Dec. 27, 1976 for the reconsideration proceedings as well as the fact that true experts on the methodology of the Inverse Problem are extremely few and known also to researchers in the fields. The following suggestions are provided on grounds of my desire to assist you, but they are left to your discretion. More specifically, I am not expecting nor requiring that you should take into account the following content of this letter.

Current status of the material under reconsideration. As stressed in my letter of application for reconsideration, the complete material is by now obsolete. In particular, the central part of the application, the monographs on the Inverse Problem, has been subjected to a profound revision which resulted from: (a) a number of invited talks in U.S. and European institutions (the list is at your disposal), (b) critical comments by several colleagues in USA, Europe and USSR and (c) an informal seminar course which I have delivered here at Harvard during the past term to a group of graduate students and researchers of the Boston Area (see the enclosed outline of the course). Nevertheless, it is my opinion that the material of the grant application PHY77-03963 should be considered as available at the time of the decision. As a result, I do not intend to disclose improved versions of the material. To be specific, I consider absurd the idea that, for a grant application, the research topic should be already worked out to utmost maturity.

Applications of the methodology of the Inverse Problem. The methodology which is the subject of my application PHY77-03963 deals with the integrability conditions for arbitrary systems of ordinary and partial differential equations to admit an analytic representation in terms of conventional Lagrange's and Hamilton's equations. Owing to the elemental nature of these equations in physics, engineering and mathematics, the methodology under consideration is expected to have a number of diversified and significant applications to both, conservative and nonconservative systems, such as, nonlinear nonconservative plasma equations, electric circuits inclusive of internal losses, trajectory problems of missile motion in atmosphere, ecc. In essence, the knowledge of a Lagrangian or a Hamiltonian renders applicable established, rigorous analytic techniques for systems which are today often treated with semiempirical approximation techniques. Also, the methods of the Inverse Problem appear to be computerizable with selfevident possible significance for, say, the in board solution of trajectory problems or optimal flight paths. Informal conversations with NASA and USAFOSR officials have confirmed the possibility of both civilian and military applications.

These possibilities are, of course, indicated in my monographs. However, they are diluted in some 1500 research pages. As a result, I considered advisable to prepare for NSF a very brief outline of these possibilities. A copy of this document, mailed to NSF sometime in March 1977 (if my recollection is correct) is enclosed for your consideration.

Again, this document is by now obsolete. The reason is that a number of applied physicists, engineers and applied mathematicians are apparently working on some of the indicated aspects and I could therefore provide much more specific data. I do not intend to disclose the names of these colleagues. The reason is that some of them are apparently with NSF support, that is, they are working with NSF grants on the methodology which I have laboriously identified and for which NSF has refused support year after year, year after year.

The indicated applications of the methodology of the Inverse Problem are those which I consider of transparent nature. In addition they are in areas outside my current research interest. In essence, my intervention is that of assistance to colleagues, when needed, in the proper use of the methodology under consideration.

The application of the methodology in which I am currently interested is of nontransparent nature. It concerns what I consider the truly fundamental problem of contemporary experimental and theoretical high energy physics: the validity or invalidity for the hadronic constituents of those relativity and quantum mechanical laws which have proved to be so effective for the atomic (as well as nuclear) constituents.

I enclose an outline of three forthcoming monographs I am currently finalizing on this topic (after many years of laborious and solitary studies) which will be published by Hadronic Press. The manuscripts are available (as well as my Harvard papers summarizing their content). Nevertheless, I do not intend to disclose them for the proceedings of reconsideration of my grant. The reason is that this part of my research proposal was only indicated, but not formally

accluded to the proposal.

In line with my application of reconsideration, I am simply asking that in your proceedings you also take into account the fact that the methods of the Inverse Problems are conceived for systems with forces not derivable from a potential, and that this is precisely the old idea that strong interactions are of this type. In different terms, the methods appear to be of some significance for the vexing problem of the nature of the strong interactions which, according to a mounting evidence, do not appear to be treatable with the simplistic idea of a potential function (that is the same as the electromagnetic interactions).

Qualified referees. As indicated earlier, I am fully aware of the difficulties of identifying "experts" in a discipline which has remained virtually ignored in both specialized mathematical and physical literature for over half a century.

At your discretion, please feel free to directly contact the following persons.

[REDACTED] REPORT
[REDACTED] Editor in Chief, *Annals of Physics* [REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]

As you know, my monographs on the Inverse Problem have been accepted for publication by Springer-Verlag (the contract was signed by both parties through the respective attorneys in December 1977). It is significant that the acceptance was based on the obsolete copies in your possession. Of course, the improved copies are also in [REDACTED] possession. Predictably, Springer-Verlag has consulted a rather significant number of professional referees solely interested in the pursuit of knowledge, rather than entangled in the financial machinery of NSF grant allocation. I am confident that, if asked, Springer-Verlag will release the file of their reports or a summary of them. Please feel free to contact either [REDACTED] in Heidelberg or [REDACTED] in New York.

[REDACTED]
[REDACTED]
In [REDACTED]
Re [REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]

The Instituut voor Theoretische Mechanica is one of the oldest and most prestigious institutes entirely devoted to Theoretical Mechanics. I had the honor of receiving invaluable assistance from several of its member over an extended period of time. In particular, this summer I had the opportunity of presenting an invited talk, with several days of detailed discussions with various experts in some of the aspects of the Inverse Problem. The Director of the Institute, Professor [REDACTED] is fully aware of my laborious search for maturity and I

am confident that, if asked, he will provide you with his independent assessment. For a formal statement on my study by Professor Mertens released for the press, see the enclosed brochure by the Hadronic Press. Notice that, to my knowledge, Springer-Verlag has not contacted Professor Mertens and his associates. As a result, this is an additional independent source of evaluation. Also, one of Professor [redacted] associates, [redacted] will likely spend next year at Harvard with me to work on certain methodological aspects of the program (see the enclosed copy of formal application).

[redacted]
[redacted]
[redacted]
[redacted]
[redacted]

[redacted]
[redacted]
[redacted]
[redacted]
[redacted]

I have been associated to Professors [redacted] and [redacted] for several years when I was at [redacted] and our scientific contacts have persisted after the termination of my appointment with this university. I am sure that they will cooperate with you in any form you desire. Notice that they have already released a formal statement on my studies for press distribution. See also the enclosed brochure by the Hadronic Press. Professor [redacted] is currently supported by NSF.

[redacted]
[redacted]
[redacted]
[redacted]
[redacted]

The [redacted] has also been interested in the publication of my monographs. I decided to publish them with Springer-Verlag for a number of reasons of nontechnical nature. It is appropriate here to acknowledge that the assistance I have received by The [redacted] for the finalization of the manuscripts has been invaluable (this was during my stay at MIT in 1976-1977). The reason is quite simple: the [redacted] again, had selected highly qualified, professional referees for my earlier versions. They did a detailed technical review of the analysis and they provided a number of criticisms on several technical aspects which have simply been invaluable for my efforts. I am sure that [redacted] will provide you with his referee file or with a summary of the reports. If you contact [redacted] please inform him of the existence of revised versions of my manuscripts (which I did not release to The [redacted] and indicate the expression of appreciation I have for the role of his referees in my achieving these improved versions.

[redacted]
[redacted]
[redacted]

The Hadronic Press is the publisher of my additional monographs on the hadronic structure. Prior to committing a rather sizable portion of the company's resources

in my research monographs, the company, of course, entered into a laborious referee process. The speculative nature of my studies called for a particular effort which resulted in the submission of the manuscripts to professional, genuine scientists in more than one continent. The results of this review have been beyond my best expectation. A formal statement released by the president of the [REDACTED] in enclosed in the separate summary of statement. I prohibited the printing of this statement in the formal brochure for press and promotional distribution because excessively positive. In any case, I am sure that the president of Hadronic Press, Inc. will be fully cooperative with you for the disclosure of the referee reports or for a summary.

You should be aware that the above persons constitute only a minor part of possible sources of qualified informations on my research. Several additional sources are at your disposal with the exclusion of physicists currently working with NSF support on the applications of those methods which have seen NSF refusal of support year after year, year after year, for their originator. You should be also aware that I have in my possession copies of all the verbatim referee reports originating from the indicated sources. Finally, I should confirm what I verbally indicated to you: by no mean I claim that I have achieved maturity in my studies. I am simply working on my laborious search for the best I can personally do. The achieving of maturity on the methodology of the Inverse Problem will likely take more than one generation, owing to implications in several disciplines, such as Nonconservative Mechanics, Nonlinear Mechanics, Optimal Control Theory, Differential Geometry, Functional Analysis, Field Theory, Continuum Mechanics, not to mention quantum mechanical and quantum field theoretical aspects.

Legal implications of NSF refereeing. I intend, of course, to avoid the expression of my personal opinion of the verbatim referee reports I have received on my grant application from NSF officials. The reason is that the best place to achieve a valuable judgment of these referees, their reports and their responsibilities, as well as the NSF responsibility in their selection and in their acceptance, is in court. You should be aware that, according to my attorneys, these reports are such to warrant a law suit on a number of independent counts and to more than one individual. The sole reason way this law suit has not been filed until now is to pay an undisclosed and tacit form of appreciation for the hospitality I am currently receiving from Harvard University. To be specific, I have not filed a law suit while being a guest at Harvard University because it is contrary to my ethical code. But, my visit here at Harvard will soon be completed and then I will be free to act according to what is, in my opinion, the best interest of this Country, as well as mine. I only hope that, in the meantime, the responsible authorities will give evidence of implementing rules for the dispersal of tax payer's money for research grants in theoretical physics in a form which is genuinely effective and, thus, truly in the interest of the Country, rather than isolated groups of scientific power. Quite frankly, I do not believe that this Country will prosper (or even survive as is) on a long range basis without the seeds of a well balanced basic research.

c.c.: [REDACTED]
[REDACTED]
[REDACTED]

Encl.

Very Truly Yours
Ruggero Maria Santilli
Ruggero Maria Santilli
Honorary Research Fellow Without Stipend

— 791 —
HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

March 7, 1978

Dr. W. GRUNER,
Special Assistant to the Director
Division of Applied Mathematics
1800 G Street
National Science Foundation
Washington, D.C. 20550

Dear Dr. Gruner,

I am contacting you to ask for the courtesy of a suspension of the reconsideration of my research grant application to NSF of 1976.

Following my application for reconsideration, I have submitted a research grant proposal to the Department of Energy with Professor Shlomo Sternberg as Principal Investigator.

Lately, we have received communication that the Division of high energy physics of the Department of Energy has approved the proposal and recommended it to the DE administration for funding. It is my understanding that a research contract is in the process of being executed between Harvard University and DE.

It is my decision to formally ask for the waiving of the process of reconsideration as soon as such a contract is executed and I hope to be able to contact you soon in this respect.

In the meantime, I would like to express the sentiments of my sincere appreciation for the genuine interest you have indicated for my case.

Very Truly Yours

Ruggiero Maria Santilli
Honorary Research Fellow

c.c.: Professor S. Sternberg

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

nsf

April 10, 1978

OFFICE OF THE
ASSISTANT DIRECTOR
FOR MATHEMATICAL AND
PHYSICAL SCIENCES
AND ENGINEERING

Dr. Ruggero M. Santilli
Harvard University
Lyman Laboratory of Physics
Cambridge, Massachusetts 02138


Dear Dr. Santilli:

This is in response to your letters of March 6 and March 7. Let me thank you first of all for your courtesy in notifying us promptly of these developments.

Second, let me express my pleasure and the pleasure of the Foundation upon learning that you have a good prospect of receiving support from the Department of Energy.

Finally, let me note that, according to your request, we intend to take no further action concerning your proposal No. PHY77-03963 unless requested in writing by you to do so. Once again let me thank you for your courtesy in notifying us of the state of your negotiations with the Department of Energy.

Very sincerely,


Wayne R. Gruner
Special Assistant to the
Assistant Director

cc:
Dr. J.A. Krumhansl AD/MPE
Dr. R.E. Kagarise DAD/MPE
Dr. M. Bardon DD/Physics
Dr. Boris Kayser PD/Physics

Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Ma. 02138

July 20, 1978

Dr. JAMES A. KRUMHANSL, Assistant Director for
the Mathematical and Physical sciences and Engineering
National Science Foundations
WASHINGTON, D.C.

Dear Dr. Krumhansl,

I would like to express my congratulations for your new post and my support for the active campaign you have initiated. I also would like to take the liberty of presenting my view on the current situation of basic, theoretical research in high energy physics. Permit me the use of a candid, nonacademic language. The situation is so grave, that the identification of the current problems in a way as clear as possible can only be beneficial. I am confident in your mature and receptive attitude.

You will probably recall me because I have been at the very edge of filing law suits against Mr. BARDON and Mr. KAYSER, both as individuals and as NSF officers, on a number of counts. One of these counts was the fact that these officers had accepted and released a referee report of clearly offensive language on my technical manuscripts which had been accepted for publication by one of the most prestigious publishers (Springer-Verlag) in one of the most prestigious series of research monographs in physics. For your convenience, I enclose copy of the front page of my first volume and of one of the NSF referee reports.

These laws suits were not filed to pay an undisclosed and tacit form of respect for the hospitality that Harvard University was providing me. As I put it in my correspondence with Mr. CARTER, the filing of these laws suits while being a guest at Harvard was contrary to my ethical code.

Subsequently, I became recipient of a research grant from the Department of Energy. I therefore instructed my Boston based and Washington based attorneys to delay indefinitely the filing of these laws suits. I also took all the necessary precautions to prohibit these attorneys from releasing the material they had collected.

I would like to stress that I do not have and never had any animosity against the indicated NSF officers. My contemplated, and intended to be, highly publicized laws suits were solely intended to draw national attention on the grave situation (in my perhaps erroneous view) of funding, promotion and support at NSF for creativity in basic research. I am never tired of repeating that this is a technologically oriented Country with a cloudy future. Such a long range future vitally depends on the capabilities of the Governmental Agencies of implementing now the seeds of a well balanced community of basic studies, in the genuine pursuit of knowledge.

I have dedicated my life to basic research. Unlike other colleagues, I have always put the pursuit of academic power subordinate to that of the pursuit of knowledge. I feel no shame in disclosing to you, as an indication of my determination, that I have put this attitude in practice to the point of being unemployed for a considerable period of time with a family of four and my wife at the graduate school.

Permit me to confess that I had lost all hopes that an improvement of the operations at the division of basic research of NSF could be achieved without grave gestures, such as laws suits, Senate Hearings, etc. This was simply due to the fact that my gentle initial attempts had met with the customary academic tool for unwanted lines: complete ignorance.

This letter is motivated by my hope that, perhaps, the objective considered can be achieved in an orderly fashion, without the grave gestures indicated. The remarks below are presented in this spirit.

I enclose a courtesy copy of a letter I wrote to Professors PANOFSKY (SLAC), WILSON (FERMILAB) and VENEYARD (BROOKHAVEN). As you can see, it is a passionate appeal that we simply cannot continue on the current basis of complete monopolistic control of basic research by the quark conjecture. It is time to implement a well balanced condition and conduction of research in which efforts along the quark models are indeed continued. But jointly we implement fundamentally different approaches to hadron structure for a comparative confrontation of physical reality.

My first appeal to you is that NSF initiates the support, even in a minimal fraction of available funds, of studies on the hadron structure which are strictly quark-non-oriented. I believe that it is virtually impossible to achieve the much needed well balanced conduction of research on the fundamental problem of contemporary physics without a well balanced policy of research grants by Governmental Agencies.

To be quite frank, I believe that an increase of funds available to NSF will be entirely ineffective, unless such a revision of policy is implemented. Indeed, if these additional funds are dispersed according to current criteria, they will result in nothing more than a further proliferation of minute incremental contributions which only in the most optimistic circumstances can hope for a future status of scientific curiosity for curious historians.

My second appeal to you is that a mere formulation of policy to achieve a well balanced conduction of research in hadron physics will be entirely ineffective, unless a profound revision of the current operations of the NSF division of basic studies is implemented.

This is the true problem. NSF operates in a complete symbiosis with academicians currently in control of the scientific power. All these fellows are academically and financially committed and dependent on the quark conjecture, as you can easily identify in all grants issued by NSF during the last decade specifically devoted to the study of hadron structure. I simply have no faith whatsoever that one of these high standing academicians, financially committed to quarks, will release a positive referee report on a proposal which is strictly quark-non-oriented.

You should not be surprised at this statement, nor you should read in it my intention of accusing my fellow researchers of scientific corruption. As a matter of fact, I believe that they are convinced of being in the right track. They are simply not conscious that their action is, in my view, strictly antiscientific. Hadron physics is not a science, that is, the manifestation of an experimentally established truth. Instead, it is the mere expression of mere opinions by groups of researchers, such as the opinion that the quarks are the physical constituents of hadrons, complemented by the opinion that they confine, etc. etc. In filing their negative reports on a quark-non-oriented proposal, these referees simply express another mere opinion that it is not the right way to go.

I have recalled my case, and the fact that it brought me so close to filing quite delicate laws suits, because we can learn by analysing it. I have been told that the referees of my proposal were "truly outstanding physicists", that is, in my candid language, physicists currently in control of the scientific power with a vital dependence on the quark conjecture for the preservation of such power. The net result is that their high academic standing did not prevent them from filing not only a negative report but one in the language which could be only justified for a frustrate mine worker. The truth of the matter is that my proposal was inspired as strictly against quark conjectures. This inspiration created such negative reaction to render these "truly outstanding physicists" blind on the physical relevance of the methods I had laboriously worked out in years of solitary and completely unsupported work, as presented in three research monographs on the so-called Inverse Problem of mechanics (MIT-CTP publications 606, 607 and 608) formally reviewed. For your informations, these methods are now applied by numerous physicists and mathematicians in differentiated problems such as circuit design inclusive of internal losses via the optimal control theory, nonlinear nonconservative plasma equations missile trajectory problems and high energy physics.

The fact remains that, in my view, NSF had funded during the last decade on hadron structure only personal opinions on an ever increasing plurality of different unknown quarks. We simply cannot continue indefinitely along these funding lines.

My specific proposals for an improvements of the operations of the basic research division of NSF and, consequently, of the dispersal of available funds are the following.

I - FORMULATION OF THE ETHICAL CODE FOR NSF REFEREES. In January 1978 I received the pleasant duty to organize a new journal in hadron physics (the HADRONIC JOURNAL). This journal is now acquiring momentum and I am very pleased of its initial results (see the enclosed letters to Professors Panofsky, Wilson and Vineyard). My very first gesture after acquiring the post of Editor in Chief was the setting of the ethical code for referees which I enclose with each request of review. It essentially states that, even in case the submitted paper is completely nonsentical, offensive language in the report must be categorically avoided. It then enters into the request that the referee conducts a selfcritical examination of the physical laws and knowledge he uses in reaching his conclusions, to the effect of ascertaining whether they are experimentally established, or merely believed to be true.

This ethical code was conceived and implemented for an editorial process without any financial aspect. I am sure you will agree with me that it becomes mandatory when delicate money aspects are involved, such as the NSF funding or lack of funding of proposals.

I have no words to stress the need that you implement such ethical code in the form you consider most appropriate. It is simply imperative, in the NSF interest, that all necessary precautions are taken to the effect that referees reports such as those I have received be categorically excluded by NSF operations. The risk for the lack of implementation of this request is clear: laws suits and Senate Hearings for alleged scientific corruption.

II - FORMULATION OF THE NSF POLICY FOR REFEREEING PROPOSALS. The HADRONIC JOURNAL is dedicated to plausible studies on fundamental issues (minute incremental paper are rerouted to other journals). In particular, my primary objective is to avoid a monopolistic presentation of research. I therefore dedicate exactly the same attention to either quark-oriented or quark-non-oriented papers. The second step I implemented after acquiring the post of Editor in Chief is that the submission of any paper to referees of only one belief be categorically prohibited. Specifically, I considered vital for objectivity that each paper, whether quark-oriented or quark-non-oriented, be submitted to referees who are quark-believers as well as most importantly quark-non-believers.

This refereeing policy was implemented, again, only for editorial purposes. I am sure you will agree with me that it becomes mandatory when delicate money aspects are involved, such as in NSF funding of research. To be specific in this truly vital aspect, if a quark-non-oriented proposal is submitted only to physicists financially committed to quarks via existing grants, this is literally equivalent, in my view, to the decision not to fund the proposal at its very reception.

I must stress that the academic status of the referee is of purely secondary consideration (unless you believe that the outstanding referees of my proposal did act properly). What must be of utmost priority in the consideration is whether the referee is or is not recipient of research grants on quarks and whether he is a believer or non believer in quarks. I can assure you that there exist physicists of proved ethical substance who are quark-non-believers. Some of them are indeed outstanding and they have simply abstained from active publications in hadron structure to avoid their association with games of scientific curiosity such as truth, beauty, up and down etc. (in the current quark language funded by NSF).

I have no word to stress to you the need to identify and strictly implement a new policy for the proper selection of the referees of each individual proposal. It is vital that each proposal, irrespective of whether quark-oriented or quark-nonoriented, is submitted to a group of referees which satisfy precise, uncompromisable criteria of differentiation in their personal beliefs and commitments. It is vital that NSF gives proof of truly effective refereeing proposals which are strictly quark-non-oriented and which are nowadays, by and large, considered outside the "scientific establishment" and thus of no scientific value by physicists academically and financially committed to the quark conjecture. It is vital that NSF begins the submission of proposals by quark believers to quark-non-believers (I would be happy to serve NSF in this latter function, and so are other more qualified colleagues, but we have all been excluded by the NSF referee process until now). The risk for the lack of

implementation of this request is clear: the monopolistic continuation of funding for studies on the fundamental problem of contemporary physics along only the quark conjecture. With full candor, unless this situation is avoided, a crisis in basic research of unpredictable proportions will be unavoidable.

III - FORMULATION OF THE NSF PRIORITIES FOR FUNDING. The enclosed letter to Professors Panofsky, Wilson and Vineyard, as you can see, is a passionate appeal to reestablish the traditional priorities of basic research which have contributed so much to human knowledge up to the first part of this century and lately abandoned. What NSF has funded in hadron physics during the last decade is essentially a sea of minute incremental contributions deprived of any contribution in basic research which is even partially comparable to the great achievements of the first part of this century.

I have a scientific duty to inform you that a series of papers in the HADRONIC JOURNAL present clear criticisms of the fundamental physical laws used for strong interactions and ask for their direct experimental verification.

I have no words to stress to you the need to keep this new horizon in due consideration. In my view, it literally creates a new situation for funding research proposals and the need to establish precise priority for fund allocations. I am sure you realize that NSF simply cannot continue to fund on minute incremental contributions in hadron physics along established trends, when the basic physical laws used in these studies are in question.

My recommendation to you is that I have made to Professors Panofsky, Wilson and Vineyard. I suggest that utmost priority be given for funding proposals in the study of truly fundamental physical problems beginning and most importantly from the fundamental physical problem whether the experimentally established knowledge for the electromagnetic interactions is applicable or inapplicable to the strong interactions in their currently known form. The funding of studies of minute secondary aspect should receive a minute secondary priority.

Whatever the priorities you select, it is essential that they are fully disclosed and advertised at the time you consider it appropriate. I am sure you will agree with me that their cryptic containment within NSF files would be ineffective. Instead, to reach the necessary effectiveness, I consider essential that our entire community of basic research is fully and adequately informed of the selected priorities.

You have a possibility of giving an invaluable contribution to basic research. But, it demands clear ideas, firm implementation against predictable opposition, and courage. The identification, release and full disclosure of clear priorities for fundings in basic research appears to be essential for a genuine contribution to the pursuit of human knowledge.

IV - INCREASE OF NSF FUNDING TO RESEARCHERS AS INDIVIDUALS RATHER THAN TO THEIR INSTITUTIONS.

I believe that a number of potentially crucial applications on truly fundamental physical problems will never reach you as a formal grant application via Institutions. The understanding of this occurs again, demands an open language. Truly new ideas generally see their inception during graduate studies. When the researcher then reaches sufficient maturity for their treatment, he is at the level of research associate or assistant professor. The filing of any proposal at this step of the academic layers via Institutions generally demands approval by the senior colleagues in the department. I know of a number of cases in which truly promising proposals were killed at the departmental level and they never reached a Governmental Agency. I can also personally testify that the filing of my own research proposals during my past academic life via Institutions demanded the overcome of such academic entanglements to go beyond the wildest imagination.

If NSF is genuinely interested in a comprehensive program for the support, promotion and assistance of creativity in basic research, I believe that it is vital to increase the number of grants to individual

and correspondingly decrease the grants to Institutions. Again, it is vital that NSF fully discloses the statutory possibility that researchers can apply as individuals, rather than via Institutions, in such a way that each researcher has full information to reach the best decision under his own circumstances. As it is now, this is a cryptic information which, even myself, I finally knew by word of mouth and after numerous years of research activity.

You must realize that this action is essential on a number of counts. The first is that a researcher with valuable ideas on fundamental problems should be free to pursue them, and not be subject to the predictable opposition of his senior colleagues with opposing vested interests. But there are other reasons in our changing community of basic studies which support the increase of grants to individuals (now virtually nonexistent at NSF, and only written in a nonimplemented statute). As you know, theoretical physicists nowadays, at the peak of their maturity and productivity (the level of associate, nontenured professor) find themselves unemployed because of lack of tenure. Other truly valuable young researchers either abandon the field, or are forced to do minute calculations as research associate to senior physicists with NSF (or other) support. A duly advertized information that grants to individuals are permitted by NSF statute and actually granted would be simply invaluable to this most productive and energetic segment of our community of basic studies.

But there is still another reason which strongly suggest the increase of grants to individuals. It is of mere arithmetic nature. Owing to the now prohibitive overheads, one grant to one institution can literally supports twice as many researchers as individuals. In essence, a theoretical physicist studying fundamental physical problems does not need huge amount of money. He simply needs money for food and shelter for his family and himself. If the funds available to NSF for basic research were properly distributed between grants to individuals and grants to Institutions in a proper proportion (at least 50 o/o in my view, money wise), this would immediately imply a substantial increase of supported research without any increase of funds whatsoever.

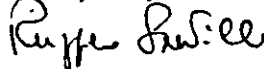
Of course, Institutions will strongly oppose such an approach. But, is the duty of NSF to support the financial condition of U.S. campuses, or to support the pursuit of human knowledge?

V - GOOD LUCK TO YOU IN YOUR DIFFICULT DUTIES.

If I can be of any assistance to you, please do not hesitate to contact me. You can trust in my utmost confidentiality.

I occasionally visit the Washington area. If you are interested in my paying you a visit to discuss in more details these issues, please let me know.

Very Truly Yours.



Ruggero Maria Santilli

RMS|cgg

P.S. In case academic entanglements will prohibit my continuation of research under DOE support (which, by statute, cannot give grants to individuals), I intend to submit a research proposal to NSF which is (a) of strictly non-quark orientation, (b) on fundamental problems (the physical laws for strong interactions) and, most importantly, (c) submitted as an individual.

* There are aspects and recent events which go beyond a letter, even written in candid language.
c.c: Mr. M. BARDON, NSF .

NATIONAL SCIENCE FOUNDATION

WASHINGTON, D.C. 20550

August 17, 1978



Dr. Ruggero Maria Santilli
367 Linwood Avenue
Newtonville, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of July 20, 1978, commenting on NSF's procedures for handling proposals and the general trend of the research we have supported in physics in the past years. I have given this matter my personal attention and have also discussed it with a number of members of my staff.

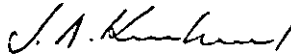
First let me respond to your comments in my role as a Federal administrator. In this capacity I feel that it is part of my responsibility to be sure that proposals received and grants made by the Foundation are handled strictly in accordance with the Foundation's policy which I believe represents a highly ethical manner of proceeding. I am, however, sympathetic to your being perturbed by intemperate comments in reviews. I should remind you in this context that comments made by referees are confidential and can be shared with the principal investigator only, following on a specific request from him. In brief, I believe that NSF's handling of proposals is carried out in an exemplary manner, and in this, I can speak from personal detailed knowledge of various proposals including your most recent one to us. My former Assistant, Mr. Wayne R. Gruner, has briefed me fully on the details of your recent dealings with the NSF.

Secondly, as a physicist I feel that while I have some passing acquaintance with the topics you mention, any comments that I might make would be somewhat uninformed in view of the limitations of my familiarity with hadron physics. Therefore, I feel it would be professionally irresponsible of me to enter into this specialized area. To guide the Foundation in this and other areas that are highly specialized, the Foundation has an Advisory Committee for each Division. Recently the

NSF Advisory Committee for Physics performed an in-depth review of NSF support of Theoretical Physics, and a copy of their report is enclosed. I believe the members of this Committee would be interested in your recent letter to me including its enclosures, and I would like to pass them on for their information. However, as one enclosure is marked "Confidential Copy", I would like your explicit permission before doing this..

Finally, I want to thank you for your letter. I know you feel strongly about these matters, and I can only admire your tenacity and dedication to your views.

Sincerely yours,



J. A. Krumhansl
Assistant Director

Enclosure

Dr. J. A. KRUMHANSL
Assistant Director for Mathematical and Physical Sciences
National Science Foundation
WASHINGTON, D.C. 20550

Ruggero Maria Santilli
367 Linwood Ave
Newtonville, Ma 02160
August 24, 1978

Dear Dr. Krumhansl,

I would like to express my appreciation for your letter of August 17, 1978. Following your request, I am glad to give you full authorization for sending a copy of my letter to you of July 20, 1978 and of all its enclosures to the members of the NSF Advisory Committee for Physics. However, I would be grateful whether you contact Professor W. PANOFSKY in respect to the release of a copy of my letter to him of July 19, 1978 prior to any decision. Indeed, this letter was intended to be restricted to the persons indicated and, in any case, Professor Panofsky should be consulted in this respect. I would also appreciate whether you include a copy of this letter elaborating certain aspects and the circumstances which lead to my letter to Professor Panofsky. Also, my letter to you of July 20 was intended for your personal amusement and not conceived to be reviewed by an NSF Advisory committee. Nevertheless, I have no objection for its release, provided that the matter remains confidential and does not become available to persons outside NSF.

The following points might have some complementary value for my letter of July 20, 1978 to you and for its enclosures.

(1) At the risk of being considered a visionary, permit me to restate that the ultimate reason behind my letters to you and to Professor Panofsky is my belief that the Division of Theoretical Physics of NSF is in a highly delicate moment. I am sure you are aware of the malcontent of one segment of our community of basic studies in regards to quark oriented studies and the amount of funds they receive. This malcontent is increased through the years, rather than decreased, because the problematic aspects of the quark models have increased, rather than decreased, through the years. I do not know whether you are aware of the fact that lately this malcontent has reached alarming proportions. I must stress that this is my personal view, based on all the informations available to me and, as such, it could be entirely erroneous. Nevertheless, I am firmly convinced that this is the case. This conviction has created my moral obligation to inform you of these impressions in my letter of July 20, 1978 for whatever their value.

(2) Besides a sincere esteem for the known scientific stature of Professor PANOFSKY, I have a sincere gratitude for the courtesy, time and assistance he has provided for me on a number of occasions. My letter to him was not the result of a one day decision. It was the climax of a series of events which left me no other conceivable alternative to serve the interest of our community. Professor PANOFSKY is entirely noninformed of this background which lead to my letter to him. In essence, certain quite valuable, but truly malcontent physicists were in the process of implementing gestures which, in my view, would be highly detrimental to the U.S. community of basic research. In my view (which, again, could be entirely erroneous) a preventive action was needed. I was in a unique position because, as you know, I am the recipient of what appears to be the first federal research grant of non-quark inspiration and, also, I am the editor of a Journal which is already emerged as dedicated to the sole pursuit of physical truth, whether of quark or nonquark inspiration. My letter to Professor PANOFSKY was conceived, intended and used as a preventive tool. But, again, it was for me reason of considerable personal regret. This is the part of my letter to you of July 20, 1978 which I referred to in the footnote of page 5 as going "beyond a letter, even written in candid language". Of course, I am glad to release the end results of this action. But under no circumstances whatsoever I intend to disclose names. At whatever price.

(3) All this commotion, so to say, boils down to a very simple argument. These highly malcontent physicists are, in my view, valuable and responsible scientists. Their requests are, also in my view,

quite reasonable. In essence, they ask that federal research support for quark-oriented studies must continue. However, jointly, NSF must initiate support of studies along fundamentally different lines to achieve a well balanced conduction of research on this fundamental problem. I must acknowledge that this request is entirely reasonable and I must endorse it in its entirety. The area which has been a primary reason of irritation (my personal case is known to you, but apparently there are others, of course, of different nature) is the current refereeing of research proposals on hadron physics by NSF. In essence, to my perhaps erroneous view, NSF sends proposals of this nature to leading physicists in the field who are experts in quark models. The criticism is that NSF should not identify quark experts with experts in hadron physics. This is the reason behind my recommendation to you of July 20, 1978 in relation to the operations for refereeing.

My personal interpretation of this occurrence is the following. It appears that NSF is not yet aware of the fact that the physics community is becoming more and more divided on the issues of hadron structure into two opposite groups: quark-believers and quark-non-believers. Physicists in the first group are known to NSF. Those of the second not yet. Thus, there has simply been the lack of sufficient information to NSF to prevent malcontent and also misunderstandings.

The point, however, remains. To understand the occurrence, you should be aware that, in essence, statements of technical criticisms by a quark-supporter on non-quark-oriented proposals are entirely and completely distrusted by quark-non-believers. A specific example might be useful. Here I am glad to expose myself in the hope that it might be of some value for our community. After studying and conducting research on hadron physics for over a decade I must honestly confess that I simply do not believe in quarks. This implies that I do not believe in the technical statements or criticisms moved by physicists when based on quark arguments. For instance, when a quark expert tells me that Einstein's special relativity is valid within a hadron because of such and such argument I consider this a mere expression of his personal opinion. Any different view would imply that the assumption that the quarks are the constituents of hadrons, complemented by the assumption that they confine, complemented by the assumption thatetc. etc., produce an unequivocal, incontrovertible, scientific truth.

It is vital, in my personal view, that NSF acquires full consciousness of this occurrence and takes all the necessary steps to prevent delicate situations which could lead to an unpredictable outcome. This essentially demands the consciousness that any critical statement of an NSF proposal based on quark arguments might be fundamentally inconclusive because the quark models are fundamentally inconclusive and they will remain so until the problem of the identification of quarks with physical particles is accomplished in an incontrovertible form. Confinement has created an area of further potential danger for NSF. The reason is that even assuming that a genuine mechanism of confinement will be achieved sometimes in the future, this will leave the problem of the experimental identification of quarks totally unchanged. It would simply shift such identification while within hadrons. In other words, confinement, if (and if) achieved, will still leave quark models fundamentally unresolved. They will remain so until a new technology emerge capable of experimentally proving that, say, the π^0 has precisely a quark and an antiquark as constituents with precisely such and such data, etc. You can now understand the profound irritation of certain physicists when criticism on their work by quark believers based on quark arguments is taken seriously and used as a decisional tool. It is here where NSF, in my view, must exercise extreme scientific caution.

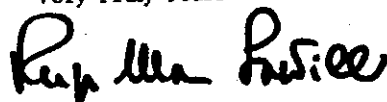
To conclude this presentation of my personal views, I am happy to report that I did achieve indeed my objective, that of quieting down excessively malcontent physicists and delaying their intended quite delicate action. This was the result of my letter to Professor PANOFKY, as well as the fact

that I subsequently received from Governmental Agencies other than NSF for my refereeing research proposals which could lead to non-quark approaches to hadron structure (this was indeed invaluable for my action). However, this simply resulted for NSF in gaining some time for studying the situation and adjusting to a fast changing situation of research in the sector. As for the future is concerned, permit me to stress that I do not intend to make a second intervention and that from here on I would like to abstain from participating in disputes ultimately related to the controversial topic of funding of quark hypothesis.

I should add that you are receiving a warm and sincere support by both quark believers and quark-non-believers. As a matter of fact, a number of physicists are relying their hopes in you. Almost needless to say, this is invaluable for how NSF is seen from outside researchers.

Speaking on personal grounds, you have my sincere esteem and my unconditional support. If I can be of any help in your rather difficult task at any time, please do not hesitate to contact me.

Very Truly Yours

A handwritten signature in dark ink, appearing to read 'Ruggero Maria Santilli', written in a cursive style.

Ruggero Maria Santilli

rms|cgg

- 803 -
HARVARD UNIVERSITY
DEPARTMENT OF MATHEMATICS

AREA CODE 617
495-2170



SCIENCE CENTER
ONE OXFORD STREET
CAMBRIDGE, MASSACHUSETTS 02138

February 14. 1980

Dr. L. F. BAUTZ
Deputy Director
Division of Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Bautz,

Following your letter of February 4, 1980, I am here respectfully submitting a proposal for support of the THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS.

The original of the proposal is enclosed to this letter jointly with two copies.

Ten additional copies have been separately mailed to you.

In addition, we have separately mailed to you one complimentary copy of the

PROCEEDINGS OF THE SECOND WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS (two volumes)

Finally, I enclose a list of mathematician and physicists, experts in the Lie-admissible formulations, in case of any assistance for the selection of qualified referees.

I remain at your disposal for any additional information or assistance you might need.

Your consideration and time has been appreciated.

Very Truly Yours

A handwritten signature in dark ink, appearing to read 'Ruggero Maria Santilli'.

Ruggero Maria Santilli
Chairman, Organization and
Admission Committee
THIRD WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS

RMS/ml
encls.

PART XXII:
REJECTION OF THE
PRIMARY I.B.R.
APPLICATION BY THE
DEPARTMENT
OF
ENERGY
IN 1981-1982

October 27, 1980

Drs. B. HILDEBRAND, D. PEASLEE, and W. WALLENMEYER
DOE, Division of High Energy Physics
Washington, D.C. 20545

Dear Bernard, David, and William,

I am happy to report to you that on October 25 (Saturday), 1980, my wife Carla and I have signed the Purchase and sale agreement for the acquisition of an 18 rooms Victorian house located inside Harvard University, one block from the Old Yard. As you can see from the enclosed copy of the purchase and sale agreement, the price is a knee trembling \$ [REDACTED]

The primary purpose for our embarking in such a venture is our firm determination to organize a new center of research called

THE INSTITUTE FOR BASIC RESEARCH

The objective of the new Institute is to gather and coordinate the best possible brains in experimental physics, theoretical physics and mathematics to pursue fundamental physical knowledge of primary energy-related orientation (strong interactions and the controlled fusion in particular). Officially the new Institute will be presented as a new research facility which "complements" the facilities already existing in the area. Unofficially and confidentially, the idea to organize the new Institute is an expression of a growing concern, nation wide, on the rather clear monopolistic restriction of research on strong interactions in leading institutions along only quark oriented lines, and the need of a more balanced use of public funds. In fact, several precautions have been taken to ensure the genuine freedom of the researchers of the new Institute, and the consideration of all valuable or otherwise promising lines.

Beginning from this morning (Monday, October 27), I have initiated the application to local lending institutions for a first mortgage of \$ [REDACTED] (for about 70 % of the value). A formal bank commitment is needed on or before November 25, 1980. If everything goes as planned, the new Institute may initiate the operations on January 1, 1981. In particular, we feel confident to be self-sufficient for the purchase of the building of the Institute.

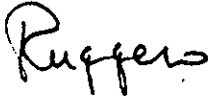
However, in regard to the operations of the new Institute we are currently facing a predictable negative cash flow for the first two years. This is due to several factors, including the need of administrative-accountant personnell (A DIRECTOR for the new Institute will be needed, but at some later time -- personally, I do not intend to take this post because I am primarily interested in conducting research). I am afraid that this negative cash flow could be detrimental for our capability to obtain a first mortgage. More explicitly, we are trying to identify revenue sources capable of providing financial selfsufficiency for the first two years of operations. However, we fear that our solutions may not result to be truly convincing to the conservative New England Banker. The entire project might therefore be jeopardized because of this aspect.

The funds needed for the first two years of operation are of the order of \$ 100,000 (\$ 45K for calendar 1981 and \$ 55K for calendar 1982, including administrative personnell, but excluding the Director). A detailed itemization is at your disposal upon request. Please see whether DOE can support the initiation of this new research facility. Also, please take into consideration that the time factor is rather crucial in this instance. In fact, we need a bank commitment on or before November 25 of this year. After that date, the property might still be available, but its price will be definitely higher, and even double (the property was located because of personal contacts with the owners, it was never on the market, and, once its availability is known, its price becomes a function of personal need owing to the quite hot location).

On more specific grounds, please consider the possibility of adding \$ 100,000 to my existing contract (\$ 45K to that for 1980-1981 and \$ 55K to its second year). A formal decision prior to November 25, 1980 on this matter would be determinant for the entire project. If this is not possible, a letter of interest would be also welcome. Alternatively, I would appreciate the authorization of releasing the name of one of you to selected lending Institutions. In this way you could verbally indicate the existence of an interest. However, please keep in mind that New England Lenders are traditionally conservative (they refuse to fund even the Polaroid for the initial operations). To be truly effective, a formal resolution on the availability of the funds, and the date appears to be needed.

As I have done in the past three years, I would like to rely entirely on your judgment and vision. The payback could be quite intriguing. In fact, a new Institute specifically organized for the genuine pursuit of fundamental knowledge in energy-related problems outside the current mumbo-jambo of academic dances could likely achieve breakthrough of fundamental character.

Best Personal Regards



Ruggero Maria Santilli
Chairman of the Board of Trustees
and Acting Director
THE INSTITUTE FOR BASIC RESEARCH

P.S. I am sorry that I did not have the time to make you a detailed report on the THIRD WORKSHOP IN LIE-ADMISSIBLE FORMULATIONS. It was a true success. We had some selected 30 participants, half pure mathematicians, and the rest theoretical and experimental physicists. Most determining was the participation of an experimentalist in neutron interferometry from Europe, Prof. Rauch. He clearly indicated that the experimental information is such to warrant doubts on conventional laws for the strong interactions. It was a quite emotional moment for all. The Proceedings look like a genuine contribution to knowledge. We shall have three volumes (rather than two for the meeting of 1979). Also, the Proceedings will be typeset. The Journal has purchased all the equipments and trained the personnell. I am typing this letter to you on a special IBM Composer to give you a first hand feeling of the selected style (I have been told that it is better than that of other Journals).

— 807 —
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

THEORETICAL STUDIES ON LIE-ADMISSIBLE FORMULATIONS

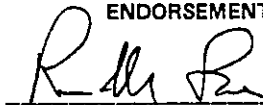
Principal Investigator
Ruggero Maria Santilli
Soc. Sec. No. 032-46-3855

Proposed Starting Date:
June 1, 1982

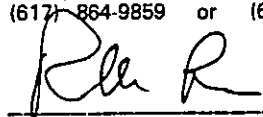
Proposed Duration:
24 Months

Amount Requested:
\$339,975

ENDORSEMENTS



R.M. Santilli
Principal Investigator
(617) 864-9859 or (617) 964-1684



R.M. Santilli
President, The Institute for Basic Research
(617) 864-9859 or (617) 964-1684

Accounting Firm of the Institute
Vaccaro and Alkon PC, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att: Mr. R. Alkon, President
Tel. (617) 969-6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
Att: Mr. J.R. Grassia, Senior Partner
Tel. (617) 956-1700

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ABSTRACT

The present application is for the continuation of research initiated by the Principal Investigator (Professor Ruggero Maria Santilli) in 1978/1979 under DOE contract ER-78-S-02-4742.A000, and continued in 1979/1980 under contract AS02-78ER-4742, in 1980/1981 under contract DE-AC02-80ER10651 and in 1981/1982 under contract DE-AC02-80ER10651.A001.

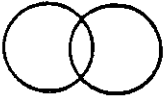
The objective of the research is to achieve experimental, theoretical, and mathematical knowledge of whether intrinsic characteristics of particles (magnetic moment, spin, etc.), as currently measured under long range electromagnetic interactions, are preserved or altered in the transition to the different physical conditions of the strong interactions.

The relevance of the research can be seen in physics, mathematics, and engineering. Particularly important is the relevance for controlled fusion. In fact, clear knowledge of the intrinsic characteristics of nucleons under strong interactions is important to achieve controlled fusion (e.g., the value of the magnetic moments of nucleons under very high pressure, densities, and temperatures is important to achieve magnetic confinement).

The available experimental basis is primarily of nuclear character, and initiates with old evidence (presented in well written treatises) that the magnetic moments of nucleons change under nuclear conditions, as apparently necessary to interpret the total nuclear magnetic moments. This hypothesis was subsequently abandoned, until its coordinated study was resumed by the Principal Investigator under DOE support. Additional experiments via neutron interferometers measure the spin precession of neutrons under joint electromagnetic and strong interactions. Available experimental data show unexplained clusters of points outside the curve predicted by conventional electromagnetic quantities and are unable to recover the 720° needed to establish the exact validity of the $SU(2)$ -spin symmetry under strong interactions. This proposal contemplates the formulation and study of a series of experiments to achieve the resolution of the problem in due time.

The proposed organization is as follows. Two Senior Research Associates (an experimentalist and a theoretician) are recommended beside the Principal Investigator, owing to the complexity and diversification of the project. The proposal also includes the organization of the Fifth (1982), and Sixth (1983) Workshop in Lie-admissible Formulations, as well as of the Second International Conference in Nonpotential Interactions to be held in 1984.

The project is expected to result in a number of articles, monographs, and conference proceedings.



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

Office of the President

August 19, 1981

Dr. DAVID C. PEASLEE
Physics Research Branch
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

Dear Dr. Peaslee,

I hereby respectfully submit the enclosed original of the research grant application entitled:
THEORETICAL STUDIES ON LIE-ADMISSIBLE FORMULATIONS
under administration of The Institute for Basic Research.
A number of copies of the application have been separately mailed to you.

The initiation date of the contract has been suggested at June 1, 1982 in order to preserve the continuity of research at the expiration of the existing contract number DE-AC02-80ER10651.A001 on May 31, 1982. The proposed duration is 24 months. The amount requested is \$ 339,975.

During the consideration of the proposal, as well as of the amount requested, I would appreciate the courtesy of keeping into account that this is the first proposal of our Institute. Adequate funding will therefore permit the growth of a new research facility with a considerable potential in the free pursue of fundamental scientific knowledge.

I hope that the application is sufficiently informative on the continuation of research under DOE support, including general information on our Institute (as Appendix A), while I remain at your disposal for any additional information you may desire.

Sincerely Yours

Ruggero Maria Santilli
President
THE INSTITUTE FOR BASIC RESEARCH and
Principal Investigator

cc.: Drs. B. HILDEBRAND, and W.A. WALLENMEYER, DOE

RMS, ml

encl.



Department of Energy
Washington, D.C. 20545

NOV 6 1981

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

Dear Dr. Santilli:

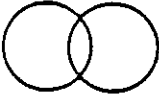
Your research proposal entitled, "Theoretical Studies on Lie-Admissible Formulations," submitted by the Institute for Basic Research has been received.

Your proposal is now under review in the Division of High Energy Physics and as soon as a decision with respect to support can be reached, you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him.

We appreciate your interest in submitting this proposal to DOE, and we will be pleased to give it review and consideration for support.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics
Office of High Energy and Nuclear Physics



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

October 22, 1981

Office of the President

Dr. DAVID C. PEASLEE
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Station J. 309
WASHINGTON, D.C. 20545

CERTIFIED MAIL

RE: Research Grant Proposal entitled
"Theoretical Studies on Lie-admissible Formulations"
Principal Investigator: R.M. Santilli
Submitted on August 19, 1981

Dear Dr. Peaslee,

As requested by you, I have separately mailed to you three parcel posts (certified) containing a collection of articles for the refereeing of the proposal above. A sample of the collection, entitled "Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions" has been enclosed to this letter for your consideration.

Please keep in mind that the Research Grant Application under consideration by your Office deals with a diversification of applications ranging from classical mechanics (trajectory problems in atmosphere) to particle physics (the open nature of the structure of the strong interactions). The selected articles for refereeing has been restricted to only one profile of the application, that regarding the intriguing situation of spin. At your discretion, we remain at your disposal to send you additional selections of articles in other aspects of the application.

Owing to this diversification of applications, we would like that the application above be considered independently from other applications by our Institute. Also, my current DOE support expires on June 1, 1981, and I would be truly grateful whether the consideration of the proposal can be expedite within reason, of course.

Yours Truly Yours

Ruggero Maria Santilli
President
RMS-pm
encls.

cc.: Drs. Wallenmeyer and Hildebrand, DOE



- 813 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 12, 1981

Drs. B. HILDEBRAND and W.A. WALLENMEYER
Division of High Energy Physics
Physics Research Branch
Department of Energy
Mail Station J-309
WASHINGTON, D.C. 20545

Dear Bernie and William,

Without a doubt, this is the most important letter I have written to you until now. As a result of your consideration of the content of this letter, the entire financing policy and planning of our Institute will be set for years to come.

Stated quite simply, the purpose of this letter is to recommend that the research initiated under your support in 1978 shall continue under your support in a way as smooth and harmoniously as possible with the scientific objectives of the Department of Energy, as well as with the various academic and national institutions supported by DOE.

I am confident of your sincere and best intention to study this possibility. Nevertheless, permit me to indicate that, from the viewpoint of our Institute, a major problem is time. In essence, a number of rather important decisions regarding the financing of our Institute are being delayed, and will be delayed to give to you the necessary time to reach a decision. However, we can delay our decisions only until mid-January 1982. Further delays beyond that date may imply excessive risks for our scientific programs. Therefore, after mid-January 1982, you should feel free to continue the investigation of the case and/or of individual proposals without any need of rush. However, you should expect the possible existence at our Institute of scientific policies which are not necessarily compatible with those of the DOE.

The purpose of this letter is precisely that of preventing this possible occurrence. On more specific terms, my proposal is the following:

1. Election of the Director of our Institute. As you know, we have an opening for the Director of general operations. We would appreciate your advice in the selection of the appropriate person as well as in the finalization of his functions. As far as I can see, the person should be well received by our neighbors as well as by DOE so that he can do a good job in smoothing out situations and resolving possible discrepancies. We are not expecting, of course, the appointment of this person by mid-January 1982. Nevertheless, we would appreciate whether you can communicate your thoughts on the opening by that time.

Drs. B. HILDEBRAND and W.A. WALLENMEYER

Page Two

November 12, 1981

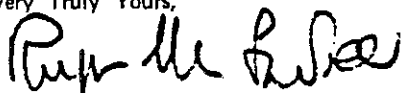
2. Possible support available at the expiration of my contract on May 31. As you know, my existing contract expires on May 31, 1982. The first application of our Institute to your Office has been that of the continuation of this support from June 1, 1982 on. As communicated separately to Dr. THEWS of your Office, it is of the utmost importance that a decision regarding this application be reached as soon as possible. Also important for our program is that we have the capability of supporting at least one additional physicist besides myself (as well as the Director, if appointed). I would appreciate whether by mid-January a decision (even informal) can be reached on the application.

3. Possible available support for the future growth of our Institute. We finally need some input in regard to future growth so that we can plan for an orderly expansion of our Institute in a way compatible with your more general plans and objectives. I would, therefore, appreciate whether by mid-January you could give us some indicative and orientational figures of ceiling for the applications we will meaningfully submit to your division. Please keep in mind that the demand for administration of grants by our Institute is great for several reasons (location, minimal overheads, genuine scientific freedom, etc.). It is, therefore, important for us that we set guidelines for growth, say for the next 4-5 years, not later than mid-January 1982.

I hope that in this letter I succeeded in communicating our sincere best intention to organize our Institute in a form compatible with more general plans at DOE. Most of all, I hope you will see in this offering the expression of my sincere gratitude for your scientifically and humanly invaluable, past support.

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

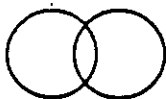
Very Truly Yours,



Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/pm

cc: Board of Governors and Officers, I.B.R.



- 815 -

THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street

Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 11, 1981

Dr. ROBERT THEWS
Division of High Energy Physics
Physics Research Branch
Department of Energy
Washington, D.C. 20545

RE: Research Grant Applications entitled
"Theoretical Studies on Lie-admissible algebras"
submitted on August 19, 1981

Dear Dr. Thews,

Following our phone conversation of November 5, I am taking the liberty of confirming or elaborating the following aspects, while I remain at your disposal for additional assistance you may need.

1. SUBMISSION OF PROPOSAL. I would like to confirm that the application has not been submitted to other Governmental or Private Agencies for funding, nor do we contemplate such a submission at this time.

As indicated in the submission letter addressed to Dr. D.C. PEASLEE of August 19, 1981, this is the first application of our Institute. Numerous scientific initiatives and activities are dependent on this application. We would, therefore, welcome any suggestion as to whether the application should be jointly submitted elsewhere. At any rate, we shall abstain from such submission unless recommended by your Office, or following consultation with your Office.

2. REFEREE PROCESS. We are concerned that, at the time of our conversation of November 5, the referee process had not been implemented since the submission in late August. We are also concerned with the fact that the experts in the physical applications of the Lie-admissible algebras are very few at this moment, on a world wide basis, while the existing specialized literature in the field has now surpassed the mark of 5,000 published pages. We are concerned that scientists in other fields, even though in good faith, may be tempted to pass judgments without a technical knowledge of the topic. Finally, we are concerned of the possibility that the reaching of a final decision on the application might take excessive time (e.g., if the referees are not selected in the field, and have to study a voluminous literature), to the detriment of all.

As you know, we are currently spending large public funds in strong interactions. Most of these funds are spent under the assumption of the validity for the strong interactions of the basic laws established for the electromagnetic ones. Our grant application, to our

Dr. ROBERT THEWS

-2-

November 11, 1981

knowledge, is the only application specifically devoted to mathematical, theoretical, and experimental studies to verify the basic laws via direct and specific experiments. Within such administrative-scientific setting, we believe that it is in the best interests of all to reach a decision on the application as soon as possible.

In view of these (and other) reasons, we are respectfully submitting for consideration by your Office the following alternatives.

Alternative I: action on the proposal without external referees. The relevance of our research, I believe, is out of the question. Our capabilities to perform have been established by the preceding four years of support. Finally, your Office has been kept fully informed of all advances. In view of these aspects, I would, therefore, gratefully appreciate the consideration of reaching a decision without external refereeing, as it was done in preceding cases of our contracts.

Alternative II: action following referee process. In this case, I would appreciate whether experts in the field of the proposal are consulted. To facilitate your task, I enclose a list of experts in Lie-admissible formulations. Finally, and most importantly, please mail to us all referee reports as soon as available.

3. REFEREEING MATERIAL. Three copies of the application were mailed (via certified parcel post) to Dr. Peaslee. Additional copies are at your disposal on request. We also mailed three sets of a collection of papers related to the problem of spin under strong interactions. The understanding specified in a letter to Dr. Peaslee was that this material relates to only one aspect of the grant application. In fact, our research applies to a considerable number of fields ranging from Newtonian mechanics, to classical field theory, to statistical mechanics, and to quantum mechanics, while the selected papers were only in the problem of spin.

All references listed in the application are available in research libraries. Nevertheless, to facilitate the referees, we would be glad to mail you additional copies.

4. INFORMATION ON OUR INSTITUTE. Following your request, I have instructed our attorney to mail you all pertinent legal data, such as the names of the Officers and of the Governors (see enclosed letter).

In regard to the internal organization, such as that of the Board of Trustees-Advisors as well as the internal operational chart, they are still under finalization at this time. You can rest assured that they will be mailed to you as soon as available.

5. EVALUATION OF OUR INSTITUTE. Permit me the liberty of suggesting that an evaluation of our Institute during the referee process be avoided as much as possible. In fact, I am concerned that such an evaluation may raise and create unnecessary problems.

November 11, 1981

(a) Legally, our Institute has the same status as that of Harvard and MIT, and we see no point in entering into this aspect at the referee level.

(b) Administratively, we can administer a limited amount of federal grants at a fraction of the cost of our neighbors (specifically, our overheads are of the order of 30% while those of other institutions range from 65% to 75%, to my knowledge.) The advantage in favor of our Institute is self-evident. However, the raising of the issue during the formal referee process may be unnecessarily detrimental to our neighboring friends.

