NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

JUN 7 1985

✓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 "Feasibility and Design Studies for a Laser Interferometer Gravitational Wave Detection System (Physics)," PHY-8504837.

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 are not available to other parties. I 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. If you have any questions, please contact Richard Isaacson, Program Director for Gravitational Physics, (202) 357-7979.

Sincerely yours,

Marcel Bardon
Director, Division of Physics

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

Enclosures

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

PHY-8504837	California Institute of Technology	01/25/84

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE

Feasibility and Design Study for a Gravitational Radiation Detector (Physics)

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

Research Performance Competence

Professor Drever holds a distinguished record of instrumental innovation in the field of gravitational physics, as the many publications and the achievements of the Glasgow group indicate. Professor Weiss is not known to me personally and his publication record is not as extensive as that of Drever, although he has written two excellent reviews on the subject of gravitational detectors.

The proposed technical approach seems at present the most promising and is being developed at other institutions as described in the proposal. The broad frequency response of the system compared to accoustic bars must make it more suitable for the exploratory phases of a new discipline than detectors working at a preselected frequency. The very substantial technical problems in achieving sensitivities of the level sought cannot be considered fully exposed in the proposal; in particular, I feel that (i) the testing of the extremely long focal length optics will be difficult; (ii) the efficiency of the intermediate length interferometers for the rejection of local extraneous noise may not be straightforward to achieve; and (iii) the light recycling scheme may be quite difficult to implement. However, none of these novel technical advances will come without the stimulus generated by the design and construction of the large facility proposed.

I am favourably impressed by the organisational plan outlined in the proposal. The procedures for project control, scheduling and work package breakdown are sound. During the course of the design study a detailed breakdown of the scientific support roles for MIT and CALTECH should be attempted. There will be a great deal of overlap, necessarily, but there may arise specific topics requiring detailed study as the project develops and the project manager must have a clear understanding as to who is carrying out the work and on what schedule.

I rate the proposal as Excellent under Criterion 1.

Intrinsic Merit of the Research

Gravitational wave research has the potential to impact Astronomy, Astrophysics, Elementary Particle Physics, Field Theory and Cosmology as well as the study of Gravitation itself. The unification of the fundamental forces is in many ways held up by the unquantizable predicament of Gravitation, detection of gravitational quanta even in large numbers will provide a sounder stepping off point for theoretical extrapolation. The growth of high energy astrophysics, in particular the study of sources whose radiation is derived from a

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

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relativistic gravitational potential, provides a strong link to gravitational wave astronomy. Both disciplines will study important and complementary aspects of condensed matter. The number of recent publications predicting large gravitational wave fluxes from γ-ray and X-ray sources bears witness to the important position which should be occupied by this new astronomy once detectors routinely achieve the sensitivities suggested in this proposal.

(An unsolicited and unbiased comment on the importance of gravitational theory may be found in Nature Vol.312, p588, 13 Dec. 1984, top of middle column.)

I rate the proposal as Excellent under Criterion No.2.

Utility of Research

Undoubtedly the techniques developed to achieve the design goals proposed will find application elsewhere, one cannot predict where or when. In this regard I do not rate this proposal as especially different from other front ranking scientific endeavours (although it might be argued that the gravitational wave observatory will require an unusual share of ingenious solutions to the many technical problems evident at this stage).

I rate the proposal as Very Good under Criterion No.3

I do not feel competent to comment on the quality of the proposal under Criterion No.4, except to repeat the obvious, which is that the goals of this ambitious project should be relatively easily understood by interested non-scientists; it has therefore the potential of being a cultural as well as a technical and scientific milestone.

