NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JAN 21 1986

Dr. Rainer Weiss
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Dr. Weiss:

I regret to inform you that the National Science Foundation is unable to support your proposal entitled "Detailed Engineering Design Study and Development and Testing of Components for a Laser Interferometer Gravitational Wave Observatory," PHY-8521626.

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 Foundation policy, I am enclosing copies of the reviews of your proposal (with identifying information removed). They are intended for your personal use only and we do not make them available to other parties. I sincerely hope these reviews will be useful to you in your future 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. If you have any questions, please contact Richard A. Isaacson, Program Director for Gravitational Physics, (202) 357-7979.

Sincerely yours,

Marcel Bardon

Marcel Bardon
Director, Division of Physics

Identical letter to:
Dr. Ronald W. P. Drever
Department of Physics
California Institute of Technology
Pasadena, California 91125

Enclosures

Copy to:
Paul H. Quinn, Associate Director
Office of Sponsored Programs

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1 (4/84)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
PHY-8521626	California Inst of Tech	10/31/85

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald W.P. Drever	GRAVITATIONAL PHYSICS

TITLE: Detailed Engineering Design Study and Development and Testing of Components for a Laser Interferometer Gravitational Wave Observatory

Please evaluate this proposal using the criteria presented on the back of this review form. Continue on additional sheet(s) as necessary.

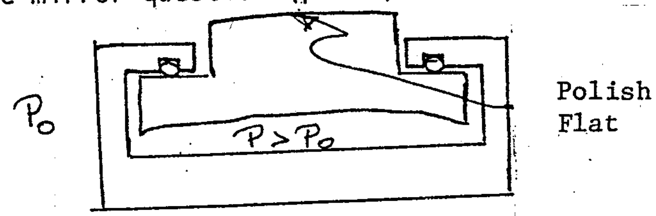
The two PI's are as individually creative as they are different in their personalities and approaches to science.

Experimental gravitational wave research is a terribly exciting new area of fundamental physics research - as exciting as it is technically difficult.

The proposal is extremely well documented.

Agree with comment (p. 13) that LIGO should be constructed as much as possible using established engineering practices ... and that the facilities should not themselves become a new experiment. Nevertheless the vacuum system may well present severe technical problems in its practical implementation.

Regarding the 4 Km focal length large concave mirror question (p. 19): a variant on the method Bernhard Schmidt used to make his first corrector plate might apply. The differential pressure ought to be controlable to the required precision.



Though I agree the ideal case is for the US "detector" to consist of a pair of gravity wave receiving systems, should economics require some retrenchment, the idea of a cooperative program with the European groups providing one of the two systems might be worth considering. A glance at the Max-Planck strain spectral sensitivity (Appendix D) together with the stated close Glasgow-Cal Tech collaboration would seem to suggest rather than rule out this option which appears to have been studiously avoided in the proposal.

I believe both MIT and Cal Tech need to commit one additional tenured position to this project as it grows. It appears at the moment that MIT is in a somewhat stronger staffing situation than is Cal Tech.

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

A

As one completely outside this science, I am reluctant to comment on the work. For what it is worth though, I do find the project scientifically very exciting - but it does seem overly expensive. 5M$ for just the design seems much too much. I compare it with the design costs of Fermilab (1965) and of the NSF financed Cornell 10 Gw Electron Synchrotron. Fermilab was, I think, ten times as complicated, maybe more, yet the design cost was comparable. The Cornell accelerator, also comparable, was essentially done out of pocket!

I think the difference is that accelerator designs essentially evolved - a big accelerator from a smaller accelerator. Hence one had pretty firm designs and design costs.

Here the project seems to be being done ab initio, from very little experience. For example, they seem to be going for 16^{-8} torr as the vacuum straight off although 10^{-5} torr would do for starters.

I would suggest a staged approach, that might cost one-tenth or one third in size and design goals. This might save considerably both in the design costs and more importantly in the cost of the ultimate observatory.

Overall Rating: Fair

B

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See Attached

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

C

The Principal Investigators, Ron Drever and Ray Weiss, both enjoy excellent reputations as experimentalists; they are extremely competent, imaginative, and ingenious in overcoming experimental obstacles. More particularly, I believe them to be the equal of any in the world in this field. I also have no doubt that the detection and analysis of gravitational waves will be of great significance for both astrophysics and physics. Detailed measurements of gravitational radiation will, for example, allow the probing of violent astrophysical events to a (physical) depth not achievable through any other known means. Will the proposed LIGO lead even to the detection of such radiation? No one can be sure; Nature could be far more cooperative or far less cooperative than our best estimates indicate. This project must therefore be approached in the true spirit of scientific adventure: our most talented scientists trying in a most difficult environment to wrest important secrets from Nature. In addition, the project will push technology in several areas of high-precision measurements which might have other applications.