(c) Scientifically, our Institute was born for the conduction of research which is currently not conducted in other local Institutions, such as the experimental verification of Einstein's special relativity under strong interactions or, more specifically, under the conditions of the controlled fusion. An appraisal of this program on a comparative basis with those of other local Institutions could only create a host of unnecessary problems. In fact, the same research should be conducted at those institutions, owing to the considerable amount of public funds spent there in strong interactions under the assumption of conventional electromagnetic law. An evaluation of our scientific program would, therefore, inevitably raise the problem (whether now or in the future) of ascertaining the reasons why the same research has been rather vigorously precluded in other Institutions until now.

Owing to these and other aspects, I would like to suggest that the referee process be restricted to the scientific merit of each individual proposal, without any consideration in regard to our Institute.

However, we would much welcome a visit by you as well as any other member of the DOE. In actuality, this would be the best way to reach an evaluation of our Institute because one direct view is better than one thousand words spoken far away. In fact, only via a direct visit one can see our building, the facility that it offers, its location inside Harvard, and the possibility that it permits to each member to have continuous scientific interactions with individual Harvard scientists.

In closing, permit me to recall the achievements permitted by the DOE support during the preceding four years (such as, the publication of three research monographs and a considerable number of papers by several authors, the support of over fifty mathematicians and physicists, the organization and conduction of four international Workshops on Lie-admissible Formulations, and more recently the organization of the First International Conference on Nonpotential Interactions, as presented in detail in the application). Also, permit me to express the hope that these scientific programs can indeed be continued under DOE support. In case I can be of any assistance, please do not hesitate to contact me.

Sincerely,



Ruggero Maria Santilli
Principal Investigator of the Application
and President

RMS/ppp

cc: J.R. Grassia, Esquire
Boston, MA

NOVEMBER 12, 1981

VERY CONFIDENTIAL MEMO

TO: Bernie and William
FROM: Ruqero
SUBJECT: comments on formal letter of same date

I am receiving rather preoccupying rumors regarding apparent pressures on you by Cambridge academicians against our Institute. I am fully aware of the difficulties of your situation. I would like therefore provide you with some background information so that you can be in a better position to reach mature decisions.

I believe I have given you proof of loyalty during the past several years. The proof I consider the best is my silence in regard to the uncountable academic dances which have occurred on my studies under your support at Harvard University and the Massachusetts Institute of Technology beginning with fall 1977 (*). This silence is due to my view that Governmental Officers should not be un-necessarily involved with vulgar academic greed. However, you should be aware that some of these episodes have been particularly serious, because they apparently imply abuses of scientific power in direct conflict with national interests, as well as the pursuit of scientific knowledge, not to mention the human profile.

I am sure you are aware that the perception of the educated society at large on the conduction of science by academic institutions is changing rapidly. The terms "academic corruption", while virtually absent only a few years ago, are now heard more and more frequently. I am not referring here to corruption in the sense of stealing money. No. I am referring to the use of academic power to prevent the pursuit or jeopardize the establishing of undesired novel scientific knowledge, which is much more damaging to society than ordinary corruption as defined in the current code of laws. A number of educated persons are now under the feeling that this type of corruption has reached such a level, to represent a serious threat to National Interests. Also, the virtually unanimous feeling is that this is something happening at the academic, and not the governmental level. You are therefore completely out of the problem, to my understanding. However, you should be fully informed of its existence, and urged not to underestimate it, so that we can initiate coordinated, preventive and containing actions. The formal letter of this date is inspired by the hope that these latter objectives are achieved in a way as smooth and orderly as possible.

However, the background scientific issues should not be ignored. Actually, they are the central aspect of the situation. Permit me, therefore, to review them as they are perceived by scientists of proved ethical standards (This, I am sure, is not reported to you by other people).

1. The experimental verification of the validity or invalidity under strong interactions of Einstein's special relativity and other basic laws must be conducted. Period. There are two ways to do it. We can achieve the objective in a scientific orderly fashion, or following a crushing scandal. Academicians who oppose these fundamental tests, either openly or in a cryptic way, are clearly corrupt in my view. In fact, the tests, in the final analysis, can also confirm the validity of the atomic laws under strong interactions. By comparison, all other experiments currently under way or under consideration, even though definitely valuable on scientific grounds, have a comparatively minute scientific importance. Also, these tests are clearly essential for future technological advances of truly fundamental character. The opposition to these tests by

(*) The sole exception I have made is the recent episode of stall by MIT of the fundamental experiment on SU(2)-spin. The reason for the exception is, first of all, that the sad episode has a quite international character involving Governmental Agencies in three different Nations; and, second, because the episode, unless monitored and controlled, has all the ingredients of reaching front pages of daily newspapers in France, Austria, and, inevitably, the U.S.A.

corrupt academicians, therefore, is a threat to National interests. At any rate, the existing direct tests DO NOT confirm the validity of the special relativity, and the need of repeating the tests is clearly unquestionable. I am referring here to the fact that the experiments available on the spinor symmetry under strong interactions do not reproduce the angle of precession predicted by Einstein's theory; the experiments on optical activity of neutron within matter are far from being convincingly in agreement with Einstein's relativity; additional experiments under nuclear forces clearly and grossly violate the T-symmetry predicted by Einstein's relativity, etc. etc. Last, but not least, we are currently spending truly large amounts of tax-payers money in strong interactions. A majority of these public funds are spent under the assumption (very often, tacit) that the basic laws are valid. You can therefore understand the concern of educated persons to a situation of this type.

Also, you should know that the scientific power of corrupt academicians, after being unchallenged for decades, has now reached blinding dimension of genuine irresponsibility. In fact, several of these "scientists" have a BIG MOUTH. They talk openly of their opposition to visitors during their known "Cambridge lunch breaks" and other occasions, with statements such as "Santilli writes long and useless letters" following a kind, respectful and detailed proposal to initiate tests. The point is that the guests smile in their presence, but subsequently, they call either me, or eventually their representatives expressing concern for the alleged existence of scientific corruption in major academic institutions. The prestige of these institutions is still mostly intact now. However, my fear is that one big scandal, and their prestige would be tarnished for generations, with a consequential major damage to the Country and science at large. But, unless these people are brought to a responsible behaviour, and their big mouth closed, the risks are real.

2. The studies on the generalization of the atomic mechanics into a form suitable for the smaller nuclear and hadronic structures must continue. I am referring to the studies initiated under your support from 1978, which have resulted into so many international initiatives and results, and whose status will be reviewed at the forthcoming First International Conference on Nonpotential Interactions to be held in France in January 1982 under financial support by the French Government, and with the participation of virtually all developed Nations, including teams from China and the U.S.S.R.

These studies are also considered by a number of observers as being important for National interests, and future technological advances involving strong interactions. For instance, the achievement of a meaningful controlled fusion will likely demand the use of a generalized mechanics (including its statistical and plasma theories) for the conditions of hadrons under very small mutual distances, high temperatures, and large energies, in exactly the same way according to which the design of a nuclear reactor via Newtonian mechanics is pure fantasy.

Academic corruption, interests, and greed are against these studies more often than you may believe. But then, the threat to national interests, in my view, is there and real. It boils down to attempt the prevention of the achievement of new knowledge which can affect directly the life of all our children.

3. The Institute is a reality. The primary reason for the organization of our Institute is the fact that the conduction of the research of aspects (1) and (2) above in other academic institutions in Cambridge has been demonstrated impossible beyond any conceivable doubt. Now that the Institute exists, I believe that it can be valuable, scientifically and administratively. In fact, we can complement the research done at other institutions, by therefore avoiding a public confrontation, while we can administer governmental funds at a fraction of the costs of other Institutions. In short, the Institute was born because it has a natural place, function, and future. It is here to stay. Academicians who oppose its existence and funding are only looking for troubles.

I though it is important you are fully informed of these things, as unpleasant as they may be, The hope is that your full knowledge of the delicate scientific moment may result to be valuable in your own, difficult administrative-scientistif function. But, whatever the future will bear, please rest assured that my loyalty and gratitude to you will not change.

— 820 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 11, 1982

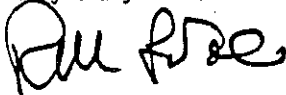
ROBERT THEWS
Division of High Energy Physics
Research Branch
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

CERTIFIED LETTER
RETURN RECEIPT
REQUESTED

Dear Dr. Thews,

In conformity with the existing regulations at the Department of Energy, I am hereby asking that you mail me promptly and in their entirety, all the referees reports on our application: "Studies on Lie-admissible Formulations".

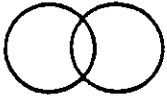
Very Truly Yours



Ruggero Maria Santilli
President

RMS-mlw

cc.: [REDACTED]



- 821 -

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

Professor. Ruggero Maria Santilli, President

February 11, 1982

Honorable JAMES B. EDWARDS,
Secretary
Department of Energy
Washington, D.C. 20585

VERY URGENT

Dear Mister Secretary,

I feel obliged to bring to your personal attention a recent preliminary decision by Dr. B. HILDEBRAND of your Division of High Energy Physics to terminate all funding of our research.

Our studies deal with a truly fundamental problem, the validity or only approximate character for the strong interactions of the electromagnetic basic physics laws. Our studies are therefore directly relevant for the totality of research on strong interactions supported by DOE and, in particular, for the controlled fusion. As you can see from the enclosed letter to Dr. W. A. WALLENMEYER, Director of the Division of High Energy Physics, the termination of our research (which is the only theoretical one in the field), may create a rather substantial administrative problem of scientific accountability vis-a-vis to the tax payer, because billions of public funds would be invested in strong interactions under the mere assumption of the belief of the basic laws.

I believe that the risks of such sizable administrative implications are inappropriate, particularly at this delicate moment for DOE of which we are all aware. You should know that, besides the lack of experimental resolution of the basic laws of strong interactions in a scientifically credible way, we have continued to see for years the spending of large public funds in theoretical research, such as those along quark conjectures, which can be at best qualified as academic exercises of curiosity without any conceivable practical value.

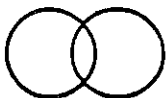
The situation in our community is therefore truly tense. The continuation of financial support for quarks conjectures and other esoteric studies, if matched with the truncation of funds for more serious work on the foundations, may have such implications, that even a "class action" cannot be excluded. The survival of DOE, let alone its effective continuation, could be at stake because of administrative unbalances.

Owing to these and other reasons, permit me the liberty of urging your personal intervention in the case, to prevent the official implementation of Dr. Hildebrand's personal views, and to ensure that public funds are truly dispersed in a more scientifically and humanly equitable way.

In case I can be of more detailed assistance to you, please do not hesitate to let me know.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President
RMS-mlw
encls.



- 822 -

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

Professor Ruggero Maria Santilli, President

February 11, 1982

Dr. ALVIN W. PRIVELPIECE, Director
Office of Energy Research
Department of Energy
Washington, D.C. 20585

VERY URGENT

Dear Dr. Privelpiece,

I feel obliged to bring to your personal attention a recent preliminary decision by Dr. HILDEBRAND of your Division of High Energy Physics, to terminate all funding of our research.

Our studies deal with a truly fundamental problem, the validity or only approximate character for the strong interactions of the basic electromagnetic physical laws. Our studies are therefore directly relevant for the totality of research on strong interactions supported by DOE, with particular reference to controlled fusion. AS you can see from the enclosed letter to Dr. W. A. WALLENMEYER, Director of your Division of High Energy Physics, our research is the only theoretical one in the problem of the basic law. Its termination would therefore create a rather substantial problem of scientific accountability vis-a-vis the tax payer, because billions of public funds would be invested in strong interactions under the mere assumption of the belief of the validity of the basic laws.

The risks of such sizable administrative implications are inappropriate in my view, particularly at this delicate moment for DOE of which we are all aware. You should know that, besides the lack of resolution of the basic physical laws for the strong interactions in a scientifically credible way, we have seen for years the dispersal of huge public funds on theoretical research, such as those along quark conjectures, which can be at best qualified as academic exercises of curiosity.

The situation in our community is therefore truly tense. Hildebrand's decision, if formally implemented, ultimately implies the continuation of large support for esoteric research, joint with the truncation of support for more serious studies on the foundations. The emerging unbalance may have such implications, that even a "class action" cannot be excluded. The survival of DOE, let alone its effective functioning, could be at stake.

Owing to these and other reasons, I am taking the liberty of urging your personal intervention in the case, to prevent the official implementation of Dr. Hildebrand's views, and to ensure that public funds are truly dispersed in a more scientifically and humanly equitable way.

In case I can be of more assistance for detailed technical implications, particularly in regard to the implications for controlled fusion, please let me know.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw
encls.



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

February 11, 1982

Professor Ruggero Maria Santilli, President

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
U.S. Department of Energy
WASHINGTON, D.C. 20545

EXPRESS MAIL
CERTIFIED

Dear Willy,

Robert Thews informed me yesterday of a preliminary negative decision on my grant application. After talking with you, I am under the impression that the decision was reached by Bernie Hildebrand without your knowledge, and, perhaps, by following questionable advice he may have received at MIT and/or other campuses.

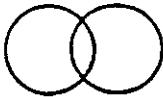
Prima facie, Bernie's decision means zero funds for our new Institute, and the continuation of millions of dollars of theoretical support to MIT and other local institutions all of us know too well. This is an extreme disparity which can only promote comparatively extreme reactions, none of which have been apparently appraised by Bernie in sufficient depth.

Permit me to recommend most warmly that you reverse Bernie's decision, establish an administratively more balanced situation in the area, and fund our Institute with small, yet sufficient means. Permit me also to recommend that, owing to the delicate implications of Bernie's position, a final, formal decision be reached as soon as possible.

The human profile. As you know, I have two children and a family to support. On June 1, 1982 my current contract terminates, and I will be without any income. The mere idea of applying for an academic (or other) job is laughable. Bernie has apparently decided to implement this situation on my part, while continuing the supply of large public funds to tenured and highly salaried faculty at MIT and at other campuses. It appears that Bernie has totally ignored the extreme exacerbation of the human conditions of our community which are inevitable from unbalances of this type.

The scientific profile. Let there be no doubt that the scientific values, results, and achievements of our grant have absolutely no match in any other line of theoretical studies under your support during the same period. We all know that millions of dollars of public funds in the hands of other theoreticians have resulted in minute advancements in established lines along quark conjectures and related fields. A comparatively minute investment of an average of \$ 60K per year has permitted our group a list of achievements too long to be repeated here. At any rate, the achievement of the generalization of the Hamiltonian Mechanics into a covering Birkhoffian form for contact interactions, or the achievement of the foundations of the hadronic generalization of quantum mechanics speak for themselves. They are substantial scientific events, no matter what other physicists say. Apparently, Bernie has decided to truncate these efforts for the generalization of the foundation of contemporary physical knowledge, in favor of huge funds invested in incremental advancements by other physicists. I am under the impression that Bernie has substantially ignored the transparent differentiation in our favor. Why?

The administrative profile. But the aspect which concerns me most is the administrative implication of Bernie's decision. We are in times of mounting pressures for scientific accountability, particularly in these periods of considerable social difficulties. The U.S. Department of Energy is spending truly immense public funds in strong interactions. All these funds are generally invested under the belief of the validity for the strong interactions of the basic physical laws of the electromagnetic ones. Even though, not admitted by possibly corrupt academicians, the technical reasons for doubt are truly substantial, and they are presented by numerous experimentalists in the Proceedings



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

February 22, 1982

Professor Ruggero Maria Santilli, President

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
U.S. Department of Energy
WASHINGTON, D.C. 20545

EXPRESS MAIL

CERTIFIED

RE: Primary research grant application for the funding of our Institute filed to your Office on August 19, 1981 under the title
THEORETICAL STUDIES ON LIE-ADMISSIBLE FORMULATIONS.

Dear Dr. Wallenmeyer,

I have been asked to submit to you this letter and the enclosed material with the request that

- it is formally included as an integral part of the application; and
 - it is informally considered for the general administrative practices of your Office.
- I remain at your disposal for any additional assistance you may need.

(1) The open problem of the basic physical laws. The virtual totality of experiments on strong interactions currently funded by your Office is based on the assumption of the validity of the basic physical laws. I am referring here to truly fundamental aspects of contemporary particle physics, such as:

- the rotational symmetry, with related Pauli's exclusion principle;
 - the time-reversal symmetry, with related reversible dynamics;
 - the Hamiltonian-unitary character of the time evolution, and related relativities; etc.
- The validity of these laws was established for the electromagnetic interactions beyond any reasonable doubt. The same laws were subsequently assumed as valid for the different physical arenas of the strong interactions without any direct tests until recently.

It is public knowledge that your Office is in the process of funding truly expensive additional experiments on strong interactions, again based on the belief of the validity of the basic laws.

This situation is reason of sincere concern to us as well as to numerous observers throughout the Country. In fact, it has been an historical pattern to test first the basic physical laws, and then consider applications and secondary particularizations. The preservation of this sound administration of Science for the strong interactions appears recommendable, particularly when the experiments under consideration for support imply truly large public funds.

(2) Some administrative implications. It has been established in the technical literature beyond any reasonable doubt that, in case the basic laws needs even a small revision, the actual, final experimental results are different. Modifications of the basic laws therefore have clear and substantial administrative implications.

Until recently, the scientific community was relatively quiet on the problem of the basic laws, and I personally see no reasons to consider the past. However, more recently there have been truly numerous and authoritative voices of doubts on the validity of the basic laws, as I shall indicate below. They are too many, too qualified, and too convincing, experimentally, theoretically, and mathematically to be ignored. Thus, the situation today is, from an administrative profile, basically different than only a few months ago.

Scientific accountability in the use of Public funds clearly calls for due consideration of these doubts in the current operation of DOE. In fact, if the experiments currently under way in Foreign Countries confirm these doubts, your funding of large experiments on strong interactions based on the old laws clearly acquires questionable tones.

(3) Additional administrative implications for the controlled fusion. Once the technical jargon is removed, the controlled fusion boils down to the laboratory construction of bound states of nucleons. The control of such phenomenon is crucially dependent on the basic physical laws. For instance, if the magnetic moments of nucleons are altered in the transition from the conditions under which they have been tested until now (long range electromagnetic interactions), to the different physical conditions of short range nuclear interactions, this implies a departure from the prediction of orthodox theories in magnetic confinement.

On administrative grounds, an alteration of the magnetic moment would therefore render a virtual waste all investments on magnetic confinement, in the sense that the actual, serious, credible, physical outcome would not be proportionate to the investment. A similar situation occurs for virtually all other administrative aspects, e.g., those for inertial confinement, etc.

This is no surprise. The basic physical laws are truly fundamental, and therefore they call for a corresponding primary administrative consideration.

(4) Some theoretical reasons for violation. The only possibility for the rotational symmetry to be exact under strong interactions is when, first, the charge distributions of a hadron is absolutely rigid and admits no deformation whatsoever, for whatever impacts and collisions with other charge distributions. Second, when a number of additional restrictive conditions are met (strict potentiality of the forces, etc.). The verification of all such primitive conditions under strong interactions is remote.

The most natural physical situation is that the charge distribution of hadrons experiences deformation under sufficient impacts and other conditions. The only debatable aspect is the amount of deformation which is permissible under given conditions.

However, deformations of the charge distributions necessarily imply the breaking of the rotational symmetry, as well as, clearly, alterations of the intrinsic magnetic moments (these latter alterations were conjectured in nuclear physics some decades ago, but later ignored for apparent reasons of scientific politics).

Furthermore, the breaking of the rotational symmetry necessarily implies that of the T- and P-symmetry, clearly, because the T- and P-operators have a crucial dependence on spin. This essentially means an irreversible particle dynamics.

Again, this is not surprising. Inspection of our environment establishes beyond reasonable doubts that irreversibility in macroscopic systems occurs via rotationally noninvariant orbits. The argument above is a mere reduction to particle levels. But the technical implications for the entirety of the strong interactions are truly vast. Equally vast are therefore the administrative implications.

But these are only the crudest arguments suggesting violation. The literature contains a diversified litany of numerous arguments, all rendering the violation quite natural. In particular, deviations in nuclear physics are expected to be small for certain physical laws (say, rotation and Pauli's Principle) but truly large for others. (For instance 1% deviation on rotation implies over 50% deviation on magnetic moment, and an even bigger deviation on reversibility, as you can read in the technical literature.

(5) Some experimental evidence for violation. The problems under consideration here have been studied at the

WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS

which I initiated in 1978 while I was at the Lyman Laboratory of Harvard and continued on a yearly basis under your support. You will recall that our first meeting was attended by "31" participants, including myself. The second was attended by "32" scientists (the topic was still too advanced). The third was attended by "33". The fourth (of 1981) was already sufficiently well known that we had to contain participation to "33".

Owing to the results of these meetings, a formal conference was organized under the title

FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR
LIE-ADMISSIBLE TREATMENT

which was held at the Université d'Orléans, France, from January 5 to 9, 1982, under support from the French Government, with about 5% support from DOE via my grant (note that the expenses of my invited lecture were paid with French funds-the support was for other members of our group).

Some 100 experimentalists, theoreticians, and mathematicians from virtually all developed Countries attended this formal meeting on the basic laws (NOTE: nonpotential interactions necessarily violate conventional laws, in fact, they are nonunitary in their time evolution and, thus, irreversible and rotationally noninvariant). The proceedings are currently in print. They comprise some 60 papers divided into three volumes for over 1,500 pages.

Official convoys from major laboratories were present. I have separately reported to you all names of physicists from Eastern Countries, with particular reference to the official convoy from the JINR of Dubna, USSR, and from Peking University.

Regrettably, no representative from major U.S. laboratories were present, particularly from those supported by DOE. This is regrettable, because I had personally solicited the participation by at least an observer via direct letters to the Laboratory Directors (Drs. Vineyard, Lederman, Panofsky, and others). Also, I repeatedly invited you to send a representative from your Office, and you apparently received repeated invitations directly from France. Our insistence was due to our sincere desire for your Office to be informed as much as possible, because of the clear administrative implications. The formal Conference at Orléans was the very best opportunity to do so, and it was unfortunate that your Office missed it.

It is impossible for me to outline the results of the Orléans Conference. I feel obliged however to state, in a way to remain in your formal file, that the experimental information presented at the Conference by distinguished experimentalists in favor of the violation is such that its ignorance can only create huge administrative problems. This letter is to suggest that you take this situation in all due account in all your future funding of the strong interactions, experimentally and theoretically.

First, the violation of the P-symmetry in nuclear physics is well established since years, as you know. We therefore only reviewed it in a marginal way.

Second, Professor Slobodrian (Quebec) and Conzett (Berkeley) reported their experimental collaboration according to which the time reversal symmetry is violated in nuclear physics. The experimental information is already sufficiently detailed to identify the origin of the breaking in the spin symmetry, as clearly stated by the experimentalists in their presentation as well as their recent paper in Phys. Rev. Letters. Other experiments (such as a rudimentary attempt at Los Alamos) were studied in details and dismissed for numerous technical reasons you may see in the proceedings. Yes, the discovery appears to be final: irreversibility originates at the elemental level of Nature. After all, this solution may be contrary to financial interests of a number of physicists, but it is the most logical and natural one.

Third, Professor Rauch (Director of the Atominstut of Wien) reported the status of our knowledge on direct, credible measures on spin under strong interactions (no mumbo-jambo theoretical assumptions of quark type in the data elaboration, but only serious measures). He made it very clear that the numbers are not final at this time. But, he made it equally clear that the currently available numbers FAVOR THE VIOLATION. In fact, the direct measures of spin his group has conducted produce a value which is 1% BELOW that predicted by the orthodox physicist assuming a perfectly rigid charge distribution. In addition, Professor Rauch reviewed the recent best measures on neutron-tritium scattering (of 1981) and indicated the existence of a region in which

".... a partial violation of Pauli's principle can be assumed."

(see enclosures).

There is no need for me to provide any additional information. That above is per se so substantial to deserve the best administrative consideration.

To give an idea of what is going on, I would like to add that, following the Conference, special meetings at very high levels were conducted in France. These meetings resulted in the formal, written recommendation to Stockholm for Professor Slobodrian and Conzett to be Candidate to Nobel Prize for 1982. I cannot disclose confidential Foreign material, but I enclose copy of my personal recommendation to the Nobel Committee.

(6) Our past DOE support. All the studies reported here were initiated with DOE support and have been conducted with DOE support ever since. In fact, the studies were initiated with grant ER-78-S-02-4742.A000 (1978 while I was at Harvard), and were continued in the subsequent years with grants AS02-78ER-4742 (1979 also while at Harvard), DE-AC02-80ER10651 (1980) and DE-AC02-80ER10651.A001 (1981).

The utmost dominant reason for these studies is the experimental resolution of the basic laws under strong interactions. The results are too numerous to be indicated here. In fact I have personally written a number of research monographs with Springer-Verlag, and too numerous articles to remember without consulting my file. In addition, I have supported as many researchers in the problem of the basic laws as possible.

At the inauguration of the Orleans Conference, soon after the Opening by the President of the University, a formal acknowledgment to DOE was pronounced, and resulted in a lasting ovation.

(7) Our pending research application. We believe that the resolution of the problem of the basic laws for the strong interactions one way or another is unprocrastinable. Permit me the liberty of being as candid as possible on this point. In fact, if the Senate and the general public at large become aware of the existence and administrative magnitude of the problem, the implications can be substantial. After all, you do not have to be a physicist to see that the test of the basic laws is more, much more important than routine work.

The only debatable issue, is how to continue the research. At this point your Office must be realistic. At our Institute we have assembled ALL the true experts in the problem I am referring to a coordinated and organized group of mathematicians, theoreticians, and experimentalists in well over 15 different Countries. By comparison, other Institutions, such as MIT, SLAC, FERMILAB, BNL, etc. have remained substantially behind, because of their rather stubborn resistance to a type of research only superficially against their academic-financial interests. At any rate, the technical literature is now well over 5,000 published pages, and it will take years for other physicists to digest it and become true experts (selfproclaimed/corrupt/experts-referees can be made in minutes).

The most effective way to continue the research is via its originators, that is, our new Institute for Basic Research.

At this point, permit me the liberty of conveying another aspect, as clearly as possible. Until now, we have initiated, conducted, and launched this new scientific current WITH TRULY MINIMAL DOE SUPPORT. In fact, our average yearly support has been of about \$ 60K (sixty thousand dollars) per years. We can certainly continue with funds of such minimal magnitude, BUT I DO NOT THINK THAT SUCH A CONTINUATION WOULD BE IN THE BEST ADMINISTRATIVE INTEREST OF DOE because it could create administrative priority imbalances.

For these reasons we have suggested in our application a very moderate increase of support for the specific purposes of

- funding the position of Director of Our Institute who should coordinate and initiate contacts with other DOE-supported laboratories and institutions;
- funding one research associate for the theoretical formulation of experiments; and
- funding one research associate for the experimental profile.

This program can be implemented with an ultimate minimum of \$ 150K (one hundred and fifty thousand dollars). This is due to our very low overheads (some 30% of salaries and wages) which is permitted by the voluntary assistance of Institute's members and their

spouses.

We are fully aware of the difficulties of your position, and of the complexity of the administrative choices. Nevertheless, we believe that it is time to set priority guidelines.

In this respect, permit me to recommend, respectfully, but as warmly as possible, that administrative priority be given to truly fundamental fields of research, and secondary attention be given to fields of secondary physical relevance. This has been the golden administrative rule which has permitted throughout the history of physics the achievement of truly fundamental advances while meeting the highest possible standards of scientific accountability vis-a-vis with the taxpayer. I beg you not you change it now in the interest of the Country as well as of DOE.

More particularly, I urge you to exercise extreme caution before committing large public funds to expensive experiments on minute technical details based on the belief of the validity of conventional laws. The doubts on the validity of these laws are now well established knowledge throughout the world. Their ignorance, rather than avoiding responsibilities, can only multiply them.

If I or any member of our group can be of assistance to you and your Office in identify some technical implications of the problem of the basic laws in the proposals under consideration for funding, please let us know.

Very Truly Yours



Ruggero Maria Santilli
President
RMS-mlw

cc.: Honorable JAMES B. EDWARDS, Secretary, DOE

Dr. A.W. PRIVELPIECE, Director, Office of Energy Research, DOE

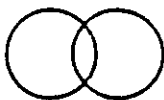
Dr. E.E.KINTNER, Ass. Director, Office of Energy Research, DOE

Dr. B. HILDEBRAND, Division of High Energy Physics, DOE

Dr. R. THEWS, Division of High Energy Physics, DOE

and

The Board of Governors, IBR



— 831 —

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

Professor Ruggero Maria Santilli, President

February 22, 1982

Honorable JAMES B. EDWARDS
Secretary
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20585

Honorable Mister Secretary,

Permit me the liberty of recommending, most respectfully, that you consider:

- (1) your personal supervision of the decision regarding our case as per enclosed formal letter to Dr. Wallenmeyer of same date;
- (2) your personal supervision in the possible funding of large, expensive experiments on strong interactions under the current conditions of mere belief of the basic physical laws, with consequential realistic possibility of invalidation of the totality of funding or most of it;
- (3) the advisability of informing President Reagan personally in case of any questionable administrative priorities or funding.

On my part, I have not, and I do not contemplate informing President Reagan of the situation without prior consultation with you. In the final analysis, I am confident of the selfcorrecting administrative capabilities at DOE which deserve all necessary time and consideration.

Nevertheless, I suggest remaining on the alert. The scientific scene is in a very delicate and tense moment. We are facing possible real questions of scientific accountability investing billions of taxpayer's money, on one side, while, on the other side, there could exist at a number of academic institutions and laboratories a real disrespect of national interests for genuine technological advancements because of truly excessive and untolerable personal greed of physicists in administrative control. A situation of this type is indeed explosive and should be monitored in my view.

Speaking on personal grounds, you can rest assured that I am seriously committed to the priority general interests of the Country even in disrespect of my personal interests, if necessary. You should therefore expect from me nothing but a behaviour as responsible as possible. The understanding, however, is that I see an equal commitment at DOE.

Best Personal Regards

Ruggero Maria Santilli
President
MRS-mlw

cc. The Board of Governors, IBR



Department of Energy
Washington, D.C. 20545

FEB 24 1982

Professor Ruggero Maria Santilli, President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letter of February 11 concerning the status of your proposal "Theoretical Studies on Lie-Admissible Formulations". External review of this proposal has been completed, and it is currently undergoing an internal review in preparation for a final decision. The comments in your letter will be considered in our review, and you can expect to be notified of a decision shortly.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics



Department of Energy
Washington, D.C. 20545

MAR 2 1982

Professor Ruggero Maria Santilli, President
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letter of February 22 with enclosed material relative to your proposal, "Theoretical Studies on Lie-Admissible Formulations". This material will be included as part of your proposal and considered in our review process.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics



Department of Energy
Washington, D.C. 20545

MAR 24 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Reference is made to the proposal submitted by The Institute for Basic Research for support of a research program entitled "Theoretical Studies on Lie-Admissible Formulations" to be conducted under your direction.

This proposal has undergone a series of external peer reviews as well as an internal DOE High Energy Physics review. The review included considerations of the recent information supplied in your letters to me of February 11 and 22, 1982.

We have carefully considered this proposal and supplementary material in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

A handwritten signature in dark ink, appearing to read "Wm A Wallenmeyer", written over a horizontal line.

William A. Wallenmeyer
Director
Division of High Energy Physics



Department of Energy
Washington, D.C. 20585

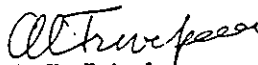
MAR 11 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

This will acknowledge receipt of your letters of February 11 and 22, 1982. Your proposal, "Theoretical Studies of Lie-Admissible Formulations" is currently undergoing a technical review in the Division of High Energy Physics. I am informed that the additional information you forwarded via your letters of February 11 and February 22, 1982, to Dr. Wallenmeyer is also being considered prior to a final decision. The Division of High Energy Physics will inform you of a decision shortly.

Sincerely,


Alvin W. Trivelpiece
Director, Office of
Energy Research



Department of Energy
Washington, D.C. 20545

MAR 24 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Reference is made to the proposal submitted by The Institute for Basic Research for support of a research program entitled "Theoretical Studies on Lie-Admissible Formulations" to be conducted under your direction.

This proposal has undergone a series of external peer reviews as well as an internal DOE High Energy Physics review. The review included considerations of the recent information supplied in your letters to me of February 11 and 22, 1982.


We have carefully considered this proposal and supplementary material in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

A handwritten signature in dark ink, appearing to read "Wm A. Wallenmeyer".

William A. Wallenmeyer
Director
Division of High Energy Physics



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

Office of the President

March 29, 1982

Dr. WILLIAM A. WALLENMEYER
Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON D.C. 20545

RE: Applications entitled:
"Theoretical Studies on Lie-admissible Algebras"
under IBR administration

Dear Dr. Wallenmeyer,

Please be reassured that I accept with grace and respect
the final negative decision by your office on our application.
This is the result also of the several conversations with you
and Dr. B. HILDEBRAND, and of the alternative interim solution
that has emerged.

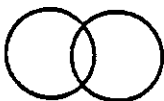
Thank you for your courtesy, consideration, and time.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-mlw

cc.: Honorable JAMES B. EDWARD, Secretary
Drs. A.W. PRIVELPIECE, E.E. KINTNER, B. HILDEBRAND, and R. THEWS, DOE
and
The Board of Governors, IBR



— 838 —

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

Office of the President

March 29, 1982

Dr. WILLIAM A. WALLENMEYER
Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
WASHINGTON D.C. 20545

RE: Applications entitled:
"Theoretical Studies on Lie-admissible Algebras"
under IBR administration

Dear Dr. Wallenmeyer,

Please be reassured that I accept with grace and respect
the final negative decision by your office on our application.
This is the result also of the several conversations with you
and Dr. B. HILDEBRAND, and of the alternative interim solution
that has emerged.

Thank you for your courtesy, consideration, and time.

Very Truly Yours

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-mlw

cc.: Honorable JAMES B. EDWARD, Secretary
Drs. A.W. PRIVELPIECE, E.E. KINTNER, B. HILDEBRAND, and R. THEWS, DOE
and
The Board of Governors, IBR



Department of Energy
Washington, D.C. 20545

APR 22 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

As per your request, enclosed are comments from reviewers of your proposal "Theoretical Studies on Lie-Admissible Formulations."

Sincerely,

A handwritten signature in cursive script, reading "Robert L. Thews".

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

Enclosures
As stated

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 1

Santilli is very knowledgeable on various modern mathematical methods. He is very methodological in search for literatures. He is certainly very competent in what he is working on.

It is rather difficult to state the significance and the merit of the proposal in terms of standard criterias, since the subject matter is not that of a main stream of current research activity in high energy physics. It may be even likely that many high energy physicists will not regard it as a branch of high energy physics. However, a nagging suspicion at least on my part (which may not be shared by many others) is that some parts of ideas, especially that of the use of Lie-admissible algebras instead of the conventional Lie-algebras may turn out to be very relevant to high energy physics some day soon. Also, I should mention that I do not agree with many views expressed by Santilli especially on unreality of quarks as well as possible violation of Lorentz covariance in strong interactions although he may turn out to be right. Nevertheless, the idea of Lie-admissible algebras is novel and intriguing, which may blossom in time to come. Together with many Workshops on the problem organized by Santilli in recent years, he is greatly contributing to development of this branch of activities which are very much ignored nearly by the majority of physicists. In this sense, the proposal has a merit and significance.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 2

I have not really followed the earlier work of the proposer on his Lie-Admissible approach, and so cannot pass on its intrinsic competence. My feelings are that this avenue is rather formal in nature and not in the mainstream of current research. With any one unorthodox idea, there is little likelihood that it will succeed and it is difficult for me to be sympathetic to a proposal of this magnitude when so much worthwhile research is going unfunded. I also believe that the experimental reasons advanced against the body of "conventional" theory leave something to be desired as well. Finally, the conferences to be sponsored, like past ones, seem devoted to exchange of ideas among the "faithful" or to try to extract support from the statements of attending "outsiders".

In summary, I feel that this proposal should not be funded on both scientific and fiscal grounds.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 3

The applicant has developed a branch of classical mechanics. He did this work first on his own, and then in collaboration with other mathematicians. I have the impression that his derivations are sound. The work demands an extensive knowledge of mathematical physics.

I believe this work could be of some practical use, although the physical research projects he suggests to pursue do not appear to me very promising on the whole. His notions like supraluminal velocities appearing in conjunction with strong interactions are not defined very carefully and are too immature to be useful.

I consider that this proposal would merit some support because something useful may come out, but I do not believe it will be very important for physics and would not give it a high priority.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 4

His typical paper surrounds with clouds of irrelevant mathematics an argument either vacuous or based on elementary errors. He has contributed nothing to the progress of science in the past and I firmly believe he will contribute nothing in the future.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 5

There are two essentially distinct aspects to this proposal. The first is a program of research in generalized mechanics, and is mathematical in nature. The second is an attempt to tie the proponent's mathematical ideas to possible new phenomena in particle physics and to possible implications for applications such as nuclear fusion research. I recommend strongly against further DOE support of either line of work. The research in generalized mechanics may be useful, but if supported by a U.S. agency it should be only after peer review by, and competition with, mathematicians doing work in related areas. I do not think that the Division of High Energy Physics of the DOE can give this aspect of Santilli's work the kind of scrutiny it requires. As to Santilli's attempt to test the fundamental laws, doing this in a useful way requires both experience with phenomenology and the ability to be careful and objective in handling experimental data. I do not think that Santilli has the needed experience or objectivity to carry out such studies. His comments on water waves on p. 6 suggest that he is not familiar with the important distinction between phenomenological Lagrangians (which usually are nonlocal) and fundamental Lagrangians, which is basic to much of what is being done now in high energy theory. His attribution on p. 24 of significance to the shift in the Rauch data from $716.8 \pm 3.8^\circ$ to $715.87 \pm 3.8^\circ$ is reading a great deal into a change of $1/4$ of a standard deviation in a one standard deviation "effect!" Is he aware of how many two or three standard deviation "discrepancies" have come, and gone, in the process of refining the tests of QED? His claim on page 19 that his work, and the small effects he would like to find, are relevant for fusion research are not substantiated either in the proposal or the cited article. They seem highly implausible, given the availability of experimental data on nuclear properties, and especially because the parameters needed for controlled fusion are typically measured on a logarithmic scale, and are not sensitive to tiny effects which can only be measured (if at all) in precision experiments.

Theoretical Studies on Lie-Admissible Formulations

Institute for Basic Research

Reviewer 6

I have read the proposal by R.M. Santilli entitled "Theoretical Studies on Lie-Admissible Formulations." In my opinion it is a non-proposal. Almost all of it is a Madison-avenue like self advertisement. After that comes about half a page in which the principal investigator asks for support to continue doing what he has been doing. I find nothing explicit in the proposal to evaluate.

In the self advertisement portion of the proposal, Dr. Santilli describes possible applications of Lie admissible systems to high energy physics. I think such applications are highly unlikely, but perhaps physicists would be in a better position to judge its applicability.

Because the document says "support me to continue my research," I went back and read some of the mathematical portions of earlier papers reporting on research supported previously by D.O.E. I found nothing exciting. A Lie admissible system is a non-associative algebra in which Lie bracket gives a Lie Algebra, i.e., the Jacobi identity holds. (The idea goes back to A.A. Albert.) Many such algebras can be obtained from associative algebras in specified ways. Dr. Santilli and his associates study such algebras. In my opinion, nothing important has emerged, even in regards to classical symplectic manifolds, Hamiltonians, etc.

If this were a mathematics proposal, I would give it a low rating. The merit of the proposal can only be its possible application to physics. Since that is implausible to me, I rate this proposal very low. It's a high risk venture with minimal payoff, especially not worth funding in these days of stringent funding.

PART XXIII:

REJECTION OF A

SECOND, PRIMARY, GROUP

PROPOSAL OF THE I.B.R.

BY THE

NATIONAL SCIENCE

FOUNDATION

AND THE

DEPARTMENT

OF ENERGY

Research Grant Proposal
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

STUDIES ON HADRONIC MECHANICS

Proposed Starting Date	Proposed Duration	Amount Requested
March 15, 1983	5 years	\$ 835,250

ENDORSEMENTS

R. M. SANTILLI

Principal Investigator and President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Soc. Sec. No. 032 46 3855
tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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5. H. C. MYUNG and R. M. SANTILLI, Foundations of the Hadronic Generalization of the Atomic Mechanics, II: Modular-Isotopic Hilbert Space Formulation of the Exterior Strong Problem, Hadronic J. 5, 1277-1366 (1982)	93
6. H. C. MYUNG and R. M. SANTILLI, Foundations of the Hadronic Generalization of the Atomic Mechanics, III: Bimodular-Isotopic Hilbert Space Formulation of the Interior Strong Problem, Hadronic J. 5, 1367-1403 (1982)	139

7.	G. EDER, Lie—admissible Spin Algebra for Arbitrary Spin, and the Interaction of Neutrons with the Electric Field of Atoms, Hadronic J. 5, 750–770 (1982)	158
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10.	R. J. SLOBODRIAN, C. RIOUX, R. ROY, H. E. CONZETT, P. VON ROSSEN, and F. HINTERBERGER, Evidence of Time–Symmetry Violation in the Interactions of Nuclear Particles, Phys. Rev. Letters 47, 1803 (1981)	174
11.	R. M. SANTILLI, Use of the Hadronic Mechanics for the Fit of the Time–Asymmetry recently Measured by Slobodrian, Conzett, et al, IBR preprint April 1982	179
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14.	Y. TOMOZAWA and S. K. YUN, Incorporation of CP Violation with a Unified Renormalizable Gauge Theory, Phys. Rev. 11D, 3018 (1975)	205
15.	S. K. YUN, New Mass Relations and Mixing Angles in an SU(5) Model of the Electroweak–Strong Interaction, Phys. Rev. 21D, 2687 (1980)	213
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ABSTRACT

The studies proposed in this application constitute a new phase of research conducted since 1978 under DOE support by a coordinated group of mathematicians, theoreticians, and experimentalists. A main objective was the identification of methods for the treatment of extended particles with action-at-a-distance/potential as well as contact/non-potential forces.

The studies were initiated at Harvard University (1978–1980), were continued thereafter at the IBR (1980–1982), and resulted into: (a) the development of a generalization of Lie's theory based on the so-called isotopies and genotopies of the envelope, which is structurally more general than the graded/supersymmetric extensions; (b) the construction of the so-called Birkhoffian generalization of the Hamiltonian mechanics for the treatment of all local, analytic, nonpotential systems; and (c) the identification of the rudiments of a conceivable generalization of atomic mechanics (the ordinary quantum mechanics) specifically conceived for strong interactions and called hadronic mechanics. The new mechanics is physically motivated by the representation of hadrons as extended objects, and mathematically suggested by the operator image of the classical, Birkhoffian realization of the generalized Lie's theory.

This proposal recommends the conduction over a five year period of a comprehensive research on the hadronic mechanics by a coordinated group of theoreticians, under the assistance of experimentalists and mathematicians, with particular reference to the following aspects.

(I) **FOUNDATIONAL STUDIES**, including: finalization of the isotopies and genotopies of the Hilbert space, quantum postulates, and basic principles of the hadronic mechanics; finalization of the quantization procedures from the Birkhoffian to the hadronic mechanics; finalization of the hadronic generalization of the atomic (unitary and antiunitary) symmetries; identification of the hadronic image of the isotopic generalization of Galilei's relativity achieved during the first phase of studies for classical closed systems with nonpotential internal forces; etc.

(II) **APPLICATIONS TO EXPERIMENTAL DATA**, including: data elaboration of the following experiments in nuclear physics (a) established breaking of the atomic parity; (b) apparent breaking of the atomic time-symmetry; and (c) apparent breaking of the atomic rotational symmetry (deformation of extended charge distributions under contact interactions); interpretation of the variation of (a), (b) and (c) from nuclei to nuclei; proof of the validity for breakings (a), (b), and (c) of the covering, hadronic, isotopic unitary and antiunitary symmetries; proof of the compatibility of these nuclear settings with gauge theories on leptonic decays; etc.

(III) **APPLICATIONS TO QUARK THEORIES**, including: proof that spontaneous symmetry breakdown is a particular case of the isotopic generalization of Hilbert spaces; application of the hadronic mechanics to the construction of quarks as composite systems of more elementary particles (representations on a bimodular Hilbert space); use of conventional atomic mechanics for the exterior treatment and of hadronic mechanics for the interior one to achieve a strict confinement of quarks (identically null probability of tunnel effects); etc.

As it occurred for the first phase of studies (1978–1982), the proposed second phase (1983–1988) is expected to imply the organization of a number of conferences, and to result in the publication of a number of papers, conference proceedings, and research monographs.



I. B. ⁸⁵¹ R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

September 28, 1982

Dr. WILLIAM A. WALLENMEYER
Director (ER-22)
Division of High Energy Physics
U.S. Department of Energy
19901 Germantown Road
GERMANTOWN, Maryland 20874

FEDERAL EXPRESS

Dear Dr. Wallenmeyer,

We hereby submit for consideration by your Division of the DOE the original, duly signed copy of a research grant proposal entitled

STUDIES ON HADRONIC MECHANICS.

Eight additional copies of the proposal have been separately mailed to you.

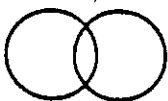
We shall remain at your disposal during the consideration of the proposal for any additional assistance you may need.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator and
President

RMS—mlw

cc.: Drs. B. HILDEBRAND and R. THEWS, DOE



- 852 -

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

September 25, 1982

Professor S. PETER ROSEN
Program Associate
Theoretical Physics Program
Division of Physics
NATIONAL SCIENCE FOUNDATION
1800 G Street
WASHINGTON, D.C. 20550

FEDERAL EXPRESS

Dear Professor Rosen,

We hereby submit for consideration by your Division of the NSF the original, duly signed copy of a research grant proposal entitled

STUDIES ON HADRONIC MECHANICS.

Fourteen additional copies of the proposal have been separately mailed to you.

We shall remain at your disposal during the consideration of the proposal for any assistance you may need.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator
and President

RMS-miw



— 853 —

Department of Energy
Washington, D.C. 20545

OCT 6 1982

Professor Ruggero Maria Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

The research proposal entitled "Studies on Hadronic Mechanics" submitted on your behalf by the Institute for Basic Research has been received in the Division of High Energy Physics

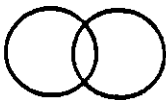
This proposal is now under review and as soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

Budget pages 60e, 60g, 60i, and 60m do not add correctly and thus the 5 year amount requested also requires correction. Please send this office corrected budget pages.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: 



I. B. R. 854

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 1, 1982

Dr. W. A. WALLENMEYER, ER-22
Director
Division of High Energy Physics
U. S. DEPARTMENT OF ENERGY GTN
WASHINGTON, D.C. 20545

RE: Applications entitled
STUDIES ON HADRONIC MECHANICS
Principal Investigator: R. M. Santilli

Dear Dr. Wallenmeyer,

I am contacting you to encourage the most comprehensive possible review of the proposal. It is my understanding that the proposal has already been sent to referees. Nevertheless, permit me to provide you with an additional list of experts in the field who are fully knowledgeable of my research. Additional copies of the proposal are at your disposal on request. At your discretion, the referees selected by your office should feel free to contact some of the experts in the field indicated in the enclosed list, in case technical advice on specific aspects is needed. In this letter, I provide you with some general information on the diversification of refereeing which appears to be needed for a serious review of the proposal.

ADDITIONAL MATERIAL. I have separately mailed to you:

- copy of the galleys of my second volume with Springer-Verlag entitled *Birkhoffian Generalization of Hamiltonian Mechanics*; and
- copy of the four volumes of proceedings of the Orleans International Conference on Nonpotential Interactions.

I would appreciate the courtesy of considering this material an integral part of the proposal. In fact, the volume on Birkhoffian mechanics constitutes the classical foundations of the proposal. It is evident that no mature judgment can be reached without at least some knowledge of this rather vast new field. Similarly, the proceedings of the Orleans Conference deal directly with the topic of the proposal and present the current state of the art in the experimental, statistical, and particle profiles of the project. Again, some (even minimal) knowledge of these proceedings is essential in order to avoid the venturing of personal feelings by the referees, rather

than technical reviews.

EXPERIMENTAL ASPECTS. I have separately listed as referees three leaders of experimental teams who are all familiar with the basic tests of the central ideas of the hadronic mechanics, as well as of the theoretical studies by our group. It appears recommendable that these experimentalists be consulted prior to reaching a final judgment on the proposal.

STATISTICAL ASPECTS. I have presented an additional list of statisticians, all experts in the relationship between irreversibility and nonpotentiality. Even though the proposal does not deal directly with statistical mechanics, the consultation of these referees appears recommendable. In fact, a primary motivation of the construction of the hadronic mechanics is to achieve compatibility and unity of thought between the experimentally established irreversibility and noncanonicity of the macroscopic physical reality, and particle mechanics. For a mature refereeing of the proposal, it is important that you consult statisticians who have an established record of scientific research in the problematic aspects underlying the unfulfilled dream of reconciliation of the current Lagrangian-Hamiltonian models in high energy physics, and the real world in our environment.

MATHEMATICAL ASPECTS. It is evident that maturity of judgment also calls for an inspection of the mathematical structure of hadronic mechanics, if nothing else, because of its novelty (isotopy of Hilbert space). A list of senior mathematicians all experts in the field has been enclosed.

PARTICLE ASPECTS. This is, of course, the central part of the proposal. A list of experts with an established record of contributions in the field is enclosed. Permit me to stress that, in our view, a referee is qualified for the physical review of the proposal if and only if he/she has a scientific record of PAPERS DEALING SPECIFICALLY WITH NONLAGRANGIAN-NONHAMILTONIAN INTERACTIONS. Otherwise, it would be the same as sending a proposal, say, on quarks, to referees without any record whatsoever of active research on quarks.

As a specific example, Professor S. Okubo has a rather extensive record of publications in the mathematical studies of Lie-admissible algebras. However, he has not published one single paper in their physical applications to contact/nonpotential interactions, nor it appears that he is knowledgeable of this vast new field. As a result, Professor Okubo would qualify as an excellent referee of the mathematical review of the proposal BUT NOT FOR ITS PHYSICAL PART.

I leave it to your judgment, of course, to consult referees without an established technical record in the field of the proposal. However, the understanding is that they may express, at best, personal feelings on the proposal, rather than professional reviews.

REJECTION OF REFEREE REPORTS. In the past, I have at times received negative referees' reports on research grant applications or on research papers without any technical contents whatsoever, or even with offensive language. Reports of this

type are generally more damaging to the institution that accepts them, than to the refereed person.

The topic of the proposal deals with a truly innovative project, the possible construction of a generalization of quantum mechanics. As such, the project may stimulate all sort of emotional attitudes, which, in turn, may result in reports potentially damaging to your office.

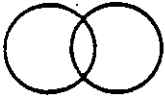
Permit me the liberty of recommending, most respectfully, that reports are inspected for scientific contents and value exactly as it is the case for the proposals, and that reports which are questionable on grounds of scientific ethics be rejected and returned to the referee.

Best Personal Regards,

A handwritten signature in black ink, appearing to read 'R. Santilli', written in a cursive style.

Ruggero Maria Santilli

cc. Drs. B. Hildebrand and R. Thews, DOE



I. ⁸⁵⁷ 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 23, 1982

Dr. M. BARDON
Director
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Bardon,

I would appreciate the courtesy of the consideration whether a decision on our main proposal entitled

STUDIES ON HADRONIC MECHANICS, NSF Ref. No. PHY-8300195

can be reached in early January 1983. However, if this is not possible for any reason, please be reassured of our full understanding.

The reasons are due to our short term forecasts. In fact, our current financial support (a \$ 40K contract with DOE) will be exhausted by March 1983. Lacking a decision by early January 1983, we will be forced to seek alternative financing of our research programs beginning from the second half of January, evidently, in order to have sufficient time prior to the deadline of March 1983. In particular, we would like to do our best to avoid the search of alternative forms of financing because of understandable, potential conflicts with an ongoing NSF consideration.

Thus, a decision by early January, whether positive or negative, would be ideal on our part, although, again, I do not know whether it is realizable from your profile. All our NSF applications have been submitted jointly to DOE, and a similar letter has been written to Dr. Wallenmeyer, Director of the DOE High Energy Physics Division.

I would like to take the opportunity of bringing to your attention the possibility that your Division considers an institutional support of the I.B.R., which would include our main proposal indicated above, as well as others already submitted or in the process of being submitted.

I assume you are aware of the fact that the research programs for which the I.B.R. was founded complement rather nicely those of other NSF institutionally supported entities, such as the Institute of Theoretical Physics at Santa Barbara. In fact, the research conducted under existing NSF support is based on the assumption of the exact validity of Einstein's special relativity for strong interactions. Our experimentalists, theoreticians, and mathematicians instead, are studying the possible need of suitable

corrections due to the extended character of hadrons, and the use of recently identified generalizations of Lie's theory beyond grading-supersymmetric extensions, called of Lie-isotopic and of Lie-admissible type.

The literature of this dichotomy is now rather vast, and estimated in the excess of 10,000 pages of printed research. To put it in a nutshell, you should first recall that conventional space-time symmetries, say, rotations, CANNOT be broken for a point-like charge, no matter what interactions you use. Point-like structures, however, are only a figment of academic imagination. In fact, hadrons have an extended size of the order of the range of the strong interactions. Once you abandon points and pass to the consideration of hadrons as extended objects, the following possibilities emerge, here expressed in nontechnical terms.

- I. The extended charge distributions of hadrons are perfectly rigid, in which case conventional space-time symmetries continue to be exact; or, a bit more realistically.
- II. The extended charge distributions of hadrons experience (small) deformations depending on the impact, interactions, and mutual penetration with other hadrons, in which case suitable correction to Einstein's special relativity must be theoretically identified and experimentally tested.

ALL current NSF support is along line I. The I.B.R. has been founded to explore alternative II. On administrative grounds we favor, of course, the continuation of primary support along line I; however, we believe essential for scientific accountability vis-a-vis the taxpayer, that NSF initiates funding also of alternative II, where at our institute or elsewhere.

On pure scientific grounds, the possibility of turning the I.B.R. into an NSF supported institution of the type existing at Santa Barbara would be ideal. In fact, this could maximize the interplay between the two alternatives in the sole interest of the pursuit of novel physical knowledge. The novelty, location, and flexibility of our Institute renders it particularly attractive. For instance, we still have open the position of Director, which could be filled by a scientist with sufficient qualifications to facilitate the flow of mutual exchanges with other NSF institutions.

Nevertheless, we beg you not to consider this latter alternative as necessary, and any other alternative considered appropriate by your office would be gratefully accepted by us.

However, permit me to express most respectfully but candidly, our fear that a continuation of the use of large public funds on the assumption of the exact validity of Einstein's special relativity for strong interactions, without a joint support for broader, potentially fundamental advances, may result to be excessively risky for the orderly condition of our community. At any rate, we are firmly convinced that the search of a suitable generalization specifically tailored for particles and interactions which were unknown at the inception of the special relativity, is necessary on scientific, economic, and military grounds.

I would like to take this opportunity to express to all of you at NSF our best wishes

for a Happy and Prosperous New Year.

Very truly yours,

A handwritten signature in dark ink, appearing to read 'Ruggero Maria Santilli', written in a cursive style.

Ruggero Maria Santilli
President and
Professor of Theoretical Physics

RMS/mlw

cc: Dr. EDWARD E. KNAPP, Director, NSF

Enclosures: Copy of title and abstract of proposals.

NATIONAL SCIENCE FOUNDATION
WASHINGTON DC 20550

January 10, 1983

Dr. R. M. Santilli,
President and Professor
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of 23 December 1982 concerning your proposal
PHY83-00195.

I understand that this proposal is presently being reviewed, but the
process has not yet been completed. I expect that it will be possible
to convey a decision to you by the end of January or soon after.