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PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE

Feasibility and Design Study for a Gravitational Radiation Detector (Physics)

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

The detection of gravitational radiation has been a goal of many groups since the first experiments of Joe Weber. I have several reservations with the proposal for a Feasibility Study for a Gravitational Wave Detector. These reservations fall into two broad categories:

1. The estimates of the gravitational wave flux reaching the earth from any type of source are still quite uncertain. For each possible type of source, the uncertainty comes from our ignorance of some parameter of the generator. The uncertainty in the flux from a pulsar, for example, arises from our ignorance of the ellipticity. The detection of gravity waves from a pulsar would, in first order, therefore simply fix the ellipticity. Likewise, the strength of a gravitational wave from a supernova depends on the shape of the core -- in fact on the deviation of the shape from spherical symmetry. For other generators, the energy and the detailed shape of the waveform depend in a complicated way on the asymmetry of the system. It is not obvious that any proposed experiment will have enough signal to noise to permit an inversion of the shape of the pulse to determine the structure that produced it. Even if such an inversion were possible, it is not clear to me exactly what the astrophysical significance of a determination of such a mechanical quantity would be. Will a knowledge of the ellipticity of a pulsar affect our understanding of its dynamics? I am not competent to answer that question, and I

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

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therefore rely on the justification in the proposal. I do not find any such justification. Indeed, the uncertainty in the magnitude of the parameter that determines the emitted gravity wave power may simply reflect the fact that the dynamics of the system are not very sensitive to its value and hence that it is not very important.

A second justification for the effort suggests that the detection of gravitational waves from the coalescence of a neutron-star binary, for example, is a useful result in and of itself. What would such a result tell us? That such events exist? That gravitational waves exist? Are there any theories which are otherwise viable which would be ruled out as a result of these experiments? Conversely, if gravitational waves were not detected, would that signify anything except that the sources were weaker or more symmetric than was at first supposed?

A third justification suggests that gravitational waves will open a "new window" just as radio astronomy did. Although this is certainly possible, I do not find the argument compelling. The various flavors of electromagnetic astronomy (optical, x-ray, radio, etc.) originate from regions having very different temperatures and levels of excitation, and different sources (or different parts of the same source) might only be observable using one or another of these methods. Gravitational radiation, on the other

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hand, arises from large scale dynamical anisotropies. While such effects are no doubt interesting, I find no reason to believe that they are first-order effects whose understanding is crucial to stellar dynamics.

I find a discussion of these sorts of issues almost totally absent from the proposal. Instead I find a catalogue of things that might be done given a receiver of a certain sensitivity without any justification for why such measurements are worth $50 M. I am not suggesting that there may not be any good reasons for detecting gravitational radiation, but I find the discussion in the proposal totally inadequate to the task of convincing me that it is worth the money and effort that are being expended.

2. I found it very difficult to evaluate the technical part of the proposal for the following reason:

It is obvious that neither Drever nor Weiss is yet in a position to design a complete system. They are therefore trying to build in as much flexibility as possible both into the mechanical design and into what they propose to do. Although this may be an inevitable consequence of the state of the program, it provides almost nothing for a referee to grab on to. The proposed effort has so many bifurcations that it is difficult to have a clear picture of what is planned. For example, how many of the ideas proposed on pp. 33, ff. will actually be tried? How can I deal with a lengthy catalogue preceded by (p. 33), "... their promise of enhancing the large antenna performances is sufficiently great that SOME (my emphasis) of them will be the subject of major research efforts in the next few years." Which ones? How can I evaluate either the level of effort or the budget without knowing this?

The proposed antennas seem to me to be non-trivial to build from a purely engineering standpoint. I realize that many of the issues that concern me are presumably answered in the suitably thick engineering studies, but a short discussion of the following points would have been useful:

a. How will the system be cooled?

b. How will the pipes be checked for leaks?

c. How can the system ever reach a pressure of 1E-8 without bakeout? If bakeout is planned, how will it be done?

Although a detailed discussion of the antenna design is perhaps more appropriate in the review of the individual proposals, it is clear that a considerable amount of development work must be done in many different areas,

2c96

including high power lasers, optical components, etc. (p. 69 ff.). Given the need for this development, and in the light of my other reservations, I recomment that the proposal for a Feasibility Study should not be funded in its current form.