Overall, the proposal seems very well prepared and clearly presented. As to its details, I have a few questions and comments: (1) Why was there no discussion of possible international collaboration? There are several competent groups in Europe developing similar instrumentation and, were a collaboration possible, the United States could realize a significant saving if it sponsored only one facility here, with the Europeans funding a "mate" in Europe; (2) I have concern that one year's study may well be too short to accomplish all the goals of the proposed engineering design, especially if the tests of the vacuum-system components imply the need for a change in this design. (The achievement and maintenance in the initial configuration of a pressure of 10^{-8} Torr may prove difficult to attain.); (3) The justification for the arms of the interferometer each being of four kilometers in length seemed a bit too qualitative. In view of the apparently large impact on cost of this length, a more detailed discussion would seem to be warranted; and, finally, (4) Why does the proposal (page 26) refer to, but not mention explicitly, which Principal Investigator would do what in regard to (i) day-to-day coordination with the Project Manager, and (ii) development of the plans for the receivers and experiments?

C1

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October 22, 1985

The proposed ultimate objective of designing, building and using a set of two or more gravitational wave detectors as an astronomical observatory is an exciting prospect. Such an observatory could yield much information, not obtainable in any other way, concerning stellar deaths, neutron stars and black holes. The two principal investigators are widely recognized to be among the most original and clever experimentalist in the world. The two institutions have strengths that are obvious. The two principal investigators have, over a period of years, developed the necessary techniques and it is time to move forward to a more complete engineering design of the instrument.

My principal worry about this program concerns the move directly to a full scale version of the instrument. For even the best experimentalists this is apt to lead to the over-design of some features of the instrument and the limitation of performance at some unexpected place. Owing to the weakness expected for the gravity wave signals, this move to a large and expensive instrument is necessary if it is to see anything interesting. In this connection, it should be recognized that the bulk of the cost is for a facility, a long vacuum tube that could be used to house some yet to be designed optical system. This flexibility is important. But the large anticipated cost is a concern. Some way of double checking the design costs prior to a final decision would be desirable.

OCT 28
DIV PH

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

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Please see attached sheets.

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

JAN

E

Review of proposal for Engineering study for LIGO

This proposal is for a $5,000,000 detailed engineering design for a $63,000,000 interferometric gravity wave sensor. The amount seems reasonable on a financial scale, but is not justified by the depth of technical details given in the porposal. The wisdom of building such a facility at all is based upon scientific arguments, including competition with other approaches to achieve the same goals, and is not at issue in these comments. I will comment only upon the substance and planning of the design study.

There are several basic questions to be addressed in such a proposed facility. The desire is that the interferometer be stable, only responding to the dimensional changes of the lengths of the arms as produced by gravitational wave deflections of the masses on the ends of the arms. There are several sources of false signals that need to be removed, principally in the area of seismic background. There are other areas of stability that must be considered regarding the stability of the interferometer itself, and the possibility of coupling erroneous signals into the system due to the intrinsic design of the interferometric cavity.

In summary, the concept appears to be to build pairs of resonant cavities whose length are determined by measurement of the optical path within the cavities. Much has been done on the demonstration of interferometric gravity telescopes at short arm lengths, up to about 40 meters. The proposed design is a scale up of a factor of 100 in arm length. This proposal does not contain a proposed design approach, but contains a smorgasboard of possibilities that would be looked at during the detailed design phase. No priority is given to the possible optical designs for the detector or the cavities. No consideration as to the size or construction of the large mirrors for the cavities is given. Neither is any estimate made of the effect of diffraction losses due to finite mirror size. The inherent stability of large optical and mechanical components for the design are critical, and no note is taken of this problem. (This reviewer has been involved in a 2.7 Km vacuum optical tunnel, so I am not saying the problems are unconquerable, I only feel that the proposers have not considered how the realities should be approached. This can lead to unstable cost growth.) In short, the design is starting at a very elementary level relative to the practical approach that is necessary to meet the goal.

The required sensitivity gain is about 100 times to get in the region of any events that are observable. A useful instrument requires a 1,000,000 times improvement. This proposal does not offer a clear path to be followed in getting to the first step. I expect that these goals can

E1

possibly be achieved, but the schedule calls for going full scale into a subcontract for the detailed design of the facility without the basic engineering and initial design being carried out and reviewed. This can lead to excessive cost growth as usually occurs when detailed design is not carried out against a specific goal.

The enthusiasm of the investigators is obviously present. However, before $5,000,000 dollars is committed, a solid and reviewed starting point for the device needs to be presented. Perhaps up to a million dollars will be required for this, which should be done before such details as site planning and vacuum system design take place. It is not clear to me what "component development" is intended, either. This should be spelled out.

In summary, if the development of a gravitational telescope of the form specified is a good scientific goal, further work toward a design should be carried out. However, a fundamental, basically engineered, starting point is needed before committing to the very expensive final design. I would estimate about a year to eighteen months, and something less than a million dollars would be well spent reaching this goal.

E2