Sincerely yours,



Marcel Bardon
Director, Division of Physics

NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

MAR - 3 1983

Dr. Ruggero M. Santilli
Division of Physics
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138


Dear Dr. Santilli:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Studies on Hadronic Mechanics," PHY83-00195.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,


Marcel Bardon
Director, Division of Physics

Enclosures

FOUNDATION		PROPOSAL EVALUATION FORM ⁸⁶²		1B	NSF FORM X-3 ✓
PROPOSAL NO. PHY-8300195	INSTITUTION INST FOR BASIC RESEARCH			PLEASE RETURN BY 11/25/82	
PRINCIPAL INVESTIGATOR RUGGERO M. SANTILLI			NSF PROGRAM THEORETICAL PHYSICS <i>PR</i>		

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability):

This proposal is a continuation of the authors' project that has been going on for some time. It is difficult to make out what it really is, but basically it seems to be concerned with various nonstandard mathematical structures of dynamics that may be relevant to physics. In principle, this is not an unreasonable enterprise. But this very verbose proposal seems rather hollow inside. I fail to see any results that are remotely persuasive or inspiring to the physicists at large. The author quotes one experimental paper on time reversal violation as a support for his ideas, but that paper is now discredited. The merit of this proposal is extremely dubious or at least cast in serious doubt. Hardekopf et al., Phys. Rev. C25, 1090 (1982).

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for reviewers' comments will be given maximum protection from disclosure.

Review A

PROPOSAL NO. PHY-2300195	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 11/25/62
PRINCIPAL INVESTIGATOR RUGGERO M. SANTILLI		NSF PROGRAM THEORETICAL PHYSICS <i>PK</i>

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
quality of the proposed research (including budget & institutional capability):

The proposal is a continuation of Santilli's line of works in the past years. He claims that he and his collaborators have laid a mathematical foundation on which physics is ready to be built. I do not agree with him. In the past five years, he and his followers have produced no solid achievement worth mentioning. None of their papers, except for one, were published in regular refereed journals where most of major mathematical and physical works have been published. I do not count the Hadronic Journal as one of them; Santilli himself is an editor and because of its low quality, many of institutions including ours stopped subscription sometime ago. The only paper of theirs which managed to get into a regular journal was the one by Ktorides, Myung, and Santilli (reference 18), which was held back for more than a year before acceptance.

Since Santilli asserts that a referee should not quarrel with his works on the basis of the "rudimentary outline presented in the proposal", I will argue in general terms. His words are often quite alien to theoretical physicists, even when he speaks of physics. As long as the part that I can understand is concerned, works of Santilli are trivial, wrong, or no more than presentations of frameworks that he wishes to work. I consulted with a few of my colleagues in our Mathematics Department. Some of them laughed at, but some other kindly took some time to look into. Their reactions to the mathematics part are roughly the same as my reaction to the physics part.

If anybody makes a proposal for a research contract, one has the obligation to present his (or her) works in the past and future in a language common to a substantial segment of the physics community. Aside from that, I do not consider that Santilli has achieved a progress that is worth continued support by NSF.

I recognize only two names of theorists among those quoted by Santilli. They are Okubo and Biedenharn. The latter declined joining Santilli according to a copy of the letter. Others have practically no track record in physics research, to my knowledge. I do not believe that anything will come out of Santilli's collaboration with them.

Santilli's institution seems to be his one-man institute. I have little knowledge of its quality since it has no history.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Review B

FOUNDATION		PROPOSAL EVALUATION FORM ⁸⁶⁴		1B	NSF FORM X-3
PROPOSAL NO.	INSTITUTION			PLEASE RETURN BY	
PHY-8300195	INST FOR BASIC RESEARCH			11/25/82	
PRINCIPAL INVESTIGATOR		NSF PROGRAM			
RUGGERO M. SANTILLI		THEORETICAL PHYSICS		PR	

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability).

Often proposals such as this on an unconventional topic by an investigator not affiliated with a well-known institution are very difficult to review. The unfamiliarity of the topic makes a thorough technical review quite time-consuming. The absence of an established, reputable sponsoring institution eliminates the somewhat reassuring safeguard that the investigator's credentials have been seriously examined and approved by a responsible body. These difficulties are certainly present in this case.

However, as is not commonly the case, this research has been funded by the DOE for the past four years. The results of this DOE supported work appear to have been nil. It is this reviewer's opinion that the research activities of the principal investigator have had no impact on the development of theoretical physics during this period. It would be a mistake to continue funding this activity.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Review C

PROPOSAL NO. PHY-8300195	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 11/25/82
PRINCIPAL INVESTIGATOR RUGGERO M. SANTILLI	NSF PROGRAM THEORETICAL PHYSICS <i>PR</i>	

STUDIES ON HADRONIC MECHANICS

Comments (continue on additional sheet(s) as necessary):
 Quality of the proposed research (including budget & institutional capability):

In my opinion this is a poor proposal and should not be funded.

The principal investigator, R. M. Santilli, has a very poor reputation among mathematical physicists and elementary particle physicists. The papers of Santilli's which I have looked at contain a large amount of inflated language but not a single interesting or significant result. In a number of articles, for example, he discusses a generalization of quantum mechanics which he names "hadronic mechanics." In all the pages of discussion no physical application of any significance is presented, however, and no nontrivial theorem, original to Santilli, is proved.

Santilli's proposed projects 1-11 appear no more likely to yield significant results than his past work has produced. His suggestions seem more likely to lead to exercises in ~~formalism~~ than to real solutions to significant physical or mathematical problems. In some of these discussions Santilli shows himself to be quite ignorant of even the basic ideas of a number of areas of modern elementary particle theory.

In his reference to lattice gauge theories, in project 1, for example Santilli apparently believes that lattices have generally been suggested as real physical objects rather than, as actually is the case, as mathematical conveniences to be removed by a limiting process.

All in all I think this is a bad proposal and is not appropriate for funding.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

Review D



Department of Energy
Washington, D.C. 20545

APR 01 1983

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Your proposal entitled "Studies on Hadronic Mechanics" is still under active consideration for funding, and will be acted upon during the next 6-month period.

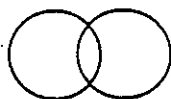
We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

A handwritten signature in cursive script, reading "Robert L. Thews".

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS



867
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

June 17, 1983

Professor Ruggero Maria Santilli, President

Dr. EDWARD KNAPP, Director
National Science Foundation
WASHINGTON, D.C. 20550

Dear Dr. Knapp,


I acknowledge receipt of the rejections of ALL applications submitted to NSF by senior members of our Institute since its founding. We are referring to four applications submitted to the Division of Physics and three to the Division of Mathematics. All applications were on coordinated mathematical, theoretical, and experimental studies on the apparent lack of exact character of Einstein's special relativity for strong interactions (as now indicated by a number of international Institutes, besides ourselves, all abroad). In particular, the applications dealt with quantitative studies that extended particles such as hadrons may experience (small) deformations under sufficiently intense external fields, with the consequential, manifest breaking of their rotational symmetry. The breaking of the Lorentz symmetry is then, under these circumstances, a known technical consequence.

For your information, some of the applications were rejected on ground of vulgarly offensive language written by manifestly corrupt referees (this was the case of some of the physical applications). Others were rejected despite the fact that the majority of the reports recommended funding quite warmly (this was the case of the applications by Professors ~~W. J. B. de Boer, J. J. de Boer, and J. J. de Boer~~, and by Professors ~~W. J. B. de Boer, J. J. de Boer, and J. J. de Boer~~ submitted to the Division of Mathematics).

The rejection of all these applications, therefore, is not the issue here. The issue is given by the premises leading to the rejections, as well as by the current lack of support at NSF of the problems addresses, despite (or because of) their truly fundamental character. Also, the processing of the applications did not stop short at the rejections, but implied additional, un-necessary damage to us. This was the case of the application by Professors ~~M. J. B. de Boer, J. J. de Boer, and J. J. de Boer~~ who hold a joint full professorship at other Institutions. Officers of the Division of Mathematics contacted these other Institutions, without any advance consultation with us, just prior to the rejection of the application, by therefore creating evident, totally un-necessary, personal problems. Another application was submitted back in November 1982 to the Division of Mathematics for support of a Workshop to be held in early August 1983. The rejection was kept for an un-reasonably long period of time, and was finally released because of my personal pressures on both the officer and the director of the division. The delay in the release of the rejection had the evident effect of preventing us from having sufficient time to seek alternative forms of funding, some of which may be incompatible with a consideration at NSF (e.g., when the use of scandalistic means is desired or rendered inevitable). Also, one referee of the primary physical application of our Institute (a group proposal for a coordinated mathematical-theoretical-experimental effort to generalize quantum mechanics for extended, and therefore deformable particles) included in the report statements to the effect that he/she: (a) had contacted one of the advisors of the project (Professor L.C. Biedenharn of Duke University); (b) had succeeded in pressuring him to withdraw from the project; and (c) had even secured a letter to the effect of documenting the withdrawal.

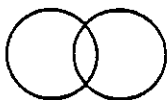
As indicated to you in preceding correspondence, individual members of our Institute are considering a national campaign aimed at having the Americal Physical Society formulate and adopt a much overdue CODE OF ETHICS, as well as having the judiciary and political systems create independent means for its strict enforcement. This letter is intended to give you and your officers all the necessary prior knowledge of the possibility that the totality of the documentation regarding our research grant applications, jointly with our individualized comments, of course, might be released to the appropriate committees of the U.S. Senate and House of Representative, as well as to the press. In case you and/or your officers have any objection to such a release, you should let us know immediately. However, in case no objection exist (or can be raised), no acknowledgement of this letter is needed.

Very Truly Yours


Ruggero M. Santilli

RMS—mlw

cc.: Drs. R.M. Sinclair, Acting Director, Div. of Phys.
E.F. Infante, Director, Div. of Mathematics
and
The White House



- 868 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

June 20, 1983

Professor Ruggero Maria Santilli, Presiden

Dr. R. THEWS
DOE, Division of Physics

RE: Main I.B.R. Application entitled
"STUDIES ON HADRONIC MECHANICS"
A group application involving experimentalists, theoreticians and mathematici
FINAL COMMUNICATION

Dear Robert,

The existence of the hadronic generalization of quantum mechanics for extended, and therefore deformable hadrons, has been proved by mathematicians experts of the symplectic geometry. A presentation will occur at our FIRST WORKSHOP ON HADRONIC MECHANICS this coming first week of August. In fact, quantum mechanics is the simplest possible realization of symplectic quantization, while hadronic mechanics is a more general one.

This result was inevitable from the generalizations of Lie's theory worked out by our mathematicians. The best way to put it physically is by indicating the identification of a suitable generalization of Einstein's special relativity for extended, deformable hadrons, as summarized in the enclosed paper in press at LETTERE NUOVO CIMENTO.

The dual Lie-isotopic/Lie-admissible structure for exterior-closed/interior open treatments is summarized in the enclosed additional note on the Lie-admissible structure of open nuclear reactions, also in press at LETTERE NUOVO CIMENTO. In particular, please inspect the final part of this note, because it will tell you when referees are being intentionally corrupt. Our Lie-admissible treatment is a mere reformulation of nonunitary time evolutions used in quantum mechanics for dissipative processes since its inception. The latter have an inconsistent algebra in the infinitesimal behaviour, and the former bypass this problem, resulting in a consistent one. The gaining of a consistent algebra then permits calculations, such as the generalization of the theorem of detailed balancing, that would be impossible with the old fashioned nonunitary time evolution. Statements that Lie-admissible algebras have no meaningful application in particle physics are therefore of questionable ethical nature, in my view. In fact, the applications have been there for decades.

The developments going on in the construction of hadronic mechanics are now too numerous for me to summarize them effectively. I restrict myself to the indication of the achievement of the unification of all dissipative Schrodinger's equations via our Lie-admissible structure achieved by a group at the University of Patras, Greece (see enclosed paper by Jannussis et al). This includes the equations first proposed by Radicati (Univ. of Milano) in the early 40's.

I believe that a representative of DOE should attend our FIRST WORKSHOP ON HADRONIC MECHANICS (August 2 to 6, 1983), because I will be unable to summarize the outcome for your office as I did for all other meetings. I leave, of course, the decision to you.

I sincerely hope that a decision on our main application can be reached in the near future. Any additional delay can only be detrimental to all.

Sincerely,

cc. Drs. Wallenmeyer and Hildebrand

University of [REDACTED]

[REDACTED]
[REDACTED]
[REDACTED]

September 8, 1983

Dr. B. Hildebrand ER-221
Chief
Physics Research Branch
Division of High Energy Physics
U.S. Department of Energy GTN
Washington, D.C. 20545

Dear Dr. Hildebrand:

It is my pleasure to address myself to you as one of the participants of the "First Workshop on Hadronic Mechanics" and to thank you for supporting such a worthwhile and outstanding meeting. I was very impressed by the quality of papers presented. I felt distinctively that, in participating, I have had the honour and opportunity to be with a group of highly competent, productive, and progressive people.

I hope the Institute for Basic Research will receive continued support in its endeavours to proceed with such meetings, and I look forward to taking part in future work of the Institute. I believe this Institute and its Director, Dr. R. Santilli, are giving excellent service to the U.S.A. and beyond that to the scientific community interested in Theoretical Mechanics at large.

The U.S. Department of Energy must be commended on having the farsightedness of supporting an institute concerned with progressive scientific work of such high quality as I have experienced by being at the above mentioned workshop during early September of this year.

Sincerely yours,

[REDACTED]
[REDACTED]
Dean of Graduate Studies

HHEL/eg

bcc: Dr. R. Santilli

[REDACTED]

[REDACTED] August 28, 1983.

Prof. Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge
Massachusetts 02138
U.S.A.

Dear Professor Santilli:

I am deeply indebted with you, with the Institute for Basic Research staff and specially with your kind family for the warm hospitality received during the Workshops.

I am also indebted for all what I learned in the two wonderful simultaneous Workshops hold at IBR. They were of the highest possible level.

Warmest regards,

[REDACTED]

Head of the Theoretical
Physics Laboratory

AJK/gbv.

[REDACTED]

14th September 1983

Professor R.M. SANTILLI
The Institute for Basic Research
Cambridge

Dear Friend,

It is my great pleasure to tell you how much I enjoyed our Summer Workshop at the I.B.R.. I greatly appreciated the open and frank scientific atmosphere which prevailed there, an atmosphere which is probably responsible for a deeper and deeper collaboration between mathematicians and physicists, and in turn, for the increasing number of results obtained. The presence of new colleagues joining us was for me an important indication of the I.B.R. workshops' success and I was particularly happy to note the lively exchanges developed in the group between what we must now recognize as scientists of three generations.

The development of the Institute is an important scientific achievement. Whatever the doubts of those of our colleagues in the world who argue that the necessity of our studies is not yet proven by experiment, it is a historical fact that the international center for non-conservative physics now exists and that, following the American pioneering tradition, it exists in the United States. I must confess that I regret for my part not having had the opportunity to organize it in Europe, but the existence of the I.B.R. in Cambridge is now an established fact. I am sure that all those who are helping you in the States — and, I think, the DOE for a great part — are perfectly aware of this fact and that, despite the current restrictions on expenditures, they will continue to ensure that the I.B.R. has sufficient financial support.

I am looking forward to seeing you again on the occasion of the next workshop, or possibly before.

Sincerely Yours,

Sept. 1, 1983

To Dr. B. HILDERBRAND ER-221,
Chief,
Physics Research Branch
Division of High-Energy Physics
U.S. Dept. of Energy - GTN
Washington, D.C.-20545, USA

c/c to
Prof. R. H. Sawbill

Dear Doctor Hilderbrand:


during the first ten days of August, 1983, I participated in the "First Workshop on Hadronic Mechanics" held at the Institute for Basic Research, Cambridge, Mass., under the direction of Prof. R. M. Santilli. Even if the I.B.R. could not provide any support this year, I succeeded in participating in that Workshop due to my strong interest in hearing about the most recent developments of the Lie-admissible formulations, especially as applied to elementary particles and to "hadron mechanics". I myself contributed a talk about the application of the methods of General Relativity to the description of hadron structure.

I would like to let you know that —even if I already expected to meet there outstanding physicists (so as Prof. Okubo)— I was very impressed by the high level also of the mathematicians and mathematical-physicists participating in the Workshop. I deem to be very profitable and promising such a collaboration among mathematicians and physicists, at the present stage of high-energy (theoretical) physics. I also enjoyed coming to know day by day —from the talks— about the very tempting theoretical framework that the Organizers of that Workshop had in mind when planning it, and that was partly unknown to me: I believe such a framework to be quite suitable for adapting quantum mechanical theories to the description of hadron structure and hadron interactions. I was also impressed by the ability of that framework in describing —as "particular cases"— the approaches by Prof. P. Caldirola (Milan, Italy) both to dissipative systems and to leptons; the latter approach succeeds in evaluating lepton masses just by introducing a "fundamental time".

I enjoyed also the workshop atmosphere, quite cooperative and open-minded, as well as the hearty participation of the Organizers, particularly of Prof. R. M. Santilli, very attent and smart scientific leader.

I hope in the future to be able again to participate in the same series of Workshops, since I think that important results are coming out —and even more will come out— from the research groups linked with the Institute for Basic Research.

Thank you for your attention. Yours sincerely,


Prof. of physics.



Department of Energy
Washington, D.C. 20545

OCT 19 1983

Dr. R. M. Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Studies on Hadronic Mechanics" to be conducted under your direction.

We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program. Due to the funding limitations which we are currently experiencing, we have found it necessary to decline support of many promising proposals such as yours.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

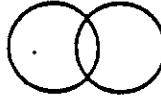
A handwritten signature in dark ink, appearing to read "William A. Wallenmeyer".

William A. Wallenmeyer
Director
Division of High Energy Physics

— 874 —
THE INSTITUTE FOR BASIC RESEARCH

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

November 10, 1983



Dr. WILLIAM WALLENMEYER
Director
Division of High Energy Physics
U.S. DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby withdraw from consideration by your Office our remaining applications, that is

Theoretical, Experimental and Applied Studies on a possible pulsating structure of the Coulomb Force of individual Electrons
Submitted on January 3, 1983 under the principal investigator
Dr. [REDACTED]

and
Studies on the Quantization of Systems with Gauge Symmetries
Submitted on July 14, 1983 under the principal investigator
Dr. [REDACTED]

The withdrawal is evidently due to the recent declination of funding of the primary I.B.R. proposal on the development of Hadronic Mechanics by your office (as well as by N.S.F.). In fact, the declination does not permit the I.B.R. to have sufficient logistic structures to administer and properly conduct other projects at this time.

Very Truly Yours

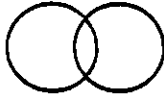
Roger Santilli
President

RMS-wls

cc.: [REDACTED]

THE INSTITUTE FOR BASIC RESEARCH

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



November 10, 1983

Drs Wallenmeyer and Hildebrand, DOE

Dear William and Bernie,

With your termination of my association to the DOE, I would like to take the opportunity to express to both of you as well as to Dr. D. PEASLEE the sentiments of my sincere appreciation and gratitude for your support during the past five years.

Everything that has been accomplished by our group during this period, including not so frequent scientific events such as the creation of new mechanics, is the result of your support, and I am sincerely grateful for it.

Under the circumstances, you should perhaps know that I am not contemplating to submit additional applications to your Office, while possible applications by other members of the I.B.R. will be discouraged, evidently, because of insufficient logistic backing. A formal letter of withdrawal of all the remaining applications is enclosed.

Nevertheless, if, sometime in the future, you foresee that my efforts can be helpful to DOE, please do not hesitate to let me know.

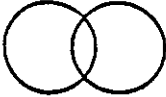
Wishing to you, your families and your Division the best, I remain

Yours, Very Truly

A handwritten signature in dark ink, appearing to be 'R. Santilli', written over the typed name.

R. Santilli

c.c.: Dr. D. Peaslee



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

January 1, 1984

Office of the President

Dr. LARRY C. BIEDENHARN, jr
Department of Physics
University of Texas
AUSTIN, Texas 78712

Dear Dr. Biedenharn,

Your regrettable, apparent, cooperation with manipulatory practices during the consideration process by the U.S. National Science Foundation of the primary research grant application of our Institute, and the easily predictable series of consequences that this will and otherwise must imply, recommend that we terminate all our contacts for the foreseeable future. Unless I hear from you, I shall therefore assume that, under the circumstances, you consider recommendable to resign from the Editorial Council of the Hadronic Journal, and I shall remove your name from it beginning from the first issue of Volume 7, that of January 1984. I shall also assume you agree on the advisability to terminate jointly all possible scientific and human contacts.

Very Truly Yours

Ruggero M. Santilli

CERTIFIED LETTER
RETURN RECEIPT REQUESTED

PART XXIV:

REJECTION BY THE

NATIONAL

SCIENCE FOUNDATION

AND THE

DEPARTMENT OF

ENERGY

OF AN APPLICATION

BY A SENIOR I.B.R.

PHYSICIST

Research Grant Application

- 878 -

Submitted to the
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

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Proposed Starting Date:
September 1, 1982

Proposed Duration:
36 Months

Amount Requested:
\$ 279,800

ENDORSEMENTS

~~_____~~
Principal Investigator
The Institute For Basic Research
~~Soc. Sec. No. 044-885596~~
Teles. Office (617) 864-9859
~~_____~~

~~_____~~
R. M. Santilli
President
The Institute For Basic Research
Soc. Sec. No. 032-46-3855
Tele. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
ATT: Mr. R. Alkon, President
Tele. (617) 969-6630

Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02109
ATT: Mr. J. Grassia, Senior Partner
Tele. (617) 956-1700

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PROPOSED BUDGET

CURRENT AND PENDING SUPPORT

CURRICULUM VITAE OF THE PRINCIPAL INVESTIGATOR

APPENDIX: General Information on The Institute For Basic Research

ENCLOSURES:

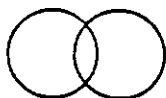
- G. [REDACTED] - [REDACTED]
[REDACTED]
[REDACTED]
- G. [REDACTED]
[REDACTED]
- G. [REDACTED] - [REDACTED]
[REDACTED]
- G. [REDACTED] - [REDACTED]
[REDACTED]
Kleinman, Gail; Displacement and Chemicality of Solid Solution
Control Letters Plus Letter FFA-260 (1976)
[REDACTED]
- G. [REDACTED]
[REDACTED]
[REDACTED]

ABSTRACT

The objective of this application is to develop a method of quantum-mechanical analysis of phase stability of pure metals, alloys, and compounds. The method will enable to calculate binding energies, enthalpies, Gibbs free energies, P-V and C-T-diagrams for a wide class of solids, both metallic and nonmetallic. The ultimate goal of the research is to develop a series of methods for theoretical (first-principle) predictions of physical properties of solids which will have direct implementation to creating new materials with given properties.

The first stage of the research will consist in developing a variational principle in the quantum-mechanical so-called Density-Functional-Formalism (DFF), which will allow to systematically determine directly electron charge-density distributions in crystalline solids, avoiding solving the corresponding Schrödinger equations for the wave functions. As an illustration of the method the calculations will be performed for some pure metals such as Titanium and Iron. The calculations of electron charge-density distributions, binding energies, enthalpies and P-V (pressure-volume) diagrams for these metals will serve as a test of the method.

The subsequent research will extend the method to the finite temperatures and two-component systems (alloys, compounds).



THE INSTITUTE FOR ⁸⁸¹BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 4, 1982

Dr. Thomas E. Walsh
Director
Electronic and Material Sciences
Air Force Office of Scientific Research
Bolling Air Force Base, DC 20332

Dear Dr. Walsh,

I hereby respectfully submit the research grant proposal entitled

**VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES
OF SOLIDS**

under administration by our Institute, and with Principal Investigator Professor [REDACTED]

Permit me to indicate that Professor [REDACTED] has been recently appointed Associate Professor at the Division of Physics of our Institute following a truly impressive variety of recommendations from distinguished scholars. We are therefore filing this proposal with full confidence that Professor [REDACTED] will indeed meet all our expectations. Also, please take into consideration that we have filed this application with the minimal possible Institute overheads (a fraction of those charged by other Institutions) in order to facilitate the funding of Professor [REDACTED] research.

As you can see, we have made an effort to file the application in a form as informative as possible, including, besides budget and other conventional parts, the identification of the state of the art in the field, curriculum, and representative papers. Nevertheless, please keep in mind that we have avoided excessive length to facilitate review. We are therefore at your disposal for any additional information you may need. The inspection of the report by Professor [REDACTED] on his personal experience of the status in the U.S.S.R. of this field of research is recommended in particular.

In closing, permit me the liberty of indicating that Professor [REDACTED] current salary expires on September 1, 1982. We are fully aware of the complex and diversified item for the consideration of a proposal. Yet, your consideration of any possibility that might expedite the consideration of this proposal would meet with our sincere gratitude and appreciation.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

cc: Board of Governors, IBR
[REDACTED]
[REDACTED]



DEPARTMENT OF THE AIR FORCE
AIR FORCE OFFICE OF SCIENTIFIC RESEARCH (AFSC)
BOLLING AIR FORCE BASE, DC 20332

JUN 1 1982

Professor [REDACTED]
Dept. of Physics
The Institute of Basic Research
Cambridge, MA 02138

Dear Professor [REDACTED]

We have completed our review of your research proposal, "VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS," assigned our Code No. 82-NE-202.

Although the proposed research is interesting, we are unable to consider sponsorship because we do not have an established or planned research program in this area. We have, therefore, not reviewed your proposal in detail. You will find enclosed all but one copy of your proposal.

We appreciate the opportunity to have considered your proposal and welcome your continued correspondence with the hope that we may be able to be of greater assistance at some future time.

Sincerely

for Clarence L. Hordner
THOMAS E. WALSH
Director
Electronic & Material Sciences

Cy to: R. M. Santilli
President



Department of Energy
Washington, D.C. 20545

JUN 4 1982

Professor Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli:

Receipt is acknowledged of the proposal entitled "Variational Method of Calculating Structural Properties of Solids" with Professor [REDACTED] as principal investigator.

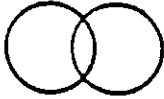
We are declining further consideration of this proposal and returning it to you at this time. This decision stems from the fact that we have completed our unsolicited proposal support actions for this fiscal year. Please be advised that this decision does not reflect adversely upon the technical merits of the proposal.

Thank you for allowing us to consider these research efforts.

Sincerely,

Louis C. Ianniello, Director
Division of Materials Sciences
Office of Basic Energy Sciences

Enclosure



— 884 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

August 20, 1982

Dr. ERIC D. THOMPSON
Condensed Matter Theory
Division of Material Research
NATIONAL SCIENCE FOUNDATIONS
Washington, D.C. 20550

Dear Dr. Thompson,

During the period September 14, 15, 16, 1982, I shall be in Washington, and I would appreciate the possibility of visiting you.

We would like to present in more detail the part of the programs of our Institute which are pertinent for your Division. Also, we would like to know the status of the proposal by Professor ~~XXXXXX~~ under consideration at your office, no. DMR-8212909.

In the meantime, I enclose a general presentation of our Institute.

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/mlw

Enclosure

cc: Professor ~~XXXXXX~~

- 885 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

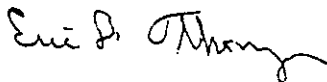
August 26, 1982

Professor Ruggero Maria Santilli
Department of Theoretical Physics
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor Santilli,

In reply to your letter of August 20, 1982, I shall have left the National Science Foundation before your visit but Dr. John Connolly will have resumed his position as program director of the Condensed Matter Theory program. I suggest that you telephone, (202-357-9737), to set up an appointment with him. Dr. Connolly has been advised of Dr. [REDACTED] proposal status. As of this date, Dr. [REDACTED] proposal is still under review.

Sincerely,



Eric D. Thompson
Program Director
Condensed Matter Theory Program
(202) 357-9737

— 886 —
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Professor [REDACTED]
Department of Physics
Institute for Basic Research
Cambridge, Massachusetts 02138

REF: DMR 8212909

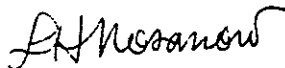
Dear Professor [REDACTED]

We regret to inform you that the National Science Foundation is unable to support your proposal entitled "Variational Method of Calculating Structural Properties of Solids." Verbatim copies of the reviews are enclosed as provided for by current NSF policy.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals in the future. In that regard, the enclosed verbatim reviews may be useful to you. If you have any questions, please contact Dr. David R. Penn, Staff Associate, Condensed Matter Sciences Section, (202) 357-9737.

Sincerely yours,



Lewis H. Nosanow
Acting Division Director
Materials Research

Enclosures.

Copy to: Ruggero Maria Santilli
President

NSF PROGRAM

CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

~~Donald K. ...~~ is an able, though probably not outstanding, solid-state theorist. He is well acquainted with current methods in the electronic structure of metals and has made useful contributions to pseudopotential theory. The proposed project on the stability of structures in metals and alloys is very similar to those being undertaken by many others in this country and Europe, stimulated to some extent by Miedema's empirical studies and Phillips' enthusiastic support for them.

What distinguishes this particular approach is the variational treatment of the density itself — without wavefunctions — which relies on his theorem (Eq. 8) through which the kinetic energy is directly related to the pressure. He purports to prove this rigorously, but I believe it is an approximation. The point is that changing the volume does not just scale the wavefunction; if there are potentials present (from the ions) it also deforms the wavefunction. Thus his approach should be classified with the Fermi-Thomas method as an approximate, in this case untested, approach. It is nevertheless interesting and worthy of support. It is a competing method which could turn out to be important.

It may be difficult to compare support here with more usual proposals from scientists in universities and industries. Institutional support would seem to be absent, in the traditional sense, but the funds would purchase the total commitment of the principal scientist. He apparently hopes to add a graduate student after the first year; I don't know if that is possible. It would be nice if it could be supported, at least partially.

Recent research achievements of the Principal Investigator(s):

OVERALL RATING: -- EXCELLENT -- VERY GOOD ☒ GOOD -- FAIR -- POOR

NSF PROGRAM

CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capabilities):

1. The theoretical foundation of the proposal is unsound. The author expressed the "kinetic energy functional" in terms the bulk modulus and the derivatives of the potential energy by using the virial theorem. This is correct for the correct density. However the author wishes the final expression to have a variational principle and, therefore, requires the expression to hold for any trial density function. His proof, as given in Enclosure 1, is false. The scaling of the density and wave function, as given by eq. of Enclosure 1, when the volume of the system is changed, does not give the corresponding density except for the homogeneous electron gas. For the usual statement of the virial theorem, this scaling of the wave function can be used because the error does not affect the first order change in energy due to the variational principle. The author cannot use eq. (8) without the help of the variational principle. The rest of the proof is, therefore, fallacious.
2. It is then proposed that the bulk modulus be expressed in terms of the density function by means of the Feynman-Hellman theorem. Again that requires the variational principle. The author's argument that the variational principle is not needed is false. His count of factor of $1/N$ is simply wrong. In infinite solids or solids with periodic boundary there is an additional peril of using the Feynman-Hellman theorem. [See, for example, L. Kleinman, Phys. Rev. B1, 4189 (1970).] In sum, the author does not have a variational principle for his expression.
3. What is billed as the "preliminary results" in sec. 3.2 is not a trial run of the procedure outlined by the author of the proposal. Instead, it is a fit of an analytic expression for the density, eq. (18), to the result of a traditional self-consistent solution. ~~Re-examination of the results shows that the agreement is only fortuitous.~~
The resulting agreement in total energy is just a routine check of the virial theorem. The Feynman-Hellman theorem is not invoked. So these "preliminary results" do not prove that his procedure will work.

In view of the serious deficiency in the theory on which the proposed computation is based, support is not recommended.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

NSF PROGRAM

CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability):

Stripped of all the verbiage what is actually being proposed for study is obtaining the electronic charge density in a solid on the basis of the density functional approach by exploiting the fact that the true electron number density $n(r)$ minimizes the total electronic ground state energy.

By itself this is not a new idea. For example, John Smith used it to obtain the electronic charge density in the vicinity of a jellium surface (PR 181, 522 (1969)). He, however, used a simple expression for the kinetic energy functional $T[n]$, viz. $T[n] = \frac{3}{10} (3\pi^2)^{2/3} n^{5/3} d^3r$. The new aspect of Krasko's proposal is the use of a much more elaborate form for this functional, that is obtained (implicitly) through the use of the virial theorem and the bulk modulus. If this approach is capable of yielding better results than those that can be obtained by the use of the simple functional quoted above, the added complexity will probably be justified. (Friedel oscillations of the charge density do not seem to be produced by the $T[n]$ given above, for example.)

I can't help thinking, however, that it would be worthwhile to test the proposed approach on some simple, but not trivial system, for which results are known, e.g. the jellium surface, before tackling the more complicated muffin-tin model discussed in the proposal.

The description of other problems to be studied, on p. 11, is too sketchy for me to comment on them.

A great deal of money and a lot of time is being requested for checking out the proposed variant of the density-functional approach, largely because a 12-month salary is being sought for the principal investigator for each of three years. I think one should be able to decide whether the method will fly for a lot less money and in a much shorter period of time.

Recent research achievements of the Principal Investigator(s):

The principal investigator, who was unknown to me before I received this proposal seems to understand the density functional formalism quite well, and to have a record of productive work, both theoretical and computational.

OVERALL RATING: EXCELLENT VERY GOOD ☒ GOOD FAIR POOR

NSF PROGRAM

CONDENSED MATTER THEORY

VARIATIONAL METHOD OF CALCULATING STRUCTURAL PROPERTIES OF SOLIDS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capabilities):

The aim set in the project is certainly timely, and the proposed means of achieving it is certainly worth studying. However, the author is not as close to being able to avoid solving the active Schrödinger equation as he seems to think, and the implementation of his program can be expected to run into problems.

More specifically, the proposed way of determining the functional $P[n]$ is not convincing. The claim on p.8 that ref.10 contains a proof of the Hellman-Feynman theorem under less restrictive conditions than it is commonly believed to be valid is exaggerated. In fact, the "proof" in ref.10 heavily relies on plausibility arguments and is at any rate limited to independent-particle approximation in which the ground state is described by a Slater determinant and the electron density can be expressed in terms of occupied single-particle wave functions. To call this the "most general formulation", as is done on p.3 of ref.10, is misleading. The agreement with the results obtained by the Kohn-Sham formalism (p.11) may be impressive in relation to the total energy, but is meaningless on the scale of alloy formation energies or structural energies.

To summarize: although it would be very useful to have a way of applying the density functional method without recourse to wave functions, the applicant is unlikely to achieve this goal along the proposed line.

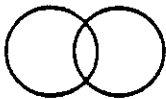
The above criticism concerns the first stage of the project (see §3.3 on p.11). The content of the further stages is very vague. This is understandable with stages 2 and 3, which concern the development of computational techniques, but cannot be accepted in connection with stage 4. The density functional theorem holds for the ground state of an electron system; its application to finite temperatures involves some fundamental unresolved problems. Stage 4 is an ambitious project in itself.

Finally, I find §3.4 of the proposal a little difficult to take seriously. With the facilities available in the Soviet Union, the Russian Menace can safely be ignored in the field for quite a while. Furthermore, for practical applications the semi-empirical methods of Miedema's are much more promising, and all information on those is available in the literature.

Recent research achievements of the Principal Investigator(s):

The achievements of the applicant bear the mark of work done in relative isolation. He seems competent in the fundamental issues, and therefore can be expected to make significant contributions in the areas of stages 1 and 4. When it comes to numerical calculations, he should seek cooperation with groups having more experience in sophisticated band calculations.

OVERALL RATING: -- EXCELLENT -- VERY GOOD ☒ GOOD -- FAIR -- POOR



I. B. R. - 891 -

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

January 3, 1983

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Dr. LEWIS H. NOSANOW
Acting Division Director
Material Research
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

RE: DMR 8212909

Application by Prof. [REDACTED]

Dear Dr. Nosanow,

We acknowledge receipt of the concluding reports on the consideration of Professor Krasko's application by your office.

Even though your decision has been negative, we appreciate the interest you have indicated in the case, your diligence in considering all possible avenues, and your receptive attitude toward the consideration of possible new applications.

We are confident that Professor [REDACTED] will understand the reasons of the negative decision. On our part, we have supported him to our best in the past, and we shall continue to provide him with our best possible support, despite the limitations of our possibilities. In particular, we shall be at Professor [REDACTED] disposal at any time in case he wishes to submit a new application.

Have a happy and prosperous New Year.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw

cc. Prof. [REDACTED]

PART XXV:
REJECTION BY THE
DEPARTMENT OF
ENERGY
OF AN APPLICATION
BY FIVE, SENIOR,
I.B.R. MATHEMATICIANS

- 893 -
Research Grant Proposal

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

MATHEMATICAL STUDIES ON LIE-ADMISSIBLE ALGEBRAS

Proposed Starting Date
July 1, 1982

Proposed Duration
48 Months

Proposed Amount
\$730,946

ENDORSEMENTS


Principal Investigator

The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859

and
Department of Mathematics
University


Principal Investigator

The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859

and
Department of Mathematics
University


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and
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and
Department of Mathematics
University


Principal Investigator

The Institute for Basic Research
tel (617) 864 9859

and
Department of Mathematics
University


R.M. SANTILLI

President
The Institute for Basic Research
Cambridge, Massachusetts
tel. (617) 864 9859
Soc. Sec. No. 032 46 3855

Accounting Firm of the Institute
VACCARO & ALKON CP,CPAS
2120 Commonwealth Ave
Newton, Massachusetts 02186
tel. (617) 969 6630

att. Mr. R. Alkon, CPA, President

Law Firm of The Institute
WASSERMAN & SALTER
31 Milk Street
Boston, Massachusetts 02109
tel. (617) 956 1700

att. Mr. J.Grassia, Sen. Partner

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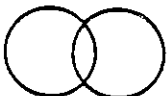
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ABSTRACT

Lie-admissible algebras were introduced in 1948 by A.A. Albert. In 1967 R.M. Santilli first pointed out that Lie-admissible algebras may be more appropriate than Lie algebras for studying physical processes. Santilli refined his idea in a sequence of papers over several years. Meanwhile a few mathematicians wrote on the structure and classification of Lie-admissible algebras as a topic in pure mathematics. With the inception of the annual Workshops on Lie-Admissible Formulations in 1978, physicists and mathematicians began to meet together to discuss their interests in Lie-admissible algebras.

Since 1978 there has been growing evidence, at first theoretical but now based on experimental results, that Lie-admissible algebras are a proper mathematical tool to formulate and solve a number of physical problems. During the Fourth Workshop held in August 1981, it became clear that physics would benefit from solutions to certain mathematical problems. They include the development of a representation theory and universal envelope for Lie-admissible algebras, and classification and structure theory especially for mutation algebras. The principal investigators propose to work on these problems and other problems that the physics will suggest during the course of the Investigation.

The applications of the mathematical tools to be developed under this research project are rather promising and of diversified nature, encompassing a number of branches of physics, engineering, and applied mathematics at large. In fact, a number of recent papers have indicated that the theory of Lie-admissible algebras can be applied to: Newtonian mechanics and space mechanics (e.g. trajectory problems under drag forces); statistical mechanics and plasma physics (e.g. statistical ensembles inclusive of inelastic collisions and nonlocal nonpotential internal forces); particle physics (e.g. for the treatment of strong interactions as of nonlocal nonpotential type due to wave overlapping of particles); computer modeling and engineering (e.g. electrical circuitry and electronics with internal losses); and other fields.



THE INSTITUTE FOR BASIC RESEARCH

Harvard Grounds, 96 Prescott Street — 896 —
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

November 4, 1981

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Station J-309
WASHINGTON, D.C. 20545

CERTIFIED MAIL

Dear Dr. Wallenmeyer,

I hereby respectfully submit the enclosed original of the research grant application entitled

MATHEMATICAL STUDIES ON LIE-ADMISSIBLE ALGEBRAS

under administration of our Institute and Principal Investigators: Professors

The INITIATION DATE has been suggested at July 1, 1982; the DURATION is recommended for 48 months; and the PROPOSED AMOUNT IS \$730,946.

As you can see in the proposal, the mathematical tools which are recommended for development have rather important and diversified applications in a number of disciplines, such as Newtonian and Space Mechanics, Statistical Mechanics and Plasma Physics, Particle Physics, Computer Modeling and Engineering, and other fields. The proposal, therefore, has all the elements for marking an important (if not historical) point in the development and pursuit of truly novel advancements in scientific knowledge.

Looking forward to hearing from you to finalize the material needed for the referee process, as well as for any additional assistance you may need, I remain,

Yours, Very Truly

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS/pm

encl.

cc: B. HILDEBRAND and R. THEWS, DOE

~~_____~~
Principal Investigators



Department of Energy
Washington, D.C. 20545

December 16, 1981

Professor [REDACTED]
Professor [REDACTED]
Professor [REDACTED]
Professor [REDACTED]
Professor [REDACTED]
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138

Gentlemen:

Your research proposal entitled, "Mathematical Studies on Lie-Admissible Algebras," has been received.

Your proposal is now under review in the Division of High Energy Physics and as soon as a decision with respect to support can be reached, you will be advised. Dr. Robert Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him.

We appreciate your interest in submitting this proposal to the Department of Energy, and we will be pleased to give it review and consideration for support.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics
Office of High Energy and Nuclear Physics

cc: Professor Ruggero Maria Santilli



Department of Energy
Washington, D.C. 20545

JUL 16 1982

Prof. [REDACTED]
Prof. [REDACTED]
Prof. [REDACTED]
Prof. [REDACTED]
Prof. [REDACTED]

Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138


Gentlemen:

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Mathematical Studies on Lie-Admissible Algebras," to be conducted under your direction.

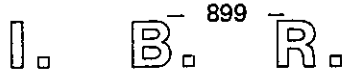
We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,


William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Prof. Ruggero Maria Santilli



Ruggero Maria Santilli, Professor of Theoretical Physics and President

[1] the WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS are now restricted only to mathematicians;

[2] a separate new series of meetings, called WORKSHOPS ON HADRONIC MECHANICS, are scheduled for the continuation of the applications to experimental and to theoretical physics according to my proposal currently pending at your office under the title of "Studies on Hadronic Mechanics"; while

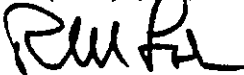
[3] the two workshops, in case funded, are intended to be conducted jointly here at the I.B.R., one in the morning and the other in the afternoon sessions.

This separation of mathematics and physics, while continuing the interaction, has been made commendable by the considerable increase of participants in this new scientific current.

Finally, I would like to express my appreciation for your subsequent letter of August 2, 1982, indicating that you had informed the offices of Senators Jepsen and Levin and Congressman Dunn. In fact, our office has abstained from any contact of this type, and I believe that this has been the case also on the part of the Principal Investigators.

In closing, I would like to take the opportunity to express my appreciation for your courtesy and time, as well as my full understanding of the difficulties of the moment.

Very truly yours,



Ruggero M. Santilli
President

RMS/mlw

cc: Drs. B. HILDEBRAND and R. THEWS, DOE
Drs. [REDACTED]



Department of Energy
Washington, D.C. 20545

AUG 2 1982

Professor Ruggero Santilli
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

This is in response to your letter of July 8 with regard to the proposal submitted by the Institute for Basic Research on behalf of Dr. [REDACTED], et al. As you know, we have declined that proposal, as is indicated in my letter of July 16 to [REDACTED] et al. We have informed Senators [REDACTED] and [REDACTED] and Congressman [REDACTED] of our action as you suggested.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

PART XXVI:
REJECTION
BY THE
NATIONAL SCIENCE
FOUNDATION
OF AN I.B.R.
WORKSHOP
IN MATHEMATICS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION		
FOR CONSIDERATION BY NSF ORGANIZATIONAL UNIT (Indicate the most specific unit known, i.e. program, division, etc.)		IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes ___ No ___ ; IF YES, LIST ACRONYM(S):
PROGRAM ANNOUNCEMENT/SOLICITATION NO.:		CLOSING DATE (IF ANY):
NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)		
THE INSTITUTE FOR BASIC RESEARCH (I.B.R.)		
ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)		
96 Prescott Street, Cambridge, Massachusetts 02138		
TITLE OF PROPOSED PROJECT		
FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS		
REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
\$ 12,280.00	one week	August 2, 1983
PI/PD NAME AND SOCIAL SECURITY NO. (SSN)*		PI/PD PHONE NO.
[REDACTED]		(617) 864 9859
PI/PD DEPARTMENT		PI/PD ORGANIZATION
I.B.R., Division of Mathematics, Cambridge, Ma		Univ. of [REDACTED] Dept. of Mathematics, [REDACTED]
ADDITIONAL PI/PD AND SSN*		ADDITIONAL PI/PD AND SSN*
ADDITIONAL PI/PD AND SSN*		ADDITIONAL PI/PD AND SSN*
FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO.:		SUBMITTING ORGANIZATION IS ___ IS NOT ___ A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).
*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950, as amended.		
CHECK APPROPRIATE BOX(ES) IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:		
<input type="checkbox"/> Animal Welfare <input type="checkbox"/> Human Subjects <input type="checkbox"/> National Environmental Policy Act		
<input type="checkbox"/> Endangered Species <input type="checkbox"/> Marine Mammal Protection <input type="checkbox"/> Research Involving Recombinant DNA Molecules		
<input type="checkbox"/> Historical Sites <input type="checkbox"/> Pollution Control <input type="checkbox"/> Proprietary and Privileged Information		
PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME	NAME	NAME
[REDACTED]	R.M.SANTILLI	
SIGNATURE	SIGNATURE	SIGNATURE
TITLE	TITLE	TITLE
Principal Investigator	President, I.B.R.	
DATE	DATE	DATE
	10-26-82	

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— Table of Contents of the Proceedings of the Second Workshop on Lie-admissible Formulations (1979)	
— Table of Contents of the Proceedings of the Third Workshop on Lie-admissible Formulations (1980)	
— Table of Contents of the Proceedings of the First International Conference on Nonpotential Interactions and Their Lie-admissible Treatment (1982)	
— M. L. TOMBER, <i>The history and methods of Lie-admissible algebras, II</i> , Hadronic J. 5, 360-430 (1982)	

1. CO-ORGANIZERS:

Professor [REDACTED]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
Department of Mathematics
University of [REDACTED]
[REDACTED]

Professor [REDACTED]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
College of Arts and Sciences
Natural Science Division
University of [REDACTED]
[REDACTED]

Professor [REDACTED]
The Institute for Basic Research
Cambridge, Massachusetts 02138
and
Department of Mathematics
[REDACTED] University
[REDACTED]

ABSTRACT

Lie--admissible algebras were introduced by A. A. Albert in 1948. In 1967, R. M. Santilli first pointed out that Lie--admissible algebras may be more appropriate than Lie algebras for the description of physical systems, because they permit the treatment of conventional potential interactions, as well as the nonpotential ones due to collisions among extended particles. Santilli refined his ideas in a series of papers and monographs over several years. Meanwhile, a few mathematicians (including the organizers of the proposed Workshop) conducted independent research on the structure and classification of Lie--admissible algebras as a topic in pure mathematics. The First Workshop on Lie--admissible algebras was held at Harvard University in August, 1978. The meeting initiated the gathering of mathematicians, theoreticians, and subsequently also experimentalists to study the Lie--admissible algebras at the pure mathematical level jointly with their applications.

The group meet again at the Second (1979), Third (1980), and Fourth Workshop (1981) held under financial support from the U. S. Department of Energy with a growing number of participants. By the end of 1981, the results were sufficient to warrant the organization and conduction of the First International Conference on Nonpotential Interactions and Their Lie--admissible Treatment, which was held on January 1982 at the Université d'Orléans, France, under joint financial support by the French and the U. S. Governments. All these studies resulted in the publication of nine volumes of proceedings for the period 1978--1982 plus a considerable number of papers.

This proposal recommends the continuation of the Workshops although specialized to pure mathematics only. The physical part is now scheduled at the Workshops on Hadronic Mechanics. The interplay between mathematics and physics will be kept via the conduction of the two workshops during the same period of time, e.g., by having the sessions on mathematics in the morning and those on physical applications in the afternoon. This proposal recommends minimal support for the logistic organization of the Workshops, as well as for travel expenses of ten mathematicians, all experts in Lie--admissible algebras and related fields. Additional mathematicians and theoreticians are expected to participate with their own support.

I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 4, 1982

Dr. ALVIN THALER
Director
Special Programs
Division of Mathematics
NATIONAL SCIENCE FOUNDATIONS
WASHINGTON, D.C. 20550

Dear Dr. Thaler,

I hereby submit for consideration by your Division the proposal entitled
FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS
with Principal Investigator Professor [REDACTED], and co-investigators Professors [REDACTED]

The original, duly signed, proposal is enclosed, while nine copies have been separately mailed to you. In case a list of experts in the field of the proposal may be of any assistance to you, please do not hesitate to let me know.

You will note that the organization of the Workshop has been made to coincide with that of the First Workshop on Hadronic Mechanics, which treats some of the most relevant physical applications of the Lie-admissible algebras. In this way, we have separated the mathematical sessions (submitted to you) from the physical ones (under consideration by DOE and NSF). Nevertheless, we continue to promote interactions between mathematicians, from one part, and physicists from the other.

I shall occasionally keep you informed of the organization of the two Workshops. Thanking you for the courtesy of your recent phone call, I remain

Yours Very Truly



Ruggero Maria Santilli
President

RMS-mlw

encl.

June 2, 1983

Dr. E.F. INFANTE, Director,
Division of Mathematics and Computer Sciences
NATIONAL SCIENCE FOUNDATION, Washington, D.C. 20550

Dear Dr. Infante,

It appears that another link in the chain of rejections of I.B.R. applications is forthcoming. In fact, Two weeks ago I contacted by phone Dr. A. Thaler of your division to inquire about our application for funding our forthcoming FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS, NSF No. MCS-8303592, Principal Investigators: Professors [REDACTED]. Dr. Thaler informed us that he expected the declination of the proposal.

I would gratefully appreciate your consideration of the case with particular reference to the following aspects.

1) It is important that rejections be communicated as soon as possible, particularly when a negative decision has already been reached. In this case, the application was filed on November 4, 1982. It appears that, without my call, the rejection of application NSF-MCS-8303592 would have remained dormant for considerable additional time, while a considerable number of distinguished scholars from numerous countries were waiting for a decision (see enclosed list of interested participants).

Despite my call two weeks ago, the formal rejection has not yet arrived. This is highly detrimental to us because a number of alternative forms of funding the workshop are directly incompatible with a formal consideration process at NSF and, as such, they cannot be initiated during such a consideration.

The net result is that considerable delays in the communication of the rejection jeopardize substantially the possibilities for alternative fundings. We are therefore going back to the old basic question I brought to your attention: that NSF at times does not stop short at the rejection of applications, but keeps going with actions that produce additional unnecessary damage.

It is my unpleasant duty to bring these issues to your personal attention. In fact, their knowledge is a necessary pre-requisite for the improvement of the relationship between NSF and the scientific community.

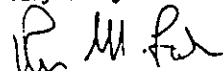
2) Please feel free to contact any member of the enclosed list. However, permit me to recommend that you do not communicate the list to NSF referees, particularly those in physics. In fact, NSF referees (in physics) would likely call members of the list to discourage their participation. This has been documented and it is a simple incontrovertible truth. For instance, one of the referees of our application to the physics division of NSF entitled "Studies on Hadronic Mechanics" (recently rejected) had the courage to put in the report itself statements to the effect that:
(a) he had contacted an advisor of the project, Professor L.C. Biedenharn of Duke University
(b) he had exercised pressures on the advisor to withdraw from the project; and
(c) he had even secured a letter from the advisor ensuring his withdrawal.

It is extremely unfortunate that the NSF division of physics did accept referee reports of this type for the decision making process involving the dispersal of public funds. In fact, the acceptance of reports of this type is much more damaging to NSF than to us.

I stressed the work "physics" because we have no record of similar occurrences in the refereeing process of the mathematics division.

Again, please accept the sentiments of my gratitude for your consideration and time.

Very Truly Yours



Ruggero M. Santilli
96 Prescott Street
Cambridge, Ma 02138

P.S. My two letters enclosed in the document for advance consultation for a group proposal/institutional support have been accepted for publication in Lettere Nuovo Cimento.

You should know that one of the letters (that on the Lie-admissible structure of open nuclear reactions) had been rejected and rejected repeatedly by the journals of the American Physical Society with truly unbelievable, and at times hysterical referee reports, totally deprived of the most minute scientific content or even a shadow of constructive criticism. The same letter was accepted in LCN in three weeks without any modification.

This, and too many other episodes, confirm the existence of a severe problem of ethics in the U.S. physics community. After all, academic dances for personal interests and straight scientific corruption have existed in the U.S. physics community since its birth, as it is the case for all human sectors in all countries. With the passing of the decades, the problem has deteriorated considerably because of numerous factors ranging from the increase of the amount of money managed by the physics community, to the total, absolute absence of any governmental or judicial control.

At this point in time, NSF is perceived as a victim of the deterioration of ethical standards. However, the acknowledgment of the existence of an ethical problem in the academic community, particularly in refereeing, should be made (at least internally) at NSF, and the necessary measures to cope should be identified and enforced. Lacking suitable action, it is easy to predict a deterioration of the way NSF is perceived by the community.

In the final analysis, the existence of a problem of ethics in physics is a rather common topic of talks these days. The lack of acknowledgement of its existence by NSF would evidently constitute a problem, and a sizable one at that.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

June 9, 1983

Dr. Ruggero M. Santilli
95 Prescott Street
Cambridge, MA 02138

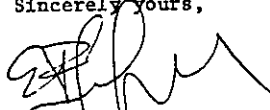
Dear Dr. Santilli:

I have just received your letter of June 2, concerning proposal MCS-8303592 entitled Fifth Workshop on Lie-Admissible Formulations with Professors H. C. Myung, R. Ohemke, A. A. Sagle and M. L. Tomber as principal investigators. I have had a brief discussion with Dr. Thaler on this matter, and I have shared your letter with him.

At this time, the review and evaluation process of this proposal has not been completed. This is the reason that you have not yet been notified of any decision on the part of the Foundation regarding this proposal.

Thank you for your letter.

Sincerely yours,



E. F. Infante
Division Director
Mathematical and Computer Sciences

cc: Dr. A. Thaler

- 911 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

JUN 13 1983

Professor [REDACTED]
Department of Mathematics
University of [REDACTED]
[REDACTED]

Dear Professor [REDACTED]

We regret that the National Science Foundation is unable to support your proposal MCS-8303592 entitled "Fifth Workshop on Lie-Admissible Formulations."

Support for conferences and symposia is derived from research funds, and in evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: other proposals received by the Foundation in the same general field of science; the distribution among fields of science within the program of the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal we would be pleased to consider other proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

Enclosures

cc: R. M. Santilli
President, Institute for Basic Research
Cambridge, Massachusetts

Alvin I. Thaler
Program Director for Special Projects
Mathematical Sciences Section

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO. MCS-8303592	INSTITUTION Institute for Basic Research	PLEASE RETURN BY 3/15/83
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM Special Projects/MSS

TITLE

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Whether or not research on Lie-admissible algebras merits support of the kind requested rests on two grounds: (1) importance of the work to physics; (2) the extent and particularly the depth of the results judged as mathematics. I cannot comment on the first part except to wonder why virtually all the work is published in the somewhat obscure Hadronics Journal. Regarding the second point, there is now a considerable body of results on Lie-admissible algebras. This is respectable work produced by competent mathematicians. In reading [REDACTED] history of the subject I get the feeling that it has developed rather unsurprisingly, with appropriate use being made of Lie algebra theory and other aspects of (mostly nonassociative) algebra. But I don't see anything of real depth. This is in conformity with the fact that the people contributing papers on the subject (I exclude Albert, who apparently only contributed the definition in passing in a paper devoted to other matters) include some good mathematicians, but none of really high international stature.

My opinion, based on the mathematics as mathematics, is that work in this field is worthy of support but does not have priority.

The frequency of these conferences seems high. The proposal conference seems well-organized and appropriate people have been invited as speakers (although it appears some light-weights are included).