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NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1 (9/83)
Supersedes All Previous Editions

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PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE
Feasibility and Design Study for a Gravitational Radiation Detector (Physics)

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

This proposal covers an essential part of an outstanding and very timely research program. The search for gravitational radiation from a number of possible astronomical sources is as scientifically exciting and fundamental as anything that is being done in physics today. While there is substantial uncertainty about the frequency of observable events for each of the types of sources which have been suggested in the 10 to 10^4 Hz frequency range, I believe the chances are considerably above 50% that one or several types will be observable with the proposed apparatus in the next 6 or 7 years. Even if this doesn't happen, the sensitivities which appear to be achieveable for different types of sources are high enough so that the corresponding upper limits would give valuable new astrophysical information. Many sources with longer periods which almost certainly would give observable signals in future laser heterodyne space experiments are expected to exist, but it wouldn't make sense to look for them before carrying out the proposed ground experiments.

The preparation for the feasibility and design studies covered in this proposal have been carried out in a thorough and careful manner, leading to a proposal which is very sound technically. Many questions still have to be settled during the design studies, like the vacuum level achieveable with particular pipe materials, gate valve arrangements, and low temperature bakeout, and the conclusions will affect the final performance. However, the only way to settle these questions is through the planned type of study. The supporting laboratory experiments at MIT to determine the results

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

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of the preliminary choice for the vacuum system design will add strong confidence in the final approach chosen.

Other technical issues exist which contribute substantial uncertainty in the final accuracy, including particularly the achieveable seismic isolation level, the laser power which can be used, and the efficiency with which light can be recycled. A lot more can be learned about these issued through the planned complimentary work at Cal Tech and MIT during the design studies, and comments on these are given in my reviews of those proposals. However, I believe there already is enough information available to support reasonable confidence in achieving within the next 6 or 7 years something like the geometrical mean of the curves given in Figs. 2.2-2.4 for the possible first experiments and for possible later experiments, at least for frequencies of 50 Hz and higher. This is sufficient sensitivity so that I believe going ahead quite rapidly with design of the gravitational wave detection facilities is strongly justified by the overall scientific importance of the results which are likely to be obtained.

Although both the expected sensitivity level and the corresponding expected frequency of observable events are to some degree subjective judgements where reasonable people can differ by an order of magnitude, my own judgment is that the probable scientific returns provide strong justification for the requested level of support. The plans for carrying out the feasibility and design studies seem very sound, and the proposal deserves an excellent rating. The PI's are among the few very best experimental physicists in the world, and their different backgrounds and viewpoints complement each other strongly. The inclusion of Kip Thorne on the Steering Committee is an excellent mechanism for making sure that any scientific disagreements will get resolved in a timely fashion.

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PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE

Feasibility and Design Study for a Gravitational Radiation Detector (Physics)

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

I think the building of the best possible gravitational wave observatory is a very worthwhile goal. The physicists involved are of the highest quality. The five kilometer system should be built. This is a very major undertaking, and I think the maximum use should be made of the present 40 meter system. All possible components of the final system should be tested in the 40 meter system, and it should be used in the interim as a gravitational wave observatory. I think the superstructure of this proposal seems somewhat complex for this stage of development and reflects the type of organization and review that is used for more complicated NASA flight programs.

OVERALL RATING: [X] EXCELLENT [ ] VERY GOOD [ ] GOOD [ ] FAIR [ ] POOR

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PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE
Feasibility and Design Study for a Gravitational Radiation Detector (Physics)

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

Common evaluation of the following NSF proposals:

PHY-8504836 Interferometric Broad Band Gravitational Antenna. R. Weiss, MIT

PHY-8504136 Investigations in Experimental Gravity and Gravitational Radiation. R. Drever, CIT

PHY-8504837 Feasibility and Design Studies for a Laser Interferometer Gravitational Wave Detection System. R. Drever, CIT-R. Weiss, MIT

1. Scientific quality:
The scientific quality of the proposals appears to be excellent. Project is feasible. The probability of achieving the proposed ultimate sensitivity is doubtful but expectation of achieving the initial phase of sensitivity is reasonable.