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

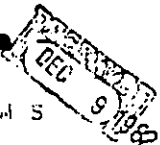
NSF Form 18 19-811
Continuation of 11 Previous Editions

PROPOSAL NO.	INSTITUTION
PRINCIPAL INVESTIGATOR	
E	

MCS-8303592

11/24

Please return
to Thaler/DMS



MENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEA
TINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This is the first time I have (in effect) been asked for a judgment on Santilli and the Hadronic Journal activities. Right away I am going to disclaim any competence to judge the physics in question. A couple of months ago I did ask the opinion of a physicist friend. He smiled and shrugged his shoulders. Well, that's not much by way of hard evidence. I shall turn to the mathematics per se.

The Albert school of nonassociative algebras has been perking along now for 40 years. It has attracted a sizeable number of able mathematicians. While largely an American enterprise, it has attracted adherents abroad, notably in the Soviet Union. But of all the numerous specialties in today's mathematical scene, it is one of the most vulnerable to the charges of being isolated and lacking significance. And indeed, except for fleeting contacts with group theory, combinatorics, and algebraic geometry, it has been largely isolated. (Of course, this could change tomorrow. Personally I expect that in due course new insights will arise that will shed a new light on the theory of general nonassociative algebras. In the meantime it seems to me that the study of various classes of algebras defined by identities has outrun its motivation and its examples. I set the dividing

(continued on a separate page)

ALL
IQ: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

tim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility, NSF, reviewers' comments will be given maximum protection from disclosure.

line at noncommutative Jordan algebras and Malcev algebras.
And that puts Lie-admissible algebras outside the pale.

The overall verdict: "good".

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO. HCS-311562	INSTITUTION Institute for Basic Research	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR	NSF PROGRAM Special Projects/MSS	
TITLE Fifth Workshop on Lie-Admissible Formulations		
<p>COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.</p> <p>This general area has not been useful or fruitful in theoretical physics, especially in particle physics.</p> <p>"Mathematical aspects are more interesting. If the proposal is to be funded, it must be judged on the basis of its mathematical merits."</p>		
<p>OVERALL RATING: <input type="checkbox"/> EXCELLENT <input type="checkbox"/> VERY GOOD <input checked="" type="checkbox"/> GOOD <input type="checkbox"/> FAIR <input type="checkbox"/> POOR</p> <p><small>Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.</small></p>		

PART XXVII:
REJECTION BY THE
NATIONAL SCIENCE
FOUNDATION OF
AN I.B.R. APPLICATION
BY TWO, SENIOR,
MATHEMATICIANS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION		
FOR CONSIDERATION BY NSF ORGANIZATION (Indicate the most specific unit known)		IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes <input type="checkbox"/> No <input type="checkbox"/> IF YES, LIST ACRONYM(S)
PROGRAM ANNOUNCEMENT/SOLICITATION NO.		CLOSING DATE (IF ANY):
NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)		
THE INSTITUTE FOR BASIC RESEARCH (I.B.R.)		
ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)		
96 Prescott Street, Cambridge, Massachusetts 02138		
TITLE OF PROPOSED PROJECT		
MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO THE GEOMETRY OF NONPOTENTIAL DYNAMICAL SYSTEMS		
REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
\$ 467,660.00	five years	March 1982
PI/PO NAME AND SOCIAL SECURITY NO. (SSN)*		PI/PO PHONE NO.
[REDACTED]		(617) 864 9859
PI/PO DEPARTMENT		PI/PO ORGANIZATION
Division of Mathematics, I.B.R. Cambridge, Massachusetts and Department of Mathematics, University		[REDACTED]
ADDITIONAL PI/PO AND SSN*		ADDITIONAL PI/PO AND SSN*
[REDACTED] Co-Inv. [REDACTED] Mathematics Department, [REDACTED] University.		[REDACTED]
ADDITIONAL PI/PO AND SSN*		ADDITIONAL PI/PO AND SSN*
FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO..		SUBMITTING ORGANIZATION IS <input type="checkbox"/> IS NOT <input type="checkbox"/> A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).
<small>*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950 as amended.</small>		
CHECK APPROPRIATE BOXES IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:		
<input type="checkbox"/> Animal Welfare <input type="checkbox"/> Human Subjects <input type="checkbox"/> National Environmental Policy Act <input type="checkbox"/> Endangered Species <input type="checkbox"/> Marine Mammal Protection <input type="checkbox"/> Research Involving Recombinant DNA Molecules <input type="checkbox"/> Historical Sites <input type="checkbox"/> Pollution Control <input type="checkbox"/> Proprietary and Privileged Information		
PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME [REDACTED]	NAME R.M. SANTILLI Soc. Sec. No. 032 46 3855	NAME
SIGNATURE [REDACTED]	SIGNATURE [Signature]	SIGNATURE
TITLE Principal Investigator	TITLE President, I.B.R.	TITLE
DATE 11-2-82	DATE 10-27-1982	DATE

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Reprints

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Academic Press New York/London (1973)

- _____

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ABSTRACT

Recent developments have lead to the identification of several new applications of nonassociative algebras, with particular reference to Lie—admissible algebras, mutation algebras, and Cayley algebras. The applications essentially deal with the description of nonpotential systems, and range from trajectory problems under drag, to computer modeling, to neural systems, etc. These developments have been promoted by the Hadronic Journal; they have been studied at four Workshops on Lie—admissible Formulations (1978—1981) as well as at the recent First International Conference on Nonpotential Interactions and their Lie—admissible Treatment; and are now coordinated by the Institute for Basic Research.

In this proposal we recommend the generalization of the familiar concept of potential dynamical systems defined by a Lie group action into that of nonpotential dynamical systems defined via the action of analytic H—spaces and nonassociative algebras. In this way, the applications of the Lie—admissible, mutation, and Cayley algebras can be unified by using reductive algebras which are tangent algebras to H—spaces that parameterize the dynamical system. This also extends the ideas of quantum dynamics and leads to (nonlinear) quadratic dynamical systems, and the utilization of nonassociative algebras for their determination. In particular, this extends the Sagle—Holmes results on the quadratic approximation for H—space multiplications, and the relationship between a Campbell—Hausdorff type formula and alternative algebras.

We then consider how these dynamical systems transform under a coordinate change and show that this leads to a new way to classify many nonassociative algebras by using a group S of coordinate changes. Thus, we may consider a direct classification of algebras by their dynamical or differential geometrical properties. For example, on a reductive space G/H , we propose to classify those algebras related by the group S which induce connections on G/H having the same geodesics.

The approach permits the determination of the physics and of the differential geometry on G/H directly in terms of the algebra inducing the connection and the corresponding quadratic approximation to the dynamical system. Thus, we shall investigate new and applicable relations between H—spaces, dynamical systems, algebras and differential geometry, by having in mind specific applications to mechanics, computer sciences, and other disciplines.



I. B. ⁹²⁰ R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 15, 1982

Dr. HARVEY KEYNES
Program Director, Modern Analysis
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Keynes,

We hereby submit for consideration by the Division of Mathematics of NSF the research grant proposal entitled

**MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND
H-SPACES WITH APPLICATIONS TO THE GEOMETRY OF NONPOTENTIAL DY-
NAMICAL SYSTEMS**

with Professor [REDACTED] as Principal Investigator, and Professor [REDACTED] as Co-Investigator.

The original, duly signed, proposal is enclosed, while nine additional samples have been mailed to you via separate parcel.

I trust in your leniency regarding the fact that its length exceeds fifteen pages. The Principal Investigators have found considerable difficulty in containing the length of the proposal to fifteen pages, owing to the novelty and diversification of the project.

I would appreciate knowing whether the consideration process takes into account the rather considerable and fast growing applications of mathematical studies on Lie-admissible algebras in particle physics, statistical mechanics, Newtonian Mechanics, and other disciplines. For this purpose, I remain at your disposal either for an outline of these applications or for the preparation of a list of physicists working in this field. Also, Professor Peter Rosen, of the Division of Physics at NSF, has a fairly complete file on this subject. I am confident you will find him very cooperative.

In addition, I would like to bring to your attention the fact that the research conducted by Professors Sagle and Holmes is very closely related and actually complementary to the studies conducted by Professors Myung, Oehmke, Tomber, Osborn, and Benkart.

Finally, I remain at your disposal for the preparation, on request, of a list of mathematician experts in Lie-admissible algebras, as well as for any additional assistance you might need.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosure

cc: Professors [REDACTED]

- 921 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

APR 14 1983

Professor [REDACTED]
Division of Mathematics
Institute For Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Professor [REDACTED]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS-8305548 for "Mathematical Studies on Reductive Lie-Admissible Algebras and R-Spaces with Applications to the Geometry of Nonpotential Dynamical Systems."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to the other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

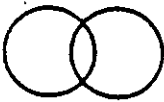
Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical & Computer Sciences

cc: Dr. R.M. Santilli
President, I.B.R.

Dr. Zbigniew H. Nitecki
Program Director for Geometric Analysis



I. B. ⁹²² R.

THE INSTITUTE FOR BASIC RESEARCH

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

April 18, 1983

Dr. E.P. INFANTE, Division Director
Mathematical & Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

RE: Research grant application entitled
"Mathematical studies on Lie-admissible algebras...."
by [REDACTED]
NSF reference number MCS-8305548

Dear Dr. Infante,

We acknowledge receipt of your letter of April 14, 1983, declining support of the proposal by [REDACTED] and [REDACTED] jointly with copies of the referees' reports. Please be reassured that our Institute shall [REDACTED] and that the same will be provided by the applicants.

Part of the I.B.R. function is to provide an independent appraisal of N.S.F. referees' reports on proposals submitted by our Institute, without any participation and prior consultation with the principal investigator and/or his/her associates. The hope is that the information may result to be of some value to N.S.F., particularly for the improvement of the refereeing process.

We are therefore taking the liberty of enclosing, most respectfully, our appraisal of the reports on the proposal by Sagie and Holmes. As you can see, we have found reports "A" and "B" to be ethically and scientifically sound, and we agree with their consideration by N.S.F. However, we have found report "C" to be definitely invalid and unsuitable for a formal consideration under a governmental process for several reasons indicates in the enclosures.

Owing to this occurrence, we would gratefully appreciate your consideration of the case and your selection of the following alternatives.

- [1] The enclosed analysis of referee's report "C" is sufficient for N.S.F. to initiate the re-consideration of the proposal, via the solicitation of a third new review; or
- [2] [REDACTED] should formally apply for a re-consideration, in case interested; or
- [3] The I.B.R. should recommend them to submit an entirely new proposal, although based on essentially the same topics.

In addition to the intrinsic mathematical merits of the proposal, we recommend that N.S.F. takes into consideration the fact that the decision whether to fund or reject the proposal by [REDACTED] (as well as the additional proposal by [REDACTED] and [REDACTED] also on Lie-admissible algebras, and the related proposal for the support of the Fifth Workshop on the topic) do imply, out of necessity, the corresponding decision whether our new Institute shall continue its activities or, regrettably, shall be suppressed.

In case I can be of any assistance for any needed additional information and/or comment, please do not hesitate to contact me.

Very Truly Yours

Ruggero M. Santilli

Ruggero M. Santilli

President

RMS:mlw

encls.

cc: Professor [REDACTED]
Dr. Z.H.NITECKI, N.S.F.

IBR APPRAISAL OF THE ENCLOSED REFEREE'S REPORT "A" ON THE PROPOSAL

"Mathematical studies on reductive Lie-admissible algebras, H-spaces,"
by [REDACTED]

The IBR considers this review ethically and scientifically sound. Indeed; the referee acknowledges explicitly the fact that the applicants, being pure mathematicians, are not expected to enter into physical (or other) applications, by therefore setting up the credibility grounds for a sound refereeing.

The only deficiency found in this report is the lack of identification of the evident fundamental character of the proposal. In fact, the project submitted by [REDACTED] and [REDACTED] deals with a generalization of a truly fundamental part of (all branches of) mathematics, Lie's theory, with evident, far reaching, mathematical and physical implications.

Despite this latter insufficiency, the IBR considers the review valid, and agrees with its consideration by NSF.

PROPOSAL NO. MCS-8305548	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY JAN 31 1983
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM

TITLE
MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

This is a long, detailed, proposal. A quick outline of the central theme is as follows. Define a nonassociative algebra to be a vector space, A , over a field F with bilinear multiplication $\alpha : A \times A \rightarrow A$. With such an algebra A , introduce another algebra, A^- , having multiplication $[x, y] = \alpha(x, y) - \alpha(y, x)$. The algebra A is called Lie-admissible if A^- is a Lie algebra.

Next, H-spaces are defined as generalizations of a Lie group, i.e., an H-space is an analytic, n -dim. manifold, M , with an analytic multiplication $\mu : M \times M \rightarrow M$ and an element (identity) $e \in M$ such that $\mu(e, m) = \mu(m, e) = m$ for all $m \in M$. (H-spaces are common in algebraic topology; differential geometric properties of H-spaces have been studied by J. Stasheff and others.) If, relative to a local coordinate patch at the identity, $F : \mathbb{R}^n \times \mathbb{R}^n \rightarrow \mathbb{R}^n$ represents μ , one expands F in a Taylor series, the second order term of which gives a bilinear form describing the "tangent algebra" of H at e . This is analogous to the derivation of the Lie algebra of a Lie group from the group multiplication. In previous work, Sagle shows (loosely stated) that for any Lie admissible algebra A there is a Lie group G and local coordinate chart at the identity (not canonical coordinates necessarily) such that A can be obtained from the second order terms of the group multiplication relative to these coordinates. A major part of this proposal deals with using tangent algebras to H-spaces, and associated differential geometric properties, to classify anti-commutative algebras.

Throughout, the abstract and proposal allude to "specific applications to mechanics, physics, computer sciences and other disciplines," but nowhere is anything even close to a specific application given. This exemplifies the "usual hope" that perhaps someone (else) will find an actual application for this nice mathematical structure! To be honest, it is a proposal to study abstract algebras and relate them to H-spaces in a way analogous to the relationship between a Lie algebra and its associated Lie group.

REPORT A

(Continued on attached page)

OVERALL RATING:	<input type="checkbox"/> EXCELLENT	<input type="checkbox"/> VERY GOOD	<input checked="" type="checkbox"/> GOOD	<input type="checkbox"/> FAIR	<input type="checkbox"/> POOR
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Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

[REDACTED] (continued)

[REDACTED] has produced some "solid" mathematical work. The text "Introduction to [REDACTED]" by [REDACTED] and [REDACTED] is a good (standard) basic text; it does not compare (at a research level) with a text such as

The (attached) Pacific J. paper "Analytic N-spaces, [REDACTED] ..." by [REDACTED] is a substantial mathematical contribution. (In my opinion, the only "solid" paper in [REDACTED] publication list.) In any case, the collaboration of [REDACTED] and [REDACTED] has shown to lead to good results.

Finally, the budget is inflated to the point of absurdity! The proposers should consider that \$18,000 (an item listed for office expenses for one year) is often the total amount for a one year grant to a mathematician (i.e., includes salary, overhead, travel; publication costs, etc.). The research proposed is not so spectacular (indeed, to a large extent it can be labeled "generalization") to warrant the level of funding requested.

REPORT A

IBR APPRAISAL OF THE REFEREE'S REPORT "B" ON THE PROPOSAL

"Mathematical studies on reductive Lie-admissible algebras, H-spaces, ..." by [REDACTED] and [REDACTED]

This referee appears to be in good faith, as evidenced by the explicit acknowledgment of lack of expertise on physical issues, and the abstention of any judgment based on that profile.

Also ethically and scientifically sound is the acknowledgment that the proposal does indeed contain "fresh ideas".

Nevertheless, the IBR disagree with the referee's appraisal of the scientific stature of [REDACTED]. Perhaps, this referee should spend some time in a research library, inspect the quotations of [REDACTED] work in physical circles (let alone in mathematical ones), and compare these quotations with those on his/her own work or that by others.

Equally gratuitous is the comment on [REDACTED]. In fact, the opposite comment would have been more pertinent. We are referring to the fact that the possible funding of the proposal would imply the possibility of major advances for [REDACTED].

Despite these shortcomings, the IBR considers this review valid, and agrees with its consideration by NSF.

PROPOSAL NO. MCS-3305546	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM

TITLE
MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE
LIE-ADMISSIBLE ALGEBRAS AND H-SPACES WITH APPLICATIONS TO

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

[REDACTED] knows a lot of the kind of mathematics which appears to be relevant to the needs of the physicists involved with the I.B.S., and it should be valuable to them to have Sagle involved too. I can make no judgment on the importance of their physics. As far as the mathematics is concerned, the proposal is a reasonable approach and contains fresh ideas---particularly attractive is the use of S in classifying algebras. The proposal is worthy of support. I am tempted to say more, that it should be supported (as regards [REDACTED]). One should keep in mind, however, that in [REDACTED] long career, while he has produced some interesting work on nonassociative algebras and then on homogeneous spaces, he really has not produced any major work. The reprints appended to the proposal are consistent with this, as they include a number of interesting observations and generalizations, but appear to contain nothing deep.

Regarding [REDACTED], his record appears to be not at all strong (distinctly weaker than [REDACTED]).

- REPORT B -

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

_____ Committee having responsibility

IBR APPRAISAL OF THE ENCLOSED REFEREE REPORT "C" ON THE PROPOSAL

"Mathematical studies on reductive Lie-admissible algebras, H-spaces,"
by [REDACTED]

The IBR considers this report invalid, and recommends NSF to abstain from its consideration during a formal governmental process.

The recommendation is based on (a) the manifest lack of knowledge of this referee of the physical applications of the Lie-admissible algebras; (b) the lack of explicit admission by this referee of such a condition; while (c) the great majority of the report is based on physical consideration, without any significant content of the true aspect of the proposal, the mathematical program.

To prevent shadows of ethical nature, this referee should have: (1) acknowledged his/her lack of expertise of the physical profile; (2) abstained from any judgment based on such a profile; and (c) restricted the report to topics of pure mathematics.

To begin, this referee states that the proposal "cites no clear case in which this relevance [of the Lie-admissible algebras in physics] is documented." This claim is false. In fact, the proposal quotes several articles by a number of physicists in which the relevance of the Lie-admissible algebras to a number of branches of physics is fully identified. The applicants were discouraged from reviewing such relevance in their proposal because the review would have been considered verbose and redundant by any expert in the field.

For the purpose of having a formal record at NSF on the issue, we mention here the following most rudimentary elements, with the understanding that the literature on the subject is already substantial.

I: Relevance of Lie-admissible algebras in quantum mechanics. It is now well known that all dissipative nuclear process have a Lie-admissible algebraic structure, and that such a structure permits the achievement of results that have not been otherwise possible until now. In fact, dissipative nuclear processes have been historically represented via non-Hermitean Hamiltonians, i.e., via non-unitary time evolutions. Their Lie-admissible re-formulation is then trivially permitted by the rules

$$A' = e^{i t H} A e^{-i t H^\dagger} \equiv e^{i B C t} A e^{-i t D B}, \quad H = B C, \quad H^\dagger = D B \\ B = B^\dagger, \quad C^\dagger = D \quad (1)$$

The relevance of the reformulation is due to the fact that the conventional form admits the infinitesimal version

$$i dA/dt = A H^\dagger - H A \quad (2)$$

which DOES NOT CHARACTERIZE A CONSISTENT ALGEBRA, trivially, because the "product" $A H^\dagger - H A$ is trilinear (being dependent on A, H AND H^\dagger). The Lie-admissible formulation, on the contrary, admits the infinitesimal form

$$i dA/dt = A D B - B C A, \quad D, C = \text{fix} \quad (3)$$

which does indeed characterize a consistent algebra, the product being a bona-fide bilinear product. In turn, the achievement of a time evolution with a consistent algebraic structure, permits the achievement of a number of physical advances that are of otherwise difficult, if not impossible derivation.

For instance, reformulation (1) permits the quantitative treatment of the irreversibility of dissipative nuclear reactions via a generalization of the principle of detailed balancing which is of direct experimental verification, and whose derivation via the conventional, nonunitary, form has remained obscure for decades. The reformulation also permits the treatment of the expected deformation of the extended charge distribution of nucleons under external strong and electromagnetic fields that also predicts experimentally measurable effects (1% deformation of the spherical shape $xx + yy + zz = 1$ into the ellipsoids $xa^{-2}x + yb^{-2}y + zc^{-2}z = 1$ for low energy neutrons in the field of Mu-metal nuclei). A number of additional direct applications (including quantum field theory) are ignored here for brevity, but the interested reader can trace them in the physical literature quoted in the proposal.

II: Relevance of Lie-admissible algebras in classical mechanics. In a way much similar to the operator case above, the "true" Hamilton's equations [those with external terms for nonpotential forces, as originally conceived by Hamilton] do not characterize a consistent algebra in the brackets of the time evolution. However, the equations can be subjected to a simple Lie-admissible reformulation by therefore achieving a consistent, bilinear, nonassociative product. The approach is not a mere mathematical curiosity. As an example, the exponentiation of Hamilton's equations with external terms is unknown, while their Lie-admissible form is. In turn, this permits the generalization of known methods of characterizing conserved physical quantities via Lie modules, into the characterization of TIME RATES OF VARIATIONS of physical quantities via a Lie-admissible bimodule, and several other developments omitted here for brevity. Significantly, the approach achieves the so-called "direct universality", that is the capability to represent in the frame of the observer all (generally nonconservative) Newtonian systems verifying minimal topological restrictions (usually, locality, regularity, and class C¹).

III: Relevance of Lie-admissible algebras in classical and quantum statistical mechanics. It is very well known that algebras and related mathematical tools have seen a limited application to statistics, when compared to particle physics. One of the reasons is the fact that virtually all collisions terms cannot be incorporated into the Hamiltonian. As a result, the brackets of the time evolutions of densities in phase space do not characterize a consistent algebra. This deficiency is removed by the Lie-admissible algebras which do permit the achievement of consistent algebras in the brackets of the time evolution for all known collisions terms in plasma equations. The implications of this occurrence are far reaching for mathematics, as any referee interested in advances can see.

Any possible residual doubt on the lack of qualification of this referee can be dissipated by the additional remarks expressed in the report. As another example, the referee states that "The principal interactions of physics are constrained by symmetries..." Such a remark applies to CLOSED, ISOLATED systems for which total conservation laws hold. All known applications of Lie-admissible algebras (as identified in the literature quoted in the proposal) are, instead, for OPEN NONCONSERVATIVE systems. Under these latter conditions, the breaking of the symmetries of the former conditions must be generally assumed to avoid evident inconsistencies (e.g., to prevent that energy is conserved for a dissipative reaction).

The mathematical part of the review, even though extremely brief, is equally deficient. As one example, the referee disclaims the relevance of the combination of nonassociative algebras and differential geometry, while such combination is notoriously basic for rigorous studies of quantization; etc. The final claim that the proposed research can be worked out by graduate students is astonishing. In fact, the proposal deals with the generalization of a truly fundamental part of contemporary mathematics. By the same token, if this report is taken seriously, NSF should abstain from funding

all faculty members [including the termination of possible support to this referee] because their research can be likely conducted by graduate students.

The [REDACTED] proposal constitutes a truly novel and fundamental project whose mathematical relevance is self-evident, and whose physical potential is equally incontrovertible. In fact, recent advances in basic knowledge have been permitted by the advances in the mathematical studies of Lie algebras (by mathematicians and physicists). There is no doubt that, lacking a corresponding mathematical study of the more general Lie-admissible algebras, physical advances along the lines indicated earlier will be suppressed.

Owing to these and other aspects, the NSF is recommended to dismiss this report from any consideration, and seek a more qualified referee.

PROPOSAL NO. MCS-8305543	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY	IV
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM		
TITLE MATHEMATICAL SCIENCES: MATHEMATICAL STUDIES ON REDUCTIVE LIE-ADMISSIBLE ALGEBRAS AND N-SPACES WITH APPLICATIONS TO			
COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.			TC)
<p>This proposal is perhaps most remarkable for its claim of relevance to theoretical physics of a rather formal conjunction of non associative algebra with general aspects of differential geometry. Unfortunately it cites no clear case in which this relevance is documented. Since it appears a priori unlikely the claim is unconvincing.</p> <p>This is of course not to say that non-associative systems may not play a significant role in physics, but only that the particular highly general and formal material proposed for investigation has no apparent non-trivial role. In particular, general classes of non-potential interactions of the types to which the proposed formalism non-trivially applies are not clearly relevant, if indeed they exist at all. The principal interactions of physics are constrained by symmetry and/or causality considerations, and it is not shown that the proposed formalism has anything useful to offer in connection with them.</p> <p>The main point remaining in the proposal would be purely mathematical work. This is tenable but the mere association of two fields such as non associative algebra and differential geometry is not in itself necessarily of much interest, and the burden of proof is on the proposer to cite interesting and non-trivial developments. What does appear is the kind of study that any competent mathematician can readily execute when needed; suitable for use as graduate student exercises, no doubt, but not clearly anything beyond that.</p> <p style="text-align: center;"><u>REPORT C</u></p>			
OVERALL RATING: <input type="checkbox"/> EXCELLENT <input type="checkbox"/> VERY GOOD <input type="checkbox"/> GOOD <input type="checkbox"/> FAIR <input checked="" type="checkbox"/> POOR			
Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.			

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

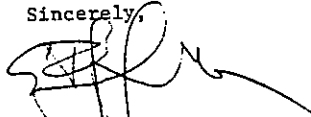
April 29, 1983

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

Dear Dr. Santilli:

I am writing in response to your letter of April 18, concerning the proposal MCS-8305548 submitted to the National Science Foundation by your organization with [REDACTED] as principal investigator. Enclosed is a copy of NSF Notice No. 84, which explains the Foundation's procedures for reconsideration of declined proposals. Please note that such procedures are initiated by the principal investigator for the proposal, rather than the officials at the submitting organization. Similarly, in accordance with NSF policy, verbatim copies of reviews are sent only to the principal investigator, and are intended to be treated as confidential information. Therefore, any discussion with you concerning these reviews would not be appropriate. The relevant program officer, Dr. Nitecki, or I will be happy to discuss this declination, and the reviews on which the decision was based, with the principal investigator for the proposal.

Sincerely,



E. F. Infante
Division Director
Mathematical and Computer Sciences

Enclosure

cc: Professor [REDACTED]
[REDACTED]

Dr. Z. Nitecki
Program Director for Geometric Analysis

PART XXVIII:
REJECTION BY THE
NATIONAL SCIENCE
FOUNDATION
OF AN I.B.R.
APPLICATION BY
THREE, SENIOR,
MATHEMATICIANS

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION

FOR CONSIDERATION BY NSF ORGANIZATION UNIT (Indicate the most specific unit known, i.e. program, division, etc.)		IS THIS PROPOSAL BEING SUBMITTED TO ANOTHER FEDERAL AGENCY? Yes <input type="checkbox"/> No <input type="checkbox"/> ; IF YES, LIST ACRONYM(S):	
PROGRAM ANNOUNCEMENT/SOLICITATION NO.:		CLOSING DATE (IF ANY):	
NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS) THE INSTITUTE FOR BASIC RESEARCH (I.B.R.)			
ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE) 96 Prescott Street, Cambridge, Massachusetts 02138			
TITLE OF PROPOSED PROJECT STUDIES ON LIE-ADMISSIBLE ALGEBRAS			
REQUESTED AMOUNT \$ 292,2210	PROPOSED DURATION Three years	DESIRED STARTING DATE March 1983	
P/IPD NAME AND SOCIAL SECURITY NO. (SSN)* [REDACTED]		P/IPD PHONE NO. (617) 864 9859	
P/IPD DEPARTMENT Division of Mathematics, I.B.R. and Dept. of Math., Univ. of [REDACTED]		P/IPD ORGANIZATION [REDACTED]	
ADDITIONAL P/IPD AND SSN* [REDACTED] tel.s (617) 864 9859/ Div. of Math., I.B.R., and Dept of Math. Univ. of [REDACTED]			
ADDITIONAL P/IPD AND SSN* [REDACTED] tel.s (617) 864 9859/ Div. of Math. I.B.R., and Dept. of Math., [REDACTED] Univ., [REDACTED]			
FOR RENEWAL OR CONTINUING AWARD REQUEST, LIST PREVIOUS AWARD NO.:		SUBMITTING ORGANIZATION IS <input type="checkbox"/> IS NOT <input type="checkbox"/> A SMALL BUSINESS CONCERN (see CFR Title 13, Part 121 for definitions).	
*Submission of social security numbers is voluntary and will not affect the organization's eligibility for an award. However, they are an integral part of the NSF information system and assist in processing the proposal. SSN solicited under NSF Act of 1950, as amended.			
CHECK APPROPRIATE BOX(ES) IF THIS PROPOSAL INCLUDES ANY OF THE ITEMS LISTED BELOW:			
<input type="checkbox"/> Animal Welfare <input type="checkbox"/> Human Subjects <input type="checkbox"/> National Environmental Policy Act			
<input type="checkbox"/> Endangered Species <input type="checkbox"/> Marine Mammal Protection <input type="checkbox"/> Research Involving Recombinant DNA Molecules			
<input type="checkbox"/> Historical Sites <input type="checkbox"/> Pollution Control <input type="checkbox"/> Proprietary and Privileged Information			
PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR		AUTHORIZED ORGANIZATIONAL REP.	
NAME [REDACTED]	NAME R.M. SANTILLI	NAME	
SIGNATURE [REDACTED]	SIGNATURE [REDACTED]	SIGNATURE	
TITLE Principal Investigator	TITLE President, I.B.R.	TITLE	
DATE 10-21-1982	DATE 10-18-1982	DATE	

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Information on The Institute for Basic Research

ABSTRACT

Lie-admissible algebras were introduced in 1948 by A.A. Albert. In 1967 R.M. Santilli first pointed out that Lie-admissible algebras may be more appropriate than Lie algebras for studying physical processes. Santilli refined his idea in a sequence of papers over several years. Meanwhile a few mathematicians wrote on the structure and classification of Lie-admissible algebras as a topic in pure mathematics. With the inception of the annual Workshops on Lie-Admissible Formulations in 1978, physicists and mathematicians began to meet together to discuss their interests in Lie-admissible algebras.

Since 1978 there has been growing evidence, at first theoretical but now based on experimental results, that Lie-admissible algebras are a proper mathematical tool to formulate and solve a number of physical problems. During the Fourth Workshop held in August 1981, it became clear that physics would benefit from solutions to certain mathematical problems. They include the development of a representation theory and universal envelope for Lie-admissible algebras, and classification and structure theory especially for mutation algebras. The principal investigators propose to work on these problems and other problems that the physics will suggest during the course of the Investigation.

The applications of the mathematical tools to be developed under this research project are rather promising and of diversified nature, encompassing a number of branches of physics, engineering, and applied mathematics at large. In fact, a number of recent papers have indicated that the theory of Lie-admissible algebras can be applied to: Newtonian mechanics and space mechanics (e.g. trajectory problems under drag forces); statistical mechanics and plasma physics (e.g. statistical ensembles inclusive of inelastic collisions and nonlocal nonpotential internal forces); particle physics (e.g. for the treatment of strong interactions as of nonlocal nonpotential type due to wave overlapping of particles); computer modeling and engineering (e.g., electrical circuitry and electronics with internal losses); and other fields.



I. B. ⁹³⁷ R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 29, 1982

Dr. HARVEY KEYNES
Program Director
Modern Analysis
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Keynes,

We hereby submit for consideration by the Division of Mathematics of NSF the research grant proposal entitled

STUDIES ON LIE-ADMISSIBLE ALGEBRAS

with Professor [REDACTED] as Principal Investigator, and Professors [REDACTED] and [REDACTED] as Co-Investigators.

The original, duly signed, proposal is enclosed, while nine additional samples have been mailed to you via separate parcel.

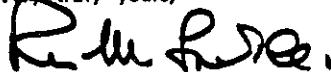
To my understanding, the proposal might qualify along the lines of a group proposal. Therefore, I trust in your leniency regarding the fact that its length exceeds fifteen pages. At any rate, the Principal Investigators have found considerable difficulty in containing the length of the proposal to fifteen pages, owing to the novelty and diversification of the project.

I would appreciate knowing whether the consideration process takes into account the rather considerable and fast growing applications of mathematical studies of Lie-admissible algebras in particle physics, statistical mechanics, Newtonian Mechanics, and other disciplines. For this purpose, I remain at your disposal either for an outline of these applications or for the preparation of a list of physicists working in this field. Also, Professor Peter Rosen, of the Division of Physics of NSF, has a fairly complete file on this subject. I am confident you will find him very cooperative.

In addition, I would like to bring to your attention the fact that the research conducted by Professors [REDACTED] is very closely related and actually complementary to the studies conducted by Professors [REDACTED] under support of the Division of Mathematics of NSF.

Finally, I remain at your disposal for the preparation, on request, of a list of mathematicians experts in Lie-admissible algebras, as well as for any additional assistance you might need.

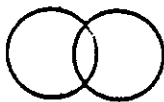
Very truly yours,

A handwritten signature in dark ink, appearing to read 'Ruggero M. Santilli'.

Ruggero M. Santilli
President

RMS/mlw

Enclosure



I. ⁹³⁹ B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

January 24, 1983

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Dr. R.E.KAGARISE
Acting Assistant Director
Division of Mathematics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Kagarise,

I am contacting you in regard to the following research grant proposals our Institute has submitted to your Division:

PROPOSAL 1:

TITLE: *Studies on Lie-admissible Algebras*

PRINCIPAL INVESTIGATORS: Professors [REDACTED] and [REDACTED]

DATE OF SUBMISSION: October 29, 1982

NSF ID NO: MCS-8303574

PROPOSAL 2:

TITLE: *Mathematical studies on reductive Lie-admissible algebras and H-spaces, with applications to the geometry of nonpotential dynamical systems*

PRINCIPAL INVESTIGATORS: Professor [REDACTED] and [REDACTED]

DATE OF SUBMISSION: November 15, 1982

NSF ID NO: MCS-8305548

PROPOSAL 3:

TITLE: *Fifth Workshop on Lie-admissible Formulations*

PRINCIPAL INVESTIGATORS: Professors [REDACTED]

DATE OF SUBMISSION: November 4, 1982

NSF ID NO: MCS-8303592

We would appreciate the indication of the period of time in which a decision on these applications could be reached. The information would be particularly valuable for our planning, particularly for Proposal 3 dealing with the organization of a meeting. In fact, we have prepared the announcement of the meeting, but we are delaying its distribution pending the decision for possible funding.

I am taking the opportunity of enclosing a general presentation of the research conducted at our Institute with the emphasis on the experimental and theoretical profiles. We hope that the presentation may give an idea of the value of the research in Lie-admissible generalization of Lie theory, along the lines of the proposals under consideration at your Division.

Very Truly Yours

Ruggero M. Santilli

Ruggero M. Santilli

President

cc: Professors [REDACTED]

RMS—mlw

encls.

NATIONAL SCIENCE FOUNDATION
WASHINGTON DC 20550

January 27, 1983

Dr. Ruggero M. Santilli, President
Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

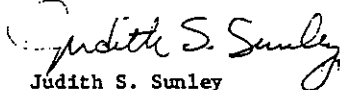
Dear Dr. Santilli:

The proposal, "Studies on Lie-Admissible Algebras," submitted by your organization on behalf of Professors ~~Myung, Oehmke and Tromber~~ has been assigned to the Algebra and Number Theory Program. It will now be reviewed and evaluated before a recommendation is made on its final disposition.

A number of factors go into the recommendation, including the vitality of the area of investigation and the potential for applications, as mentioned in your letter. Nonetheless, I must point out that budgetary considerations force the declination of a number of very strong proposals every year. It is impossible to forecast the outcome of the evaluation process at this early stage.

Finally, I am enclosing copies of a letter which has been sent to Professors Myung, Oehmke and Tromber. Please feel free to respond to the questions raised there if such a response could clarify the situation.

Sincerely yours,


Judith S. Sunley
Program Director for
Algebra and Number Theory

Enclosures

- 941 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Professor [REDACTED]
Department of Mathematics
Michigan State University
[REDACTED]

Dear Professor [REDACTED]

We have received your proposal entitled, "Studies on Lie-Admissible Algebras" submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at Michigan State University. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.

While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than Michigan State University.


Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:

[REDACTED]
Vice President
Finance and Operations
Michigan State University

R. M. Santilli, President
Institute for Basic Research



- 942 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON D C 20550

January 27, 1983

Professor [REDACTED]
Department of Mathematics
University [REDACTED]
[REDACTED]

Dear Professor [REDACTED]

We have received your proposal entitled, "Studies in Lie-Admissible Algebras," submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at the University of Northern Iowa. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.


While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than the University of Northern Iowa.

Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:
Authorized Organizational Representative
University of [REDACTED]

R. M. Santilli, President
Institute for Basic Research



- 943 -
NATIONAL SCIENCE FOUNDATION
WASHINGTON DC 20550
January 27, 1983

Professor [REDACTED]
Department of Mathematics
University of [REDACTED]
[REDACTED]

Dear Professor [REDACTED]

We have received your proposal entitled, "Studies on Lie-Admissible Algebras" submitted through the Institute for Basic Research. While it is not clear where the proposed work would be performed, it appears to us that it would be carried out at the University of Iowa. If this is so, we will need a letter signed by the appropriate university business officer (not the department chairperson) stating that the necessary university facilities, etc., will be made available without charge, if such is the case, or what arrangements the university deems appropriate. The arrangements for secretarial service, etc., should also be specified.

While such matters as health insurance, workman's compensation, social security, etc., are not properly our concern in a specified case, we suggest that you may wish to explore your situation in such matters during periods when you are employed by an institution other than the University of Iowa.


Sincerely yours,

Judith S. Sunley
Program Director for
Algebra and Number Theory

Copy to:

[REDACTED]
Vice President for Educational
Development and Research
University of [REDACTED]

R. M. Santilli, President
Institute for Basic Research





February 8, 1983

Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, D.C. 20550

Dear Ms. Sunley:

I am writing to you to answer the questions raised in your letter of January 27, 1983 to Professor [REDACTED] related to his proposal "Studies in Lie-Admissible Algebras" submitted through the Institute for Basic Research.

Professor [REDACTED] work will be carried out at the University [REDACTED]. He will retain his office and access to any other university facilities which his work requires. [REDACTED], chair of the department of Mathematics and Computer Science has assured me that the department will also provide the necessary secretarial support (estimated by Professor [REDACTED] at 40 hrs.) for [REDACTED] work under the grant.

Thank you for your mention of the matter of fringe benefits. The university will continue to provide all normal annual benefits for Professor [REDACTED] during the period of the grant.

If you have any further questions about the University's support for Professor [REDACTED], do not hesitate to contact me or [REDACTED].

Sincerely,

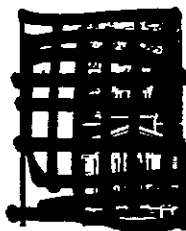

[REDACTED]
Administrator

HJB/ds

cc: [REDACTED]
[REDACTED]

The University [REDACTED]
[REDACTED]

- 945 -



1047

February 9, 1983

Ms. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, D.C. 20550

Dear Ms. Sunley:

In response to your letter of January 27, 1983 concerning the proposal "Studies on Lie-admissible Algebras" submitted by the Institute for Basic Research I would like to make the following comments.

During selected time periods I would be employed by the Institute of Basic Research to perform the activities as delineated in the proposal; most likely during some Summer months.

At no time would there be a conflict or overlap of my employment by the University [REDACTED] and my employment by the Institute of Basic Research.

All administrative expenses accrued in the implementation of the grant would be the responsibility of the Institute of Basic Research.

If secretarial funds are provided by the grant and if it is necessary to have typing done in [REDACTED] such typing would be privately contracted and paid from the grant funds.

It is anticipated that no University facilities will be required.

Sincerely,

[REDACTED]

Professor and Chairman
Department of Mathematics

Approved by:

[REDACTED]

[REDACTED]
Research Coordinator
Office of the Vice President
For Educational Development and Research
Acting for [REDACTED]
Vice President and Dean

UNIVERSITY

VICE PRESIDENT FOR FINANCE AND OPERATIONS AND TREASURER
CONTRACT AND GRANT ADMINISTRATION
[REDACTED]
[REDACTED]

February 2, 1983

Dr. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, DC 20550

Dear Dr. Sunley:

In response to your letter of January 27, 1983, [REDACTED] University would be pleased to provide office space, library privileges and other customary services, specifically, including secretarial work to Dr. [REDACTED] during the period of his proposed grant with the National Science Foundation through the Institute for Basic Research.

Very truly yours,

[REDACTED]
[REDACTED] Assistant Director
Contract and Grant Administration

APPROVED: [REDACTED]

[REDACTED] Assistant Vice President

[REDACTED] UNIVERSITY

VICE PRESIDENT FOR FINANCE AND OPERATIONS AND TREASURER
CONTRACT AND GRANT ADMINISTRATION

February 2, 1983

Dr. Judith S. Sunley
Program Director for
Algebra and Number Theory
National Science Foundation
Washington, DC 20550

Dear Dr. Sunley:

In response to your letter of January 27, 1983, [REDACTED] University would be pleased to provide office space, library privileges and other customary services, specifically, including secretarial work to Dr. [REDACTED] during the period of his proposed grant with the National Science Foundation through the Institute for Basic Research.

Very truly yours,

[REDACTED]
Assistant Director
Contract and Grant Administration

APPROVED:

[REDACTED]
Assistant Vice President

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

February 10, 1983

Professor Ruggero M. Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

I am replying to your letter of January 24 to Dr. Kagarise concerning three proposals submitted to the National Science Foundation by the Institute for Basic Research.

MCS-83-03574, "Studies on Lie-admissible Algebras", was logged in on November 17, 1982.

MCS-83-05548, "Mathematical studies on reductive Lie-admissible algebras and H-spaces, with applications to the geometry of nonpotential dynamical systems", was logged in on December 30, 1982.

MCS-83-03592, "Fifth Workshop on Lie-admissible Formulations", was logged in on November 17, 1982.

As stated in Grants for Scientific Research, NSF-81-79h, enclosed, "applicants should allow 6 to 9 months for review and processing... Every effort is made to reach a decision and inform the applicant promptly." We do aim for the 6 month end of the range but vagaries of the return of reviews by the ad hoc mail reviewers, changes in workloads, etc., do sometimes make that impossible. I have checked with the program directors who have been assigned the responsibility of handling these proposals and have been told that the review and evaluation process seems to be proceeding routinely.

Please feel free to write or to call (202-357-7341) if you have any further questions.

Sincerely yours,



William G. Rosen
Head
Mathematical Sciences Section



I. B. ⁹⁴⁹ 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 3, 1983

Ms. JUDITH S. SUNLEY
Program Director
National Science Foundation
WASHINGTON, D.C. 20550

RE: Applications entitled
STUDIES ON LIE-ADMISSIBLE ALGEBRAS

Principal Investigators: Drs. ~~Frank J. Yndestad, Jr. and~~
and ~~William J. Yndestad~~

Dear Ms. Sunley,

It is our understanding that, following your request of January 27, all administrations of the principal investigators have provided you with a formal authorization for a possible administration of the contract by the I.B.R.

We hope that the answers you received are satisfactory, and that the consideration of the proposal can now proceed toward a speedy resolution.

Your interest in the proposal is sincerely and gratefully appreciated.

Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

cc: Drs. ~~Frank J. Yndestad, Jr. and~~

— 950 —
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Mathematical and Computer Sciences
Mathematical Sciences Section

APR 21 1983

Dr. [REDACTED]
Department of Mathematics
University [REDACTED]
[REDACTED]

Dear Dr. [REDACTED]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS83-03574 for "Studies on Lie-Admissible Algebras."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,

E. F. Infante
Division Director
Mathematical and Computer Sciences

cc: Dr. R. M. Santilli, President
Institute for Basic Research
Cambridge, Massachusetts 02138

Dr. Judith S. Sunley
Algebra and Number Theory

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO. PCS-8303574	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM ALGEBRA AND NUMBER THEORY	
TITLE MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS		

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Whether or not research on Lie-admissible algebras merits support of the kind requested rests on two grounds: (1) importance of the work to physics; (2) the extent and particularly the depth of the results judged as mathematics. I cannot comment on the first part except to wonder why virtually all the work is published in the somewhat obscure Hadronics Journal. Regarding the second point, there is now a considerable body of results on Lie-admissible algebras. This is respectable work produced by a competent mathematicians. In reading Tomber's history of the subject I get the feeling that it has developed rather unsurprisingly, with appropriate use being made of Lie algebra theory and other aspects of (mostly nonassociative) algebra. But I don't see anything of real depth. This is in conformity with the fact that the people contributing papers on the subject (I exclude Albert, who apparently only contributed the definition in passing in a paper devoted to other matters) include some good mathematicians, but none of really high international stature.

My opinion, based on the mathematics as mathematics, is that work in this field is worthy of support but does not have priority.

Comments on the individual mathematicians applying: [REDACTED] has done almost no research since early in his career, and even then did only a very small amount; a weak case for support.

[REDACTED] did some significant work on nonassociative algebras earlier in his career and then some other work (semigroups) before apparently recently turning to Lie-admissible algebras. He is a competent mathematician who can make reasonable contributions but I don't foresee him doing something profound; worthy of support.

[REDACTED] His early work was industrious but apparently unexciting. Then in 1978 he began publishing copiously on Lie-admissible algebras in the Hadronic Journal. Since he is, in volume, by far the largest contributor to the subject, my remarks about the subject apply especially to him; worthy of support.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO. HCS-8303574	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM ALGEBRA AND NUMBER THEORY	MD
TITLE MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS		

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

I have to disclaim any ability to judge the relevance of the proposed research to physics. I turn to the mathematics per se.

Of the numerous specialties in today's mathematical scene, the Albert school of nonassociative algebras is unusually vulnerable to the charge of being isolated and lacking significance. Of course this could change tomorrow. In the meantime it seems that the study of various classes of algebras defined by identities has outrun its motivation and its examples. Overall verdict: "good".

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act; 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
Supersedes All Previous Editions

PROPOSAL NO. MCS-8303574	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR C	NSF PROGRAM ALGEBRA AND NUMBER THEORY	

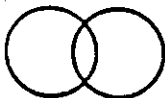
TITLE
MATHEMATICAL SCIENCES: STUDIES ON LIE-ADMISSIBLE ALGEBRAS

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Some aspects of this proposal look interesting, but at the present time, I do not think that the theory of Lie-admissible algebras has proved its importance. Perhaps my evaluation should be taken as a challenge to the proposers to uncover deeper mathematical or physical phenomena. When and if they can do this, their proposals will be stronger. I would be very happy to see my evaluation proved wrong by future significant discoveries by the proposers.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.



I. B. ⁹⁵⁴ - R.

THE INSTITUTE. FOR BASIC RESEARCH

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

April 30, 1983

Dr. E.F. INFANTE, Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Infante,

I feel obliged to express my extreme reservation regarding the way your division has handled the application by Professors [redacted] (Univ. of [redacted] and IBR), [redacted] (Univ. of [redacted] and IBR), and [redacted] ([redacted] Univ.), NSF ref. no. MCS-8303574. I am referring first to the fact that, early in 1983, your office contacted the administration of each primary affiliation of the applicants to request authorization for the IBR administration of the (possible) grant. This contact was made without any prior knowledge whatsoever, either to us here, or to any of the applicants. The subsequent rejection then seems to confirm rumors repeatedly heard in academic corridors, that at times NSF does not stop at the rejection of grants, but goes ahead with actions which, whether intentionally or accidentally, have the net result of discrediting or otherwise damaging the applicants, and/or their administrative conduits.

When, on the top of this, you see that the numerical majority of the referees (the 2/3) warmly recommends funding, that the applicants are all senior, full professors, and that the content of the proposal is truly of potentially fundamental advances, then you can dispel shadows of political manipulations, including the possibility that the negative decision was the result of pressures by corrupt elements outside your division.

You should keep in mind that the preceding application rejected by your office had also received the favorable support by the numerical majority of the referees (2/3) and was equally of high caliber, both for the applicants and for contents. I am referring to the application by Professor [redacted] (Univ. [redacted] and IBR) and [redacted] ([redacted]), NSF ref. No. MCS-8305548. The question we must therefore naturally ask from this repetitive pattern, is whether your chain of rejections (and additional ones now expected) is a form of language to express a conceivable opposition by NSF and its academic affiliate against the organization of our Institute as currently conceived, that is, in a way independent from academic-financial-ethnic interests at Harvard University and at the Massachusetts Institute of Technology, which way is evidently essential for genuine advance.

In the primary interest of NSF, I would like to recommend that the entire situation of our various applications to both your division as well as to the division of physics be reviewed, possibly with direct consultation with the NSF Director's Office and The White House Office of Science and Technology.

In particular, I suggest that my original suggestion be considered in the appropriate diversification of aspects. I am referring to the granting of an institutional support to the IBR which combines all our projects in experimental physics, theoretical physics, mathematics, international meetings, and related activities.

As you know, the IBR was funded by a group of independent experimentalists, theoretician and mathematicians for the specific purpose of attempting a generalization of Einstein's special relativity for strong interactions. Our experimental, theoretical, and mathematical programs are all deeply inter-related toward this single goal. The elements for success are there at all levels. I am referring here to the technical capability of available laboratories to conduct the needed tests; to the theoretical elaborations; and most importantly, to the backgrounds mathematical research.

A (very brief) summary paper on the generalization of the special relativity is enclosed, while over 10,000 pages of published research are at your disposal, including five research monographs, nine volumes of reprints of international meetings, and a massive number of papers.

As you can see, the main idea is that hadrons, since are extended in space, are deformable under sufficiently intense external fields and/or collisions, that is, their spherical charge distribution $xx + yy + zz = 1$ can be deformed into ellipsoids $xa^2x + yb^2y + zc^2z = 1$ with consequential, manifest, breaking, first, of the rotational symmetry and, second, of the entire Lorentz symmetry, although in a sufficiently small amount.

Our experimental program (which we could not submit to the NSF division of physics because of difficulties beginning with the presentation) is centered in the repetition of the experiment Prof. Rauch (Director of the Atominstitut of Wien, Austria) has been conducted since 1975 via neutron interferometry. NOTE THAT HIS LATEST MEASURES CONFIRM THE BREAKINGS.

Our theoretical program is centered in the achievement of the so called isotopic and genotopic liftings of quantum mechanics in its various aspects, as an operator image of the generalization of classical Hamiltonian mechanics already achieved by our group (the so-called Birkhoffian mechanics), and as described in more details in our summer meetings.

Finally, our mathematical studies concern truly vital information for the above theoretical and experimental research. It can only be done, to our knowledge, via a generalization of the very heart of contemporary mathematics, Lie's theory. This is exactly the topic of the chain of proposals you have rejected.

Apart from evidently manifest, large scientific implications, I feel obliged to bring to your personal attention, to the attention of the NSF Director and to the attention of The White House Office of Science and Technology, the potentially significant military damage which may result by this NSF posture of chains of rejections. Admittently, none of these applications has been studied so far. However, you are aware that Einstein's special relativity permitted the discovery of two new basic weapons that have changed the face of the world, fission and fusion bombs. It is known in the scientific environment that, by no means, these are the only ways of extracting weapons (or energy, if you prefer) from hadrons. Simply we have not yet found other alternatives. Then a possible generalization of the special relativity specifically conceived for hadrons has a self-evident potential for truly basic, new, military applications I leave to your imagination while, on my part, I intend to be silent at this time.

The difficulties we have encountered in the conduction of our research have been simply beyond the wildest imagination. In fact, the program started under DOE support while I was a member of Harvard University, but, the moment it was clear in its objectives, I was forced to leave Harvard despite the availability of support, and we even received a formal prohibition to hold our third workshop on Harvard premises because occurring a few weeks after the termination of my employment (although its organization was an important part of my contract). Similar interferences and opposition occurred at MIT leaving us no other alternative than that of organizing an independent institute of research.

These occurrences should be openly, plainly, and clearly indicated as a necessary condition to achieve a maturity of judgment in our grant applications. In fact, the selection of referees from these local institutions would be a mere farse under the circumstances. At any rate, all the numerous episodes are fully documented and well known to many. Most importantly, the clear identification of this situation is essential for your achievement of maturity of final decisions.

The ultimate reasons for this organized, at time hysterical opposition we have encountered is also known and it is not related to our persons (in fact I personally have several friends at both Harvard and MIT). It is due to the vested, organized, academic-financial-

ethnic interests that surround or, better, are based on Einstein's special relativity, as I am sure all of you can imagine if not personally knowledgeable of it. These interests are therefore opposed to the very idea of the generalization of the special relativity.

However, their credibility is virtually null, and their political nature is manifest. In fact, you do not have to be a physicist or a mathematician to see that a sphere can be deformed into an ellipsoid, with consequential breaking of spherical symmetry (the breaking of the Lorentz symmetry is then a mere technical consequence).

Thus, our strength rests on the fact that the ordinary taxpayer can readily see the academic dances regarding the currently assumed exact validity of the special relativity in particle physics. In fact, every taxpayer can see that a sphere can be deformed, and that the academic baron is doing dances for his/her own personal interests, but certainly not in the interest of advances, whenever he/she claims that extended particles are absolutely rigid (a necessary condition to salvage the special relativity).

The implications for NSF regarding this situation are staggering. I have attempted a number of times to bring them to the attention of the various officers throughout a number of years (over a decade) with total and complete failure until now.

Quite openly, I have now reached a point where I begin to have doubts on the orderly communication of the information, and that perhaps a full disclosure to the U.S. (as well as the international) community is more appropriate. We should not forget that NSF is spending truly large amounts of taxpayers money on the MERE BELIEF of the exact character of the special relativity for strong interactions, while ALL our research grant applications throughout the years on the problem have been rejected. This includes the rejection of the primary application of the IBR, that on physics, made quite recently. Needless to say the application was exactly on the fundamental aspects of the generalization of the special relativity (liftings of the Hilbert space).

At any rate, time is running out for all, the NSF and the IBR. Our institute was funded as a result of (for us) immense sacrifices. It has been operated now for two years without one penny of governmental support. As chief executive officer I must take all the necessary precaution to prevent further damage to its members resulting from the rather massive chains of rejections we have seen during this period. Most importantly, we must take a number of decisions that are predictably difficult for all.

I am therefore recommending that, whatever decision may be taken by your office or by the NSF Director's Office, if any, should be taken in the very near future. There is simply no more time for additional time-extensive investigations and considerations.

Very Truly Yours



Ruggero Maria Santilli
President

cc.: ~~XXXXXXXXXX~~ and Dr. E. Knapp, NSF Director

PART XXIX:
REJECTION BY
THE NATIONAL SCIENCE
FOUNDATION OF
AN I.B.R.
APPLICATION BY
TWO SENIOR
PHYSICISTS

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

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE
PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

Proposed Starting Date

March 1983




Proposed Duration



One Year


Amount Requested

\$ 95,400

ENDORSEMENTS


Principal Investigator
(experimental physics)
 National Laboratory

I. B. R., Cambridge, MA


Co-Investigator
(theoretical physics)
The Institute for Basic Research
Cambridge, Massachusetts 02138



R. M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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ENCLOSURES

- [1] [REDACTED]
[REDACTED]
- [2] [REDACTED]
[REDACTED]
[REDACTED] internal note no. 403 (1970)
- [3] [REDACTED]
[REDACTED]
[REDACTED] Nuovo Cimento 2, 402 (1971)

REFERENCE ADDED IN PROOF:

- A. [REDACTED]
Physical Review Letters 14, 10, p. 403 (1965)

ABSTRACT

Despite well known technological advances, the electric field of electrons is still assumed to be of the type conceived by Coulomb back in 1785, that is, to be constant at fixed distances. Santilli recently remarked that, if the electron has any dynamical structure (e.g., it is an elementary oscillation of space), its field will likely possess an explicit time dependence. He therefore proposed the hypothesis according to which the electric field of individual electrons has an oscillatory behaviour in time with frequency of $7.57 \times 10^{20} \text{ sec}^{-1}$, by therefore being inclusive of both attractive and repulsive actions. Their separation results into a pulsating force and occurs during the interactions of individual pairs of electrons, positrons, or electron-positrons. When a sufficiently large collection of electrons is considered, all pulsating effects disappear owing to the very high frequency, and the conventional Coulomb law is recovered. A number of conceivable experiments were indicated, including those via the use of the positronium.

Independently from these studies, [redacted] and his associates proposed the hypothesis that the positronium admits the C-violating decay $^1S_0 \rightarrow 3\gamma$ besides the conventional C-conserving decay $^3S_1 \rightarrow 3\gamma$, and predicted the ratio of the rates of these decays to be of the order of 10^{-10} . The hypothesis was formulated on the basis of a number of similarities existing between the decay of the positronium and that of $K^0_{S,L}$ particle. The established CP violation of the latter then suggested a conceivable C-violation of the former. [redacted] hypothesis can be tested today thanks to advances in particles accelerators and detectors.

[redacted] hypotheses are clearly inter-related, inasmuch the former provides a theoretical background of the latter. This proposal recommends the conduction of a comprehensive study of the hypotheses considered, ranging from theoretical studies of mutual compatibility and compatibility with existing data, to the feasibility study of a number of experiments. The study of possible implications of the hypotheses for computers, solid state, and other systems is also recommended.



Department of Energy
Washington, D.C. 20545

OCT 28 1982

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

Dear Dr. Santilli:

I have reviewed your preliminary proposal, "Theoretical and Experimental Studies on a Possible Pulsating Structure on the Coulomb Force of Individual Electrons", and find that the subject matter is substantially distinct from that research eligible for support within the reach of the Atomic Physics Program.

As a result of the above, it should come as no surprise to you that I am not able to refer to you any work that treats the same subject as described in your proposal.

It appears appropriate ^{for you} to discuss the intent of your above-stated research with members of the Office of High Energy and Nuclear Physics. Since you have or have had support from that Office, you are probably better informed than I on whom to contact.

Sincerely,

J. V. Martinez
Fundamental Interactions Branch
Division of Chemical Sciences
Office of Basic Energy Sciences

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

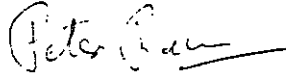
October 12, 1982

Dr. R. M. Santilli, President
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

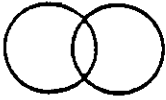
Dear Dr. Santilli:

Thank you for your letter of 4 October 1982 concerning the preliminary draft of the proposal by ~~XXXXXXXXXXXX~~. After examining it, I would suggest that the Elementary Particles Program under Dr. David Berley would be suitable for the experimental aspects and the Theoretical Physics Program under Dr. Boris Kayser for the theoretical aspects.

With best wishes,

A handwritten signature in cursive script, appearing to read "Peter Rosen", with a horizontal line extending from the end of the signature.

S. Peter Rosen
Program Associate for
Theoretical Physics



I. B.⁹⁶³ - R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 6, 1983

Dr. DAVID BERLEY
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Berley,

I respectfully submit for consideration by N.S.F. the enclosed original application entitled

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING
STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

with Principal Investigator Professor [REDACTED]

The application is submitted to you because of its primary emphasis on the formulation of experiments for the resolution of the problem at some future time. Theoretical aspects are considered, but only in a way subordinate to this primary goal.

The application has been submitted jointly to your Office and to the Division of High Energy Physics of the Department of Energy. Any additional submission will be promptly communicated to you.

I may add that an informal advance consultation on the project was submitted to DARPA (Dr. C. Romney, Deputy Director). It was agreed that the proposal is of too basic character to be within DARPA's guidelines. However, it was also agreed that, in case the basic aspects are positively resolved, we shall contact DARPA again because of a number of rather intriguing possibilities of military applications of this possible pulsating effect, let alone conventional non-military applications.

Your consideration of the proposal is appreciated. In case you need any additional assistance, please do not hesitate to let me know.

Very Truly Yours

Ruggero M. Santilli
President

RMS-mlw



I. B. R. - 964 -

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

January 6, 1983

Dr. W.A.WALLENMEYER, Director ER-22
Division of High Energy Physics
DEPARTMENT OF ENERGY - GIN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I respectfully submit for consideration by your office the research grant application entitled

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE PULSATING
STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL ELECTRONS

under the Principal Investigator, Prof. [REDACTED]. The original is enclosed, while nine additional copies have been separately mailed to you.

The proposal is submitted jointly to your Office and to the Division of Physics of the NSF. Any additional formal submission will be promptly communicated to you.

I may add that an informal, advance-consultation on the project occurred with Dr. C. Romney (202 694 3035), Deputy Director of DARPA. It was agreed that the proposal is of too basic character to be within DARPA's guidelines. However, it was also agreed that we should keep DARPA informed of possible positive outcomes of the basic aspects of the projects because of rather intriguing military possibilities (non-military possibilities are self-evident for the project).

You should be informed that the military possibilities are foreseen as conceivable at this time when the project submitted here is combined with our main research grant application on the development of the hadronic mechanics (the mechanics specifically conceived for the interior of strongly interacting systems). In fact the possibilities deal with the case when electrons are totally immersed within hadrons (in which case the insufficiency of conventional quantum mechanics is more transparent), and deal with systematic studies on the problem whether fusion and fission are the only forms of hadronic weapons, or other forms are possible.

Needless to say, all our applications to your Office, including this one, have been prepared without any mention whatsoever of military applications. A verbal report of possibilities that might be worth considering has been made to Dr. Romney, while an informative report is currently under preparation for his office (only). Confident on your benevolent understanding, I would like to report from now on military possibilities only to DARPA's Office. Jointly, I would like to encourage you to enter into direct contact with Dr. Romney, with full confidence that you will find him most cordial and cooperative. No information on the matter has been released to NSF until now.

Very Truly Yours

Ruggero Maria Santilli
President

cc.: Dr. C. ROMNEY, Deputy Director, DARPA, 1400 Wilson Blvd, Arlington, Va 22209
Drs. B. HILDEBRAND and R. THEMES, Div. of High Energy Physics, DOE



- 965 -

Department of Energy
Washington, D.C. 20545

FEB 1 1983

Dr. [REDACTED]
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. [REDACTED]:

The Department of Energy's Division of High Energy Physics has completed its review of your proposal, "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons," and has referred this proposal to the Division of Nuclear Physics for final action. We in the Division of Nuclear Physics have examined the proposal and find that the proposed research topics are not appropriate for consideration by the Division of Nuclear Physics. Therefore, we must advise you that we cannot support this research proposal. Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

Enloe T. Ritter
Director
Division of Nuclear Physics

cc:
Div. of High Energy Physics, DOE
R. M. Santilli, I.B.R.



Department of Energy
Washington, D.C. 20545

MAR 22 1983

Dr. R. M. Santilli
Institute of Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

We are in receipt of your letter of February 4, 1983 to Dr. Ritter of the Division of Nuclear Physics concerning the proposal "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons" submitted by Dr. [REDACTED]. The proposed research appears to be appropriate for consideration by the Division of High Energy Physics. With your permission, we are initiating the technical review process. As soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

We appreciate your interest in submitting this proposal and will be pleased to give it consideration for support.


Sincerely,

for

William A. Wallenmeyer
Director
Division of High Energy Physics

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 8 1963


Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Gentlemen:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Theoretical, Experimental, and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons," PHY83-06700.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,



Rolf M. Sinclair
Acting Director, Division of Physics

Enclosures



NATIONAL SCIENCE FOUNDATION		PROPOSAL EVALUATION FORM	15,	NSF FORM X-3
PROPOSAL NO. HPS-3305700	INSTITUTION INST FOR BASIC RESEARCH		PLEASE RETURN BY 5/23/83	
PRINCIPAL INVESTIGATOR 		NSF PROGRAM DIR-MATH & PHYSICAL SCIEN		

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE
PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL
ELECTRONS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability)

The research proposed here is a theoretical and experimental study of the speculative hypothesis that the electric field of the electron oscillates in time. Experimental study of the C violating decay $^1S_0 + 3\gamma$ is also proposed.

Testing the oscillating electric field hypothesis might be of interest but the proposal has weak points discussed below.

The hypothesis is completely speculative and has no support from experiment or from current theoretical ideas. Thus it seems unlikely a priori that the hypothesis would be confirmed. In fact, the proposal does not clearly state how the results of the positronium decay experiment would bear on the oscillating field hypothesis.

Santilli's work has emphasized general questions, rather than the more phenomenological work described in the proposal.

The study of applications in macroscopic systems seems unpromising because the hypothetical oscillations produce no known observable effects on the atomic scale.

The institutional capability of the Institute for Basic Research is unknown to this reviewer.

OVERALL RATINGS: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER A

NATIONAL SCIENCE
FOUNDATION

(PROPOSAL EVALUATION FORM)

19.

NSF FORM X-3

PROPOSAL NO. MP3-3305700	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 2/23/65
PRINCIPAL INVESTIGATOR XXXXXXXXXX		NSF PROGRAM DIR-MATH & PHYSICAL SCIEN

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE
PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL
ELECTRONS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability):

This may well be the most marginal research proposal that I have ever been asked to review. Not only are the foundations for the work very speculative but there is no clear statement of the line of investigation to be followed and the results expected from this line of investigation. Most of the proposal consists of papers describing the speculations of the principal investigators each of which states the ideas are only partially worked out and gives promise of more detailed future publication. There is no clear reference to experiments carried out in the past which shed some light on the questions under consideration. This proposal should clearly be rejected in its present form.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER B

PROPOSAL NO. MPS-0306700	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 3/23/33
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM DIR-MATH & PHYSICAL SCIEN

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE
PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL
ELECTRONS

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability)

It is astounding that such nonsense as this can be promulgated as a serious research proposal. The original ludicrous speculation that the charge on the electron varies rapidly and harmonically in time was published in an unrefereed journal (Hadronic Journal). Rather, it was in a journal in which the [REDACTED]. Two charges interacting will have the same time-variations so as to produce a result in accord with experiment. How this can possibly be compatible with relativity is not made clear. Under no circumstances should precious resources be wasted on such trash.

Worse than Poor

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal request from Chairpersons of Congressional committees having responsibility for NSF reviewers' comments will be given maximum protection from disclosure.

REVIEWER C

PROPOSAL EVALUATION FORM		15	NSF FORM X-3
SAL NO.	INSTITUTION	- 871 -	
APS-8306700	INST FOR BASIC RESEARCH	PLEASE RETURN BY 3/23/83	
PRINCIPAL INVESTIGATOR		NSF PROGRAM	
		DIR-MATH & PHYSICAL SCIEN	

THEORETICAL, EXPERIMENTAL, AND APPLIED STUDIES ON A POSSIBLE
PULSATING STRUCTURE OF THE COULOMB FORCE OF INDIVIDUAL
ELECTRONS

Comments (continue on additional sheet(s) as necessary):
Quality of the proposed research (including budget & institutional capability):

This proposal suggests the theoretical investigation of the hypothesis that the charge on the electron (and other point-like particles) is an oscillating function of time (Eq. 3. p. 4). The viability of the idea is discussed semiclassically and nonrelativistically. Even limiting the discussion in this way, I have serious problems with the discussion presented. Furthermore, the most serious questions which occur cannot be addressed in such a restrictive framework.

A few of the specific problems which have occurred to me are listed below. References are to the included paper [redacted] (1982).

- (1) I think the evaluation of the integrals on page 779-780 is incorrect, and that a correct evaluation of (2-23) will lead to $\delta(\omega_{cm} - 2\omega)$ and $\sigma(\omega_{cm} + 2\omega)$ terms, thereby leading to gross energy nonconservation in electron-electron Rutherford scattering.
- (2) According to condition (2) two separated interacting macroscopic charge distributions have a time independent Coulomb interaction. On page 774 it is claimed that a proton behaves like a macroscopic charge distribution. On the other hand Fig. 1a tells us that the electron charge oscillates with average value zero. I do not see how to combine these ideas in a way which would lead one to conclude that the electron proton interaction is the normal electrostatic one. A vanishing interaction seems a more reasonable consequence.
- (3) The relation of the hypothesis to Maxwell's equations is not mentioned, but since photons are referenced there must be some idea of an electromagnetic field implied.

Finally, I find I do not understand the "prima facie" arguments which would suggest that such a hypothesis should be seriously considered. This immediately raises questions about charge conservation, which is inconsistent with the basic hypothesis, and how electrons which are remote from one another but at varying distances manage to keep these charge oscillations in phase so as to lead to a maximal repulsive interaction. Similar remarks would apply to the maximal attractive interaction between an electron and positron.

I conclude that the proposal is inadequately motivated and insufficiently developed to warrant support at this time.

OVERALL RATING: -- EXCELLENT -- VERY GOOD -- GOOD -- FAIR ☒ POOR

verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER D

PHY83-06700
Institute for Basic Research
██████████

TYPED FROM HANDWRITTEN REVIEW

"I cannot help but recommend against providing any support for the proposed research.

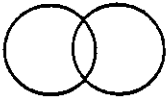
"To begin with, insofar as the pulsating structure of the Coulomb law is concerned, Santilli's arguments for its existence are devoid of merit. The process $e^+ + e^- \rightarrow 2\gamma$ is explained to extraordinary accuracy by the theory of quantum electrodynamics. Secondly, two numbers appear for the [frequency] of the oscillation. The first is on the order of $7.57 \times 10^{20} \text{ sec}^{-1}$ which implies that processes which probe distances on the order of $2 \times 10^{-10} \text{ cm}$ should see it. It would seem that this should be felt in experiments which involve energies on the scale of 1 Mev or less. This would imply that X-rays of heavy atoms should be affected in an already observed way. The second number is on the order of 10^{-21} seconds and avoids this difficulty, however Bhabbe scattering experiments at SLAC check the Coulomb law down to distances of 10^{-16} cm or times $\sim 10^{-26} \text{ sec}$. Hence there is no reason to believe anything interesting can be seen at the level suggested in this proposal.

"In addition to the problems which I have with respect to the scientific merit of this work, I have examined enough of Santilli's publications to have become convinced that they are of poor quality. I think that any NSF monies spent in further support of work of this caliber will be wasted. As proposed half of the grant (it would seem) will go to support of ██████████ either as a co-investigator, or in the guise of a senior research associate. Given my opinion of his previous work and the low quality of the present proposal I cannot in good conscience countenance such a waste of funds.

"Insofar as institutional capabilities; so far as I know the IBR has none."

Overall rating: "Very Poor"

REVIEWER E



- 973 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 20, 1983

Dr. R. THEWS
DOE, Division of Physics

RE: Application entitled
"EXPERIMENTAL AND THEORETICAL STUDIES ON THE POSSIBLE PULSATING STRUCTURE
OF THE COULOMB FORCE OF INDIVIDUAL ELESTRONS"
Principal Investigator: [REDACTED]
FINAL COMMUNICATION

Dear Robert,

I enclose a joint paper with [REDACTED] regarding the main hypothesis
of the application in print at LETTERE NUOVO CIMENTO.

I have contacted several colleagues in this topic and none of them had
a truly scientific objection against its plausibility. You should recall
that the hypothesis has solid grounds of compatibility with experimental
data at the nonrelativistic/quantum mechanical level. As it has been the case
for all nonrelativistic advances, a relativistic extension may be found
sooner or later.

In particular, the application has been rejected by NSF, as you eventually
know. What you should additionally know is that the referee reports are
of an incredible mumbo-jambo nature, totally deprived of the most minute
scientific content. It is politics brought to unbelievable extremes of
antiscientific behaviour. I have abstained from commenting to NSF on their
reports. However, I would be delighted to indicate to you their lack of
any value whatsoever.

I believe that, for the peer review to be a bit more valuable, applicants
should inspect the referee reports and communicate their comments PRIOR
to any decision by Governmental Agencies. I do not know whether this
procedure can be implemented by DOE, and, whatever the case, I shall respect
your decision.

In short, I believe that the hypothesis is too basic and important to be left
at the level of mumbo-jambo academic dances. It must be either proved or
disproved beyond reasonable doubts. This is the objective of the application:
hire a U.S. young physicist to prove or disprove the hypothesis via a genuine
scientific process.

Sincerely,

P.S. Some conceivable military applications have been indicated to Dr. Romney
of DARPA.

Cc. Drs. Wallenmeyer and Hildebrand.



Department of Energy
Washington, D.C. 20545

JUL 21 1983

Professor [REDACTED]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [REDACTED]

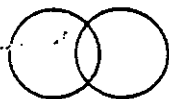
Your proposal entitled "Theoretical, Experimental, and Applied Studies on a Possible Pusating Structure of the Coulomb Force of Individual Electrons" is still under active consideration for funding, and will be acted upon during the next 6-month period.

We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon, CP, CPAS



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

Professor Ruggero Maria Santilli, President

September 20, 1983

Dr. R. L. THEWS
Physics Research Branch
Division of High Energy Physics
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled:
"Theoretical, Experimental, and Applied Studies on
a Possible Pulsating Structure of the Coulomb Force
of Individual Electrons"

Principal Investigator: [REDACTED]

Dear Dr. Thews,

Following your request, we are pleased to authorize the
retention of the proposal by your office for any addi-
tional period of time.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw



Department of Energy
Washington, D.C. 20545

NOV 15 1983

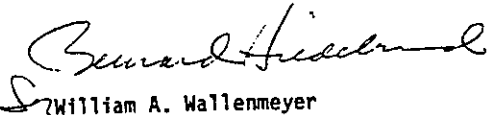
Dr. Roger Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

As you requested in your letter of November 10, 1983, we are considering as withdrawn your proposals entitled "Theoretical, Experimental and Applied Studies on a Possible Pulsating Structure of the Coulomb Force of Individual Electrons" under the Principal Investigator [REDACTED] and "Studies on the Quantization of Systems with Gauge Symmetries" under the Principal Investigator [REDACTED].

Your interest in submitting these proposals to the Department of Energy is appreciated.

Sincerely,


William A. Wallenmeyer
Director
Division of High Energy Physics

CC: [REDACTED]
[REDACTED]

PART XXX:

REJECTIONS BY THE

NATIONAL SCIENCE

FOUNDATION

AND THE

DEPARTMENT OF

ENERGY

OF AN I.B.R.

APPLICATION BY A

SENIOR PHYSICIST

Research Grant Proposal

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

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Proposed Starting Date

March 1983

Proposed Duration

Two Years

Amount Requested

\$ 175,300

ENDORSEMENTS

[REDACTED]
Principal Investigator
The Institute for Basic Research
Cambridge, MA, and
Department of Physics
University of **[REDACTED]**
Tel. (617) 864 9859

R. M. SANTILLI

President

The Institute for Basic Research

Cambridge, Massachusetts 02138

Soc. Sec. No. 032 46 3855

Tel. (617) 864 9859

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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ENCLOSURES:

- ~~Violation of the principle of conservation of energy and nonrelativistic mechanics (1933-1934)~~
- ~~On the foundations of quantum mechanics and the admissible algebra: the Heisenberg uncertainty principle and the Sommerfeld-Hilbert paradox (1933-1934)~~
- ~~On the foundations of quantum mechanics and the generalization of the atomic mechanics, in particular, the problem of the scattering of particles (1933-1934)~~

ABSTRACT

Recent studies by a number of scholars have indicated the possibility that the Hilbert space admits a new generalization, called isotopic, which is structurally more general than available extensions, e.g., of rigged- or C^* -type. This implies the possibility of generalizing the various aspects of quantum mechanics into a form capable of representing extended particles under conditions of mutual penetration, and which admit as classical image the Birkhoffian generalization of Hamiltonian mechanics for contact nonpotential interactions.

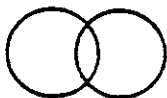
Along these studies, the Principal Investigator has indicated the possibility of generalizing the conventional potential scattering theory into a form called "nonpotential" primarily for its classical image, while its actual technical structure is that of the isotopic generalization of time evolutions, eigenvalue equations, perturbative expansions, etc. The existence of a corresponding isotopic generalization of the formal, abstract, theory of scattering has also been indicated.

Owing to the well known scientific and administrative relevance of the scattering theory in the data elaboration of high energy experiments, this proposal recommends a comprehensive study of the generalized scattering theory, for the primary purpose of ascertaining whether or not it constitutes a viable alternative to the current data elaboration of experiments implying mutual distances of particles smaller than their size.

The proposed research is articulated into three parts:

- A first part of foundational character, for the study of formal aspects;
- A second part of phenomenological character, for the study of formalisms ready for applications; and
- A third part of experimental character, for applications to the re-elaboration of existing experiments and for comparative analysis with available elaboration via the conventional potential scattering theory.

The research team recommended comprises the Principal Investigator (██████████), a U.S. expert in scattering theory to be hired full time as Senior Research Associate, and a number of advisors and/or consultants for experimental, theoretical, and mathematical aspects.



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 19, 1982

Dr. W. A. WALLENMEYER, ER-22
Director
Division of High Energy Physics
U. S. Department of Energy GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby submit for consideration by the Division of High Energy Physics of the U. S. Department of Energy, the research proposal entitled .

Studies on Nonpotential Scattering Theory

with Professor [REDACTED] as Principle Investigator. The original, duly signed, proposal is enclosed. The needed amount of additional copies have been separately mailed to you.

As you will notice, this proposal is deeply linked to the proposal currently under consideration by your office entitled "Studies on Hadronic Mechanics". Therefore, I remain at your disposal to mail you additional copies of the latter proposal, in case needed.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 19, 1982

Dr. S. PETER ROSEN
Program Associate
Theoretical Physics Program
Division of Physics
NATIONAL SCIENCE FOUNDATION
1800 G Street
WASHINGTON, D.C. 20550

Dear Dr. Rosen,

I hereby submit for consideration by the Division of Physics of the National Science Foundation, the research proposal entitled,

Studies on Nonpotential Scattering Theory

with Professor [REDACTED] as Principle Investigator. The original, duly signed, proposal is enclosed. The needed amount of additional copies have been separately mailed to you.

As you will notice, this proposal is deeply linked to the proposal currently under consideration by your office entitled "Studies on Hadronic Mechanics". Therefore, I remain at your disposal to mail you additional copies of the latter proposal, in case needed.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw



Department of Energy
Washington, D.C. 20545

OCT 29 1982

Professor [REDACTED]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [REDACTED]

The research proposal entitled "Studies on Nonpotential Scattering Theory" submitted on your behalf by the Institute for Basic Research has been received in the Division of High Energy Physics.

This proposal is now under review and as soon as a decision with respect to support can be reached you will be advised. Dr. Robert L. Thews of this office will be concerned with the technical aspects of the review. If you should wish to inquire about the status of the proposal, please feel free to communicate with him on (301) 353-4829.

We appreciate your interest in submitting this proposal and will be pleased to give it consideration for support.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS



Department of Energy
Washington, D.C. 20545

MAY 12 1983

Professor [REDACTED]
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [REDACTED]:

Your proposal entitled "Studies of Nonpotential Scattering Theory" is still under active consideration for funding, and will be acted upon during the next 6-month period.

We hereby request your permission to retain the proposal for this extended period of consideration and shall notify you of our decision regarding support as soon as possible.

Sincerely,

Robert L. Thews
Physics Research Branch
Division of High Energy Physics

cc: Vaccaro & Alkon CP, CPAS

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 8 1983

Dr. [REDACTED]
Department of Physics
96 Prescott Street
Institute for Basic Research
Cambridge, Massachusetts 02138

Dear Dr. [REDACTED]:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Studies on Nonpotential Scattering Theory," PHY83-02271.

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research support by the Foundation; and, finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

As part of a Foundation effort to ensure that all principal investigators better understand the decisions made on their proposals, we are including copies of the reviews received (with identifying information removed).

Sincerely yours,



Rolf M. Sinclair
Acting Director, Division of Physics

Enclosure

Copy to:

Dr. Ruggero M. Santilli
President

PROPOSAL NO. PHY-8512271	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 1/1/83
PRINCIPAL INVESTIGATOR XXXXXXXXXX		NSF PROGRAM THEORETICAL PHYSICS

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability)

I have no confidence in the soundness of the approach to physics taken by this investigator or the institution with which he is associated. An example that leads to such a lack of confidence is the following.

In the second paper attached, equations (3.1), (3.2), and (2.4) lead to $U_-(t, t_0) = [U_+(t_0, t)]^{-1}$ for $t \geq t_0$ if U_+^{-1} exists. If U_+^{-1} does not exist then we do the same with (3.11) and (3.12), which imply [with the initial condition $U_+(t, t) = U_-(t, t) = 1$] that for $t_0 \leq t$

$$U_+(t, t_0) = \exp[iH \int_{t_0}^t dt' \mu]$$

$$U_-(t_0, t) = \exp[iH \int_{t_0}^t dt' \lambda]$$

and hence $U_+(t, t_0)$ must have an inverse for some $t \geq t_0$. But then it follows that $\mu = \lambda$, and there is nothing new.

The works of the Santilli-group and other works by the principal investigator are, in my mind, characterized by the use of mathematical tools without judgment. I have no objection to the use of abstract mathematics in physics when necessary. Here my feeling is that the tools are running away from the physics.

I know that physicists have to be wary of such judgments and it is easy to produce examples in the history of modern physics where similar judgments were in error. Nevertheless one has to use one's best sense and should not be intimidated by these historical precedents into believing that every far-out idea is worth supporting. Perhaps to be far-out is a necessary condition for substantial progress, but it surely is not a sufficient condition.

OVERALL RATING: -- EXCELLENT -- VERY GOOD -- GOOD -- FAIR -- POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PROPOSAL NO. PHY-83-227*	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY 11/6/83
PRINCIPAL INVESTIGATOR XXXXXXXXXXXX		NSF PROGRAM THEORETICAL PHYSICS

STUDIES ON NONPOTENTIAL SCATTERING THEORY

Comments (continue on additional sheet(s) as necessary):

Quality of the proposed research (including budget & institutional capability):

This proposal is based on a (trivially) incorrect assumption. The proponent believes that elementary particle interactions are analyzed in terms of an S-matrix derived from a non-relativistic, single-particle Schrödinger equation with a static potential: cf. in particular paragraphs 4 and 6 on page 4 and paragraph 4 on page 5 of the Proposal. (The proponent could have convinced himself of the incorrectness of this assumption by simply consulting any current textbook on elementary particle physics or quantum field theory.)

The proposed research intends to maintain this basic framework (which is known to be incompatible with the special theory of relativity), proposing instead to modify the laws of quantum mechanics, in this referee's opinion, without cogent physical reasons for doing so. To my knowledge, the only experimental evidence claimed to support this scheme (originated by R.M. Santilli) is to be found in a paper by Ktorides et al., Phys. Rev. D22, 892 (1980). In that paper, the authors claim that deviations of the radii of light (!) nuclei from the liquid-drop formula, $R = r A^{1/3}$, arise from a breakdown of the Pauli exclusion principle, rather than from, say, shell structure (which is ignored in that paper).

I also note that all references in the Proposal (with the exception of a book authored by R.M. Santilli) are to be found in Hadronic Journal, edited by R.M. Santilli, who is also involved in this Proposal and is directing the Institute for Basic Research. Although I have no specific reason to doubt the integrity of the refereeing process of Hadronic Journal, the list contained in the Proposal does indicate some reluctance on behalf of the staff of the Institute for Basic Research to submit their work to the criticism of other members of the Physics Community, by publishing in other journals (Phys. Rev., Nucl. Phys., etc.)

I conclude that the proposed research is most likely to be irrelevant from the point of view of the development of particle physics and it should not be funded under any circumstances.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552, and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

NATIONAL SCIENCE
FOUNDATION

PROPOSAL EVALUATION FORM

File
NSF Form 18 (9-81)
Supersedes All Previous Editions

PROPOSAL NO. PHY83-02271	INSTITUTION Inst for Basic Research	PLEASE RETURN BY ASAP
PRINCIPAL INVESTIGATOR	NSF PROGRAM Theoretical Physics	

TITLE

Studies on Nonpotential Scattering Theory

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Quality Dr. Santilli has for a number of years been conducting research in a rather unconventional direction. The present principal investigator is one of his associates. The basis is a generalization of the mathematical structure of nonrelativistic quantum mechanics. It permits breaking of many symmetries and (therefore) of conservation laws. Violation of time reversal invariance is an example. It is essentially fed in by hand (put R₇S). There is at present no established experimental evidence in favor of the proposed generalization.

Since the proposed dynamics is not relativistic I do not understand the proposals claim of relevance for high energy physics.

The proposal also says nothing of the relation (if any) of this work to conventional high energy physics.

The principal investigator has a respectable number of publications. Most of these have been off the main stream such as tachyon theory and his work on Lie-admissible algebras. He has a very wide range of interest, from strong interactions to black holes.

The budget. Item G6 is so large presumably because of the 1 1/2 offices which belong to the private Institute for Basic Research rather than to a university. Item G3 is not clear to me, especially in view of the rather abstract nature of the work and the large arbitrariness that is available for quantitative comparison with experiment.

Summary. The theory seems to me to have too much arbitrariness to be useful: one can fit anything with it and at the same time one loses the symmetries that makes conventional theories beautiful. It is not motivated by experimental evidence, and it has not been shown to be in any way superior to our present theories.

OVERALL
RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☒ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PROPOSAL NO. PHY83-02271	INSTITUTION Inst for Basic Research	PLEASE RETURN BY ASAP
PRINCIPAL INVESTIGATOR XXXXXXXXXX	NSF PROGRAM Theoretical Physics	PR
TITLE Studies on Nonpotential Scattering Theory		
COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.		
<p>This proposal provokes a very mixed reaction from the present reviewer. The basic trouble is that in the research program, of which the present proposal is a part, matters which may be physically and mathematically deep and interesting are mixed with matters which are trivially irrelevant. Example: On p. 1320 of the paper <u>Foundations of the Hadronic Generalization of the Atomic Mechanics</u> II Myung and Santilli, one finds the statement "But there are other reasons to suggest a departure from the original idea of Atomic Mechanics. They are given by the <u>clear experimental evidence according to which, in the transition from the two-body problem under electromagnetic interactions to that under strong interactions, there is the disappearance of excited states.</u> In fact, while the hydrogen atom and the positronium admit an infinite variety of excited states, no excited state has been experimentally established until now for the deuteron. The same situation may occur also for other composite particles supposed to be of two-body nature, such as the π^0.</p> <p>This drastic change in physical behaviour is, perhaps, the most forceful experimental evidence suggesting a revision of the Atomic Mechanics into a form specifically conceived for the strong interactions..."</p> <p>This statement is really foolish. It has been understood for fifty years that in Schrödinger mechanics short range forces in the two body problem will yield only a finite number of bound states. A precise form of this argument was the core of Wigner's argument in 1932 that the small binding energy of the deuteron implies a short range for the neutron-proton force. Is it on grounds like this that the authors propose we abandon conservation of probability in hadronic mechanics? If so, this reviewer would have to rank the proposal <u>poor</u>.</p> <p>On the other hand, the research program is making a serious attempt to enlarge the framework of hadronic mechanics and in the course of that work is considering a number of interesting problems. (The reviewer regards the book of Santilli <u>Foundations of Theoretical Mechanics I</u> very useful. In it, the inverse problem of mechanics is studied: which equations of motion are derivable from an action principle?). The attempt to generalize quantization from Hamiltonian to Birkhoffian mechanics is laudable.</p> <p style="text-align: right;">(continued on page 2)</p>		
OVERALL RATING:	<input type="checkbox"/> EXCELLENT <input type="checkbox"/> VERY GOOD <input type="checkbox"/> GOOD <input type="checkbox"/> FAIR <input type="checkbox"/> POOR	
Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.		

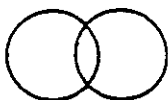
Comments on:

2.

Studies on Nonpotential Scattering Theory
R. Mignani

As far as the specific research program proposed on non-potential scattering theory is concerned, the reviewer again has mixed feelings. The previous work of the proposed principal investigator [REDACTED] as described in Hadronic Journal [REDACTED] is not very impressive - a few simple formalities. On the other hand, the research proposal, if carried out successfully, would enable one to confront the very speculative ideas of the general research program with experiment and that is good.

For reasons which should be evident from the above statements this reviewer will not give an overall rating to the proposal. However, in his opinion the very speculative motivations of this research are more likely to be fruitful in yielding greater understanding of more conventional approaches to hadronic mechanics than they are to give a description of Nature.



- 991 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

June 20, 1983

Dr. R. THEWS
DOE, Division of Physics

RE: Research grant application
"STUDIES ON THE NONPOTENTIAL GENERALIZATION OF THE SCATTERING THEORY"
Principal Investigator: [REDACTED]
FINAL COMMUNICATION

Dear Robert,

I would like to confirm the main objective of the application, to hire a young U.S. physicist for the research under [REDACTED] supervision. Please keep in mind this main objective because referees will likely indicate that I will pocket the money (this was the case of referees at NSF for the same proposal).

Also, a number of developments are going on already in the generalization. They are expected to appear in European Journals. This is due to the known hysterical oppositions at the Journals of the APS on our studies, I reported to Bernie time ago. As the situation now stands, we do not foresee any submission to APS journals for the foreseeable future, except when we see the appearance of papers of excessively manifest manipulatory nature, or with massive omissions of references (which have already occurred).

You should not be surprised at this. An entire new mechanics, the Birkhoffian Mechanics, was built without one single paper appearing in APS journals, as documented in a tacit form by scanning the references of my Vol. II with Springer-Verlag. We are having a mere continuation. In fact, we expect the construction, this time, of the hadronic mechanics, without one single paper appearing in APS journals.

This is an aspect that should be identified as clearly as possible, to prevent misjudgments in the processing of the application.

Please keep in mind that the existence and non-triviality of the nonpotential scattering theory is beyond any doubt, as additional material, besides that of the application, can prove. The only debatable aspect is the compliance of the theory with experiments.

This is another important point you should keep in mind. In fact, your referees are likely the same as those of our papers submitted to APS journals, that is, persons whose minds have been deformed by politics beyond the levels of scientific ethics.

In short, we are fully aware of your difficulties in the processing of this (and other) application. For this, you can count on our understanding.

Sincerely,

Ruggero

cc. Dr. Wallenmeyer and Hildebrand.



Department of Energy
Washington, D.C. 20545

OCT 17 1983

Professor [REDACTED]
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [REDACTED]

Reference is made to the proposal submitted by the Institute for Basic Research for support of a research program entitled "Studies on Non-potential Scattering Theory" to be conducted under your direction.

We have carefully considered the proposal in the light of our existing commitments and limitations on funding and regret that we will not be able to support the proposed research program. Due to the funding limitations which we are currently experiencing, we have found it necessary to decline support of many promising proposals such as yours.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

William A. Wallenmeyer
Director
Division of High Energy Physics

cc: Dr. R. M. Santilli

PART XXXI:

REJECTION BY THE

NATIONAL

SCIENCE FOUNDATION

OF AN I.B.R.

APPLICATION BY

A SENIOR

APPLIED MATHEMATICIAN

Research Grant Proposal

submitted to the
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

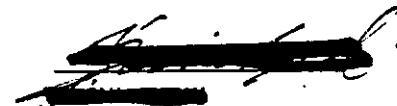



STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

Proposed Starting Date
January 1, 1984

Proposed Duration
Three Years

Amount Requested
\$ 185,230

ENDORSEMENTS


Principal Investigator
Department of Mathematics
University of 

Tel. 



R. M. SANTILLI
President
The Institute for Basic Research
Cambridge, Massachusetts 02138
Tel. (617) 864 9859
Soc. Sec. No. 032 46 3855

Accounting Firm of the Institute
VACCARO & ALKON CP, CPAS
2120 Commonwealth Avenue
Newton, Massachusetts 02166
tel. (617) 969 6630

Law Firm of the Institute
JOSEPH R. GRASSIA, ESQUIRE
44 School Street, Suite 500
Boston, Massachusetts 02108
tel. (617) 227 6060

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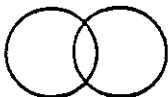
ENCLOSURES:

1. Table of Contents of
[REDACTED]
2. [REDACTED]
3. [REDACTED]
4. [REDACTED]
5. [REDACTED]

ABSTRACT

The successes of gauge theories in explaining some phenomena in physics of elementary particles emphasize the importance of an understanding of the quantum nature of gauge fields. The present quantum theories of gauge fields use either the Feynman path integral approach, in which one has a problem of the existence of path integrals, or a gauge condition approach, in which one has a problem of the independence of the resulting theory from the gauge condition used in its formulation. The aim of the proposed project is to study the mathematical problems appearing in attempts to develop an intrinsic, gauge invariant, canonical quantization theory, using as a guideline the geometric quantization theory.

The difficulties with a canonical quantization of gauge theories stem from the fact that gauge invariance leads to constraints given by the vanishing of the generators of gauge transformations. According to P. A. M. Dirac [1950], one should quantize the extended phase space and require that the physical states are gauge invariant. An alternative invariant approach is a quantization of the reduced phase space. In sufficiently regular cases both approaches are possible and yield equivalent results. [V. Guillemin and S. Sternberg, 1982; J. Śniatycki, 1982]. In the case of non-linear gauge fields, the regularity conditions are not satisfied: the constraints have quadratic singularities [J. Arms, J. Marsden and V. Moncrief, 1981], the reduced phase space is not a manifold, and there may be a loss of essential information incurred in reduction [J. Śniatycki, 1982]. In this case, one can generalize the process of reduction leading to the reduced Poisson algebra which need not be the Poisson algebra of a symplectic manifold [J. Śniatycki and A. Weinstein, 1982]. The problem of quantization of reduced Poisson algebras and its equivalence to the quantization of the corresponding generalized phase spaces are to be investigated.



THE INSTITUTE FOR ⁰⁰⁷BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 14, 1983

Dr. W. A. WALLENMEYER, ER-22
DIRECTOR
Division of High Energy Physics
U. S. Department of Energy GTN
WASHINGTON, D.C. 20545

Dear Dr. Wallenmeyer,

We hereby submit for consideration by your Division the
research grant application entitled,

STUDY OF QUANTIZATION OF SYSTEMS WITH
GAUGE SYMMETRIES

Principal Investigator: [REDACTED]

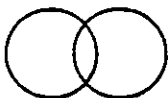
The original, duly signed application, is enclosed. Nine
additional copies have been separately mailed to you.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosure



— 998 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 14, 1983

Professor E. F. Infante
Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Professor Infante,

We hereby submit for consideration by your Division the
research grant application entitled,

STUDY OF QUANTIZATION OF SYSTEMS WITH
GAUGE SYMMETRIES

Principal Investigator: [REDACTED]

The original, duly signed application, is enclosed. Nine
additional copies have been separately mailed to you.

We trust that you will select the appropriate program
within your Division for the consideration of this proposal.

Very truly yours,

Ruggero M. Santilli
President

RMS/mlw

Enclosures

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Division of Mathematical Sciences

JAN 16 1983

Professor [REDACTED]
Division of Mathematics
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Professor [REDACTED]

We regret to inform you that the National Science Foundation is unable to support your proposal no. MCS-8317816 for "Study of Quantization of Systems With Gauge Symmetries."

In evaluating each proposal submitted to the Foundation, a number of factors are considered. They include the following: the scientific merit of the proposal and its merit in relation to the other proposals received by the Foundation in the same general field of science; the relation of the proposal to contemporary research in the field; the distribution among fields of science within the program of the Foundation; the geographical distribution of research supported by the Foundation; and finally, the funds available for research support. Thus, many excellent proposals cannot be supported for reasons aside from intrinsic merit, although this is an important consideration.

In accordance with a recently instituted policy within the Foundation, I enclose copies of the reviews of your proposal. They are intended for your personal use only and are not available to other parties. We sincerely hope these reviews will be useful to you in your research endeavors.

Even though we are unable to support this proposal, we would be pleased to consider other research proposals which you might wish to submit.

Sincerely yours,



E. F. Infante
Division Director
Division of Mathematical Sciences

cc: Dr. R. M. Santilli
I. B. R. President

Su-Shing Chen
Program Director for Geometric Analysis

PROPOSAL EVALUATION FORM ¹⁰⁰⁰ FO

NSF Form 1B (9-81)
Supersedes All Previous Editions

AL NO. CS-8317816	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM	
TITLE MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES		
<p>COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.</p> <p>The proposer is a leader in the areas of geometric quantization and classical field theory. The problem of quantization of gauge fields is a very hard one but for which much progress has been made by physicists; many of them work in the Euclideanized framework, using hard analytic tools like the Atiyah-Singer Theory.</p> <p>On the spacetime side it is not clear what the analogue to these tools is. In any event, it is missing from this proposal (and that of virtually every other worker). In this context, the proposal to investigate the differences between geometric type quantization before and after reduction, the effect of singularities and various ramifications of these ideas is an excellent program. Perhaps the work of Moncrief (Phys. Rev D <u>18</u> (1978) 983) would provide a good example here.</p> <p>The proposal to bring the P.I. to the Boston area for a half year is <u>very good</u>. However, it perhaps is not justified for three consecutive years.</p>		
<p>OVERALL RATING: <input type="checkbox"/> EXCELLENT <input checked="" type="checkbox"/> VERY GOOD <input type="checkbox"/> GOOD <input type="checkbox"/> FAIR <input type="checkbox"/> POOR</p> <p>Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.</p>		

PROPOSAL NO. MCS-8317816	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY NOV 9 1983
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM

TITLE

MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH
GAUGE SYMMETRIESCOMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

The application of geometric quantization methods to gauge theories is long overdue. [REDACTED] program seems to me to be very appropriate at this time. Having had long experience and success in both of these areas I think he is highly qualified and should be supported.

OVERALL RATING: ☒ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PROPOSAL NO. MCS-6317816	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY SEP 28 1983
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM	

TITLE
MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH
GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Professor [REDACTED] is one of the experts in geometric quantization and its application to quantum mechanics. The proposed research lies in the main stream of activity in this field and is likely to lead to interesting results.

The work should be supported.

OVERALL RATING: ☐ EXCELLENT ☒ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PROPOSAL NO. MCS-8317816	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN BY
PRINCIPAL INVESTIGATOR [REDACTED]	NSF PROGRAM GEOMETRIC ANALYSIS PROGRAM	
TITLE MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES		
COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.		
<p>[REDACTED] is clearly a competent differential geometer, doing interesting, although not terribly exciting research in the theory of symplectic manifolds. [REDACTED] proposes to study the problem of the quantization of systems with constraints, especially field theories with constraints. Given the importance of gauge theories in modern physics, this is clearly an important subject. [REDACTED] proposes to study this problem using the method of geometric quantization. I must confess extreme skepticism about the success of such a program, given the fact that there does not seem to be a canonical procedure for choosing the proper polarization to successfully quantize nonlinear systems in situations where everyone agrees what the correct quantization should be. Despite these reservations, I find some merit in the proposal, and were [REDACTED] a U.S. scientist applying for the usual summer salary grant, I would have made a rating of good, and indicated that I regarded this proposal as in the borderline area, somewhere near the bottom of that area (and therefore, in the current situation, probably just below the cut-off for support).</p> <p>As it stands, we have a scientist from Canada who proposes to visit an institution in the United States, and asks for 6 months' salary. The amount of money he is asking for is roughly enough to support something like 2.5 of the typical grants for younger scientists. Given the fact that a number of people certainly roughly as good as [REDACTED] have had grants refused because of the tight budgetary constraints, I regard it as completely wrong to seriously consider funding this proposal.</p> <p>In summary, on purely scientific judgment, I would rate this proposal as good, but taking into account the amount of funds asked, and other considerations, I would rate this proposal as poor.</p>		
OVERALL RATING: <input type="checkbox"/> EXCELLENT <input type="checkbox"/> VERY GOOD <input checked="" type="checkbox"/> GOOD <input type="checkbox"/> FAIR <input checked="" type="checkbox"/> POOR		
Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.		

PROPOSAL NO. MCS8317816	INSTITUTION INST FOR BASIC RESEARCH	PLEASE RETURN TO
PRINCIPAL INVESTIGATOR [REDACTED]		NSF PROGRAM GEOMETRIC ANALYSIS

TITLE
MATHEMATICAL SCIENCES: STUDY OF QUANTIZATION OF SYSTEMS WITH GAUGE SYMMETRIES

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.)
CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

Typed at NSF from handwritten copy.

Geometric Quantization has generated much mathematical interest in recent times, but unfortunately it has not as yet succeeded in providing much new insight into the problem of quantization of physical theories. My impression is that they have considerable difficulty going beyond the old Bohr-Sommerfeld theory. Nevertheless, it is an interesting avenue of approach and well worth supporting. [REDACTED] has been quite conspicuous in this area. Although this proposal seems quite vague as to what are the new features which might enable him to succeed where so many others have failed, his past performance indicates that much good and interesting work could result. I'm a bit trouble however about the financing of this research. The relationship of [REDACTED] to I.B.R. is never mentioned; the request is for 1/2 year salary in each of the successive 3 years. Is [REDACTED] still in [REDACTED] or will he be in Boston? In the proposal he refers to research to be conducted by a student - where? I could see supporting this project in the usual 2/9 summer fashion, but I don't find it of such high priority as to require such an expensive crash program. This is the kind of work done best at a university campus with the concomitant educational spinoff.

OVERALL RATING: ☐ EXCELLENT ☒ VERY GOOD ☐ GOOD ☐ FAIR ☐ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

PART XXXII:
REJECTION BY THE
DEPARTMENT OF
ENERGY
OF AN APPLICATION
BY SANTILLI UNDER
THE SMALL BUSINESS
INNOVATION RESEARCH
ACT.

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BUDGET , 16

Tables of Contents of

R.M.Santilli, Foundations of Theoretical Mechanics,

Volume I (1978) and II (1982), Springer-Verlag, New York/
Heidelberg/Berlin

Curriculum Vitae of Principal Investigator



HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

January 19, 1983

Dr. R. GAJEWSKI,
SBIR Program Manager
U.S. DEPARTMENT OF ENERGY-GTN
WASHINGTON, D.C. 20545

Dear Dr. Gajewski,

Following a kind mailing by Dr. W. WALLENMEYER of the SBIR Program Solicitation, we hereby enclose a Phase I Research Proposal entitled

DEVELOPMENTS AND APPLICATIONS OF BIRKHOFFIAN MECHANICS

which essentially consists of a continuation of research previously conducted under contracts between DOE and Hadronic Press numbers DE-AC02-80ER-10651, A001 and A002.

We would like to respectfully bring to your attention the fact that the current support will be exhausted by mid March 1983. As stated in the application, all research personnel and facilities will be terminated by the Hadronic Press upon such exhaustion of support. Their likelihood of resumption at some later time is in doubt at this moment. Any possibility of speedy consideration of the proposal for possible continuity of support would be gratefully appreciated.

In the hope of facilitating the review task, I enclose a list of eminent scholars throughout the world who are familiar with the project and who could provide you with a speedy review or refer you to qualified referees (that is, referees with a record of research in non-Lagrangian/non-Hamiltonian mechanics).

The scientific outcome seems to be truly promising and deserving DOE consideration. In fact, the birth of the Birkhoffian generalization of Hamiltonian mechanics (see the enclosure of the application) marks a rather momentuous development of mechanics, with far reaching implications of scientific and military nature, and of classical as well as quantum mechanical character.

Very Truly Yours

Ruggero Maria Santilli
Principal Investigator
RMS-mlw

cc. Drs. W. WALLENMEYER and B. HILDEBRAND, Division of High Energy Physics, DOE

ENCLOSURE: Original contract.

Nine additional copies are mailed separately.



- 1010 -

Department of Energy
Washington, D.C. 20545

January 31, 1983

Dr. R. M. Santilli
Principal Investigator
Hadronic Press, Inc.
Post Office Box 7
Nonantum, MA 02195

Dear Dr. Santilli:

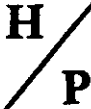
Your proposal entitled "Developments and Applications of Birkhoffian Mechanics," has been received in the Small Business Innovation Research Program Office and assigned the number 0011. Please refer to this number in any future communication you may have with the Department concerning your proposal.

Thank you for participating in the Department of Energy's SBIR Program.

Sincerely,

A handwritten signature in cursive script that reads "Carolyn Klose".

Carolyn Klose
SBIR Program Office



HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

February 2, 1983

Dr. R. Gajewski,
S.B.I.R. Program Manager
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled "Developments and Applications of
Birkhoffian Mechanics".

Dear Dr. Gajewski,

Soon after submitting the proposal, we realized that the limitation on length included the enclosures. In fact, the indication of "no additional attachments" is only at the end of the paragraph 4.3, while before reference is made only to "no more than 20 pages excluding the budget." My curriculum, being of 19 pages, is then cause of invalidation. If this is indeed the case (which is unclear to us), we would gratefully appreciate the courtesy of considering one of the following two alternatives.

- (a) We are hereby authorizing your office to detach and dispose of all enclosures, by reducing the proposal to only the presentation (pages 1 through 15) and the budget. In case this cannot be realized at your office for any reasons,
- (b) Kindly remail to us all copies of the proposal (including the original, if necessary, and at your discretion). We shall then detach all enclosures here and remail the reduced copies to you.

Thanking you in advance for your courtesy and time, we remain

Yours Very Truly

Ruggero M. Santilli
Principia Investigator

RMS-m1w



Department of Energy
Washington, D.C. 20545

March 2, 1983

Dr. R. M. Santilli
Hadronic Press, Inc.
Post Office Box 7
Nonantum, MA 02195

Dear Dr. Santilli:

I am sorry to inform you that your proposal, "Developments and Applications of Birkhoffian Mechanics," has been found deficient in the following respects:

The cover page of the proposal is not signed by the Principal Investigator and the Corporate/Business Official, and the proposal, including attachments, exceeds 20 pages. (See Sections 7.1 and 4.3 of the Solicitation, DOE/ER-0153.)

Therefore, the proposal must be declined.

The effort you took in preparing and submitting the proposal is very much appreciated.

Sincerely,

A handwritten signature in cursive script, reading "Ryszard Gajewski".

Ryszard Gajewski
SBIR Program Manager



HADRONIC PRESS, INC.

NONANTUM, MASSACHUSETTS 02195, U.S.A.

March 7, 1983

Dr. R. GAJEWSKI
SBIR Director
Department of Energy
WASHINGTON, D.C. 20545

RE: Application entitled
"Development and Applications of Birkhoffian
Mechanics", DOE-SBIR NUMBER 0011.

Dear Dr. Gajewski,

Our records disagree with the content of your letter of March 2 declining the consideration of the proposal.

- [1] The original proposal was duly signed by the President of Hadronic Press and by myself as principal investigator, and mailed to you via certified mail. The additional 10 copies, as customaries for all copies of all proposals, were not signed. No mention whatsoever of the lack of signature was indicated by Ms. C. Klose of your office in the acknowledgment of the arrival of the proposal dated January 31, 1983. (see enclosed copy).
- [2] Soon after submission, we realized that the enclosure of my curriculum would cause invalidation owing to its length (19 pages). We therefore wrote you asking the removal of all enclosures from the proposal and their disposal, or the return of the various copies to us for such removal and subsequent remailing to you (see enclosed copy of our letter dated February 2, 1983). Since we did not hear from you, we evidently assumed that your office had indeed removed all enclosures.

Please do not interpret this letter as a petition for you to reconsider your decision. We merely intended to establish a record on the peculiarities of the case.

Very Truly Yours

R.M. Santilli
Principal Investigator

RMS-mlw
encls.



- 1014 -

Department of Energy
Washington, D.C. 20545

March 28, 1983

Dr. R. M. Santilli
Hadronic Press, Inc.
Nonantum, Massachusetts 02195

Dear Dr. Santilli:

Thank you for the information provided in your letter dated March 7, 1983. Based on that information, your proposal "Development and Applications of Birkhoffian Mechanics" (0011) will be processed and evaluated.

Sincerely,

A handwritten signature in cursive script, reading "Ryszard Gajewski".

Ryszard Gajewski
SBIR Program Manager



Department of Energy
Washington, D.C. 20545

June 30, 1983

[REDACTED]
President
[REDACTED]
[REDACTED] BOX 7
[REDACTED]

Ref: SBIR Proposal 0011. "Developments and Applications of Birkhoffian Mechanics"

Dear Mr. [REDACTED]

We have completed the review of over 1,700 proposals submitted in response to the Department of Energy's Small Business Innovation Research (SBIR) Program Solicitation which ended March 1, 1983, including the referenced proposal submitted by you. Unfortunately, the budget for the program allowed only about one hundred to be funded; regrettably, yours is not among them.

Let me assure you that every proposal was examined thoughtfully. Each was evaluated by scientists or engineers knowledgeable in the subject area of the submittal, with a final review by my office. Realizing how much effort went into the preparation of proposals, and recognizing the value to the nation of the research and development potential that they represent, the Department has proceeded with extraordinary care to come to the most equitable decisions possible.

When all was said and done, hard choices had to be made. We had to eliminate many excellent proposals simply because there were others which were even better. The abundance of submissions compelled particularly close attention to the appropriateness and responsiveness of each proposal to the program requirements and the scope of the technical topics, as defined and described in the Program Solicitation. Confronted with the choice between a high quality proposal that was more closely responsive to a stated topic and one that was less so, we had to opt for the former.

We plan to send out our next SBIR solicitation in December 1983, and you will receive a copy automatically. Should you find in it technical topics under which your firm could submit a proposal, I hope you will consider another submittal.

Sincerely,

Ryszard Gajewski
Ryszard Gajewski
SBIR Program Manager

PART XXXIII:

SUPPRESSION OF THE

TESTS OF THE ROTATIONAL

SYMMETRY

SECTION A:

DIFFICULTIES AT THE

ILL-LABORATORY

OF GRENOBLE, FRANCE

Prof. Dr. R.M. SARTILLI
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, MA 02138

ETATS UNIS

Grenoble, le 22nd October 1981

V/lettre du

Notre référence à rappeler : HRF/ep/6824

Re: Discussion of proposals in subcommittee III

Dear Colleague,

Please find enclosed a copy of a letter to Prof. H. Rauch containing the decisions of the subcommittee 'Fundamental and Nuclear Physics'.

Best regards

H.R. Faust

H.R. FAUST
College Secretary College III

Encl.



INSTITUT FÜR VORLESUNG PAUL LANGEVIN

Prof. H. RAUCH
Atominstitut der Österr. Universitäten
Schüttelstr. 115
A-1020 Wien

AUTRICHE

Grenoble, le 21st October 1981

V/Lettre du

Notre référence à rappeler : HRF/ep/6818

Re: Discussion of proposals in subcommittee III

Dear Colleague,

In the subcommittee meeting 'Fundamental and Nuclear Physics' held on October 14, 1981 your proposal 'Test of SU(2) symmetry breaking due to strong interaction by neutron interferometry' proposal no. 03-13-034, was extensively discussed. Before allocating measuring time, the members of the subcommittee ask for more detailed information concerning the theoretical background of the proposal. Furthermore the following questions have been raised :

- Is there any information on SU(2) breaking from presently known data from other experiments and what are the current limits ?
- Are there any theoretical predictions for the outcome of the proposed experiment ?

The subcommittee decided to postpone the proposal and asks to resubmit it with a new formulation of the problem including the additional information.

With best regards,

H.R. FAUST
College Secretary College III



INSTITUT MAX VON LAUE - PAUL LANGEVIN

B.P. 156 X - 38042 - GRENOBLE CEDEX, FRANCE

DECISIONS OF THE SCIENTIFIC COUNCIL OF OCTOBER 15

(NB. Please inform your co-proposers of this decision).

If you have been allocated beam time you are requested to communicate immediately with Mr. G.A. Briggs, stating any preferential dates other than those given on the proposal form. The provision of neutron beams is extremely expensive and a perturbation of the instrument schedules and the consequent inconvenience to other users caused by non-compliance with this request is no longer acceptable.