2. Capability of investigators:
The principal investigators appear to be highly competent and experienced. The supporting members of each of the laboratories are relatively inexperienced. Their past records appear good but proposals are very ambitious for such small and inexperienced groups.

3. Significance of the proposals:
The reviewer does not feel well qualified to make a firm judgment on the value of research in this field. However, it is my judgment that unless the highest sensitivity is achieved, the scientific payoff is rather doubtful. I believe such work should be supported but perhaps on a more modest scale. The work if successful would make an important contribution to the field of gravity and to the related field of astrophysics.

4. Effect of research on the infrastructure of science and engineering:
The activity in this field is likely to have little impact outside of the field.

(continued)

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

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5. Comments concerning the general approach to the construction program:
This is an exceedingly ambitious program for such a small core group with its limited experience, particularly with regard to operating a relatively large facility. The whole program is couched in terms of an elaborate national or international facility which would accommodate as many as 20 different experiments. This is too grandiose a project for the current tenuous state of the capability of the instrumentation and an uncertain physics output. By making a much more modest approach one can verify both the ability to make the detectors work with the desired sensitivity and to make observations demonstrating the usefulness of the physics results. After this demonstration and the development of a larger core of experienced personnel, one can then go ahead with a more ambitious proposal building on this experience.

6. An alternate mode of proceeding:
I think that the scope of the eventual construction proposal should be scaled down or phased in some way. This would also have some impact on the cost of the joint design proposal. I feel that to initiate now the construction of such an elaborate facility as proposed is unwise both from the point of view of being premature, and also from the point of view of flexibility and the capability to respond to the demands of the next generation of development.

If I understand it, the ultimate construction cost is primarily proportional to the length of the arms. At the arm length of five kilometers, my impression is that the intercept for zero length is about 20 percent or less. Hence by making an initial system of only about 1/3 the length, a factor of 2 can be saved in the overall cost.

I am concerned about choosing the full 48-inch diameter vacuum tube. It makes the cost of the system much higher, requiring not only larger pipes but also more and larger pumps. It also increases the cost of the valves and the difficulties of working with the system. The important advantage stated for the larger pipe is the ability to run a lot of different independent antennae simultaneously in the same pipe. This has an accompanying disadvantage because it couples the experiments in the same tube, end and corner stations, keeping them from being truly independent insofar as interruptions arising from access to the equipment in the vacuum of the station enclosures. My suggestion would be to build a 24-inch diameter vacuum system about 2 kilometers long on a site which would accommodate 5 kilometer arms. Use scheme (1) for the tube without earth cover. Then expect, in a second phase, to extend the length of the facility and add parallel paths in one or more additional tubes if the scientific results justify further expansion. This is a much more natural growth process rather than a plan to build at the outset an elaborate and expensive setup to provide a "national centerpiece" facility for the field.

The overall program as proposed is an expensive one. Amortizing the capital costs over 10 years and adding the operational costs and the support of the individual groups, the program will run a cost of roughly $10 million per year. This is comparable to the total funds now being put into the field. While I do support the program in general, I feel that this is a very large initial commitment to make for an activity which has a number of risks to the achievement of its goals. I believe that the more modest approach would ultimately achieve the same goals in about the same time, but minimize the risk and the cost.

If the overall scope is reduced as suggested, perhaps as much as one million dollars could be cut off the 17-month funding costs for the design work in such areas as engineering design, valve testing, vacuum test bed, prototype vacuum system, and independent cost estimate. The time chart appears optimistic for construction funding in view of the inherent bureaucratic requirements and present state of development. FY88 may be a more likely date for initiation of construction.