RAUCH H
ATOMINSTITUT
SCHUETTELSTRASSE 115
A-1020 WIEN
AUTRICHE

Exp. Number : 3-13- 34
(to be quoted in all replies)

Title : TEST OF SU(2)-SYMMETRY BREAKING
DUE TO STRONG INTERACTION BY
NEUTRON INTERFEROMETRY

The above proposal has been accepted : NO (SEE BELOW)

Instrument : Beam allocation : days

Instrument : Beam allocation : days

The local contact for this experiment is :

The above proposal has been refused for the following reason :

ADDITIONAL COMMENTS :

SEE LETTER REF. HRF/EP/6818

Atominstitut der
Österreichischen
Universitäten

Prof. H. Rauch

Schüttelstraße 115
A-1020 Wien
Tel. (0222) 72 51 36
AUSTRIA

- 1020 -

for information
I appreciate your
accountant reaction
Yours sincerely:
H. Rauch

Directors
Institut Laue-Langevin
BP 156X Centre de Tri
F-38042 Grenoble Cedex
France

Wien, 4/11/1981

Dear Directors,

With some astonishment we got the information that our proposals mentioned below are either rejected, or postponed or shortened in the beam allocation by the Scientific Council of the ILL. The proposed experiments represent a mixture of routine investigations where the scientific outcome is rather well assured and some more speculative experiments which can give highlights to basic physical investigations.

Exp. No. 5-16-145 (rejected) "Observation of Triple Laue-Reflection Curves" (Bonse/Rauch). A standard experiment for D18 which should demonstrate a central reflection peak with a width of 0.002 sec of arc with a better background ratio than obtained by an earlier experiment. Information about the lateral spread of the wave packet can be obtained and applications of this central peak effect for special adjustment problems can be envisaged.

Exp. 5-16-144 (rejected) "Investigation of Metal-Hydrogen Systems near the α - β -Phase Boundary" (Rauch/Bonse). Again a typical experiment for D18 where the capacity of the interferometric hydrogen and deuterium determination is demonstrated in an earlier experiment. New samples and a proper thermostat are now available to perform such investigations with an accuracy higher than a factor of 10 compared to conventional methods. Not only the content of hydrogen (deuterium) can be determined but also the amount of free and precipitated hydrogen can be extracted. Three weeks would be enough for this experiment.

Exp. 3-13-34 (postponed) "Test of SU(2)-Symmetry Breaking Due to Strong Interaction by Neutron Interferometry" (Rauch/Santilli). Within a Cambridge/USA - Grenoble - Wien cooperation the 4π -spinor-symmetry experiment of 1975 and 1978 should be repeated in order to observe any nonlinear effect on the symmetry factor caused by the additional strong interaction introduced into the coherent beams of the interferometer. For fundamental physics any observation of a deviation from 4π -symmetry would be a sensation but also a more accurate value for the symmetry factor (at present 715.87 ± 3.8 deg) would be very useful.

Exp. 3-13-36 (shortened from 10 to 2 weeks) "Development of Single Crystal Interferometers for Thermal Neutrons" (Bonse/Rauch). Here I want to mention that at D18 more than 90% of the experiments are still performed using our first interferometer crystal tested successfully 1974 at our TRIGA-reactor in Wien. The ILL should be extremely interested to get new interferometer crystals for D18 not only to replace the old crystal but also to get interferometers with alternative beam paths.

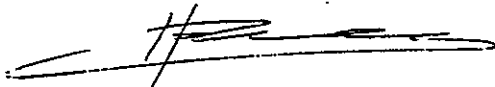
Exp. 3-13-036 (shortened from 16 weeks to 18 days) "Precision Measurements of Coherent Scattering Lengths b_c " (Bonse/Rauch). This proposal represents the routine application of D18 and any element or isotopic value should be remeasured if suitable samples are available.

Our group in Wien is especially concerned about these decision of the Scientific Council because some of the proposals are part of thesis works where most of the preparatory work has been done at our home institute and substantial financial support has been given to these experiments. The students are trained with a comparable interferometer set-up at our small reactor and therefore an effective use of D18 is guaranteed. No additional financial support from ILL is requested. Is there a possibility to present the details and the background information of the proposed experiments to the Subcommittees or to the Scientific Council? In principle we can perform some interferometer measurements at our small research reactors and

we have been invited by some reactor institutes in Europe to install interferometer set-ups with them but we are still of the opinion to continue the Grenoble - Dortmund - Wien cooperation in the field of neutron interferometry, because an effective use of D18 is last but not least also in our interest. We welcome other applicants for D18 but we suggest an effective use joint experiments during the first period as a User Instrument. We are still involved in neutron interferometer projects and on the average we would like to ask for an access to D18 (via proposals etc.) of about 5 weeks per year. Please inform us if this seems to be nonrealistic at all in order to push some alternatives.

I will visit ILL during the week from 23 to 27 November 1981 and would be very grateful to get the opportunity to discuss all related problems with you in detail and hope for a satisfactory solution.

Yours sincerely,

A handwritten signature in dark ink, appearing to be 'H. R. ...', written over a horizontal line.

Copy: Prof. Bonse, Dortmund
U. Kischko, ILL



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

October 29, 1981

Office of the President

Professor H. R. FAUST
Institut Max von Laue - Paul Langevin
F-34042 Grenoble, France

CERTIFIED AIR MAIL LETTER

Dear Professor Faust,

Please accept the sentiments of my sincere appreciation for the courtesy of mailing to me a copy of the report HRF/ep/6818 addressed to Professor H. RAUCH, Director of the Atominstut of Wien, Austria, regarding the decision by the subcommittee on 'Fundamental and Nuclear Physics' to postpone the proposal entitled "Test of the SU(2)-symmetry breaking due to strong interactions by neutron interferometry" (number 03-14-034), and to request additional theoretical information. I am taking here the liberty of providing some of the information requested. I would like to stress that I did not have time to consult with Prof. Rauch in Wien (your letter arrived this afternoon). Therefore, I am solely responsible for the contents of this letter.

The theoretical background of the proposal has been discussed extensively at the four yearly WORKSHOPS ON THE LIE-ADMISSIBLE FORMULATIONS held here, first at Harvard (1978, and 1979) and then at our Institute (1980 and 1981). A detailed theoretical study of the theoretical background would call for the reading of the five volumes of the PROCEEDINGS of these Workshops, plus a predictable number of papers and monographs in mathematics and physics.

To simplify the task of your committee, I have separately mailed to you:

- [1] A collection of seven selected papers specifically devoted to the experiment (five theoretical papers and two experimental ones) under the title "Primary bibliography on the problem of the exact or approximate validity of the SU(2)-spin symmetry under strong interactions".
The understanding is that the reading of the seven papers is grossly insufficient for a true understanding of the theoretical studies. In particular, the study of the quoted papers in Lie-admissibility by the mathematicians of our group is an understandable prerequisite.
- [2] A copy of my second volume "Foundations of Theoretical Mechanics, II: Birkhoffian Generalization of the Hamiltonian Mechanics", now in press at Springer-Verlag in the series "Textbooks and Monographs in Physics."
This monograph provides, in my view, a necessary classical background.
- [3] A very limited number of additional, recent papers not yet available in print, such as the paper by Mignani (Rome) on the preliminary construction of a nonpotential scattering theory. The parallel experimental paper on the apparent T-violation in nuclear physics is also included.

In addition to the inspection of this literature, I remain at your disposal for any additional element you may need. Simply let me know the information and/or paper desired, and you can count on my best possible assistance (most of the material has been published in the HADRONIC JOURNAL which, I understand, is not widely available in France).

In addition, as a member of the Organization Committee, I am happy to invite you as well as any interested member of your Subcommittee to attend the 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 9, 1981. Copy of the announcement as well as of the registration is enclosed for your convenience.

For your information, the problem of the spin under strong interactions will be an important part of the Conference at its various levels (mathematics, theoretical and experimental physics). Participation will therefore give to the members of the committee the opportunity to talk directly to the originators of the studies.

Permit me here the liberty of touching on some of the interesting issues raised in your letter, in the hope that the ideas presented below may be of some value. I reserve myself to enter into more details, as soon as I have more informations on the reasons which resulted in postponement of an experiment of clearly fundamental physical relevance.

CLASSICAL PROFILE. The first profile which should be taken into account is that, thanks to the contributions by a large number of mathematicians and physicists beginning from the past century, we can today state with confidence that the conventional Hamiltonian/Lie symplectic mechanics has been generalized into a covering form, which, for certain historical reasons, has been called Birkhoffian Mechanics [2]. This mechanics preserves the derivability from an action principle, the Lie algebra character of the time evolution, and the symplectic geometric structure but, the action functional has the most general possible (Pfaffian) integrand; the product of the Lie algebra has the most general possible (regular) realization; and the symplectic structure is the most general possible (exact) two-form.

What your Subcommittee should take into account is that the generalization has been constructed at all levels of Hamiltonian formulations, ranging from variational principles, to the transformation theory, to symmetries and first integrals, to the canonical perturbative theory, etc. Most importantly for this letter, it has been proved in the literature (Sarlet and Cantrijn) that the conventional Hamilton-Jacobi equations admits a consistent generalization of Birkhoffian type. The confrontation of the possible existence of a generalized formulation of quantum mechanics is then inevitable (see below).

The physical applications of the Birkhoffian mechanics are rather forceful. In fact, the insistence of the preservation of the Hamiltonian Mechanics often implies perpetual-motion type of approximation (often tacit), unless properly treated. The Birkhoffian mechanics is directly universal for all Newtonian systems satisfying certain smoothness and regularity conditions. The spinning top, as an example, rather than being studied under the perpetual motion approximation of an exact $SO(3)$ symmetry and conserved angular momentum, can be treated in its more physical realization, that under the presence of a drag torque with consequential nonconservation of the angular momentum, and breaking of the $SO(3)$ symmetry.

In essence, the transition from the trivial Hamiltonian Mechanics to its covering Birkhoff form permits the transition from the description of a system of point-like particles under long range, action-at-a-distance interactions to extended objects under potential forces as well as the most general possible nonpotential (but still local) ones due to internal collisions and any conceivable contact interaction (universality). Most intriguing is the capability of preserving total conservation laws without resulting into perpetual-motion internal approximation. This is the notion of closed nonselfadjoint interactions which is the starting point of the Orléans Conference of 1982. A rather forceful image is given to our Earth when seen by an outside observer as isolated from the rest of the universe. The system verifies all the ten Galilean conservation laws. But, internally, the Galilean symmetry is grossly violated (spinning top with drag torques, Space Shuttle during re-entry in atmosphere; damped oscillators; etc.).

In summary, we can confidently state that a new concept of interactions has been established at the classical level. Besides existing in Nature, the available theoretical formulations have reached a rather remarkable maturity and sophistication, which I could only grossly touch in my review [2]. Regrettably, these generalized techniques are still known only to a restricted circle of researchers. We hope with the Orléans Conference to propagate a bit the information, of course, to colleagues not solely interested in preserving Hamiltonian Mechanics as the final form of our classical description.

QUANTUM PROFILE. Permit me to stress from the outset that, unlike the classical profile, all the studies at the quantum mechanical level are still of tentative and conjectural character, mathematically and physically. There are, however, a number of points which have transpired rather clearly.

First, the existence of the Birkhoffian generalization of the Hamiltonian Mechanics has reversed the situation. The old criticism was that no generalization of quantum mechanics was possible because of the lack of a consistent classical image. Now the criticism is the opposite one. Until a quantum formulation admitting the Birkhoffian (rather than the Hamiltonian) mechanics as the classical image has not been built, ALL our microscopic descriptions, whether of Heisenberg or other conventional type, remain tentative and conjectural.

To state it differently, the complexity of the Newtonian world was usually bypassed via simplistic point-like assumptions of the elementary constituents, and the regaining of the potential-Hamiltonian-unitary description at the microscopic level. Today the situation is different. Physicists respect the reduction, but jointly ask for explicit studies of theoretical compatibility, that is, the proof that the quantum description of potential type of a large number of point-like particles is truly compatible with an experimentally established nonpotential macroscopic form. Any orthodox physicist who actually sits down and initiates mathematical studies of compatibility will soon discover a host of problems.

In summary, a considerable number of contributions have established that the customary reduction of Newtonian contact nonpotential interactions to potential quantum mechanical ones, even though conceivable, is, first of all, a mere personal conjecture by individual researchers at this time; and, second, that the conjecture is plagued by numerous problems of internal consistency (one can readily see a gross violation of the correspondence principle to begin with).

This situation has stimulated a truly intriguing effort by a growing number of scientists from virtually all over the World to attempt the unthinkable: generalize quantum mechanics into a covering discipline for particle wave packets under long range potential forces, as well as conventional contact nonpotential forces due to mutual wave penetration. Permit me to confess that, without any doubt, to eyewitness the birth of so many and so disparate ideas by so many people, has been for me the most stimulating experience of my research life. But, again, the efforts, in my view, are at the very beginning, and so much remains to be done.

However, thanks to the participation by distinguished mathematicians the structure of a conceivable covering mechanics can be identified with a considerable degree of confidence. I am referring to the mathematical structure (a layered generalization of Lie's theory at all its levels/enveloping algebras-Lie algebras-Lie groups/ of Lie-isotopic and of Lie-admissible type, one for the exterior treatment and one for the interior one). The actual construction of the mechanics is still ahead in the future, in my view, even though several specific generalizations exists (of Heisenberg's equation, of Heisenberg's uncertainty principle, of Galilei's relativity, etc.). Needless to say, the most important part will remain the confrontation of the prediction of the theory with experiments.

THEORETICAL PREDICTIONS. In your letter you correctly asked for theoretical predictions. The generalized theory predicts that hadrons, when interpreted as they are in nature (extended charge distributions with a wave packet), exhibit an alteration of their magnetic moment and of their spin (as well as of other Poincaré quantities) when in conditions of mutual penetration with other particles or under very strong fields. No alteration is predicted by the theory when (a) hadrons are treated via point-like abstractions; (b) the mutual distances are large compared to the electromagnetic radius of the particles (atomic structure), or the fields are sufficiently weak; and (c) when the magnetic moment is not altered in the transition from elm interactions to the strong. This is, in essence, my prediction of 1978 [1] via the proposal of breaking the $SU(2)$ -spin symmetry at its enveloping level (replacement of the associative envelope $A(SU(2))$ with a nonassociative Lie-admissible form).

Since that time, the proposal has been studied by a number of mathematicians and physicists. More recently, Professor G. EDER of the Atominstitut in Wien made fundamental advances in the problem [1]. Most important for this letter is Professor Eder's discovery that, even when the magnitude and third component of the spin are the conventional ones, the SU(2)-spin symmetry can still be grossly broken because of alterations of the space structure of hadrons (under the conditions considered) which result in non-SU(2) values of the other components as well as of the higher Casimirs.

Permit me to be candid on this important point. I do not know whether the SU(2)-spin symmetry is exact or broken under strong interactions. However, the technical profile is such that I would fear statements of extreme confidence in the exact value. In fact, the studies available, even though grossly incomplete and inconclusive, are sufficient to restrict confidence statements of this type to the level of scientific politics, rather than that of the true pursuit of human knowledge.

OTHER EXPERIMENTAL INFORMATION. Your Subcommittee has also asked the important question whether there are additional experimental informations, independent from Professor Rauch's proposal, which might indicate similar results. The answer is YES, with a number of understandings. First, one must realize that the SU(2)-spin was conceived for the atomic structure and experimentally established under elm interactions. The same notion was then assumed to persist under strong nuclear interactions, but without a direct experimental information until recently. As a result, the SU(2)-spin is simply assumed as valid and used in the data elaboration.

You can rest assured that a considerable effort is under way here at our Institute for the purpose of re-inspecting a number of experiments directly dependent on the value of the spin under strong interactions. Regrettably, a number of papers are written in a way too cryptical for a theoretician to reconstruct the data elaboration without the presence of the experimenters. However, a number of them show clear sign of being along the lines you indicated. Here, I want to indicate at this time only preliminary indications of compatibility of the covering theory (called Hadronic Mechanics) with the experiment by Forte et al (Phys. Rev. Lett. 26, 2088 (1980)). You can easily arrive at the same conclusion after a minimal study of the Lie-isotopic and Lie-admissible techniques. In fact, the asymmetry of the experimental results is a typical effect of our Lie-admissible mutation of the charge distribution of the neutron while within the inside of the atoms. At the same time, the orthodox plane wave description, while being unable to reach truly effective quantitative interpretations, is substantially nonconvincing to any physicist who rejects the use of too many constants.

But there are more data to be reinspected. I shall here and in the future abstein myself from presenting the case, again, until I am aware of the reasons resulting in the delay of Professor Rauch's experiments.

PROFESSOR RAUCH'S EXPERIMENT. I assume your Subcommittee is aware of the fact that Professor Rauch experiment of 1978 [1] is the SOLE experiment available until now which measures directly quantities related to spin under strong interactions. I assume you are aware of the fact that the initial reading was 716.8 ± 3.8 deg, and that this reading has been recently revised because of updated constants and other reasons, resulting in the value 715.8 ± 3.8 deg. I assume, finally, that your Subcommittee is aware of the fact that THIS CURRENT EXPERIMENTAL VALUE DOES NOT REPRODUCE THE 720 deg NEEDED TO ESTABLISH THE VALIDITY OF THE SU(2)-SPIN SYMMETRY UNDER STRONG INTERACTIONS ON TRUE SCIENTIFIC GROUNDS (that is, outside scientific politics).

As soon as these points are fully known, the need to repeat the experiment is, in my view, self-evident. After all, which experiment could resist a comparative analysis of value with the fundamental relevance of Professor Rauch experiment?

Permit me to express my position in this respect as clearly as possible.

- (1) I support unconditionally Professor Rauch for the repetition of the experiment NO MATTER WHAT THE RESULT IS, that is, whether in favor of against orthodox views. The same support is shared by all the TRUSTEES of our Institute as well as by all members (some of whom are orthodox physicists, and other no).
- (2) I believe that the experiment must be repeated first by Professor Rauch and his associates, and the possibility that other groups in other parts of the World re-do the experiment before Professor Rauch does should be prevented as much as possible. This is clearly due to our scientific ethics. In fact, the idea, as well as the scientific courage, belong to Professor Rauch. It is very regrettable that your delay in acting on the proposal has substantially increased in my view the possibility that other groups arrive before Professor Rauch. I assume you are fully aware of this, and ready to assume the related scientific responsibility.
- (3) Third, and perhaps most importantly, I am in favor of the repetition of the experiment by Professor Rauch in exactly the same way he did it the first time, and with the same data elaboration. To be explicit, I shall object against any alteration of the data elaboration as desired by Professor Rauch. This is clearly necessary to prevent the repetition of the experiment with different data elaboration which might have been conceived to reach compatibility with orthodox views.

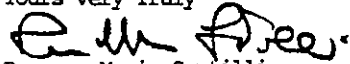
But, apart from any scientific reason, permit me to confess as candidly as possible the ultimate reasons of my drive (which, as you eventually know, has resulted in a number of initiatives, ranging from the organization of a new Journal to that of a new Institute of research). I believe that we have an ethical duty to test and establish our beliefs via experiments. We are currently spending truly large public sums in strong interactions. A considerable portion of these sums is spent under the assumption of the validity of the SU(2)-spin symmetry (and other fundamental elm notions) under strong interactions. How long can we procrastinate these fundamental tests without forcing a public investigation of scientific accountability? How long we can continue along the current lines without risking a severe judgment by future historians?

In closing, permit to beg you not to interpret this letter as intended to be offensive. If this has been the case, please accept my most sincere apologies. The primary intent of this letter is to be as clear as possible for an important page of physics, with considerable implications of nonscientific character. My intention is that the more clearly the problems are identified, the better for the true pursuit of physical knowledge.

In closing, I also would like to re-iterate that the request by the Subcommittee for additional theoretical information is fully sound, and meets my full approval. However, I fear that an excessively long theoretical study on the possible violation, for an experiment which is predictably expected to reproduce the preservation of the SU(2)-spin may be excessive, and may put the Subcommittee into dangerous areas such as that of Point (2) above. I mentioned this point in the hope that, perhaps, you will then see the intend of this letter of been useful to you, that is, of providing sufficient knowledge of the various aspects to reach a mature decision.

Hoping to have the pleasure of meeting you at Orléans this coming January, I remain

Yours Very Truly



Ruggero Maria Santilli
Professor of Theoretical Physics

RMS-pm
encls.

cc. Professors H. RAUCH and E. EDER, ATominstitut, Austria
Professors J. FRONTEAU and A. TELLEZ-ARENAS, Univ. of Orleans, France

MAILGRAM SERVICE CENTER — 1028
MIDDLETOWN, VA. 22645

western union

Mailgram



4-0307209318002 11/14/81 ICS IPHMTZZ CSP BSNB
1 6179641664 MGM TDMT NEWTON MA 11-14 0959P EST

RUGGERO MARIA SNANTILLI
28 CROSS ST
WEST NEWTON MA 02165

THIS MAILGRAM IS A CONFIRMATION COPY OF THE FOLLOWING MESSAGE:

TDMT NEWTON MA 11-14 0959P EST
INT DIRECTORS INSTITUTE LAUE-LANGEVIN
CENTRE DE TRI
38042GRENOBLE (FRANCE)
OUR INVESTIGATIONS GRENOBLE AFFAIR REVEALED ELEMENTS POTENTIAL
SCIENTIFIC SCANDAL OF INTERNATIONAL PROPORTIONS, SUGGEST URGENT
REINSTATEMENT PROFESSOR RAUCH EXPERIMENTS WITH PARTICULAR REFERENCE
TO INITIATION EXPERIMENT 3-13-34 ON SPIN SYMMETRY WAS ORIGINALLY
SCHEDULED IN JANUARY 1981. FOR BEST INTERESTS OF ALL PARTIES I
RECOMMEND TELEGRAPHIC COMMUNICATION TO ME OF FINAL DECISION IN ORDER
TO CONTAIN THE EPISODE AS MUCH AS POSSIBLE.
RUGGERO MARIA SNANTILLI

COL 38042GRENOBLE 3-13-34 1981.

22103 EST

MGMCOMP

TO REPLY BY MAILGRAM, SEE REVERSE SIDE FOR WESTERN UNION'S TOLL - FREE PHONE NUMBERS

PROF. DR. T. SPRINGER

Prof. R. M. Santilli
Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge
Massachusetts 02138
U.S.A.

Grenoble, le 17 November 1981

Veuillez du

Notre référence à rappeler : TS. JMA

Dear Professor Santilli,

I was sorry to learn of your very strong reaction concerning the postponement of the experiment 3.13.34 on spin symmetry. As you are probably aware, decisions to accept or refuse proposals at the ILL are taken in the sub-committees concerned. The difficulty concerning this proposal arose because it is not sufficiently self-explanatory. Sub-committee members are unable to look into original papers quoted in proposals, the number of proposals to be treated in one session of the sub-committee being of the order of one hundred. The information they asked for in particular was :

- 1) Is there any information on $SU(2)$ breaking from presently known data from other experiments and what are the current limits?
- 2) Are there any theoretical predictions for the outcome of the proposed experiment?

Consequently the proposal has not been refused but postponed and a re-submission asked for with a new formulation. I will do my best to prevent any delay and have already spoken with the co-author, Prof. Rauch, on the telephone. I will be meeting with him on 24 November to discuss the matter further.

Yours sincerely,



T. SPRINGER
Director

Telex sent out 17/11/81



THE INSTITUTE FOR BASIC RESEARCH - 1030 -
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

November 30, 1981

Professor Ruggero Maria Santilli, President

Professor T. SPRINGER, Director
Institute Laue-Langevin
F-38042 GRENOBLE, France

Dear Professor Springer,

I would like to confirm our phone conversation of today with particular reference to the following aspects.

(1) The solution most scientifically expeditious would be to leave the original proposal 03-13-034 in full standing and still formally under consideration by your Institute. The additional theoretical information requested by the sub-committee would then be nothing but part of the ordinary process of consideration.

(2) Some of the difficulties in the case have been created by the formal rejection of the proposal by the sub-committee in its current form, with indication of possible resubmission at some eventual future time. Since each of the investigators is an Officer at an Institute of Research, the fulfillment of the possibility calls for the repetition of the administrative iterim to receive new authorization to sign. This, in turn, has a number of evident and predictable implications. The advantage of solution (1) over the current status is clear, in my view. In fact, solution (1) would not need any new authorization of signature, and the consideration of the proposal could be restricted solely to due scientific process.

(3) The requests for theoretical information were answered by me in my letter to Mr. Faust of October 29, 1981 (copy of which is at your disposal), as well as in the collection of seven reprints of articles in the field of the proposal, and in the copy of my Volume II of "Foundations of Theoretical Mechanics" with Springer-Verlag, which were separately air mailed to Mr. Faust (again, additional copies are available on request). In case this information is insufficient, I would appreciate a request of specific technical elements which may be additionally needed.

(4) In case the members of the committees (or one of their representatives) are interested in a serious study of the mathematical, physical, experimental, as well as historical setting underlying the proposal, I suggest participation to the First International Conference on Nonpotential Interactions to be held at the Université d'Orléans, France, from January 5 to 10, 1982. Copies of the Conference Announcement as well as of the registration form were mailed to Mr. Faust on October 29, and additional copies are available on requests.

(5) You will kindly provide the names of the individual members of the sub-committee as well as of the chairman, so that, in the future, scientific material can be duplicated and mailed directly to them. This would also avoid possible errors in our current information on the matter.

I can be reached at this office until December 21, 1982. The courtesy of a communication prior to that time would be appreciated and, I believe, would be in the best interest of all. From December 22 until January 10 I will be in Europe.

Very Truly Yours

Ruggero Maria Santilli
Co-investigator of former proposal 03-13-034
RMS-vf
cc. Mr. Faust, ILL.

date: Aug. 3, 1981

RESEARCH PROPOSAL

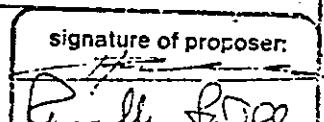
for nuclear physics experiments
at the ILL-HFR

dead lines for submission: february 15 and august 31

N° of proposal (to be filled in by ILL)	title: <u>Test of SU(2)-symmetry breaking</u> <u>due to strong interaction by neutron</u> <u>interferometry</u>		proposer(s): H.Rauch " R.M.Santilli "
3-13-034			
exact address of applicant(s):		telex: _____	
Prof.H.Rauch 3/ Prof.R.M.Santilli			
Atominstitut der Institute for Basic Research Osterr. Universitäten Harvard Grounds Schüttelstrasse 115 96 Prescott Street A-1020 Wien, Austria Cambridge, MA 02138, U.S.A.		tel: <u>72 51 36 (H.R.)</u> 19-43 222	
participants and measuring team:		suggested local contact:	
G.Badurek, H.Rauch, J.Sumhammer, S.Hammerschmied		U.Kischko A.K.Freund	
IMPORTANT In order to facilitate the mailing of letters concerning this proposal, please indicate below your name and full postal address and the person to whom all letters must be sent.		additional information on the proposal: (to be filled in by the applicant) keyword n° : new proposal: <input checked="" type="checkbox"/> continuation: * <input type="checkbox"/> resubmission: <input type="checkbox"/> date of report on previous measurements: n°(s) of previous proposal(s) about same subject: n°(s) of previous proposal(s) about related subjects: 03 - 05 - 013	
H.Rauch Atominstitut Schüttelstrasse 115 A-1020 Wien, Austria			

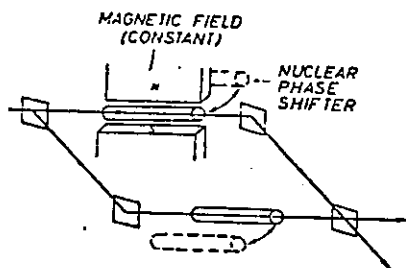
* An application for further beam time must be supported by a report on the previous measurements

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instruments to be used <input type="checkbox"/> Lohengrin PN1 <input checked="" type="checkbox"/> GAMS PN3 + Pair Spectrometer PN4 <input type="checkbox"/> BILL PN2 <input type="checkbox"/> OSTIS PN6 <input checked="" type="checkbox"/> Special Neutron Beam (please specify*) D18	
estimated measuring time: 3 weeks	target material: Bi
details of special material or equipment supplied by the user, proposed location and lay-out A Bi-phase shifter will be introduced within the coherent beams of the neutron interferometer at positions with and without magnetic fields (0.5 kg).	definite starting date: <small> 1-3 APRIL FOR PROPOSALS SUBMITTED IN OCTOBER 4-6 APRIL FOR PROPOSALS SUBMITTED IN MARCH 1 JULY 2 JULY 3 JULY 4 JULY 5 JULY 6 JULY </small>
safety aspects: is there any danger associated with the proposed sample or experimental condition? yes <input type="checkbox"/> uncertain <input type="checkbox"/> no <input checked="" type="checkbox"/> if yes what are the risks ?	
auxiliary equipment to be supplied by ILL (see guide-lines)	
electronics:	
mechanical equipment:	
others none	
data evaluation:	
data format required: standard p18	data reduction by whom? J. Summhammer where? ILL and Wien
expenses the ILL is expected to cover (see guide-lines)	
no separate expenses (experiment financed from Austrian Fonds zur Förderung der wissenschaftlichen Forschung and from	
signature of proposer: 	

Last year's theories have been published which violate the Pauli-principle and the validity of standard quantum mechanics in the region of strong interaction /1,2/. These theories violate also the 4π -symmetry if the spinor rotation occurs at a magnetic field region where strong interaction is present too. Since the first verification of the 4π -symmetry factor has been achieved at SI12 (now D18) in 1975 /3/ and a precision measurement using well defined magnetic fields within Mu-metal sheets has been performed 1978 and yield a periodicity factor of $\alpha_0 = 716.8 \pm 3.8 \text{ deg} / 4/$. The whole magnetic phase shift is given as $\alpha = \gamma m \lambda \int B ds$ ($\gamma \dots$ gyromagnetic ratio, $m \dots$ neutron mass, $\lambda \dots$ neutron wave length, $B \dots$ magnetic induction, $\int ds \dots$ path integral). Using D18 and a measuring procedure as shown in the figure it is possible to reach an accuracy

of $\Delta\alpha/\alpha_0 \sim 10^{-4}$ concerning the influence of additional strong interaction introduced by a Bi-phase shifter. The usual nucleus phase shift $X = k(1-n)D$ ($k \dots$ wave number, $n \dots$ index of refraction, $D \dots$ thickness is compensated and a hypothetical dependence $\alpha = \alpha(\lambda)$ can be extracted.



Justification for the performance of the experiment at the ILL and list of references

D18 is optimally suited for this kind of experiment because magnetic and nuclear phase shifts can be applied to the coherent neutron beams. High intensity is needed to reach the required accuracy.

References

- /1/ C.N.Ktorides, H.C.Myung, R.M.Santilli; Phys.Rev. D22 (1980) 892
- /2/ R.M.Santilli; Hadronic Journal 4 (1981) 1166
- /3/ H.Rauch, A.Zeillinger, G.Badurek, A.Wilfing, W.Bauspiess, U.Bonse; Phys.Lett. 54A (1975) 425
- /4/ H.Rauch, W.Wilfing, W.Bauspiess, U.Bonse; Z.Physik B29 (1978) 281



INSTITUT MAX VON LAUE - PAUL LANGEVIN

Prof. R.M. Santilli
The Institute for Basic Research
Harvard Grounds
96 Prescott Street
Cambridge, Massachusetts 02138
Etats Unis

Grenoble, le 8th December 1981

V/lettre du

Notre référence à rappeler : GAB/AT 231

Dear Professor Santilli,

Thank you for your letter of the 30th November 1981.

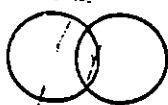
May I reassure you, that from the point of view of the Institute there is absolutely no problem in re-submitting your proposal 3-13-34 to the Scientific Council. I will of course, in order to conform with the request of this committee, require additional theoretical justification from you and this can be sent to me directly to be attached to the original proposal as an addendum. It would be preferable if your scientific justification was in a condensed form in order to facilitate photocopying for onward transmission to the committee members, but all this can be simply added to the original proposal, as an appendix.

Regarding the names of the nuclear physics sub-committee, these are as follows: Drs. SPECHT, LEROUX, VINH MAU, LYNN, SANDARS and SHOTTER. The chairman is Prof. Dr. SCHULT, Institut für Festkörperforschung der KFA Jülich, Postfach 1913, 5170 JÜLICH, W. Germany and it would be preferable for reasons of protocol if you addressed yourself to Prof. Schult in the first instance.

Any additional scientific material relevant to the proposal should be sent directly to the Scientific Secretariat at the Institute who will ensure distribution.

Yours sincerely,

PROF. T. SPRINGER
Director



- 1036 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

December 16, 1981

Professor Dr. SCHULT
Institut für Festkörperforschung der KFA JÜlich
Postfach 1913, 5170 JÜLICH, West Germany

Dear Professor Schult,

I am contacting you in your quality of Chairman of the Sub-Committee that considered the former application by Professor RAUCH on the repetition at ILL of his important experiment on the spinor symmetry done there in 1978. The primary motivations of the application were clearly indicated in the application it-self, and stem from the fact that an up-dating of the old measures of 1978 DOES NOT produce an angle of precession which is inclusive of the 720 deg needed for the exact SU(2)-spin symmetry. This, in turn, is more in agreement with modern Lie-isotopic and Lie-admissible theories which break and generalize the SU(2)-spin symmetry under strong interactions. The fundamental character of the SU(2)-spin symmetry for the totality of contemporary physics, then renders the repetition of the experiment truly important.

In the hope of preventing misrepresentations, permit me to review the situation to the best of my understanding and knowledge.

- I. At your Sub-Committee meeting "Fundamental and Nuclear Physics" held on October 14, 1981, Professor RAUCH's proposal number 03-13-034 was rejected. An informal suggestion to submit a new proposal sometime in the future was conveyed. However, as a result of the action taken at the meeting indicated, no formal consideration of Professor RAUCH's proposal is currently active or otherwise pending at ILL.
- II. The proposal made by our Institute has been either rejected, or it has not been acted upon. I am referring to the proposal I made by phone to Professor SPRINGER, Director of the ILL, and then confirmed via my letter to him of November 30, 1981, according to which your Sub-Committee should:
 - (a) reconsider the vote of October 14 as soon as possible (see below);
 - (b) keep proposal 03-13-034 under active, formal consideration by ILL; and
 - (c) ask for any additional information which may be needed to reach a decision at the appropriate future time.

Permit me to stress that the lack of immediate approval of the application DID NOT constitute a problem. The request of additional theoretical information also DID NOT constitute a problem because it is a routine for most applications. Our difficulties, communicated verbally to Professor SPRINGER, originated from the rejection of the application, as confirmed by the need that we have to prepare a new one (Case I) rather than keeping the application under formal consideration and asking for additional material (Case II).

Permit me to give you an indication of our difficulties, as well as, and perhaps most importantly, of the delicate profile related to our disclosure of your decision to the necessary colleagues.

For me to submit a new application, there is the evident authorization by our Board of Governors who, in turn, acts following the advice of an international body of distinguished scientists scattered throughout the World (our Institute has mainly an international character with minimal in house presence at this time). In order for me to apply for such authorization, I have to report the existing rejection, and be prepared to answer predictable requests of explanations.

On the morning of January 5, 1982, I have to deliver my invited opening talk of the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT, which, as you know, will be held at the Université d'Orléans, France. The existing deviations from the 720 deg in Prof. Rauch's experiment may be an indication of the possible existence of a nonpotential component in the nucleons-nuclei interactions. His experiment is therefore important for the Conference, jointly with a number of additional experiments.

Owing to these reasons, it was natural that most of the Organizers and/or the Members of the Advisory Committee of the Conference, had been informed of the application by Professor RAUCH at ILL to repeat the experiment. I am referring here, for instance, to Professor BOGOLUBOV, Director of the Joint Institute for Nuclear Research in Dubna, U.S.S.R., Professor PICOZZA, Deputy-Director of the Italian National Laboratories in Frascati, Italy, Professor BIEDENHARN, now at the Stanford Linear Accelerator Center, U.S.A., Nobel Laureate Professor Prigogine, Director of the Center for Statistical Mechanics of the University of Texas at Austin, U.S.A., and numerous additional distinguished scholars.

It is equally natural to expect that the participants to our First International Conference will ask the status of Professor RAUCH's application to you and to your Sub-Committee at ILL.

In order to prevent completely un-necessary aggravations and impressions, I provided by best effort to submit Proposal II above, but, as I can see from a recent letter from Professor SPRINGER, the proposal has not need acted upon or it has been tacitly rejected. Permit me to ask you directly that Proposal II be considered by you and by your Sub-Committee as soon as possible, and that the answer be communicated to me PRIOR to the inauguration of the Conference on January 5, 1982.

In the consideration of the case you should keep in mind that Proposal II would

- (1) imply no acceptance whatsoever of the experiment at this time;
- (2) prevent my asking a new authorization to sign a new application; and
- (3) permit us to communicate at the Orlean International Conference that "Professor Rauch's application for the repetition of his fundamental experiment of 1978 is currently under formal consideration at ILL, and that a decision is expected in the near future" (this communication is precluded at this time).

In case you reject also Proposal II, and insist in the preservation of the original decision (re-submit a new application), it would be recommendable that you give me guidelines for the presentation of the situation at my opening talk.

I shall soon leave for Europe. I have therefore asked my secretary to enclose my schedule until January 5, so that you can reach me in case needed. In case of letters, please use my address at the Conference in Orleans (and exclude other mailing addresses).

You should not expect additional communications on the matter from my part.

Very Truly Yours



Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-vf

cc.: Professors SPRINGER, SPECHT, LEROUX, VINH MAU, LYNN, SANDARS, and SHOTTER. FAUST
~~and ILL Scientific Secretariat~~

January 14, 1982

TEST OF SU(2)-SYMMETRY BREAKING DUE TO STRONG INTERACTIONS
Proposal number 03-13-034

Additional information prepared by R.M.Santilli

Basic information

1. The original measure of the spinor symmetry of neutrons at the ILL experiment of 1978 was 716.8 ± 3.8 deg for two spin flips. The measure was therefore inclusive of the 720 deg predicted by the exact SU(2)-spin symmetry.
Ref.: Rauch et al. Z. Physik **B29**, 281 (1978)
2. Up-dated physical constants and other reasons have suggested a re-inspection of the 1978 measure. The new value is 715.8 ± 3.8 deg which, as such, it does not include the theoretically expected 720 deg.
Ref.: IBR publication, October 1981.
3. In addition to the problem of periodicity, there is the problem of the clusters of points clearly outside the sinusoidal behaviour of the intensity modulation predicted by the exact SU(2)-spin symmetry, as well as additional aspects not sufficiently clear at this time (such as the phase of the intensity and polarization modulation).
Ref.: R.M.Santilli, Hadronic J. **4**, 1166 (1981)

It is submitted that this information alone is sufficient to warrant the repetition of the experiment. The improvement on uncertainty can be identified by Professor Rauch on request.

Theoretical information.

4. The nonconservation of the angular momentum is experimentally established in all open classical, Newtonian or statistical systems (e.g., the spinning top with drag torques, continuous variation of angular momenta in plasma, etc.). This experimental occurrence is an indication of the breaking in nature of the rotational symmetry.
Ref.: Santilli, "Foundations of Theoretical Mechanics", II (in press), Springer
5. In 1978 the hypothesis of the breaking of the SU(2)-spin symmetry was formulated for the case of open strong systems, e.g., for one neutron in interaction with a nucleus which is considered as external. The hypothesis was based on the idea that the charge distribution of the neutron is deformed under wave overlapping due to strong fields. The recovering of the exact symmetry for the close implementation of the system is understood.
Ref.: Santilli, Hadronic J. **1**, 574 (1978) and subsequent Proceedings of Workshop
6. Upon refinements of the original hypothesis, it was predicted that the mutation (or fluctuation) of the spin of the neutron in the external field of a nucleus is of the order of 1%.
Ref.: G. Eder, Hadronic J. **4**, 2018 (1981)

It is submitted that this provides additional motivation for the repetition of the experiment. In fact, the current number as per paragraph 2 implies a deviation from the exact SU(2)-spin symmetry which is exactly equal to the order of magnitude predicted by the theories of paragraphs 4-6 (called, on mathematical grounds, of Lie-isotopic or Lie-admissible type).

It is stressed that the basic motivation for the repetition of the experiment remains that of paragraphs 1, 2, and 3, inasmuch the theoretical studies of paragraphs 4, 5, and 6 are tentative at this time.

Speculative information.

7. The violation of the P-symmetry in nuclear physics is established, and so is its origin in the spin component (which is responsible, say, for the optical activity of neutrons in matter).
Ref.: Forte, Ramsey, et al, Phys. Rev. Letters 26, 2088 (1980)
8. Experimental evidence on the additional violation of the T-symmetry in nuclear physics has been recently achieved by a collaboration Berkeley-Quebec-Bonn. In particular, it has emerged that the most likely origin of the T-breaking is the spin component of the nuclear force, by therefore indicating the possible origin of this additional breaking in the spin.
Ref.: Slobodrain et al, Phys. Rev. Letters 47, 1803 (1981)
9. Recent unpublished and tentative calculations have indicated that the deviation of the current available measures of paragraph 2 from the exact SU(2)-spin symmetry (1%) are sufficient to interpret the breaking of the T- and P-symmetry, by therefore indicating the possibility that the ultimate origin of the P- and T-violation lies in the SU(2)-spin.

It is submitted that this speculative information provides additional elements of judgments favoring the repetition of the ILL experiment of 1978 by Professor Rauch. The rationale is that the identification of the breaking of the discrete symmetries, even though a fundamental step in physics, is per se insufficient, and calls for the experimental identification of the origin of the breaking themselves. The argument of paragraphs 7,8, and 9 is established in classical, Newtonian and statistical mechanics. For instance, a large collection of rotationally invariant orbits constitutes a reversible system. Thus, the most direct way to reach compatibility with the experimentally established violation of the T-symmetry in statistics is by making sure that at least some of the orbits of the constituents of the system are not rotationally invariant. This latter aspect is requested any how by the experimental evidence of varying angular momenta of parts of the statistical system.

A corresponding knowledge in particle physics is not available at this time. The experiment by Professor Slobodrian has established the origin of the irreversibility at the particle level. The corresponding experiment by Professor Rauch on the spinor symmetry is inconclusive in its available form. Even though theoretically the most probable origin of the T-breaking can be identified with that of the breaking of the rotational symmetry, the corresponding experimental test is lacking.

These aspects were discussed at the recent FIRST INTERNATIONAL CONFERENCE ON NON-POTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT Univ. d'Orléans, France, January 5 to 9, 1982. A qualitative and classical, but majestic illustration was provided by slides of the NASA missions on Saturn. Each and every chunk of ice of Saturn's rings has the rotational symmetry. The rings then also have globally the rotational symmetry. But they constitute a reversible system (all continuous and discrete symmetries are verified in this case). Saturn itself is a more complex system. Globally, it verifies the rotational and discrete symmetry, e.g., for its orbit in the solar system or its intrinsic rotation. However, in the interior of the system, all conventional, continuous and discrete symmetries are broken, as clearly illustrated by the NASA slides. This confirmed that the classical origin of the breaking of the discrete symmetries lies in the continuous ones. Several talks were then presented at the Conference to indicate that the same situation is expected at the particle level following the fundamental results by Professor Slobodrian, not only to achieve compatibility with the discrete breaking, but also to achieve compatibility with the classical, experimentally established situation.
Ref. Proceedings of the First International Conference on Nonpotential Interactions and their Lie-admissible treatment, to be published in 1982.

INSTITUT FOR KERNPHYSIK
DER KERNFORSCHUNGSANLAGE JÜLICH GmbH
EXPERIMENTELLE KERNPHYSIK II
DIREKTOR: PROF. DR. OTTO SCHULT

— 1040 —

D-5170 Jülich, den January 6, 1982
Postfach 1913
Telefon (02461) 614408
Telex-Nr.: 833556 kja d

Prof. Dr. R.M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138
U S A

Dear Professor Santilli,

I have received your letter dated December 16 only today, and I therefore want to answer immediately.

All members of the Sub Committee 'Fundamental and Nuclear Physics' including myself as chairman follow in their work the basic principle to give the physics that is planned at the ILL the maximum possible support. I can assure you that the attitude of all of us is a very constructive one indeed.

Our work is based essentially on the information given in the proposals. The proposal 'Test of SU(2)-symmetry breaking due to strong interaction by neutron interferometry' by Prof. H. Rauch and Prof. R.M. Santilli (see enclosure) has been discussed on the basis of the brief information that has been given by the authors. It was the feeling of all members of the Sub Committee that more information is required for us to recommend the experiment to be carried out at the ILL. The Sub Committee has therefore postponed its final decision and asked for additional information (see the letter of October 21 to Prof. Rauch).

From your various letters to Prof. Springer and Dr. Faust and your letter of December 16 I conclude that you have great interest in this experiment to be carried out as soon as possible. In this context I am very interested to give you any support and I therefore ask you to please proceed according to the letter of October 21 by Dr. Faust. It will help if you are brief, as I have been in my letter of October 16 (copy is enclosed).

A few remarks on statements that you have made in your last letter are in order:

1. If 'an up-dating of the old measures of 1978 DOES NOT produce an angle of precession which is inclusive of the 720 deg needed for the exact SU(2)-spin symmetry', why has this information then not been given in the proposal dated August 3, 1981 where the figures are 716.8 ± 3.8 which clearly is consistent with 720° ?
2. What is the up-dated value and its error?
3. What is the Lie-isotopic and Lie-admissible prediction for SU(2)-spin symmetry breaking? Please give me a number and its uncertainty in case your theory allows the estimation of uncertainties of the theoretical prediction.
4. Your statement that the Sub-Committee has rejected Prof. Rauch's proposal does simply not correspond with the facts. The proposal has been postponed which is explicitly stated in the letter by Dr. Faust. I would like to urge you to read this letter carefully because it is expressis verbis stated 're-submit it (the proposal) with a new formulation of the problem including the additional information'. The Sub-Committee and the college secretary have carefully chosen this way which in fact avoids all the problems that you are dwelling on in the rest of your letter.
5. I vaguely understand that you have formal problems with proposals. Nobody in the Sub Committee is interested in formalities and everybody will definitively give you any support; but please give us if possible briefly and clearly the information we have asked for. Let me stress that this is our basis for a fast decision.
6. I can assure you that neither Prof. Springer nor the Sub Committee will tacitly reject a proposal. I must inform you though that I cannot accept your letter of November 30 as an answer to the questions risen by the Sub Committee. Nor can your discussion with Prof. Springer be considered by the Sub Committee as a new proposal.
7. I am convinced that outstanding scientists will ask at a conference what the new numbers are, rather than about the status of an application.

By the way: I do not care about rumors but about facts. Therefore, I am very much looking forward to your short and clear answer of our questions and I would rather not take your statement 'you should not expect additional communications on the matter from my part' as your final answer.

Yours sincerely,

Otto Schult

Copies to:
Prof. Springer, Director at the ILL
Dr. Faust → Members of the Sub Committee
Prof. Rauch

O. Schult



1043
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

January 14, 1982

Professor Dr. OTTO SCHULT
Institut Für Kerphysik
der Kernforschungsanlage Jülich
Postfach 1913
D-5170 JÜLICH, West Germany

Dear Professor Schult,

I would like to express my sincere appreciation for your cooperative letter of January 6, 1982. By reading it, I have the impression that the two of us would have resolved all difficulties in a brief and friendly phone back in October 1981. Regrettably, it took me almost three months to know your name [perhaps in the future ILL should indicate the name of the subcommittee members in all communications regarding their consideration of applications-- this would save time and help all].

I enclose the data you recommended that I submit, in their shortest possible presentation, with references in case additional information is needed. In case it is insufficient, please let me know.

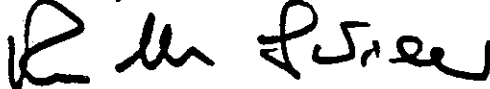
In Orléans, Professor Rauch and I discussed the case, and decided that it is best that, from now on, the ILL application be carried forward by Professor Rauch alone. After all, I am not an experimentalist.

Our Orléans conference was indeed a success (I shall try to let you have a complimentary copy of the Proceedings which are expected to be some three volumes). The climax was reached at the third day of talks, when Professor Slobodrian reported about the clear and firmly stated results of the violation of the T-symmetry in nuclear physics in his experiment with Conzett et al (Phys. Rev. Letters 47, 1803 (1981), Dec. issue). The origin of the breaking in the spin component of the nuclear force was also identified as quite probable. It was a pity that Professor Rauch did not have more recent data on the rotational symmetry (which is at the basis also of the P-violation experiment by Ramsey et al, as you know). This would have permitted a "grand unification of symmetry breakings"... But we hope that this will be possible in the near future.

During my opening talk I avoided, of course, any reference to the ILL case. In private conversations I tried to be as elusive as possible. However, the question when the experiment by Professor Rauch will be repeated was voiced by several participants from numerous Countries. The reasons are clear: we are currently spending truly large amounts of public funds on strong interactions, most of which are crucially dependent on the rotational symmetry. I am sure you will agree that

scientific accountability calls for the experimental resolution one way or another.

Very Truly Yours

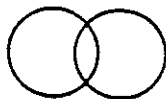
A handwritten signature in black ink, appearing to read 'R. M. Santilli', written in a cursive style.

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

RMS-vf

cc. Professor Rauch

Professor Springer and Dr. Faust, ILL



— 1045 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 30, 1982

Professor Dr. Schult
Institut für Festkörperforschung
Der KFA Jülich
Postfach 1913, 5170 JÜLICH, West Germany

Dear Professor Schult,

I would appreciate the courtesy of an indication concerning the study by your subcommittee on the proposal by Professor Rauch on the test of the spin-symmetry for the strong interactions. Since your letter of January 6, 1982, and my answer of January 14, 1982, I have not heard from you or from the ILL Laboratory in Grenoble.

I would like to take the liberty to enclose a brief paper of mine illustrating the fundamental character of Professor Rauch's proposal, this time in regard to the experiment by Professors Slobodrian and Conzett on the violation of the time-reversal symmetry.

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

Very truly yours,

Ruggero Maria Santilli
Professor of Theoretical Physics
and President

Enclosure

RMS/miw

cc: Professors SPRINGER, SPECHT, LEROUX, VINH MAU, LYNN, SANDARS,
SHOTTER, and FAUST

INSTITUT FÜR KERNPHYSIK
DER KERNFORSCHUNGSANLAGE JÜLICH GmbH
EXPERIMENTELLE KERNPHYSIK II
DIREKTOR: PROF. DR. OTTO SCHULT

D-5170 Jülich, den May 12, 1982

Postfach 1913
Telefon (02461) 614408
Telex-Nr.: 833556 kfo d

Prof. Dr. R.M. Santilli
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138
U S A

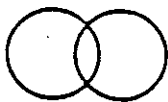
Dear Professor Santilli,

as you can see from the enclosed copy it takes only one week between the recommendation of our Sub-Committee and the letter by Dr. Maier to Prof. Rauch. May I recommend that you speed up your cooperation with Prof. Rauch accordingly? As I am very busy I do everything to get things in order and we really have. It would be nice if you could also follow this example by more intense interaction with Prof. Rauch.

Yours sincerely,

Otto Schult

O. Schult



THE INSTITUTE FOR ¹⁰⁴⁷BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

May 22, 1982

Professor O. SCHULT
Jülich, West Germany

Dear Professor Schult,

Your note of May 12 concerning the resolution of your subcommittee on Professor Rauch's spin experiment arrived jointly with a similar one from Professor Rauch.

It was our expectation that your subcommittee would provide our Institute with the courtesy of sending us a copy of this resolution, to save time to all parties. After all, I was a co-investigator and, as you know, we are providing the Nobel Committee with all information relevant for the Slobodrian-Conzett experiment on time-asymmetry, and this includes Professor Rauch's experiment.

Evidence indicate that this expectation of courtesy was erroneous. I guess it is an indication of contemporary academic costume which escapes my comprehension.

Very Truly Yours



Ruggero M. Santilli
President

cc.: Professor Springer ILL Grenoble



Prof. H. RAUCH
Atominstitut der Osterr. Universitäten
Schüttelstr. 115
A-1020 Wien

AUTRICHE

Grenoble, le 8th April 1982

Viettre du

Notre référence à rappeler : BM/ep/6978

Re: Discussion of proposals in Subcommittee III

Dear Colleague,

In view of the additional information received concerning your proposal no. 03-13-034, the subcommittee 'Fundamental and Nuclear Physics' recommends to perform the proposed experiment.

With best regards


B. MAIER

PART XXXIII

SUPPRESSION OF THE

TESTS OF THE ROTATIONAL

SYMMETRY,

SECTION B:

DIFFICULTIES

AT THE U.S.

NATIONAL SCIENCE

FOUNDATION

Research Grant Application

- 1050 -

Submitted to the
NATIONAL SCIENCE FOUNDATION

by

The Board of Governors of
THE INSTITUTE FOR BASIC RESEARCH

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

entitled

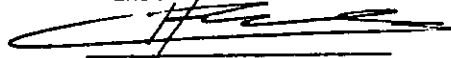
EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND
ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION

Proposed Starting Date:
June 1, 1982

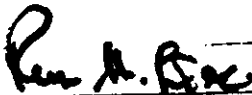
Proposed Duration
12 Months

Amount Requested:
\$ 94,900

ENDORSEMENTS



H. Rauch
Principal Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36



R. M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
Tel. (617) 864-9859



U. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 36



R.M. Santilli
President
The Institute for Basic Research
Soc. Sec. No. 032 46 3856
Tel. (617) 864-9859

Accounting Firm of the Institute
Vaccaro and Alkon CP, CPA
2120 Commonwealth Avenue
Newton, Massachusetts 02166
Att.: Mr. R. Alkon, President
Tel. (617) 969 6630

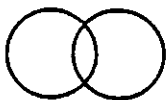
Legal Firm of the Institute
Wasserman & Salter
31 Milk Street
Boston, Massachusetts 02108
Att.: Mr. J. Grassie, Senior Partner
Tel. (617) 955 1700

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2. H. Rauch, A. Zeilinger, G. Badurek, A. Wilfing, W. Bauspiess, and U. Bonse, Phys. Lett., 54A, (1975), 425	
3. G. Badurek, H. Rauch, A. Zeilinger, W. Bauspiess, and U. Bonse, Phys. Rev. D, 14, (1976) 1177	
4. H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik, B29, (1978) 281	
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D. Theoretical Papers	
6. C. N. Ktorides, H. C. Myung, and R. M. Santilli, Phys. Rev. D22, (1980), 892	
7. G. Eder, Hadronic J., 4, (1981), 2018	
8. R. M. Santilli, "Use Of The Hadronic Mechanics For The Best Fit Of The Time-Asymmetry Recently Measured By Slobodrian, Conzett, Et Al", IBR, Preprint April 1982	

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. Wilfling, W. Bauspiess, and U. Bonse (Z. Physik B29 (1978), 281) via the test of the 4π 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* and at the recent *First International Conference on Nonpotential Interactions and their Lie-admissible Treatment* held at the Université d'Orléans, France, from January 5 to 9, 1982, or the inclusion of additional new physical effects.



— 1053 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

April 27, 1982

Dr. PETER S. ROSEN,
Program Associate for Theoretical Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Rosen,

I hereby respectfully submit one original and seven copies of the research grant proposal entitled

EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION

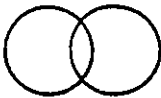
under administration by our Institute, and with Principal Investigator Prof. H. RAUCH, Director, Atominstitut der Österreichischen Universitäten, Wien, Austria, who is an undisputed experimental leader in the field of the proposal.

As you can see, the proposal has been made as brief as possible, thanks also to its experimental character. However, we would appreciate your consideration of the advisability whether we should prepare a collection of experimental and theoretical papers in the problem of the proposal, for referee's assistance. Please let us know at your convenience whether such collection of papers should be prepared or not. Also, please keep in mind that the experiment could be initiated this summer, and a solicit resolution would be appreciated, of course, within the time schedule of NSF. I understand that you are in the theoretical division of NSF. In case you pass the proposal to an officer in the experimental part, kindly let me know his name.

Finally, permit me the liberty of recommending, if at all needed, that NSF exercises extreme care with respect to the proved ethical standards of the desired referees. The proposal is for an open problem that is clearly at the foundation of current physical knowledge. Individual referees might therefore be tempted to discourage the conduction of the experiment to protect personal academic-financial interests, to the detriment of the true pursuit of novel human knowledge. At any rate, the proposal reaches your desk after years of documented opposition by a number of physicists who have been trying, whether openly or cryptically, to prevent the conduction of this experiment. I believe that it is in the best interest of NSF as well as the international physics community that you are informed of the existence of this opposition so that you can take appropriate precautionary measures. Also, I believe that the proposal and the contemporary scientific climate in the USA warrant a serious consideration of the ethical profile from the outset. Needless to say, we are fully confident that NSF will indeed meet our best expectations.

Sincerely

Ruggero Maria Santilli
President
RMS-mlw
encls.



- 1054 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 8, 1982

Dr. R.M. SINCLAIR
Division of Physics
NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

Dear Dr. Sinclair,

We appreciated the courtesy of your recent note of June 29. As you can see from the enclosed copy, we have communicated the situation to Prof. Rauch in Wien in a form which is the best possible for NSF.

I would like to take the opportunity also to enclose copy of a few lines prepared by our members here concerning the possibilities of rather intriguing advances in QCD and all that permitted by the hadronic mechanics. These possibilities are virtually unknown in the community at large. They have not been developed until now pending the availability of a more detailed formulation of the new mechanics. It appears that we are now approaching that point. Within a year or so, some of the possibilities considered here could therefore reach maturity for papers.

You will be amused to know that all these possibilities are rather crucially dependent on the deformations of the charge distribution of hadrons measured by Rauch, as well as by other data, such as the time-asymmetry measured by Slobodrian, Conzett et al, the variation in the space-asymmetry from one nucleus to another measured by Ramsey et al; etc.

The irony is that these preliminary measures of symmetry breakings are generally opposed by physicists in quark theories, thus resulting in a potentially self-damaging posture, as it has been the case a number of times in the past history of physics.

Sincerely,

Ruggero M. Santilli

RMS-mlw

cc.: Drs. M. Bardon and P.S. Rosen, NSF.

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

MAY 28 1982

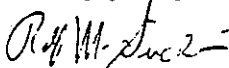
Ruggero Maria Santilli
President
The Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

I have been asked to consider the proposal you sent to Dr. Peter Rosen of this office. I return it herewith because I feel it is inappropriate for consideration by the Foundation for the following reasons:

1. The main thrust of the proposal seems to be for work done by foreign scientists in a foreign laboratory. The involvement of members of the U.S. scientific community is nowhere made clear. Please refer to the enclosed publication NSF 81-79 ("Grants for Scientific and Engineering Research"), p. 4, which gives our policy on funding work in foreign institutions.
2. The proposal is excessively brief in experimental details and fails to describe what would be done and by whom, and would probably be impossible to have reviewed.
3. Our Division of International Programs advises me that to consider the proposal as an international collaboration under the U.S.-France Program a number of extra steps have to be taken. These are described in the enclosed draft of the guidelines for the U.S.-France Cooperative Science Program. Since there is no corresponding Program with Austria, they cannot consider that aspect of your proposal.

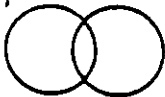
Sincerely yours,



Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

Enclosures

cc: M. Bardon



- 1056 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

June 3, 1982

Dr. ROLF M. SINCLAIR
Program Director
Atomic, Molecular, & Plasma Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Sinclair,

I acknowledge receipt of your letter of May 28, communicating to us the decision by NSF not to consider the proposal by Professor Rauch on the fundamental test of the rotational symmetry under strong interactions.

Please rest assured that our Institute accepts this decision with grace and respect. Also, please rest assured that the same acceptance will be provided to all future NSF decisions on research grant applications submitted by our Institute to your office or any other NSF office.

As president of the IBR, one of my functions is to communicate NSF decisions to the principal investigators in a way as smooth as possible. Another function expected from me is to provide NSF with sufficient information to reach all the necessary maturity of judgment.

The comments below are respectfully submitted to you in the hope of achieving these objectives toward both Professor Rauch's team, as well as NSF.

COMMENTS ON PROFESSOR RAUCH'S APPLICATION. Permit me to provide additional information on the application. Regrettably, your office decided to reject the proposal without any prior consultation with us, while a courtesy phone call to us would have rendered this letter unnecessary.

The primary use of the proceeds of the proposal are anticipated for U. S. physicists by specific desire of Professor Rauch as well as of the IBR. Actually, a reason why our Institute supports the proposal is to have U. S. experimentalists and theoreticians trained in the field under the supervision of Professor Rauch, who is an undisputed leader in the field on a world wide basis [see below for the advisability of this training].

The names of the U. S. recipients of possible funds were not indicated because we anticipated consultations with the interested Federal Agency on the specific guidelines for their selection [advertising under the Equal Opportunity Right, etc.].

The budget was not prepared in the NSF forms, and was submitted as received from Europe, precisely to stress its preliminary character, as well as the need of specific guidelines from NSF for its finalization.

Also, Professor Rauch is Director of the Atominstitut der Oesterreichischen Universitaeten of Wien, and, as such, he needs no salary from U. S. grants.

In short, we acknowledge that the proposal was incomplete on administrative grounds, and we assume all the responsibility for possible misleading impressions that may have resulted.

On scientific grounds, however, permit me to disagree with your view expressed in point 2 of your letter, to the effect that the proposal is excessively brief and of potentially impossible review. The proposal deals with a fully established experimental setting, that of neutron interferometry, that is now well known by experts in the field; it identifies the proposed experiments in all necessary technical details; and it includes copies of five experimental papers in the field. The references of these papers consists of the virtual totality of the literature in the field. For these reasons, the proposal is fully sufficient for a technical review by experts.

Needless to say, as it is the case for scientific papers, no research grant application is perfect. Professor Rauch's application can be improved in a number of ways, e.g., by listing additional experiments, or by adding copies of additional papers in the experimental techniques in case a reviewing by non-experts in the field is desired.

COMMENTS ON POSSIBLE IMPLICATIONS FOR NSF. The submission of Professor Rauch's application is clearly the best opportunity to identify the relevance of the underlying physical problem for the general NSF programs in strong interactions. Permit me the liberty of presenting a few comments in this respect, in the hope that they can be of some value for NSF either now or in the future.

Theoretically, the situation is so simple to appear paradoxical. One of the conditions for the exact character of the rotational symmetry for hadrons is that their charge distribution is perfectly spherical. This property is evidently verified under long range electromagnetic interactions, say, for the proton of an hydrogen atom. However, the preservation of a perfectly spherical charge distribution under sufficient impacts due to strong interactions cannot be sustained on true scientific grounds. In fact, the possibility of small deformations for sufficiently intense collisions is rather natural.

The use of the conventional quantum mechanics leads to a perfectly rigid and spherical charge distribution. The use of a covering mechanics currently under

study for the strong interactions [called hadronic mechanics] permits the representation of possible deformations [they are technically achieved via the replacement of the enveloping, associative algebra of operators with more general Lie-admissible forms]. Studies by several theoreticians along this alternative line, particularly those by Professor Eder, predict about 1% deviation from a perfectly spherical charge symmetry for neutrons within the intense fields in the vicinity of nuclei.

Experimentally, the situation is at a considerably advanced stage, although so much remains to be done. In fact, Professor Rauch initiated the experimental test of the rotational symmetry via neutron interferometry back in 1975. Since then, his team has continuously improved the experimental techniques, by repeating the experiments several times through the years [for this reason he is the initiator and undisputed experimental leader in the test]. The point you should be fully aware of is that the recent measures yields exactly the 1% deviation predicted by Professor Eder et al.

It should be stressed that these latest measures are still tentative at this time, and in need of verifications, first, by Professor Rauch's team itself, and, second, by independent U. S. experimentalists. Actually, the unsettled character of the available measures is precisely the reason for the application.

Administratively, the situation is quite delicate, and deserving the best consideration by NSF. In fact, NSF is spending large amounts of public funds in strong interactions. A considerable portion of these funds is spent under the belief [by grant recipients and NSF officers] that the rotational symmetry is exact under strong interactions. However, physics is based on experiments. Scientific accountability and lack of discrimination among equally probable scientific views, demand the experimental resolution of the exact or only approximate validity of the rotational symmetry. After all, this symmetry is not a minute detail. It is at the foundations of virtually ALL contemporary physical knowledge with self-evident implications at the level of National interests.

The interplay between academic and governmental circles should also deserve the best possible NSF attention. It is public knowledge that the possible experimental detection of a deformation of the charge distribution of hadrons under strong interactions would cause considerable damage to several academicians in various institutions. For this reason, experiments such as that proposed by Professor Rauch, are opposed by organized academic interests; as documented in a variety of cases.

For the orderly condition of our community, it is essential that Federal Agencies such as the NSF, continue to provide evidence of their independence from conceivable academic lobbying toward scientifically discriminatory and administratively unbalanced uses of public funds.

Needless to say, NSF has brilliantly accomplished this duty in the past. I have achieved my objectives if this letter provides you with valuable information for the continuation of this difficult duty in a rapidly changing scientific scene.

After all, the number of physicists now accepting the plausibility of a possible breaking of the rotational symmetry due to deformations of the charge distribution, is increasing considerably in time. Also, the literature directly or indirectly relevant to the problem has reached rather substantial proportions [I am referring to some 10 volumes of proceedings of Workshops and Conferences, plus several research monographs, plus a large number of papers]. Even though this literature continues to be ignored by academicians financially committed to the exact rotational symmetry, it should not be ignored by NSF. In fact, its ignorance could one day prove to be excessively and unnecessarily risky.

THE IBR RECOMMENDATIONS. Upon due consultation with our Board of Governors, as well as colleagues and advisors, permit me to submit the following alternatives for consideration by NSF.

Alternative A: Confirm the rejection of Professor Rauch's application.

In this case, permit me to ask the courtesy of sending me a new letter of rejection mentioning only the lack of agreement between Austria and the U.S., and abstaining from additional remarks, particularly those in point 2 of your current letter [that might be interpreted as being offensive, or, at extreme, even as a manifestation of the desire by NSF not to consider the experiment]. The IBR shall then communicate the rejection to the Atominstitut in Wien via a copy of this modified letter. For reasons communicated verbally to Professor P.S. Rosen of your office, we believe that, in this instance, it is recommendable to proceed in the smoothest possible way. As a gesture of courtesy on our part, we are therefore returning your original letter hereby enclosed.

Alternative B: Accept the consideration of Professor Rauch's application in a modified form.

In this case, we would be most grateful to receive the following guidelines.

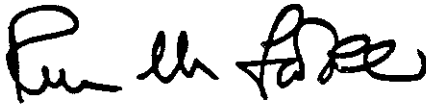
- (a) Criteria for the identification of the U.S. recipients;
- (b) Administrative guidelines for the itemization of funds that can be allocated for the ILL-Laboratory in France under current NSF rules; and
- (c) Suggestions for the technical improvements of the presentation vis-a-vis the refereeing process, as well as any other pertinent data.

Alternative C: Recommend the submission of a new application ON THE SAME EXPERIMENT to be done entirely in the U.S. by U.S. physicists.

In this case, we can provide our best efforts to put together a team comprising U.S. experimentalists in neutron interferometry, as well as theoreticians in favor and against the exact rotational symmetry, in such a way to achieve the best possible diversification of data elaboration and scientific maturity. The administration of this possible new proposal needs not necessarily be conducted by the IBR, and can be conducted by other Institutions. In this latter case, however, the cost is expected to be higher because of overheads generally higher than those practiced by the IBR. Also, to achieve credibility, alternative Institutions should not have a record of opposition to the experiments and underlying theoretical studies. The emphasis on the same experiment is referred here to the test of the rotational symmetry, but its realization can be different than that suggested by professor Rauch.

Whatever the final decision will be, NSF can count on the best possible collaboration, understanding, and backing from the IBR.

Very Truly Yours

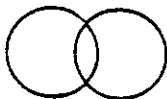
A handwritten signature in black ink, appearing to read 'Ruggero Maria Santilli', written in a cursive style.

Ruggero Maria Santilli
President and
Chairman of the Board of Governors

RMS-mlw

cc.: Drs. M. Bardoni and P.S. Rosen, NSF

encl. Original letter by Dr. Sinclair of May 28, 1982



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

Office of the President

June 23, 1982

Drs. R.M.SINCLAIR, M. BARDON, and P.S.ROSEN
Division of Physics
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Drs. Sinclair, Bardon, and Rosen,

I would like to confirm my recent informal conversation with Peter Rosen, indicating the consideration of Rauch's application by the Division of Nuclear Physics of the DOE.

I would like to confirm also our tentative plans to inform Wien (and Grenoble) that the proposal is under consideration by DOE, and that it is not being considered by NSF

"as a result of a sound judgment to avoid un-necessary duplications of Governmental efforts."

We believe that this is the smoothest possible way of communicating the NSF decision. Nevertheless, as verbally indicated to Peter Rosen, we would appreciate the courtesy of a final communication from you on the matter [an informal "go ahead" by phone would be o.k. for us].

As a gesture of courtesy on our part, we are enclosing copy of a report prepared for Dr. Ritter at DOE providing a scientific elaboration of the proposal. The open physical issues addressed in this report have an evident administrative relevance. We therefore hope that the information may be valuable to you, and we shall attempt to keep you informed in the future of major developments.

Very Truly Yours

Ruggero Maria Santilli
President

RMS-mlw

encl.

cc. Dr. E.T.RITTER, Div. Nucl. Phys., DOE

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 23 1982

Dr. Ruggero Maria Santilli
President and
Chairman of the Board of Governors
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of June 3, 1982. Please note that the proposal you submitted was not rejected. It was just not possible for us to consider it.

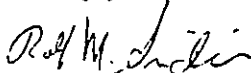
I enclose again our letter of May 28, 1982. The first two reasons given therein indicate why we felt the proposal referred to therein could not be considered by the Foundation. The third states in addition why it did not fit the further requirements of our international programs.

As regards the second of these points - the brevity of the proposal - I refer you to pp. 6-21 of the enclosed booklet NSF 81-79 ("Grants for Scientific and Engineering Research"), which describes the information that should be included in a research proposal to the Foundation. Your inclusion of background material is of course of help to us and to the reviewers. It would still be necessary for us to have a description of the proposed new experiment in appropriate detail, with quantitative assessments where necessary, together with the other material called for, in order to arrange for proper review.

We are not in a position to give advice on which scientists should be involved, nor can we suggest specific technical improvements in a proposal.

I hope these additional remarks are of help to you and your colleagues.

Sincerely yours,



Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

Enclosures: our letter of May 28, 1982
NSF 81-79

cc: M. Bardou

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 29 1982

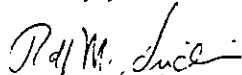
Dr. Ruggero Maria Santilli
The Institute for Basic Research
96 Prescott Street
Cambridge, Massachusetts 02138

Dear Dr. Santilli:

Thank you for your letter of June 23, 1982 (which crossed with our letter to you of that date).

We are informed by your letter that you have submitted a proposal to DOE that is basically the same as one you had sent earlier to NSF, and thus you do not wish the Foundation to consider your proposal further.

Sincerely yours,


Rolf M. Sinclair
Program Director for Atomic,
Molecular, & Plasma Physics

cc: M. Bardon
S. P. Rosen

PART XXXIII

SUPPRESSION OF THE

TESTS OF THE ROTATIONAL

SYMMETRY,

SECTION C:

REJECTION OF AN

I.B.R. APPLICATION BY THE U. S.

DEPARTMENT OF ENERGY

FOR A JOINT

AUSTRIA—FRANCE—U.S.A.

COLLABORATION

Research Grant Application

Submitted to the

~~NATIONAL SCIENCE FOUNDATION~~
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

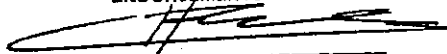
EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND
ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION

Proposed Starting Date:
June 1, 1982

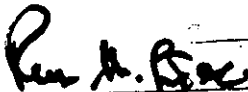
Proposed Duration
12 Months

Amount Requested:
\$ 94,900

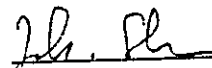
ENDORSEMENTS



H. Rauch
Principal Investigator
Atominstitut
Wien, Austria
Tel. (0222) 75 51 38



R. M. Santilli
Co-Investigator
The Institute for Basic Research
Cambridge, Massachusetts USA
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J. Summhammer
Co-Investigator
Atominstitut
Wien, Austria
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R.M. Santilli
President
The Institute for Basic Research
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Legal Firm of the Institute
Wasserman & Salter
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Boston, Massachusetts 02109
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3. G. Badurek, H. Rauch, A. Zeilinger, W. Bauspiess, and U. Bonse, Phys. Rev. D, 14, (1976) 1177	
4. H. Rauch, A. Wilfing, W. Bauspiess, and U. Bonse, Z. Physik, B29, (1978) 281	
5. S. Hammerschmied, H. Rauch, H. Clerc, and U. Kischko, Z. Phys. A, (1981) 302	
D. Theoretical Papers	
6. C. N. Ktorides, H. C. Myung, and R. M. Santilli, Phys. Rev. D22, (1980), 892	
7. G. Eder, Hadronic J., 4, (1981), 2018	
8. R. M. Santilli, "Use Of The Hadronic Mechanics For The Best Fit Of The Time-Asymmetry Recently Measured By Slobodrian, Conzett, Et Al", IBR, Preprint April 1982	

Some of the primary experimental papers by Professor Rauch and his team, in the topic of the proposal, during the period 1975-1982, are reproduced in the application to DOE. Their listing will not be repeated in this additional material.

The scientific value of these experiments can be better focused by the fact that they constitute the ONLY experiments currently available on the direct measure of spin. In fact, ALL the remaining experiments in strong interactions, both in nuclear physics and high energy physics, ASSUME the exact validity of the spin symmetry in the data elaboration.

BACKGROUND OF THE ILL-LABORATORY. All experiments considered here were conducted by Professor Rauch at the Laue-Langevin Laboratory in Grenoble, France. The proposal recommends the conduction of the proposed experiments also at the ILL-Laboratory.

This is due to a number of scientific and logistic reasons, including the availability at the ILL-reactor of a high flux D18 user set up particularly suited for the proposed experiment.

The ILL-Laboratory is well known and actually used by several U. S. physicists. No additional information is therefore needed here.

THE IDEA OF THE EXPERIMENT. The objective is to achieve a direct experimental test of the SU(2)-spin symmetry under joint strong and electromagnetic interactions. This objective is made feasible by a branch of experimental physics known under the name of neutron interferometry.

In simple terms, a perfect cristal neutron interferometer (see Figure 1) is constituted by a neutron beam subjected to coherent splitting into two branches, and then to a coherent recombination via the use of a perfect cristal. The neutron beam is generally monochromatic, unpolarized, of low energy and of high flux. The perfect cristal is generally a Si cristal with extremely low impurities shaped with three vertical slabs, as indicated in the figure.

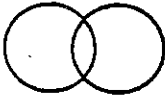
An important feature of the set up is the possibility of having a wide angle of separation of the two branches. This feature permits the application of an electromagnet to one (or both) of the branches of the beam for precession (spin flip). Since neutrons are Fermions, a minimum of two complete spin flips (720 deg) are needed to permit the same coherent recombination as that without spin flips. The value of the magnetic field needed to produce two spin flips of the neutrons is 7,496 G. The gap of the electromagnet is generally 1 cm. A typical beam cross section is $2 \times 1.5 \text{ mm}^2$. The cristal wavelength is 1.83 Å.

If both branches of the beam are subjected to spin precession, the intensity of the exiting beam is modulated, as per Figure 2. The periodicity of the modulation is then a measure of the angle θ of spin flip. Note that the measure is as direct as experimentally possible, in the sense that the periodicity of the modulation does not require theoretical models in the data elaboration.

The test was first conducted in 1975 under electromagnetic interactions only, and yielded the

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 4π 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* and at the recent *First International Conference on Nonpotential Interactions and their Lie-admissible Treatment* held at the Université d'Orléans, France, from January 5 to 9, 1982, or the inclusion of additional new physical effects.



— 1068 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

June 16, 1982

**RE: ADDITIONAL INFORMATION RELATED TO THE RESEARCH GRANT PROPOSAL
ENTITLED**

**"Experimental Verification of the SU(2)-spin Symmetry under Strong and Electro-
magnetic Interactions via a joint AUSTRIA-FRANCE-U.S.A. Collaboration"**

**Principal Investigator: Professor H. Rauch
DOE REF. NO. P82206041**

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545
Mail Stop ER-23 GTN

Dear Dr. Ritter,

We would like to express our appreciation for your consideration of the proposal by Professor Rauch, as well as for the courtesy of your time and cooperation during our recent phone conversation.

It appears that additional information may be useful for the review of the proposal. I shall therefore attempt to outline in this letter a number of aspects, beginning with *prima facie* motivations, and then passing to conceptual, theoretical, and speculative arguments. Some of the conceivable implications are also discussed for completeness. Finally, a few comments on the budget are appropriate because they are not included in the proposal.

On our part, we do not contemplate the release of additional scientific material for review, unless unexpected novel developments occur. Nevertheless, we shall remain at your disposal to provide any additional assistance you might need, such as copies of the Proceedings of the various Workshops and Conferences relevant for the proposal, copies of individual papers, delivery of informal presentations on the topic at your office or other locations, etc.

BACKGROUND OF PRINCIPAL INVESTIGATOR. Professor H. RAUCH is the Director of the Atominstitut der Osterreichischen Universitaeten of Wien, Austria. He is an undisputed leading experimentalist in the field of the proposal. In fact, he initiated the experiment proposed in the DOE application back in 1975. Since that time, his team has repeated the experiment several times, by improving apparatus, operation, and approximation.

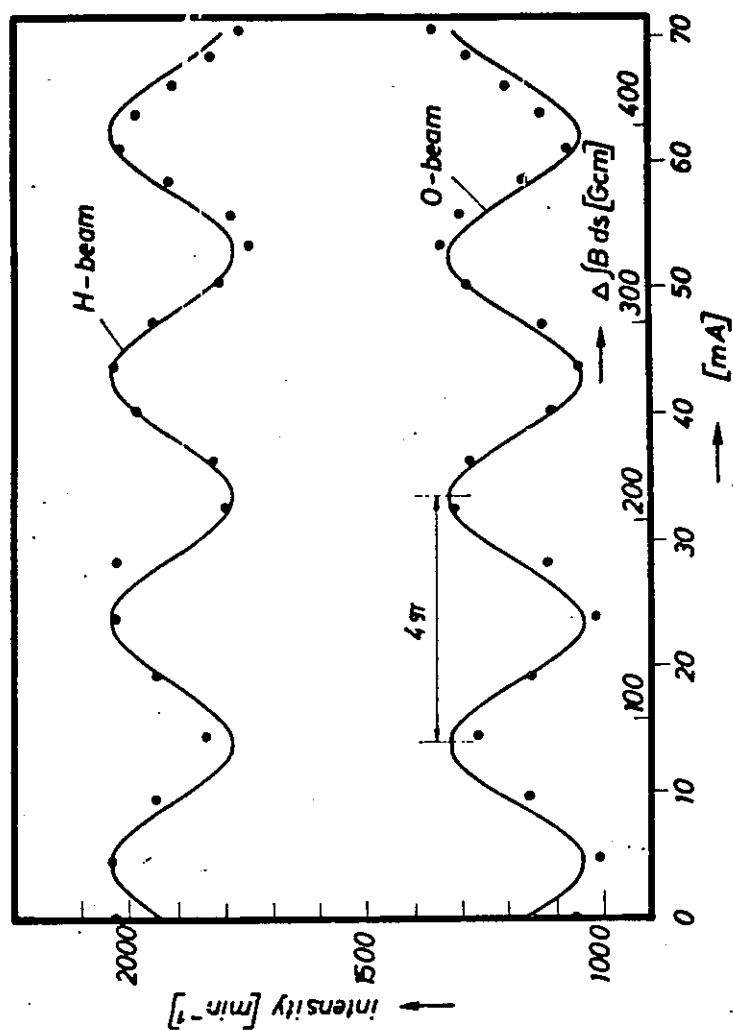


FIGURE 2

angle

$$\alpha = 704 \pm 38 \text{ deg} \begin{cases} \text{Max } 742 \text{ deg} \\ \text{Min } 666 \text{ deg} \end{cases} \quad (1)$$

which is inclusive of the 720 deg needed for the exact SU(2)-spin symmetry, as expected [See paper 2 enclosed in the DOE application].

The objective of the proposed experiment is to repeat the test under joint electromagnetic and strong nuclear interactions. This can be achieved in a number of ways. The most direct one is by filling up the electromagnet gap with matter suitably selected to enhance the neutron-nuclei interactions. In fact, the spin flips of the neutrons beam now occur under joint electromagnetic interactions and nuclear interactions due to the matter within the gap.

The strong component can be enhanced in a number of ways [which are contemplated for consideration but not mentioned in the DOE application]. One possible way is to repeat the experiment with a progressive increase of the width of the matter to be penetrated by the neutron beam (say, 3cm, 4cm, and 5cm). Another possibility is to repeat the experiment with an increasing number of spin flips (say, 2, 4, 6, etc.). Other possibilities are offered by different strong interactions in the two branches of the beam. Note that these possibilities could also permit the test of the SU(2)-spin symmetry under strong interactions characterized by linearly varying data of width, spin flips, etc. For a study of these alternatives, see Vol. C of ref. 2 of this letter and refs. 40,41 in particular.

The test can be conducted with high accuracy, typical of neutron interferometers, which is of the order of

$$\frac{\Delta \alpha}{\alpha} = 10^{-4} \quad (2)$$

and which is fully sufficient for the desired objectives (see below). It should be noted that the achievement of the accuracy demands a variety of experimental considerations for: corrections for diamagnetisms, spin-orbit interactions, nuclear polarization; avoiding temporal instability; reducing stray fields; etc.

PRIMA FACIE MOTIVATION FOR THE EXPERIMENT. During the last repetition of the experiment by Rauch's team in 1978, the magnet gap was filled up with Mu metal sheets. This was done only to reduce stray fields. In fact, the experimenters did not have in mind the test of spin under joint strong and nuclear interactions. Their measures resulted in the modulation of Figure 3 with periodicity [See paper 4 enclosed in the DOE application].

$$\alpha = 716.8 \pm 3.8 \text{ deg} \begin{cases} \text{Max } 720.6 \\ \text{Min } 713.0 \end{cases} \quad (3)$$

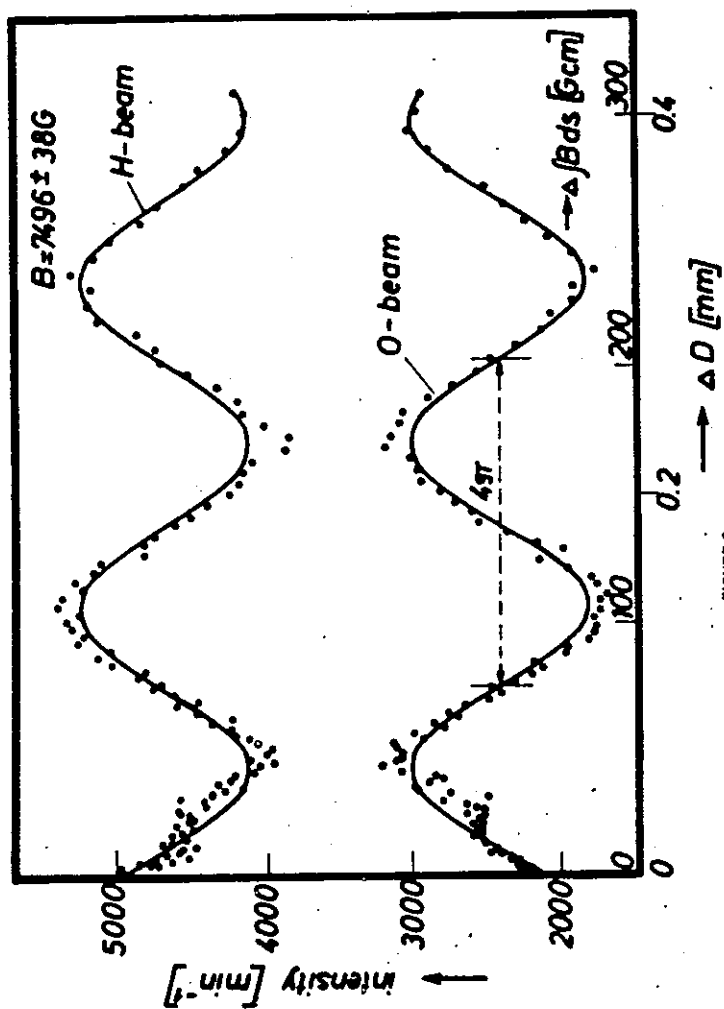


FIGURE 3

More recently, the team has reinspected these measures for a number of reasons, including the availability of improved value of nuclear constants. The best measure available at this time is given by [See paper 1 enclosed in the DOE application].

$$\alpha = 715.87 \pm 3.8 \text{ deg} \begin{cases} \text{MAX } 719.67 \\ \text{MIN } 712.07 \end{cases} \quad (4)$$

This value does not contain the 720 deg needed for the exact character of the SU(2)—spin symmetry under strong interactions. The need to resolve the issue via new experiments is then consequential.

It should be stressed that value (4) is only one of the *prima facie* reasons. At a deeper inspection, a number of additional data are not in agreement with the predictions of the exact rotational symmetry in a scientifically convincing way. For instance, we have clusters of points outside the minimal and maxima predicted by the exact symmetry (see fig. 3); the phase between the intensity and the polarization modulation does not appear to include the predicted 90 deg in a truly clear way; the average values of the modulations of all experiments conducted until now is BELOW 720 deg contrary to statistical expectations [this has been called "angle slow-down effect"⁴⁰]; etc.

Note that these *prima facie* motivations are only those originating from a direct inspection of the experimental data available at this moment. Several additional motivations of theoretical, as well as historical character, exist and will be indicated below.

CONCEPTUAL MOTIVATIONS. As we know well, neutrons and protons are not points. Instead, they are extended objects with a charge radius of the order of 10^{-13} cm, as it is the case for all hadrons.

The extended character of hadrons creates a fundamentally new situation vis-a-vis to SU(2)—spin symmetry. In fact, a necessary condition for the rotational symmetry to be exact for an (extended) hadron under strong interactions, is that its charge distribution remains perfectly spherical, irrespective of the intensity of the external fields, and of the impact with other hadrons.

This rigid view is clearly untenable on objective scientific grounds. In fact, the existence in nature of perfectly rigid objects can be advocated by religious arguments similar to those used by the Catholic Church at Galileo's trial, but not by scientific arguments.

It is natural to expect that the charge distribution of hadrons experiences deformations under strong and electromagnetic interactions, depending on the local physical conditions, that is, the intensity of the external field, the energy of the collision with other hadrons, etc. Needless

to say, the deformation is expected to be very small [see below for theoretical predictions].

Stated in words as simple as possible, *the proposed experiment has been conceived to measure possible deformations of the charge distribution of a hadron under sufficient external fields.*

This is the first concept needed for the understanding of the proposal. Second, recall that, by comparison, leptons can be very well approximated as being point-like. The possibility of a deformation of the charge distribution is evidently absent in this case.

Another important concept is that no test whatever is recommended for the rotational symmetry of leptons, as well as, more generally, for all particles that are truly "elementary".

Third, let us note that the possible deformation of the charge distribution of hadrons is expected to exist for *all interactions*, including most importantly the electromagnetic ones. More specifically, consider the physical conditions relevant for the proposed experiments, that is, *neutrons within the intense fields in the vicinity of nuclei*. The deformation of their charge distribution can be expected not only from strong interactions, but also from the electromagnetic ones. It should be stressed that we are referring to the electromagnetic interactions neutron-nuclei and NOT neutron-electromagnet (the latter being too long range and weak to produce measurable deformations).

The first theoretical prediction of a breaking of the rotational symmetry for extended hadrons under strong interactions was done in ref.¹¹ and thereafter studies in papers.^{17,18} The first prediction of the same deformation under short range electromagnetic interactions is that of ref.¹⁹ [reproduced in the DOE application] The cases of the weak and gravitational interactions are under study.

APPARENT INSUFFICIENCIES OF THE "ATOMIC MECHANICS". To my best knowledge, the "Atomic Mechanics" (i.e., the conventional quantum mechanics) does not appear to be capable of representing hadrons experiencing a deformation of their charge distribution.

Conceptually, this can be understood by recalling that the mechanics was conceived for long range electromagnetic interactions of the point-like electrons (hence, the emphasis on its "atomic" character), while the physical situation we are interested in is basically different.

The deficiency can be seen in more detail at the technical level.

The structure of the Atomic Mechanics is entirely set up for the representation of elementary, point-like, structureless, particles. This is the case beginning at the basic axioms of the Hilbert space \mathcal{H} over the field \mathbb{C} of complex number, with: inner product

$$\langle a | a' \rangle = \int a a' \quad (5)$$

enveloping associative algebra \mathcal{OZ} of local-differential Hermitean operators A, B, C, \dots with conventional product AB

$$\mathcal{OZ}: AB = \text{ASSOCIATIVE PRODUCT} \quad (6)$$

attached Lie algebra

$$L: [A, B]_{\mathcal{OZ}} = AB - BA \quad (7)$$

Lie group

$$G: e^{\theta X} = 1 + \frac{\theta}{1!} X + \frac{\theta^2}{2!} XX + \dots \quad (8)$$

etc. This theory can only represent interactions at a collection of isolated points, as evident from the local-differential character of the operators, and several other features.

For the case of neutron interferometers, the predictions of the Atomic Mechanics are based on Pauli's realization of $SU(2)$ spin

$$\vec{S} = \frac{1}{2} \vec{\sigma}, \quad \hbar = 1$$

$$SU(2): (\vec{S})^2 |> = \frac{3}{4} |>, \quad S_3 |> = \pm \frac{1}{2} |> \quad (9)$$

$$[S_i, S_j]_{\mathcal{OZ}} = i \epsilon_{ijk} S_k$$

The spinorial wave function of the neutrons transforms according to the law

$$\psi' = e^{i\chi} e^{-i\sigma_3 \frac{\alpha}{2}} \psi \quad (10)$$

where χ and α are the magnetic and nuclear phase shifts, respectively.

The intensity and polarization modulations for the out-going beam can then be written, after

simple algebra,

$$I' = \psi'^{\dagger} \psi' = \frac{I}{2} (1 + \cos \chi \cos \frac{1}{2} \alpha) \quad (11)$$

$$\vec{P}'(\vec{P} = \vec{0}) = \frac{\sin \chi \sin \frac{\alpha}{2}}{1 + \cos \chi \cos \frac{\alpha}{2}} \cdot \frac{\vec{\alpha}}{\alpha}$$

Note that the angle of modulation α is precisely the angle measured by the experimenters.

The divergencies indicated earlier are apparent deviations from the predictions of formulae (11) for the case of the exact symmetry.

The point-like characterization of the neutron and of its charge distribution in formulae (9) through (11) is evident. Equally evident is then the need to attempt a representation of the neutron closer to physical reality.

THE INTRIGUING PREDICTIONS OF THE "HADRONIC MECHANICS". A comprehensive study of the insufficiencies of the Atomic Mechanics for strong interactions was initiated in 1978 by a coordinated group of scholars comprising mathematicians, theoreticians, and experimentalists. The studies included the organization of four WORKSHOPS ON LIE-ADMISSIBLE FORMULATIONS held in Cambridge-USA from 1978 until 1981, and of the FIRST INTERNATIONAL CONFERENCE ON NONPOTENTIAL INTERACTIONS AND THEIR LIE-ADMISSIBLE TREATMENT held at the University of Orléans, France, in January 1982.

These studies have resulted in some ten volumes of Proceedings,¹⁻³ five research monographs,⁴⁻⁸ and a predictably large number of papers [see the bibliography⁹].

These efforts are devoted to a generalization of the Atomic Mechanics into a covering form called "Hadronic Mechanics", which is capable of representing the *EXTENDED* character of hadrons. The understanding is that the interactions are then given by a combination of the conventional action-at-a-distance/potential/Hamiltonian terms plus new contact/nonpotential/non-Hamiltonian terms [note that points can only interact at a distance, while extended particles have additional contact interactions for which the notion of potential energy has no physical basis].

The realization of the generalized mechanics is permitted by mathematical studies on the existence of two progressive generalizations of Lie's theory called of Lie-isotopic and of Lie-admissible type.²⁵⁻³⁰

The need for such a mathematical generalization is self-evident. The notion of particle used in Atomic Mechanics is technically realized via a Lie group. As a result, no genuine advancement

in the notion of particle is possible without a generalization of the very structure of Lie's theory (Lie group, Lie algebras, and enveloping associative algebras).

The simplest generalization is given by the Lie-isotopy theory. It is characterized by the generalization of the envelope $\mathcal{O}\mathcal{L}$ of operators A, B, C, \dots into the form

$$\mathcal{O}\mathcal{L}^*: A * B = ATB \quad (12)$$

where T is a suitable operator (Hermitean, bounded, and positive) fixed for all products. The attached Lie algebra L^* is then characterized by the generalized product

$$L^*: [A, B]_{\mathcal{O}\mathcal{L}^*} = A * B - B * A = ATB - BTA \quad (13)$$

The underlying generalized group is now given by the expansion in $\mathcal{O}\mathcal{L}^*$, i.e.,

$$G^*: e^{\frac{\partial}{\partial X} T} = 1 + \frac{\partial}{1!} X + \frac{\partial^2}{2!} X * X + \dots \quad (14)$$

Structures $\mathcal{O}\mathcal{L}^*$, L^* and G^* are called associative-isotopic envelopes, Lie-isotopic algebras, and Lie-isotopic groups, respectively, because they preserve the original enveloping, algebraic and group character, respectively.

Heisenberg's time evolution of the Atomic Mechanics

$$i\dot{A} = [A, H] = ATH - HTA, \quad T = \hbar^{-1} = 1 \quad (15)$$

is then generalized into the isotopic form proposed in ref.¹¹

$$i\dot{A} = [A, H]^* = ATH - HTA, \quad T = T(\vec{\epsilon}, \vec{p}, \dots) \quad (16)$$

As well known, law (15) describes the time evolution of a point-like particle under external action—at-a-distance/potential/Hamiltonian fields (or a collection of such particles). The capability

of the covering law (16) to represent the time evolution of an extended particle can be seen in a number of ways, e.g., from possible integrodifferential realizations of the isotopy operator T , from the fact that the forces CANNOT be reduced all to a Hamiltonian forms (hence, they are also of contact type); etc.

The theory is technically made possible by an underlying isotopic generalization \mathcal{H}^* of the Hilbert space \mathcal{H} ¹⁴⁻¹⁶ with inner product

$$\langle a | * | a' \rangle = \langle a | T | a' \rangle = \int_{a, a'}^* = T^{-1} \int_{a, a'} \quad (17)$$

and with corresponding generalizations of: operators (Hermitean, unitary, antiunitary, etc.); quantum postulates (observability, states, time evolutions, etc.); and numerous other aspects of the conventional Atomic Mechanics. In particular, Planck's unit

$$I = \hbar = 1, \quad IA = AI = A \quad (18)$$

is generalized into the integrodifferential (left and right) unit operator

$$I^* = \hbar^* = T^{-1}(\vec{r}, \vec{p}, \dots), \quad I^* A = A * I^* = A \quad (19)$$

which is expected to represent the increased complexity of the energy exchanges for short range interactions among extended particles.

Also, the atomic eigenvalue equation is generalized into the isotopic form

$$H^* | \rangle = H T | \rangle = \lambda^* | \rangle = \lambda | \rangle, \quad \lambda^* = I \lambda \quad (20)$$

which is, structurally, the most general possible one under an *associative* enveloping algebra.

These advances have lead to a generalization of Schrödinger's equations of the type^{15,20}

$$i \frac{\partial}{\partial t} \psi(t, \vec{r}, \vec{p}) = H(\vec{r}, \vec{p}) * \psi(t, \vec{r}, \vec{p}) \quad (21)$$

Under certain realizations of the \vec{r} and \vec{p} operators, eq. (21) has been proved to be equivalent to equations (16), and to admit as a classical limit the Birkhoffian generalization of the Hamiltonian Mechanics.⁵ This latter aspect is important to confirm that the underlying theory is not one

for massive points in perpetual-motion conditions.

A review of the theory is impossible in a letter. Therefore, we can indicate here only the essential ideas of the "hadronic spin" used in the data elaboration of the proposed experiment.

Consider a neutron beam under the condition that possible deformations of the charge distribution are ignorable. Under these circumstances, the "atomic spin" (9) is applicable. Suppose now that the same beam enters a region of fields of high intensity, such as when in the vicinity of nuclei. We then assume the hadronic spin characterized by

$$\vec{S}^* = I^* \vec{S} = T^{-1}(\vec{r}, \vec{p}, \dots) \vec{S} \quad (22)$$

Suppose now, in first approximation, that the T-operator can be averaged to a constant

$$\frac{1}{VT} \int dV \int dt T = c \approx 1 (= \hbar^{-1}) \quad (22)$$

as conceivable for certain nuclear conditions. Then, we have the eigenvalues equations in χ^*

$$(\vec{S}^*)^2 |> = \frac{3}{4} |>, \quad S_3^* |> = \pm \frac{1}{2} |> \quad (23)$$

namely, the magnitude and third component of spin are the conventional ones. Nevertheless, a number of new features emerge. First, a study reveals that the remaining two components of spin transform according to a mixture of conventional rotations and deformations¹⁹

$$U_3^* \vec{S}^* U_3 = M_3 N_3 \vec{S}^* \quad (24)$$

$$M_3 = \begin{pmatrix} \cos \alpha & -\sin \alpha & 1 \\ \sin \alpha & \cos \alpha & 1 \\ 0 & 0 & 0 \end{pmatrix}, \quad N_3 = \begin{pmatrix} \beta & 0 & 0 \\ 0 & \beta & 0 \\ 0 & 0 & 1 \end{pmatrix}$$

by therefore confirming the desired objective.

Next, one sees the possibility of representing ANOMALOUS magnetic moments via CONVENTIONAL values of spin (and charge). This is a typical situation of NUCLEAR (AND NOT

ATOMIC) physics, which has essentially escaped true understanding until now.

For applications to a number of other aspects of nuclear physics that have remained obscure via the use of the Atomic Mechanics, we refer the interested reader to the technical literature.^{2,3}

What is important for the DOE application is that the isotopic generalization of the Hilbert space, of the quantum postulates, and of the atomic time evolutions, permit a direct interpretation of a number of nuclear phenomena that are apparently outside the capability of the Atomic Mechanics.

The main idea of the data elaboration of the proposed experiment is now predictable, and consists in the generalization of basic law (10) into the corresponding form in \mathcal{OZ}^* , i.e.,

$$\psi' = e^{i\chi} e^{-iG_3 T \frac{\alpha}{2}} I^* \quad (26)$$

This yields intensity and polarization modulations DIFFERENT than the atomic ones (11). Most importantly, the isotopic term enters directly in the argument of the periodicity of the modulation. Thus, deformations of the charge distribution result into different values of the periodicity, as expected.

A number of explicit forms of the generalized intensity and polarization modulation have been studied, some of them via the still more general "Lie-admissible (non-associative) extension of \mathcal{OZ} ", and others are contemplated to be investigated if the proposal is funded.

As an example, we quote the generalized laws computed in ref.⁴⁰ via a Lie-admissible mutation of \mathcal{OZ}

$$I' \cong \frac{I}{2} \left[1 + \varepsilon \cos \chi + (1 + \varepsilon) \cos \chi \cos \left((1 + \varepsilon) \frac{\alpha}{2} \right) \right] \quad (27)$$

$$\vec{P}'(\vec{P}=0) \cong \sin \chi \sin \left((1 + \varepsilon) \frac{\alpha}{2} \right) / \left[1 + \varepsilon \cos \chi + (1 + \varepsilon) \cos \chi \cos \left((1 + \varepsilon) \frac{\alpha}{2} \right) \right]$$

That computed in ref.¹⁹ yields the angle

$$\alpha' = \alpha \left[1 + (1 - \varepsilon^2) \frac{\alpha^2}{12} + \dots \right] \quad (28)$$

as one can see in the papers reproduced in the DOE application.

Most importantly, ref.¹⁹ predicts 1% deviation from the perfectly rigid charge distribution for neutrons within the intense fields of nuclei. This prediction is **CONFIRMED** by best values (4) currently available.

In addition, law (28) predicts an angle α' which is ALWAYS smaller than that of the Atomic Mechanics. This prediction too is confirmed by the average values of all measures conducted until now, as indicated earlier.

A number of additional predictions of the Hadronic Mechanics that are confirmed by available measures, calls for an in depth technical knowledge of the field, and cannot be indicated here in a meaningful way.

To summarize, the application submitted to DOE with Professor Rauch as Principal Investigator recommends the conduction of an experiment for the future resolution of the following different predictions in the spin behaviour of a neutron beams under certain physical conditions identified in the proposal:

1. The Atomic Mechanics predicts two complete spin flips for a total of 720 deg; while
2. The Hadronic Mechanics predicts a smaller rotation of the order of 710 deg.

The available best measures do not include 720 deg and favor the prediction of 710 deg. The resolution of the difference (of about 1%) is well within current experimental capabilities in neutron interferometry, with the understanding that experimental results under this proposal must be subjected to additional independent verifications [see last part of this letter].

The differences in prediction can be conceptually reduced to the fact that:

- 1'. The Atomic Mechanics represents neutrons as massive, structurless points. Under these assumptions, the rotational symmetry CANNOT be broken; while
- 2'. The Hadronic Mechanics represents neutrons as extended charge distributions. Under these conditions, the spherical charge distribution can experience small deformations under sufficiently intense external fields, with consequential small rotational-asymmetry.

The quantitative treatment of the different predictions is made possible by the underlying mathematical structures of the theories, that is:

- 1''. The Atomic Mechanics is based on the conventional Lie theory realized via operators on a conventional Hilbert space; while
- 2''. The Hadronic Mechanics is based on a generalization of Lie's theory realized via operators on a generalized formulation of Hilbert spaces.

It should be kept in mind that:

- 1" : *The classical image of the Atomic Mechanics is given by the Hamiltonian Mechanics for massive points under perpetual-motion conditions; while*
- 2" : *The classical image of the Hadronic Mechanics is given by the Birkhoffian Mechanics for extended systems under superpositions of action-at-a-distance/ potential and contact/nonpotential forces.*

Also, it may have some value to know that the DOE application under consideration has been submitted following studies conducted over the period 1978–1982 under DOE support by a coordinated group of mathematicians, theoreticians, and experimentalists.

Finally, the DOE application under consideration is the EXPERIMENTAL PART of a comprehensive research program submitted to DOE and including

- *A THEORETICAL PROPOSAL by a group of physicists for a coordinated study of the Hadronic generalization of the Atomic Mechanics; as well as*
- *A MATHEMATICAL PROPOSAL by a group of mathematicians for a coordinated study of the mathematical structure underlying the physical theories.*

Copies of these additional proposals are available on request.

POSSIBLE IMPLICATIONS OF THE PROPOSED EXPERIMENT. The rotational symmetry is not an aspect of secondary physical significance. In fact, it is at the foundation of virtually ALL contemporary knowledge in particle dynamics. A study of the proposal by Professor Rauch without the consideration of its possible implications, would therefore be grossly deficient. By the same token, this letter too would be deficient without at least touching some of the possible implications.

The first predictable implications are those related to Galilei's relativity and Einstein's special relativity. The rudimentary review made in this letter is sufficient to indicate the inapplicability of these relativities to the Hadronic Mechanics, and the need for their suitable generalization.

In fact, these conventional relativities are realized via (unitary) Lie groups acting in the Hilbert space \mathcal{H} and, as such, they cannot act in \mathcal{H}^* . Also, the time component of these relativities is Hamiltonian, while the Hadronic Mechanics demands the incorporation of contact non-Hamiltonian interactions. Third, all unitary and antiunitary transformations ALTER the Lie-isotopic product and cannot be symmetries of eqs. (16).

The list of insufficiencies of Galilei's and Einstein's special relativities for extended particles treated via the Hadronic Mechanics could continue, but it is not needed for the scholar familiar with the writings of the originators (rather than their followers). In fact, both Galilei (and Einstein) stated quite clearly that their studies were conceived for "massive point" (and "point-like particles")

moving in vacuum under action-at-a-distance forces. The physical conditions we are referring to here are fundamentally different. The generalization of the relativities at a suitable future time is therefore unavoidable, despite a predictable academic resistance.

At any rate, a feverish effort is now well under way to generalize the relativities via covering forms permitted by the Lie-isotopic structure (14).

It should be kept in mind that, at the classical level, rather comprehensive studies have been conducted (some of them dating back to the past century) for the non-Hamiltonian generalization of conventional Galilean formulations. A review of these studies is reported in monograph⁵, including a Lie-isotopic generalization of Galilei's relativity.

The studies for a parallel operator generalization of the relativity are well under way. After all, the studies reviewed in this letter are based on such generalization. In fact, the integrated form of the isotopic time evolution (16), i.e.,

$$T^*(H): A' = e^{i\theta H\tau} * A * e^{-i\theta H} \quad (29)$$

is the time component of the desired generalized relativity, while the isotopic covering of the rotational symmetry used in the data elaboration of Professor Rauch's experiment

$$SU^*(2): A' = e^{i\theta_f J_f T} * A * e^{-i\theta_f T J_f} \quad (30)$$

$J_f \in SU(2)$

is the rotational component of the desired covering relativity.

Needless to say, these studies are at the very beginning and so much remains to be done. At any rate, I hope that this letter has communicated at least in a small way the contagious scientific enthusiasm underlying these studies [we had planned one volume for the Proceedings of the recent Orleans International Conference, but we have been forced to increase them to FOUR VOLUMES — see ref.³].

Other predictable implications are related to the basic laws and principles of the Atomic Mechanics and, inevitably, Pauli's exclusion principle. It is understood that Professor Rauch's experiment DOES NOT refer to the validity of Pauli's principle in the arena for which it was conceived, the atomic structure. It is also understood that the proposed experiment DOES NOT refer to the approximate validity of Pauli's principle in nuclear physics, which is well established by now.

Nevertheless, in the opinion of an increasing number of scholars, *it is time to submit the exact validity of the exclusion principle in nuclear physics to direct, specific, and detailed tests.*

When neutrons experience a conceivable small deformation of their charge distribution, they are no longer exact Fermions, and comparatively small departures from Pauli's exclusion principle follow.

The view expressed by an increasing number of scholars¹⁻³ is that it is time to abandon personal views in this fundamental problem, and initiate quantitative experimental studies. The preliminary information already exists, and FAVORS possible small deviations from Pauli's principle theoretically predicts in ref.¹¹. In fact, recent data on neutron-tritium scattering experiments apparently permit a small overlapping of the wavepackets of the incident s-neutron with those of the two s-neutrons of the tritium core, contrary to the exclusion principle [ref.³¹ reproduced in the DOE application].

Needless to say, and as stressed repeatedly by Professor Rauch in his articles and invited talks, the experimental information currently available is highly tentative. But this is precisely the reason for suggesting additional measures. After all, the exclusion principle is merely assumed in current data elaborations of nuclear physics without the beautiful, historical and direct verifications that occurred in atomic physics.

For a full assessment of the implications under consideration, the additional study of the discrete symmetries is necessary.

Consider the time-reversal symmetry. It is represented by an antiunitary operator depending explicitly on spin. For the case of spin $\frac{1}{2}$, the atomic symmetry is characterized by

$$\tau = e^{-i\pi J_z} \quad (31)$$

It is evident that a possible small rotational-asymmetry in the charge distribution would necessarily imply a consequential time-asymmetry. In turn, a time-asymmetry in the evolution of each hadron would have rather profound implications for the virtual entirety of physics. For instance, it would imply a resolution of the historical open problem of the origin of the irreversibility of our macroscopic reality.

Intriguingly, available measures by a collaboration Berkeley/Quebec³³ have indicated a rather clear time-asymmetry. Subsequent measures at Los Alamos³⁵ have put in question the magnitude of the time-asymmetry of ref.³³. The problem is now under intense experimental study in the USA, Canada, Europe and, apparently, the U.S.S.R. A resolution is therefore possible in the near future.

Nevertheless, *there is a rather general consensus that the time-reversal symmetry is violated in nuclear physics (and, expectedly, under all strong interactions) although in a small amount.*

Evidently, the proposed experiment is not directly concerned with the time—reflection symmetry. However, *the establishing of a rotational—symmetry would be the most powerful indirect verification of the time—symmetry of hadrons.*

A much similar situation occurs for the space—reflection symmetry, whose violation is established in nuclear physics, as well known. In fact, this symmetry too is explicitly dependent on the rotational symmetry via realizations of the type

$$P = \int dV \frac{\mathbf{r}}{s_3} |\hat{\mathbf{r}}, s_3\rangle \langle s_3, \hat{\mathbf{r}}| \quad (32)$$

Again, *the verification of a rotational—symmetry via the proposed experiment would confirm the space—symmetry, e.g., that of experiment.*³²

As an amusing comment, permit me to note a rather odd academic situation. A frequent attitude is that of accepting the space—symmetry, as experimentally established anyhow, but of rejecting the existence of a joint time—symmetry. This is odd because against all teaching by Einstein (on the equivalence of space and time), as well by Dirac (who explicitly recommended the joint space—and time—symmetries since 1949, that is, much before the discovery of the space—symmetry.

A serious study of the experiment proposed by Professor Rauch calls for a technical evaluation of the problematic aspects underlying conventional attitudes of this type in order to separate the pursuit of knowledge from established scientific interests.

For instance, to achieve scientific credibility for the exact time—reflection symmetry joint with the broken space—reflection symmetry, one must solve a number of technical problems of consistency for the conventional formulation of Einstein's special relativity. Clearly, these consistency problems would be resolved by a confirmation of measures (4).

Along similar lines, to achieve credibility for an exact rotational symmetry, joint with the experimentally established space—symmetry [say, that of the Tin isotopes] one must solve additional problems of consistency. We are referring here to a proof that the Atomic Mechanics can represent the difference in space—symmetry in the transition from one isotope of Tin to the other. Again, these problematic aspects would be resolved by a confirmation of measures (4), trivially, because deformations of the charge distribution depend directly on the local physical conditions and, thus, they vary from nuclei to nuclei.

Similarly, to achieve credibility for the exact time—reflection symmetry joint with the established macroscopic irreversibility, one should solve a host of technical problems of consistency between the macroscopic and the microscopic descriptions. As an example, one should prove that the experimentally established NONCANONICAL character of the time evolution of Newtonian systems of the real world can be consistently reduced to a large collection of conjectured UNITARY time evolutions of the particle constituents.

Again, situations of this type would be resolved by a confirmation of measures (4). In actuality, the unity of thought in physics would be rather beautifully expressed by the fact that the Newtonian, Statistical, the Particle Mechanics are nothing but different realizations of the same generalized formulation of Lie theory. As an example, the Birkhoffian Mechanics and the Hadronic Mechanics are nothing but different realizations of the same Lie-isotopic theory, one via functions, and the other via operators.

The regaining of the unity of scientific thought in the various branches of science was one of the objectives of the recent Orléans Conference on Nonpotential Interactions, and an inspection of the Proceedings³ is recommendable for the refereeing of the experiment proposed.

In essence, we are attempting to convey the idea that it is time to limit the compartmentalized conduction of physics via phenomenological models tailored for each individual aspect of particle dynamics. It is time to test the compatibility of the basic assumptions not only within the limited field considered, but also at the level of the general unity of physics.

An ultimate aspect focused by Professor Rauch's experiment is therefore the dichotomy currently existing between

- the ESTABLISHED NONHAMILTONIAN character of the physical reality of our environment, versus
- the CONJECTURED HAMILTONIAN character of particle dynamics,

and which would be resolved via a NONHAMILTONIAN generalization of particle dynamics.

In such a setting, the restriction of the scientific vision to the Hamiltonian particle mechanics without a general view, can likely prove to be a temporary expedient.

USE OF PROCEEDS. It may be valuable to provide information on the use of the proceeds of the proposal, if funded. It is clear that Professor Rauch, being Director of the Atominstitut of Wien, does not need a salary from U.S. grants. The same situation occurs for other participants in Austria and in France.

The proceeds of the proposal are primarily intended for U. S. physicists by specific desire of Professor Rauch as well as of the Board of Governors of the IBR.