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PRINCIPAL INVESTIGATOR	NSF PROGRAM
Ronald Drever	Gravitational Physics

TITLE

Feasibility and Design Studies for a Laser Interferometer Gravitational Wave Detection System

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

While this proposal is only for the engineering design and prototype studies for a large gravitational wave antenna, I have chosen to review it as a proposal to contruct one. There is no question about the high abilities of the PI's, but I have serious reservations about this project nonetheless. I accept the scientific importance of detecting gravitational radiation, and agree that if this were done it would be of great value to physics and astronomy, just as were earlier advances in radio, X-ray, and infrared measurements. This project does however, differ from all those earlier advances in the riskiness of its goals and the resources demanded. In all other cases, development has been able to be incremental, with some sources detected early and more being found as detectors improved; usually, indeed, application of a detector to astronomy followed its use and development for brighter terrestrial sources (often man-made). In this case the only sources available are astronomical ones, and so the full technology must be developed, at great cost, to detect weak sources that may not even be there. (The only other projects of equivalent cost are particle accelerators, but here too development has been incremental, with useful measurements at each step.)

The riskiness and boldness of this project are not a reason not to support it, but do suggest that the haste shown here is unwise. It has been shown that an antenna sensitive enough to probably detect existing sources is theoretically possible, but so are lots of things that are nonetheless not practical. A lot of technology that does not now exist will need to be created to approach the theoretical limits, including high-power stabilized lasers (100 W vs .3 W now used), modulators and single-mode fibers to handle that power, low-loss mirrors 25 times bigger than now made and flat to 1/300 wavelength, displacement transducers 60 dB better than those now used, and 1-ton masses in high-Q suspensions.

All of this is likely, but none of it guaranteed; until the prospects are clearer, a large-scale facility would appear to run a good risk of merely putting a very expensive upper bound on gravitational radiation. Some of the remarks in the proposal about available signal-to-noise are optimistic; in practice, a S/N of.5 is not going to be convincing evidence of anything, much less enough to actually learn anything about the details of what is recorded.

I sympathize with the PI's desire to have a large-scale facility available soon so that detection of gravitational radiation may be done as soon as possible, but a long development time is a consequence of a project of this magnitude, and it is their choice to pursue it.

(TO CONTINUATION SHEET)

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

REVIEWER

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One part of the plan that seems especially weak is the simultaneous construction of two antennas. Certainly two are needed to give convincing results, but to build them together is to abandon all the advantages of the learning curve. I expect that once a system is running, a long time will be spent finding and removing unexpected sources of noise; if construction were staggered these lessons could be applied to the second antenna as it is built, so the total time to be working would probably be about what it would be anyway.

Having said all this, I must agree that most of the prototyping studies described in this proposal will need to be done before a large-scale facility is built, and that having done them, the costs would be much clearer. This is especially true of the proposed work on the vacuum system. I would therefore favor funding this, though perhaps over a longer duration than the 18 months given here. I can however see no use in the engineering design study at this stage, which is what I take the JPL work order to cover (the proposal is vague on this).