The understanding of this point is important for the review of the proposal. The rationale is that

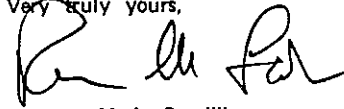
- (a) Professor Rauch's team is currently the leading one in the test of the rotational symmetry;
- (b) an intensification of experiments in the field can be predicted, particularly if the current deviations from the perfectly rigid charge distribution are confirmed; and
- (c) the U.S.A. has all the technology for additional experiments in National and private laboratories.

In a situation of this type it is advisable that U. S. experimentalists are trained in the field of the proposal UNDER PROFESSOR RAUCH'S SUPERVISION at the ILL—Laboratory in Grenoble. Once this training is achieved, the experimentalists are ready for a continuation of the tests in the U.S.A.

The proposal submitted intends to achieve exactly this objective, with particular reference to the training of young U. S. experimentalists. The understanding is that the budget cannot be restricted to this purpose only, and a participation in the logistic expenses sustained by Austria and France must be contemplated.

The submission of a detailed budget, including itemization of all expenses and personnel (or the criteria for their identification) is contemplated for submission to DOE at some future time.

Very truly yours,

A handwritten signature in dark ink, appearing to read 'R. M. Santilli', written over a horizontal line.

Ruggero Maria Santilli
President, IBR
and Co-investigator

RMS/mlw

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RUGGERO MARIA SANTILLI
28 CROSS ST
WEST NEWTON MA 02165

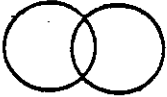
THIS MAILGRAM IS A CONFIRMATION COPY OF THE FOLLOWING MESSAGE:

TDMT NEWTON MA 11-14 1007P EST
INT PROFESSOR RAUCH
ATOMINSTITUT SCHUETTELSTRASSE 115
1020WIEN (AUSTRIA)
SUGGEST SENDING ME AUTHORIZATION BY TELEGRAM TO SUBMIT PROPOSAL
WITHOUT ZEILINGER SIGNATURE. POSSIBLE OFFICIAL SUPPORT BY US
GOVERNMENT MAY BE IMPORTANT TO RESOLVE GRENOBLE IMPASSE. BEST WISHES
FOR YOUR TRIP TO GRENOBLE
RUGGERO MARIA SANTILLI

COL 115 1020WIEN

22111 EST

MGMCOMP



- 1095 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

October 22, 1981

Office of the President

Dr. DAVID C. PEASLEE
Division of High Energy Physics
Physics Research Branch
DEPARTMENT OF ENERGY
Mail Statikn J-309
WASHINGTON, D.C. 20545

Dear Dr. Peaslee,

I enclose copy of the proposal
EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG
AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION
as per our phone conversation of October 19, 1981.

As you can see, the proposal has been signed by the Principal Investigator Professor H. RAUCH, as well as by the co-investigators Professor J. SUMMHAMMER and myself, but (regrettably), it has not been signed by an additional co-investigator recommended by Professor Rauch, namely, Dr. A. ZEILINGER (a member of the Atominstitut of Wien, currently spending the 1981-1982 academic year at M.I.T.).

As a result, please do not consider this letter a submission of the proposal.

While the M.I.T./Dr. Zeilinger case continues to be investigated, I would appreciate the courtesy of your recommendation on the following aspects.

- (1) As you know, the proposal is of international nature and the Principal Investigator (Professor Rauch) is the Director of the Atominstitut of Wien. According to DOE regulations, does Professor Rauch need an appointment at our Institute (the administrative conduit) to qualify as Principal Investigator?
- (2) Is my Social Security Number in the Proposal sufficient according to DOE regulations, or additional investigators must have the S.C.N.?
- (3) Is there any additional regulatory aspect we should be aware of in order to file the application in a proper way?

Your assistance in the finalization of the proposal would be sincerely appreciated. Thanking you in advance,

I remain, Yours Sincerely

Ruggero Maria Santilli

RMS-pm

cc.: Drs. Wallenmeyer and Hildebrand, DOE



- 1096 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

November 10, 1981

Dr. ROBERT L. THEWS
Division of High Energy Physics
Office of High Energy and Nuclear Physics
DEPARTMENT OF ENERGY
Mail Station J-309
WASHINGTON, D.C. 20545

RE: Conversation of October 5 regarding the possible submission to DOE of a research grant proposal entitled "EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION", with Principal Investigator Professor H. Rauch.

Dear Dr. Thews,

I would like to confirm our phone conversation of October 5 regarding the administrative requirements for our possible filing of the proposal, with particular reference to:

(1) Prof. Rauch Position. There is no need that Prof. Rauch acquires a U.S.A. Social Security Number because no funds of the proposal would be used for his personal salary. However, it is recommendable (if not necessary) that Professor Rauch has a formal appointment at our Institute. The appointments of Full Professors we are currently issuing are sufficient (I am referring here to our joint appointment as members of our Institute in a way compatible with existing academic appointments).

(2) Use of proceeds. You suggested that, in case known, the names of the persons who would receive the funds should be indicated in a letter, and that those persons should have an U.S. Social Security Number, if possible. I do not have this information. I shall therefore do my best that the information be released to you in case the application is founded.

(3) MIT Stall. As you know, the proposal has been stalled by the MIT suggested member, Dr. Zeilinger, and his superiors (see my letter to Dr. Zeilinger of October 29). As also indicated to your Office, there is no need that Dr. Zeilinger signs the proposal; or, in case his superiors will authorize him to do so, he is welcome to sign. The important point is that a formal written resolution on whether to sign or not to sign be reached as soon as possible. In fact, additional un-necessary and un-explained delays may give the impression that MIT might be interested in stalling the proposal, which I presume is not the case.

Owing to these (and a number of other circumstances), permit me the liberty of asking your friendly intervention.

I am suggesting here the possibility that you contact Dr. Zeilinger or his superiors (Dr. Shull, Head of the Nuclear Physics Division at MIT) and Dr. Feshback (Chairman of the Department of Physics at MIT), to the effect of suggesting a solicit resolution of the issue (Yes, Dr. Zeilinger will sign; or No, Dr. Zeilinger will not sign). Equivalently, in case of a continuation of the current status of lack of any decision, we would appreciate an indication of the reasons.

However, in case you think that this direct contact between your office and MIT is inappropriate at this time, please ignore my request. You can count on our best understanding.

Very Truly Yours



Ruggero Maria Santilli
President

RMS-pm

cc. Professor H. RAUCH. Atominstitut
Schuettelstrasse 115, A-1020 WIEN, Austria.



Department of Energy
Washington, D.C. 20545

January 3, 1982

Dr. Ruggero M. Santilli
President
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

This is in reply to your letter of December 11, 1982. As you probably surmise from the newspaper accounts, the Department of Energy's budgetary situation in Fiscal Year 1983 is somewhat confused. We are functioning under Continuing Resolution and expect to be under Continuing Resolution for the indefinite future. I very seriously doubt that any favorable action on your proposal will be possible. Certainly favorable action in early January does not appear to be at all likely.

Sincerely,

A handwritten signature in cursive script that reads "Enloe Ritter".

Enloe T. Ritter
Director
Division of Nuclear Physics

Dr. Wallenmeyer, Director
High energy physics Divisio
Division of high energy Physics
Department of Energy
WASHINGTON, D.C.

Tel. (301) 353 3367 —

I am happy to report formal authorization from LL-Laboratory
Grenoble to proceed test of spin symmetry under strong
interactions via a collaboration Austria-France-USA.
DOE application with Professor Rauch as principal
investigator is forthcoming.

Best Regards

Ruggero Maria Santilli, President
The Institute for Basic Research

Telegram mailed March 3, 1982



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

Office of the President

April 27, 1982

Dr. WILLIAM A. WALLENMEYER, Director
Division of High Energy Physics
DEPARTMENT OF ENERGY
Washington, D.C. 20545

Dear Dr. Wallenmeyer,

I hereby submit most respectfully the enclosed original and seven copies of the research grant proposal entitled

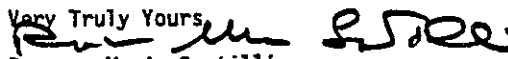
EXPERIMENTAL VERIFICATION OF THE $SU(2)$ -SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION,

under administration by our Institute, and with Principal Investigator Professor H. RAUCH, Director, Atominstitut der Österreichischen Universitäten, Wien, Austria, who is an undisputed experimental leader in the field of the proposal (neutron interferometry)

As you can see, the proposal has been made as brief as possible, thanks also to its experimental character. However, I would appreciate your consideration of the advisability for us to prepare a collection of experimental and theoretical articles in the problem, for referee's convenience. Please let us know whether or not we should prepare this collection of articles. Also, please keep in mind that the experiment could be started this summer, in case funded. We would therefore appreciate a speedy consideration of the proposal, of course, within the time schedule of your Office. The connection with the other proposal currently pending at your Office under administration by our Institute should be kept in mind. In fact, the proposal by Professors BENKART, MYUNG, OEHMKE, OSBORN, and TOMBER deals with the development of the basic mathematical tools to treat the deformation of the charge distribution of hadrons under strong interactions, as preliminarily detected by the available measures of this proposal.

Finally, permit me the liberty of recommending, if at all needed, that extreme care be exercised in the desired referees, with particular reference to their proved ethical standards. The proposal is for an open problem that is clearly at the foundation of contemporary physical knowledge. As such, individual referees might be tempted to discourage the conduction of the experiment in order to protect personal academic-financial interests, to the detriment of the true pursuit of novel human knowledge. At any rate, this proposal reaches your desk after years of documented opposition by a number of physicists who have been trying, whether openly or cryptically, to prevent the conduction of this fundamental experiment. This opposition apparently originates within circles of researchers financially and academically committed to the conjecture that quarks are the constituents of hadrons. In fact, the finalization of the current experimental measure by Professor RAUCH of a small (1%) deformation of the charge distribution of hadrons under strong interactions, could have rather profound, negative implications for the quark conjecture (even though the physical value of the unitary models of Mendelev-type classification of hadrons would be essentially untouched). I believe that it is in the best interest of DOE as well as of the international physics community that you are informed of this opposition, so that you can take the appropriate precautionary measures. Also, it appears appropriate, owing to the nature of the application and its international character, that the ethical profile be focused from the outset. Needless to say, we are confident that your Office will indeed meet all our expectations.

Very Truly Yours


Ruggero Maria Santilli
President

RMS-mlw, encls



Department of Energy
Washington, D.C. 20545
Mail Stop ER-23 GTN

JUN 11 1982

Professor R. M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

In Dr. Wallenmeyer's letter of June 7, 1982, he informed you that he was forwarding your proposal entitled "Experimental Verification of the SU(2)-Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration" to the Division of Nuclear Physics. The proposal is now under review in this Division, and you will be advised as soon as a decision has been reached.

We would appreciate knowing whether this proposal is being submitted to any other Federal agency or whether there are any other sources of Federal support.

If you should wish to inquire about the status of this proposal, please feel free to get in touch with me.

Sincerely,

A handwritten signature in cursive script that reads "Enloe T. Ritter".

Enloe T. Ritter, Director
Division of Nuclear Physics

cc:

H. Rauch, Atominstitut
J. Summhammer, Atominstitut
H. Willard, NSF

STRICTLY CONFIDENTIAL

June 21, 1982

Dear Dr. Ritter,

Permit me the liberty of recommending, most respectfully, that no referee for Prof. Rauch's proposal is selected from MIT, Harvard and other local institutions of the Boston area. The conflict of interest between the proposal and the research currently conducted at these institutions would then invalidate a fair referee process.

The recommendation is the result of a number of years of interference in the conduction of the studies underlying the test of the spin-symmetry (and, thus, of Einstein's special relativity) under strong interactions. Some of the episodes indicate such gross academic greed to be hardly believable. Yet, the existing documentation speaks for itself. At any rate, members of the research teams underlying the project have been forced in more than one occasion to hire attorneys. Therefore, we have been very close more than once to an open confrontation in court houses and news media. The fact that you did not read about these episodes in the Washington Post, or Paris Soir, or other newspapers, is the best evidence of my personal commitment to an orderly condition of our community.

Lately, I recommended the Division of High Energy Physics of DOE to abstain from contacting these institutions in regards to a primary research grant application for our new institute of research. Apparently, my recommendation was not followed and backfired considerably both internally in the local institutions (which, after all, do house ethically sound scholars), as well as externally (via new unbelievable extremes of open interferences). This created predictable, un-necessary aggravations. I have abstained from reporting these episodes to Drs. Wallenmeyer and Hildebrand as a form of respect for them, and for the difficulties of their work.

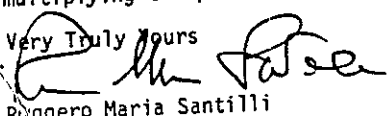
Therefore, I believe that DOE can be only damaged by contacting MIT, Harvard, and other similar institutions on fundamental experiments such as the test of spin/Einstein's relativity under strong interactions. After all, the existence at these institutions of vested, organized academic interests favoring the preservation as much as possible of old knowledge, is well known.

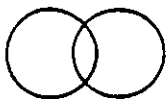
The pursuit of knowledge as well as of national interests for novel advancements is in the hands of governmental officers such as you. I am fully aware of the difficulties of this task. For this reason you should count on my best possible assistance and backing. As indicated by phone, permit me to beg you to contact me confidentially in case delicate situations arise during the consideration process. In fact, we can join forces to identify the smoothest possible way.

But, most of all, permit me to stress that: (a) the application is for experiments; (b) it is of truly fundamental physical nature; and (c) it can eventually result to be either in favor or against old knowledge. Physicists opposing the test on scientific grounds are therefore of clearly questionable ethical standards. In fact, why should they oppose tests that may eventually result to be in favor of their own views?

This latter question is at the foundation of the existing problems here in the Cambridge college community. The decision to organize our new institute of research was taken by a number of concerned scholars, businessmen, and observers precisely in favor of an orderly condition of our research. However, the consultation of the local institutions for IBR grant applications, such as the latest one to the Division of High Energy Physics might have done, undermine exactly these efforts, by therefore multiplying the possibilities of an open confrontation.

Very Truly Yours


Ruggero Maria Santilli



- 1103 -
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Professor Ruggero Maria Santilli, President

July 1, 1982

Dr. Enloe T. Ritter, Director
Division of Nuclear Physics
Department of Energy
Washington, D.C. 20545
Mail Stop ER-23 GTN

Dear Dr. Ritter,

As a gesture of courtesy I would like to pass to you information that recently became available to our Institute concerning the experiment by Slobodrian, Conzett, et al. on the violation on the time-reflection symmetry under strong interactions.

1. Assuming that they are correct, the four measures conducted by Hardekopf, et al at Los Alamos are not sufficient to establish the identity of the polarization of the forward reaction with the analyzing power of the backward reaction. This is according to a theoretical study conducted here at IBR. Copy of a diagram is enclosed for your information.
2. Professors Slobodrian and Conzett have found serious experimental reasons to doubt the validity of the four measures at Los Alamos. Copy of letters from Slobodrian to Veese are enclosed on a *confidential basis*. Experimentalists contacted by us have indicated that the apparent inconsistencies of the Los Alamos measures are truly sound.
3. The Québec-Berkeley experimental group has repeated again their measures and found values very close to the original ones. It appears that a communication by the experimentalists on these additional measures will be made publicly available in the near future.

In addition to the direct information, you should also keep in mind the considerable amount of indirect information supporting the violation of the time-reflection symmetry under strong interactions.

I am referring here, for instance, to:

- a. The available measure by Rauch's experimental team on the apparent deformation of the charge distribution of neutrons in the field of nuclei. As you know, the underlying rotational-asymmetry, if confirmed, will imply a necessary violation of the time symmetry.

Copy of a paper by Rauch is enclosed

- b. An increasing number of theoretical studies indicate the existence of new, rather substantial, problematic aspects in the relationship between the experimentally established macroscopic irreversibility and the conjectural particle reversibility. These problems were studied at our recent International Conference at Orléans [see for instance a paper by Tellez-Arenas]. It is clear that the best resolution of this historical problem is that along the experiment by Slobodrian, Conzett, et al.
- c. An additional array of problematic aspects is currently surfacing for a joint time-reversal symmetry combined with the established, broken space-reversal symmetry. I am referring to inconsistencies in the structure of the Special Theory of Relativity. After all, Einstein taught us the equivalence of space and time and Dirac has stressed since 1949 his expectation of a joint space-asymmetry and time-asymmetry.

I hope that this information is of some value to you. I shall continue to keep you informed of the most salient events in this important physical problem.

Wishing you a happy summer.

I remain,

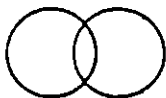
Very truly yours,

Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Drs. W. A. Wallenmeyer, B. Hilderbrand, R. Thews, DOE.
Drs. R. M. Sinclair, M. Bardon, and P. S. Rosen, NSF



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

Professor Ruggero Maria Santilli, President

July 7, 1982

Professor H. RAUCH, Director
Atominstiut der Osterreichischen Universitaeten
Schuettelstrasse 115
A-1020 WIEN, Austria

Dear Professor Rauch,

It is a pleasure to inform you that your application entitled
"Experimental verification of the SU(2)-spin symmetry under strong and electromagnetic interactions via a joint Austria-France-U.S.A.collaboration"
is under active consideration by the U.S. DEPARTMENT OF ENERGY, thanks to the interest of Dr. ENLOE T. RITTER, Director of the Division of Nuclear Physics, and following a kind referral to such division by Drs.W. WALLENMEYER and B. HILDEBRAND of the Division of High Energy Physics.

I would like to take this opportunity to inform you that your application will not be jointly considered by the U.S. NATIONAL SCIENCE FOUNDATION as a result of a sound judgment to avoid un-necessary duplication of Governmental efforts.

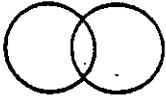
I have separately mailed to you more detailed information concerning the consideration of your proposal at DOE. Wishing you the best success in the funding of your important experiment, and in its actuation, I remain

Yours Very Truly

Ruggero Maria Santilli
President

RMS-mlw

cc.: Drs. E.T.RITTER, W.WALLENMEYER, and B. HILDEBRAND, DOE, and
Drs. R.M.SINCLAIR, M. BARON, and P.S. ROSEN, NSF
Board of Governors, IBR



- 1106 -

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

Professor Ruggero Maria Santilli, President

July 8, 1982

Dr. E.T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545

RE: Research proposal by Prof. H. RAUCH
"Experimental verification of the
SU(2)-spin symmetry"

Dear Dr. Ritter,

I enclose a self-explanatory letter to Professor Rauch in Wien indicating the status of his applications to DOE and NSF. As you can see, your consideration of the proposal is the only active one. The same proposal is not under consideration by any other Governmental, Corporate, or Private Institution. No additional submission of the proposal is contemplated in the future.

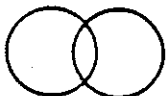
I am taking this opportunity to enclose a few lines prepared by members of our Institute regarding the possible applications of the hadronic mechanics to quark theories, QCD, and all that. As you can see, these possibilities are rather intriguing. Particularly relevant is the possibility of a genuine advancement in the vexing problem of confinement of quarks, which would become apparently possible if one differentiates the physical laws of the center of mass of a hadron in an external elm field (exterior problem) from possible generalized laws for the constituents (interior problem). As you know, the lack of achievement of quark confinement in a form truly credible, is one of the most delicate aspects of contemporary physics, scientifically and administratively.

Intriguingly, all the possibilities for quark theories under consideration are dependent rather crucially on the confirmation of the deformation of the charge distribution of hadrons under strong interactions, according to the proposal by Professor Rauch under consideration at your office. In fact, the measures would likely force the abandonment of conventional physical laws for the interior problem only, and set the way for more general laws. In turn, this would permit the attempt of genuinely novel advances in strong interactions, including conventional quark theories.

Best Personal Regards

Ruggero Maria Santilli
President

RMS-mlw



— 1107 —
THE INSTITUTE FOR BASIC RESEARCH
Harvard Grounds, 96 Prescott Street
Cambridge, Massachusetts 02138, tel. (617) 864 9859

Office of the President

September 22, 1982

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

Dear Professor Rauch,

I would like to inform you that I have recently visited Dr. Ritter of the Division of Nuclear Physics of NSF, which is currently considering your proposal. I am pleased to report that Dr. Ritter is genuinely interested in the project, and he is doing his very best. Apparently, half of the referee's reports have already been received, and the remaining half is expected in the near future. After that, a decision will be based on budgetary considerations in view of the following occurrences.

As you know, Reagan's policies have implied substantial limitations in the current budget for research. However, Congress is expected to act this coming fall on the research budget for next year. This latter decision includes the budget not only for DOE at large, but also the budget specifically for Dr. Ritter's division.

As a result of this situation, in case there are no funds available in the budget for the current year, it may be advantageous to allow Dr. Ritter to delay the decision until the budget for next year is known.

In the meantime, I am happy to inform you that we have received an extension of our own contract for theoretical studies. Please keep in mind that I can make available to you funds up to a maximum of \$1,500. My only restrictions are of administrative nature, in the sense that the funds should be dispersed in the same way as we did for you and Professor Eder. I am referring more specifically to: (a) consulting agreement with any person you recommend; (b) the release to us of subsequent publications in any journal [not necessarily the Hadronic Journal]; and (c) the nature of the research should be experimental or theoretical in strong interactions.

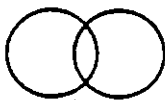
The contract has been legally executed and, therefore, the availability of the funds is guaranteed. However, please keep in mind that the actual release of funds is done one month after an individual request. Please feel free to phone me at any time at your convenience at home [(671) 964 1684] or at the office for the use of these funds.

Best regards,

A handwritten signature in black ink, appearing to read 'Ruggero'.

Ruggero M. Santilli
President

RMS/mlw



I. B. ¹¹⁰⁹ R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

October 22, 1982

RE: Application: EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-FRANCE-USA COLLABORATION

Principal Investigator: Professor H. Rauch

Submitted to the Nuclear Physics Division of DOE on June 21, 1982

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
WASHINGTON, D.C. 20545
Mail Stop ER-23 GTN

Dear Dr. Ritter,

I have attempted to call you twice this week because I have good scientific news. In fact, the Berkeley-Québec-Bonn experimental group has repeated the measures on time-asymmetry in nuclear physics and confirmed the original findings, contrary to the disclaim by the Los Alamos group. A copy of their recent paper is enclosed.

This result renders much urgent, if at all needed, the repetition of the tests on spin symmetry by Professor Rauch according to the pending proposal. In fact, the measures indicate quite clearly that the origin of the time-asymmetry is in the spin component, that is, in the spin part of the time-reversal operator

$$\tau = e^{i\pi J_2} \mathcal{C}$$

Again, the theoretical interpretation is so natural, to be trivial. It is given by the possible deformation of the charge distribution of nucleons in the conditions of the reactions considered, with consequential loss of rotational symmetry.

Regrettably, I have also to report an escalation of academic opposition against the experimental-theoretical study of this fundamental physical problem. This opposition has taken the form of a blatant misuse of academic authority, with manifest misconducts in refereeing processes. Therefore, it has been necessary to force the issue of ethics in academia, by taking several actions, including formal requests of resignation of academicians holding editorial posts at the Journals of the American Physical Society.

I can personally testify to the DOE efforts and successes in avoiding academic pressures of doubtful ethical motivations. In regards to the current consideration of the proposal on the test of the rotational symmetry, we fear the possibility that your office might be exposed to outside pressures by corrupt academicians interested in preventing the acquisition of experimental knowledge on the possible deformation of the charge distribution of hadrons for the perpetration of personal gains, in disrespect of the most elemental National and human values. In order to prevent even the vague feelings of possibilities of this type, as well as to permit the smoothest possible conduction of the examination, it is evident that our direct communications are of utmost importance.

But, most of all, we should always keep in mind the true values here. In fact, there is absolutely no doubt whatsoever that the proposal to test the rotational symmetry is, by far, the most important and fundamental project on your desk at this time.

Very Truly Yours

Ruggero M. Santilli
President

RMS--mlw

encls.



I. B. R. - 1111 -

THE INSTITUTE FOR BASIC RESEARCH

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

December 11, 1982

Dr. ENLOE T. RITTER
Division of Nuclear Physics
Department of Energy
Washington, D.C.

RE: I.B.R. application entitled
"Experimental verification of the
SU(2)-spin symmetry"

Dear Dr. Ritter,

I would appreciate the courtesy of your consideration whether a decision on the application can be reached in early January 1983.

Such a consideration appears recommendable on account of a number of aspects, some of which reported to Drs. Wallermeyer, Hildebrand, and Thews at the DOE Division of High Energy Physics, and others that are specific for your case.

In particular, in case you foresee possibilities for funding, kindly consider the suggestion of my recent letter to you of November 18, 1982, concerning the possible signature of a contract with initiation of support at some subsequent, postdated, specified time.

However, if, for any reason, you are not in a position to reach a decision in early January 1983, you can count on my full understanding. In this case, I would be grateful for the mere indication of the time projection for a possible decision.

I would like to take this opportunity to express to you and to your family our best and sincere wishes for a happy holiday.

Yours, Sincerely

Ruggers

Ruggiero M. Santilli



Department of Energy
Washington, D.C. 20545
Mail Stop ER-23 GTN

November 12, 1982

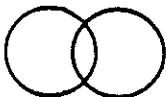
Dr. Ruggero M. Santilli
President
Institute for Basic Research
96 Prescott Street
Cambridge, MA 02138

Dear Dr. Santilli:

The Division of Nuclear Physics has now had your proposal "Experimental Verification of the $SU(2)$ -Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration" under review for six months. Unless you have an objection, we would like to retain this proposal under active consideration for another six months. Please contact me if you have questions or wish to discuss this action.

Sincerely,

Enloe T. Ritter
Director
Division of Nuclear Physics



- 1113 -
I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

November 18, 1982

Dr. ENLOE T. RITTER
Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY
Mail Stop ER-23 GTN
WASHINGTON, D.C.

RE: Research grant proposal No. P8206041 entitled
EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYMMETRY UNDER
STRONG AND ELECTROMAGNETIC INTERACTIONS BY A JOINT AUSTRIA-
FRANCE-USA COLLABORATION
with Principal Investigator Professor H. RAUCH

Dear Dr. Ritter,

We would like to express our appreciation and approval for your decision to retain the proposal under active consideration for another six months, as expressed in your letter of November 12, 1982. We are fully aware of your current difficulties created by the lack of normal operations under a formal budget. You can, therefore, count on our full understanding, cooperation, and backing.

Permit me to take this opportunity to bring you updated on scientific developments which are relevant to the consideration of the proposal.

FORMAL APPROVAL AT THE ILL-LABORATORY. You will be pleased to know that the Laue-Langevin Laboratory has formally approved the repetition of the experiment, and authorized Professor Rauch to proceed whenever the availability of funds will allow him to do so. Copy of the formal communication is enclosed. Note that the approval implies also the confirmation of the cost sharing of the experiment as far as the French part is concerned. The Austrian cost sharing is ensured by the Principal Investigator, as Director of the Atominstut in Wien.

The ILL approval is the result of a rather extensive investigation by a subcommittee in nuclear physics under the personal supervision of the Director, Professor Springer. The subcommittee was headed by Professor Schultz of Julich, West Germany, and included Professors Specht, Leroux, Vin Mau, Sandars, Shotton, Faust, and others. The extensive nature of the investigation, and the personal supervision by Professor Springer, were suggested by rather unfortunate external interferences that apparently caused a halt of the process.

Quite regrettably, this occurred just prior to our First International Conference on Nonpotential Interactions, held under support from the French Government (and DOE) at the Université d'Orléans, France, in January, 1982. The problem of the possible deformation of the charge distribution was a central topic of the conference and, of course, Professor Rauch was one of the central invited speakers.

I am sure you will share our pleasure in seeing a smooth and orderly resolution of a case which had reached at times quite considerable tensions.

CONFERENCES AND WORKSHOPS. The *First International Conference on Nonpotential Interactions* resulted in four volumes of proceedings for over 2,000 pages, and was attended by numerous mathematicians, theoreticians, and experimentalists including formal convoys from the JINF of Dubna, U.S.S.R., and the University of Peking, China (see enclosures).

Two workshops have now been planned for summer, 1983. Professor Rauch's experiment is fundamental for both. In fact, we shall have the *FIRST WORKSHOP ON HADRONIC MECHANICS* in afternoon sessions on August 2-7, 1983 (see the enclosed preliminary announcement). Jointly, during morning sessions, we shall have the *FIFTH WORKSHOP ON LIE-ADMISSIBLE FORMULATIONS* of pure mathematical character. The former workshop deals with a generalization of quantum mechanics based on the possible deformation of charge distributions, while the second deals with the novel mathematical theories needed to represent the phenomenon (isotopies and genotopies of Lie algebras).

SCIENTIFIC IMPLICATIONS OF THE PROPOSED EXPERIMENTS. We have reported to you earlier some of the implications illustrating the truly fundamental character of the experiments. Here are a few additional comments.

(A) **ORIGIN OF THE IRREVERSIBILITY OF NATURE.** As you know, the measures by Conzett, Slobodrian et al. indicating a possible violation of the time-reversal symmetry in open (non-conservative) nuclear reactions have been recently confirmed, and a new paper has been submitted for publication. The experimenters insist that the violation occurs in the spin component of the nuclear forces.

Rauch's experiment on a possible deformation of the charge distribution of nucleons under external strong interactions is therefore a rather forceful indirect verification of the Conzett-Slobodrian experiment.

Regrettably, the case is afflicted by a number of prejudices. For instance, few physicists have done actual calculations to prove, as we do in the hadronic mechanics, that the time-asymmetry specifically applies to OPEN nonconservative nuclear reactions (e.g., when the target is fixed and external), and that it disappears in the reformulation of the setting into a closed form inclusive of the external target. In fact, the center-of-mass trajectory of any system isolated from the rest of the universe, is expected to be time-reversal-invariant. Also, few physicists have studied the compatibility of the time-asymmetry in open nuclear reactions with the apparent lack of time-asymmetry in spontaneous decays of hadrons via semileptonic processes.

These considerations are important because they also apply to the deformation of the charge symmetry of hadrons. In fact, the rotational-asymmetry measured by Rauch, and which should be confirmed or denied by the proposed experiments, is also for neutrons in the EXTERNAL field of the nuclei of the fixed target. Again, if one passes to the formulation of the interaction into a closed form inclusive of the target, we do not see interest in studying the problem of the charge symmetry, the system being isolated.

(B) CONSTRUCTION OF HADRONIC MECHANICS. As you know, the deformation of the charge distribution of hadrons does not appear to be describable via conventional quantum mechanics, owing to the intrinsically point-like representation of the particles and of their constituents.

Rauch's experiment is at the foundation of the current efforts to attempt a generalization of quantum mechanics into a covering discipline called Hadronic Mechanics.

The main idea is to generalize the structure of the Hilbert space via the so-called modular and bimodular isotopies (i.e., generalizations of the associative product, from the conventional form AB to the isotopic form $A \circ B = ATB$ where T is a suitable operator).

As shown by Professor Eder (also of the Atominstitut in Wien), the covering mechanics is capable of predicting the deformation of the charge distribution as measured by Professor Rauch, while preserving the conventional values of the magnitude and third component of spin. A separate research grant proposal, based on the experiments by Professor Rauch, Conzett-Slobodrian, et al, is currently under consideration by the High Energy Physics Division of DOE for the construction of the hadronic mechanics. I am confident that Drs. Wallenmeyer, Hildebrand, and Thews will cooperate with you for any additional information you may desire.

(C) APPLICATIONS TO NUCLEAR PHYSICS. You will be pleased to know that a host of new possibilities have been already stimulated by Professor Rauch's experiments. For instance, Professor Eder has shown how the anomalous character of the nuclear magnetic moments can be interpreted quite naturally by a conceivable deformation of the charge distribution in the interior conditions only.

The idea is not new, and, regrettably, its study was abandoned for apparent reasons of academic politics. For instance, Blatt and Weisskopf state quite clearly in their *Theoretical Nuclear Physics* (p.31) that "it is possible that the intrinsic magnetism of a nucleon is different when it is in close proximity to another nucleon." (see enclosure)

Numerous additional insights are coming out, all valuable for nuclear physics. I am referring here to the apparent possibility to suppress the atomic spectrum of energy, in order to resolve the vexing problem of lack of excited states in the deuteron, and several other developments.

(D) GENERALIZATION OF GALILEI'S RELATIVITY. As you know, conventional, classical and quantum mechanical relativities (Galilei's and Einstein's) apply to closed systems of point-like particles with only potential internal forces. However, inspection of nature soon reveals the existence of classical systems which are closed in the conventional sense (of verifying usual total conservation laws), yet the internal forces are of nonpotential/non-Hamiltonian type (think of our Earth as seen from an outside observer).

You will be pleased to know that I have recently completed my studies for a possible classical generalization of Galilei's relativity for closed non-Hamiltonian systems. The generalized relativity is presented in my Volume II of *Foundations of Theoretical Mechanics* entitled *Birkhoffian Generalization of Hamiltonian Mechanics*, which is just about to be released by Springer-Verlag. Incidentally, the monograph establishes the classical foundations of the rotational-asymmetry.

I am now feverishly working at the expected, corresponding generalization of the quantum mechanical Galilei's relativity via the structurally more general foundations of the hadronic mechanics.

Needless to say, Professor Rauch's experiment is absolutely fundamental for this task, evidently, because the rotational symmetry is at the foundations of all relativities. Note that a corresponding generalization of Einstein's special relativity is expected to be mandatory for the interior strong problem (only).

(E) CONSTRUCTION OF A NONPOTENTIAL SCATTERING THEORY. As you know, all contemporary elaboration of scattering experiments is done via the use of a theory which is strictly conceived for "potential scattering" that is, for point-like abstractions of hadrons or of their constituents.

A group of scientists headed by Professor Mignani (Univ. of Rome, Italy) is constructing a generalization of the theory which is called of nonpotential type mainly for its classical image while, technically, the theory is based on the isotopic lifting of the Hilbert space. In particular, the researchers have shown that THE REPRESENTATION OF HADRONS AS EXTENDED PARTICLES MAY IMPLY A CHARGE IN THE CROSS SECTION, that is, the possibility of reviewing the data elaboration of current experiments in high energy physics in which particles reach the conditions of mutual prenatation of their charge distributions.

The experimental information at the foundation of Mignani's nonpotential scattering theory is, again, Rauch's measure of deformation of the charge distribution, and the consequential time-asymmetry by Conzett and Slobodrian. The possible administrative implications alone are staggering.

I hope that this brief and nontechnical outline gives you an idea of the importance of Rauch's experiment, which is evidently such to dwarf any other study of minute incremental nature.

I also hope this may give you an idea why past academic interferences in this case have forced very moderate and peaceful scientists at the very edge of clamorous public gestures. In fact, academic interferences are, in this instance, too much beyond what a normal, ethically sound physicist can tolerate.

POSSIBLE POST-DATING OF CONTRACT. In summary,

- Professor Rauch is in a position to repeat the experiment at the ILL-Laboratory at any time now;
- Here, at the IBR, we are ready to hire a U.S.A. experimentalist to be trained by Professor Rauch at the ILL-Laboratory for his subsequent, independent continuation of studies in the States, as pointed out in the existing proposal, and our additional elaboration dated June 16, 1982; and
- Numerous scientific developments are under way, all crucially dependent on the funding of the proposed experiment.

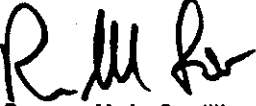
We are aware that current regulations do not permit back-dating of DOE contracts. However, we understand that current regulations permit the POST-dating of contracts. We are referring to the possible signature of contracts for initiation of funding at some specified subsequent date.

Please consider this possibility. In fact, we have here the capability of financing Professor Rauch's experiments, of course, after signature of a contract. In particular, we would have no problem in financing the experiment even for delays of the order of six months (and even possibly more)

in the reimbursement by DOE. The necessary pre-requisite for such a financing is, of course, the signature of the contract.

I do not know whether this possibility makes administrative sense. I pass it to you as an expression of our sincere desire to collaborate.

Best Personal Regards,



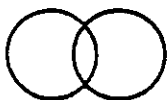
Ruggero Maria Santilli
President

RMS/mlw

Enclosures

cc: Drs. W. WALLENMEYER, B. HILDEBRAND, and R. THEWS, DOE

- ENCLS.
- 1 - Copy of ILL—authorization.
 - 2 - Table of Contents of Proceedings of Orléan Conference.
 - 3 - List of participants of the Orléans Conference.
 - 4 - Front Page of new paper by Slobodrian et al.
 - 5 - Quotation from Blatt and Weisskopf
"Theoretical Nuclear Physics".
 - 6 - Outline of "Birkhoffian Generalization of Hamiltonian Mechanics".
 - 7 - Announcement of First Workshop on Hadronic Mechanics.



I. B. R.

THE INSTITUTE FOR BASIC RESEARCH

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

December 11, 1982

Dr. ENLOE T. RITTER
Division of Nuclear Physics
Department of Energy
Washington, D.C.

RE: I.B.R. application entitled
"Experimental verification of the
SU(2)-spin symmetry"

Dear Dr. Ritter,

I would appreciate the courtesy of your consideration whether a decision on the application can be reached in early January 1983.

Such a consideration appears recommendable on account of a number of aspects, some of which reported to Drs. Wallenmeyer, Hildebrand, and Thews at the DOE Division of High Energy Physics, and others that are specific for your case.

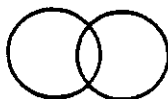
In particular, in case you foresee possibilities for funding, kindly consider the suggestion of my recent letter to you of November 18, 1982, concerning the possible signature of a contract with initiation of support at some subsequent, postdated, specified time.

However, if, for any reason, you are not in a position to reach a decision in early January 1983, you can count on my full understanding. In this case, I would be grateful for the mere indication of the time projection for a possible decision.

I would like to take this opportunity to express to you and to your family our best and sincere wishes for a happy holiday.

Yours, Sincerely

Ruggero M. Santilli



I. B. - 1119 - R.

CERTIFIED MAIL

THE INSTITUTE FOR BASIC RESEARCH

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

January 15, 1983

Ruggero Maria Santilli, Professor of Theoretical Physics and President

Dr. ENLOE T. RITTER, Director
Division of Nuclear Physics
DEPARTMENT OF ENERGY - GIN
Washington, D.C. 20545

RE: Research grant proposal number P8206041 entitled
"EXPERIMENTAL VERIFICATION OF THE SU(2)-SPIN SYM-
METRY UNDER STRONG AND ELECTROMAGNETIC INTERACTIONS
BY A JOINT AUSTRIA-FRANCE-U.S.A. COLLABORATION"
Principal Investigator: Prof. H. Rauch (Austria)

Dear Dr. Ritter,

I feel obliged to convey my distress because of your letter of January 3, 1983 concerning your inability to fund the fundamental test of spin by H. Rauch et al. I feel also distressed by my inability to reach you by phone.

On my part, I would like to confirm what indicated earlier, that you can count on my best possible assistance to facilitate your task. For instance, I can provide my services to see whether the current budget of \$94,900 can be slashed in half by eliminating deferrable items, such as the hiring of a U.S. experimentalist to be trained by Rauch for future continuation of the experiments in the States. The understanding is that, since the amount requested is already unusually small for an experiment, I cannot cooperate for reduction of costs below a level of decency vis-a-vis the expenditures supported by Austria and France.

On your part, you should consider dismissing any connection of this case with the Continuing Resolution owing to the extremely minute amount of funds involved, particularly when compared to the degree of scientific accountability vis-a-vis the taxpayers that is at stake here. In fact, as you know well, your office is spending large public funds in nuclear physics in projects based on the mere belief of the exact validity of the rotational symmetry. Statements to the contrary should be taken with extreme care because they might be motivated by the intention to manipulate basic physical knowledge in the benefit of vested academic-ethnic interests. In fact, the idea that the extended charge distributions of nucleons are perfectly rigid has no scientific basis. After all, Rauch's current measures, as you also know well, show deviations from the exact symmetry quite clearly, and this renders the repetition of the experiments simply unprocrastinable.

You should be fully aware that the experimental test of spin in nuclear physics is, as it must be, a breaking point, and, as such, it can provoke a host of undesired problems ranging from class actions to the solicitation of Governmental investigations. In fact, no ethically sound scholar can silently accept the scientific, economic and military implications caused by the indefinite deferral of the test.

The rotational symmetry is at the foundation of contemporary physical knowledge. The suppression of its direct verification which has been successfully achieved until now by vested, organized, academic-ethnic interests, has all the ingredients of a scientific crime against this beautiful Land, against our children who have to live on it, and against the pursuit of novel human knowledge.

Very Truly Yours

R. M. Santilli

Ruggero Maria Santilli

TO BE MAILED TO: Dr. G. KEYWORTH II, Science Adviser, The White House
Drs. D. P. HODEL, Secretary, and S. BREWER, Assistance Secretary, DOE

March 31, 1983

CONFIDENTIAL

Dr. E.T. RITTER
DOE

Dear Dr. Ritter,

As you know, my distress letter to you of January 15, 1983 was the result of my inability to reach you by phone. It is perhaps in the DOE and your best interest that I disclose the reason of the distress.

It is due to rumors circulating by the end of 1982 that Drs. C.G. SHULL and A. ZEILINGER of the MIT nuclear physics division were running, under financial support by your division, the fundamental test of the spherical symmetry of the charge distribution of neutrons under intense external fields, via neutron interferometers.

As you know, this test is precisely the test that H. RAUCH has been conducting since 1975 and which is the subject of Rauch's experimental proposal under consideration by your office. Also, as you eventually know, Zeilinger is a former associate of Rauch.

The reason for the distress is that the conduction of the experiment had been recommended by our group to Dr. Shull since the time I was at Harvard, back in 1978. In fact, Dr. Shull laboratory has all the equipment for the conduction of the test in a few months. The test had also been recommended to the various salient physicists at MIT. Regrettably, despite our most respectful possible attitude, and despite our sincere desire to establish a scientific collaboration, our proposal met with an unprecedented disinterest, opposition, and interference from the part of MIT people, apparently, because we indicated the possibility of a deformation of the charge distribution (which is manifestly against the vested MIT interests).

The rumors that MIT was finally running Rauch's experiment, combined with the history of MIT opposition, then resulted in our distress. In fact, I personally do not believe that MIT can run such experiment without scientific manipulations. I am one of a few who admit it openly. However, the number of people who share this view is considerable, and growing.

Very regrettably, rather than decreasing, the rumors of MIT running Rauch's experiment under DOE support are increasing.

I beg you to clarify the situation in the best interest of all. A phone conversation on the topic in early January would have prevented our distress. Your reassurance now that you are unaware of the occurrence would be invaluable to prevent further deteriorations in a situation that MIT people have already brought to rather extremes of tension.

Sincerely,


Ruggero M. Santilli

cc. Dr. B. HILDEBRAND, DOE (only copy)

P.S. A number of theoretical articles dealing with the generalization of the theory of rotations for deformed charge distributions have appeared at the I.B.R. or are under finalization. In case you are interested in inspecting them because of possible usefulness for Rauch's proposal, please let me know. Thank you.



- 1121 -

Department of Energy
Washington, D.C. 20545

JUL 25 1983

Professor R. M. Santilli
President
The Institute for Basic Research
Harvard Grounds, 96 Prescott Street
Cambridge, MA 02138

Dear Professor Santilli:

The Division of Nuclear Physics has completed its review of the research proposal entitled "Experimental Verification of the $SU(2)$ -Spin Symmetry Under Strong and Electromagnetic Interactions by a Joint Austria-France-USA Collaboration."

We regret to advise you that we cannot support this research proposal.

Your interest in submitting this proposal to the Department of Energy is appreciated.

Sincerely,

Enloe T. Ritter
Director
Division of Nuclear Physics

cc:
H. Rauch
J. Summhammer
NSF, Harvey Willard

PART XXXIV:

LACK OF

CONSIDERATION BY THE

NATIONAL SCIENCE

FOUNDATION

OF AN I.B.R.

COMPREHENSIVE

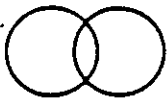
EXPERIMENTAL—THEORETICAL—MATHEMATICAL

PROPOSAL TO TEST EINSTEIN'S

SPECIAL

RELATIVITY

UNDER STRONG INTERACTIONS



I. B. R. - 1123 -

THE INSTITUTE FOR BASIC RESEARCH

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

Ruggero Maria Santilli, Professor of Theoretical Physics and President

May 11, 1983

Dr. E. F. INFANTE, Division Director
Mathematical and Computer Sciences
NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Dear Dr. Infante,

Please accept the sentiments of our sincere gratitude for the courtesy of your phone call this afternoon. The possibility of an informal discussion with you has been simply invaluable for us.

After due consideration, we believe that the most appropriate form of consideration of the research conducted at our institute is a collective form incorporating our experimental, theoretical, and mathematical research.

For this reason, we have abstained from recommending to IBR principal investigators of NSF applications to apply for a reconsideration of their proposals. In fact, owing to the novelty of our program and other factors, the reviewers of mathematical proposals are not expected to reach full maturity of judgment without a joint inspection of the physical studies. Similarly, the reviewers of our physical applications may have major deficiencies in judgment without an inspection of the experimental status of the research, as well as of the underlying mathematical studies.

To our understanding, a consideration of this type goes beyond the scope of each individual NSF Division. We would therefore appreciate the courtesy of your bringing the case to the attention of the appropriate NSF Officer suitable for a joint consideration of our experimental, theoretical, and mathematical program. For this purpose, I enclose a preliminary documentation for advance consultation with tentative title:

EXPERIMENTAL, THEORETICAL, AND MATHEMATICAL STUDIES ON A CONCEIVABLE GENERALIZATION OF THE SPECIAL RELATIVITY FOR EXTENDED AND DEFORMABLE STRONGLY INTERACTING PARTICLES.

This documentation essentially consists of the combination of the following five deeply inter-related proposals previously submitted to the NSF Divisions of Physics and Mathematics in an independent way.

- Experimental Part:** Experimental Verification of the SU(2)-Spin Symmetry Under Strong and Electromagnetic Interactions via a joint Austria-France-U.S.A. collaboration.
Principal Investigator: H. Rauch, Professor of Physics and Director, Atominstitut, Wien, Austria
- Theoretical Part:** Studies on Hadronic Mechanics
Principal Investigator and Coordinator: R. M. Santilli, Professor of Physics and President, IBR

Studies of Nonpotential Scattering Theory

Principal Investigator: R. Mignani, Associate Professor of Physics, University of Rome, Italy, and Professor of Physics, IBR

Mathematical Part: Studies on Lie-admissible Algebras

Principal Investigators: H. C. Myung, Professor of Mathematics, University of Northern Iowa and IBR; R. H. Oehmke, Professor of Mathematics, University of Iowa and IBR; and M. L. Tomber, Professor of Mathematics, Michigan State University and IBR

Mathematical Studies on Reductive Lie-admissible Algebras and H-Spaces with Applications to the Geometry of Nonpotential Dynamical Systems

Principal Investigators: A. A. Sagle, Professor of Mathematics, University of Hawaii and IBR; and J. P. Holmes, Associate Professor of Mathematics, Auburn University

Upon due consultation, kindly let us know:

- (a) the appropriate NSF office and officer where to submit the application;
- (b) the form of application which is most appropriate to our case; and
- (c) the necessary administrative guidelines to comply with, possible advice on the structure and size of the budget, and the minimal number of copies needed to apply.

The alternative forms of submissions appear to be the following.

I. **SUBMIT A GROUP PROPOSAL.** In this case, kindly let us know the restructuring of the enclosed advance presentation which is needed, besides the unification of the now separate budgets into one single form.

II. **APPLY FOR INSTITUTIONAL SUPPORT.** In case this latter form is preferable, kindly let us know the appropriate modifications of the enclosed presentation.

III. **OTHER ALTERNATIVES RECOMMENDED BY NSF.** In case the application for institutional support is recommended, NSF should be aware of a number of intriguing and promising possibilities.

Facilities. As you know, we have purchased a rather unique building, a charming Victorian within the compounds of Harvard University, known as The Prescott House. Our building has three floors suitable for the housing, in due time, of three divisions: one of pure mathematics, one of physics, and one of applied research.

Personnel. You should be aware that the position of Director of the IBR is vacant in the expectation of funding the Institute. According to our charter, the Director is in charge of all logistic operations, including the handling of IBR applications to Governmental Agencies, while the position of President is more similar to that of Chairman of the Board of a corporation. Since I am primarily interested in conducting research, I am rather eagerly waiting for the moment we can appoint the Director of our Institute. In case of an institutional support, we would be glad to fill up this position, as well as any needed additional one, in conformity with NSF regulations and recommendations.

Programs. In case of institutional support, you should keep in mind that the enclosed proposal would be a mere germ for future growth along lines set forth by National priorities and other NSF needs. To put it more explicitly, we would welcome the addition of any research program recom-

mended by NSF, even if completely independent from our current interests. Diversification of scientific inquiry is in fact a primary long term objective of the IBR.

Permit me to indicate that the current moment is rather unique in its gathering of qualified experimentalists, theoreticians, and mathematicians, or of novelty and unity of scientific thought. We are therefore firmly convinced that our group deserves a serious consideration by our peers. After all, our group can be easily dispersed via the prevention of financial support, but its possible future re-gathering would be difficult, assuming that it could be at all possible.

We do not believe that our research is the way the scientific community must necessarily follow, and we are not seeking passive acceptance by our peers. We are merely seeking constructively critical scientific interactions, mutual respect and consideration, particularly in those areas in which we share a common scientific accountability with our peers. The understanding is that the IBR has not been organized to clone the research conducted at other institutions, but rather to complement them with alternative avenues. In fact, we believe that America can be best served via a sufficiently diversified pursuit of basic advances, rather than research railed between preset narrow lines.

We believe that the past achievements of our group are sufficient to establish our credibility and qualification for further advances. In fact, during the first five years of activities, our group has produced:

- experimentally, rather forceful indications of the approximate character of the Lorentz symmetry in particle physics, in both its continuous and discrete parts, e.g., Rauch's measures of apparent deformation of neutrons in the intense fields of nuclei with consequential rotational-asymmetry; the measures by Slobodrian, Conzett et al on the apparent irreversibility of open nuclear reactions; and others;
- theoretically, we have accomplished a generalization of classical Hamiltonian mechanics into the so-called Birkhoffian mechanics; we have established the foundations of a generalization of Galilei's relativity for closed (isolated) systems of extended particles with contact/non-Hamiltonian internal forces; and we have identified the elements of a corresponding generalization of quantum mechanics (via isotopies and genotopies of the Hilbert space and enveloping algebras of operators) which appears capable of representing the experimental information indicated above; and, last but not least;
- mathematically, we have initiated two progressive and complementary generalizations of the very heart of contemporary mathematics, Lie's theory, called Lie-isotopic and Lie-admissible theories, which have solidly established themselves as the structural foundations of the generalized, classical and quantum mechanics indicated earlier.

As a gesture of consideration for your person, I have separately mailed to you complimentary copies of two recent monographs of our group published in 1983, that by Professor H. C. MYUNG on flexible Lie-admissible algebras (Hadronic Press), and my own monograph reviewing the construction of the Birkhoffian generalization of Hamiltonian mechanics (Springer-Verlag). I am sure you are aware of other monographs by members of our group and their advisors, such as that by Professor A. A. SAGLE on Lie algebras (Academic Press, 1973), or that by Professor G. EDER on nuclear forces (The MIT Press, 1968), or the two volumes on angular momentum by Professor L. C. BIEDENHARN (Addison-Wesley, 1981), and others.

During the first five years of operation, our group has also organized and conducted five international meetings emphasizing the interplay between experimentalists, theoreticians, and mathematicians, that resulted in the publication of some nine volumes of proceedings. Finally, our group has produced over 200 papers published in a considerable variety of mathematical and physical journals.

Ruggero Maria Santilli
President

Theoreticians: Professors [REDACTED] C. BIEDENHORN (Duke Univ.) R. BROUWER
[REDACTED]
University of California [REDACTED]
of Toronto, Canada, and [REDACTED] (SR),
[REDACTED]
[REDACTED]
[REDACTED]
[REDACTED]

Experimentalists: Professors [REDACTED]
[REDACTED] [REDACTED], [REDACTED], Australia,
[REDACTED]

RMS/mlw

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NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

May 20, 1983

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

Dear Mr. Santilli: *

I have received your letter of May 11, together with the "Preliminary Documentation for Advance Consultation".

As you requested, I am hereby forwarding a copy of your letter and the attached "Preliminary Documentation for Advance Consultation" to the Acting Assistant Director for Mathematical and Physical Sciences.

Please accept my gratitude in advance, for the two research monographs you have mailed to me, and which I look forward to receiving.

Sincerely yours,



E. F. Infante
Division Director
Mathematical and Computer Sciences

Preliminary Documentation for Advance Consultation

with the

NATIONAL SCIENCE FOUNDATION

on the submission of a collective proposal entitled

EXPERIMENTAL, THEORETICAL, AND MATHEMATICAL STUDIES ON A POSSIBLE
GENERALIZATION OF EINSTEIN'S SPECIAL RELATIVITY FOR EXTENDED, DEFORMABLE
STRONGLY INTERACTING PARTICLES

submitted by the Board of Governors

of

THE INSTITUTE FOR BASIC RESEARCH

96 Prescott Street

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- Paper 4: A. Tellez-Arenas, Short range interactions and irreversibility in statistical mechanics, Hadronic J. 5, 733 (1982), p. 29
- Paper 5: R. M. Santilli, A Possible Lie-admissible, time-asymmetric model for open nuclear reactions IBR preprint DE-82-9 (1982), submitted for publication, p. 38
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SUMMARY

Recent theoretical and experimental studies conducted at a variety of institutions (N. Bohr, Denmark; FERMILAB, Batavia, IL; Atominstitut, Austria; I.B.R., Cambridge, MA; et al) are providing increasing indications of apparent departures from the exact validity of the conventional Lorentz symmetry in a variety of sectors of physics, ranging from open nuclear reactions to unified gauge theories. The ultimate roots of the occurrence have been identified in the possibility that hadrons and their constituents, when represented as extended charge distributions, admit (generally small) deformations of their spherical symmetry under sufficiently intense external fields. This results, first, in a manifest rotational-asymmetry, and then in a consequential asymmetry of the Lorentz boosts, including a deformation of the light cone.

This application recommends the conduction of a comprehensive study of the issue with the following primary objectives: (1) Continuing the experimental measures via neutron interferometry of the apparent deformation of the charge distributions of hadrons under intense external fields; (2) Continuing the theoretical studies on a generalization of Einstein's special relativity specifically conceived for extended and, therefore, deformable particles, called Lorentz-Isotopic Relativity, of the related generalization of quantum mechanics called Hadronic Mechanics, as well as of a consequential generalization of the potential scattering theory called Nonpotential Scattering Theory; and, last but not least, (3) Continuing the studies on the generalized mathematical structures underlying the Lorentz-isotopic relativity and the hadronic mechanics, which have resulted to be generalizations of conventional formulations of Lie's theory called Lie-isotopic and Lie-admissible theories. The application also includes the study of several novel developments of quark theories, such as the construction of quarks as bound states of more elementary particles, the search for a strict confinement with identically null probability of tunnel effects of free quarks, possible generalization of spontaneous symmetry breakings via the isotopic liftings of the Hilbert space; etc.

The proposal is articulated into the following coordinated parts:

- Part I: Experiments. Principal Investigators and coordinators:
H. Rauch and J. Summhammer;
- Part II: Theoretical Studies: Principal Investigators and coordinators:
R. M. Santilli and R. Mignani; and
- Part III: Mathematical Studies: Principal Investigators and coordinators:
H. C. Myung, R. E. Oehmke, M. L. Tomber, A. A. Sagie, and
J. P. Holmes

The experiments are scheduled for conduction at the ILL-Laboratory in Grenoble, France via a France-Austria-U.S.A. collaboration. The theoretical and mathematical studies are scheduled for conduction at the I.B.R. as well as at a number of other institutions in the U.S.A., and abroad.

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