There is a large gap in this and previous work on antenna design which I would like to see addressed. Figures 2.2 through 2.4 make it clear that most of the expected sources emit at frequencies of a few hundred hertz and below (in any case bar detectors partly cover higher frequencies). The major noise source in this band is seismic motion; to judge from these figures it is reducing this, not lowering the shot noise alone, that will make the most sources detectable. However, all of the estimates of this come from a very thin database: essentially measurements at one (very quiet) location to 10 hertz, with generalizations about noise elsewhere based on visual estimates made in 1959, and extrapolation to higher frequencies. Some later measurements (G. Frantti, Geophysics, vol 28, 547-562) show that the extrapolation is roughly valid out to 100 Hz, but also that the low levels assumed are not common, 10 times larger not being rare. It is common seismological experience also that high-frequency noise varies substantially with local wind, and that spectra often show narrow lines from distant cultural sources. Given that ground motion is a major source of noise, it would seem right for more effort to be put into studying it so that more realistic estimates can be made of the amount of isolation needed. The effort required of a proper field program would not be large relative to the amount to be spent on other tests. To postpone this to a check made at various sites, rather than first trying to understand the behavior and causes of ground motion, is to give a too minor role to a potentially major problem. As one example of insufficient attention being paid to this, I would point to the list of sites proposed, which includes locations in Maine and various parts of the West. Any location in the Northeast is going to very noisy because of cultural moise and the high Q of the local rocks, which allows efficient propagation. Locations in the West are a poor idea for a different reason: in many places (including all those shown in California, Idaho, Arizona, and Utah) there is a good chance of significant seismic activity nearby during the 20-year projected life of the facility, and during the months or years that such activity might last the usefulness of the facility would be greatly reduced because of frequent "strong" ground motion. (It need be strong only relative to background noise).

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APPENDIX II

PROPOSAL TO THE NATIONAL SCIENCE FOUNDATION
Cover Page

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 X ; IF YES, LIST ACRONYM(S):
GRAVITATIONAL PHYSICS, DIVISION OF PHYSICS	

PROGRAM ANNOUNCEMENT/SOLICITATION NO.:

CLOSING DATE (IF ANY):

NAME OF SUBMITTING ORGANIZATION TO WHICH AWARD SHOULD BE MADE (INCLUDE BRANCH/CAMPUS/OTHER COMPONENTS)

CALIFORNIA INSTITUTE OF TECHNOLOGY

ADDRESS OF ORGANIZATION (INCLUDE ZIP CODE)

PASADENA, CALIFORNIA 91125

TITLE OF PROPOSED PROJECT

FEASIBILITY AND DESIGN STUDIES FOR A LASER INTERFEROMETER GRAVITATIONAL WAVE DETECTION SYSTEM

REQUESTED AMOUNT	PROPOSED DURATION	DESIRED STARTING DATE
$6,700,000.00	17 months	June 1, 1985

PI/PD NAME AND SOCIAL SECURITY NO. (SSN)*		PI/PD PHONE NO.
RONALD W.P. DREVER (CALTECH)	013-34-8339	(818) 356-4291
RAINER WEISS (MIT)	128-26-6401	(617) 253-3527

PI/PD DEPARTMENT / PI/PD ORGANIZATION

DREVER: DIVISION OF PHYSICS, MATHEMATICS, & ASTRONOMY, CALIFORNIA INSTITUTE OF TECHNOLOGY
WEISS: CENTER FOR SPACE RESEARCH, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

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:

- ☐ Animal Welfare
- ☐ Human Subjects
- ☐ National Environmental Policy Act
- ☐ Endangered Species
- ☐ Marine Mammal Protection
- ☐ Research Involving Recombinant DNA Molecules
- ☐ Historical Sites
- ☐ Pollution Control
- ☐ Proprietary and Privileged Information

PRINCIPAL INVESTIGATOR/ PROJECT DIRECTOR	AUTHORIZED ORGANIZATIONAL REP.	OTHER ENDORSEMENT (optional)
NAME: RONALD W.P. DREVER	NAME: D. Mack	NAME: RAINER WEISS
SIGNATURE: R. W. P. Drever	SIGNATURE: D. Mack	SIGNATURE: Rainer Weiss
TITLE: PROFESSOR OF PHYSICS, CALTECH	TITLE: Director of Finance Controller (CFO)	TITLE: Professor of Physics, MIT
	NAME: Paul H. Quinn	
	SIGNATURE: Paul V. Quinn	
	Associate Director, Office of Sponsored Programs, MIT	
DATE: OCTOBER 30, 1984	DATE	DATE: NOVEMBER 1, 1984