R. Weiss

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Gravitational Radiation Detector Projects Report to the

National Science Foundation from the

*Technical Review Sub Committee

April 22, 1979

Sub *Committee Members:

Richard D. Deslattes, National Bureau of Standards
David Edwards, Ohio State University
Stephen Jacobs, University of Arizona
David Nygren, Lawrence Berkeley Laboratory
Arthur Rich, University of Michigan
David Wilkinson, Princeton University

1. Introduction

Within the present decade, major gravity wave detection projects have taken shape in more than a dozen laboratories in half as many countries. These experimental programs face formidable obstacles, they are costly in time and in money and they consume the attention of talented investigators. In the United States alone, work is underway in six universities. The dominant financial support for this field comes from NSF. At the FY 78 level of $777,000/year, and with major growth in the offing, the Foundation commissioned an examination of the goals, methods, and progress of the field.

To assist in carrying out the needed program study, the Foundation appointed a Technical Review Committee whose membership is listed above. The Committee was asked to "assess and make recommendations concerning: a) the objectives, accomplishments and future potential of the research; b) adequacy of present facilities and the need and relative priority for new or improved facilities; c) the adequacy of personnel necessary for achieving both short- and long-term research goals; d) appropriate distribution of resources to the different approaches to efficient

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realization of research goals; and e) appropriate milestones for all projects for the next three years".

The present document represents the response of the Technical Review Committee to the Foundation's Charge. Members of the Committee came from diverse areas of experimental physics. Although none had experience in the area of gravitational radiation detection, some Committee members had experience in precision measurement techniques while others had a background in the area of cryogenic systems. There was also experience in certain astrophysical problems on the part of several. Our information (from which this report was derived) was obtained by reviewing technical material furnished by each of the U.S. experimental projects and by day-long visits by at least three Committee members to each site in the U.S. During these visits, Committee members received detailed technical presentations and had the opportunity for extended discussions with principal and associate investigators as well as graduate students and others connected with the effort. In addition we obtained interviews with appropriate administrative representatives through whom we attempted to gather an impression of the climate under which each project proceeds.

Our study of the field and examination of the U.S. efforts leads us to an assessment which includes perceptions of both great difficulty and great promise. Evidently, the work could well lead to results of great interest in general relativity, cosmology and astrophysics. Overcoming the inherent difficulties will require extension of known technology into regions of very low temperatures and macroscopic quantum systems. In addition it will involve development of interferometry with sensitivity of a fraction of a nucleon radius over kilometer lengths. Though difficult and, not without risk, we feel the work should go forward and that the present U.S. effort is adequate provided currently proposed expansion is funded. The future may entail still more massive efforts possibly requiring regional, national, and international cooperation. The remainder of this report attempts to supply its reader with some impression of the basis from which these conclusions were derived.

2. Scientific Background

Most signals detected on or near earth from distant astronomical sources arise from incoherent superposition of microscopic processes. In the case of potentially detectable sources of gravitational radiation, we have, instead, cooperative activity involving large masses in coherent motion. It is this fact that makes it possible to think of detecting gravity waves from astronomical sources in spite of the small coupling constant. Laboratory scale replication of the Hertz experiment for gravity waves appears, however, impossible at the present time.

Although clearly permitted by general physical principles, direct detection of gravitational radiation from astronomical sources has not yet been accomplished and may not be for some years into the future. The present efforts to construct apparatus for the detection of gravitational radiation fall, predominantly, into two classes. The first uses resonant bars instrumented in such a way that small changes in their free vibrations may be detected with high sensitivity. The second hopes to use almost-free masses separated by large distances to obtain broad-band response to gravitational waves from both pulse and cw sources. Before getting into the particulars of these efforts, we address the broader questions of scientific motivation and current estimates of likely signal levels.

Scientific motivation for these efforts includes the obviously desirable direct confirmation of the dynamical features of Einstein's now 60-year old gravitational theory. Beyond the detection of such signals and verification of their tensor properties, the real potential for the field lies in its evident capability to open a new astrophysical window. This new kind of observational astronomy, though perhaps far in the future, would permit study of many types of unusual objects some of which are otherwise destined to remain inaccessible to non-gravitational observation. Through their gravity wave signal structure, we can hope to see the coherent rapid motion of matter under unusual conditions of pressure, density, and temperatures. Similarly, objects and processes obscured to electromagnetic or neutrino investigation by intervening dust or plasma should be detectable by means of gravitational signals because their scattering and absorption are weak.

Progress toward these goals is being made (see following section) but difficulties remaining are numerous and substantial. At the very least, outstanding further developments are required in transducer technology, low-noise amplifiers, interferometry and seismic isolation. It is not without significance that in response to these problems, active reexamination of the fundamental detection limitations arising from quantum mechanics is well underway.

3. Sources and Detectors

It is difficult to characterize succinctly the entire array of potential gravitational radiation sources without major omissions. Nevertheless, without some cursory attempt to do so it is impossible to convey the present and near future relationships of detector refinement to likelihood of event or signal detection. It must be emphasized before beginning that all estimates have large uncertainties. They are usually strongly model dependent and also are dependent on details such as the impact parameter of a black-hole collision or the asymmetry of matter collapse. It is also (preversely) the case that those objects about which relatively rigorous calculations are possible tend to be among the weakest of radiators whose detection most likely lies far into the future.

Many authors have attempted to summarize present estimates in diagrams such as that shown in Fig. 1. This is based on the summary of Thorne* which is representative of the range from other sources. Along the abscissa are indicated the characteristic frequencies, ν, (top) and periods, τ, (bottom) for the indicated sources. In the case of pulse sources, a spectral bandwidth is assumed to be of the order of τ^{-1}. The ordinate scale indicates dimensionless strain $h = \delta\ell/\ell$ (left side) and source strength (right side) in GPU (1 GPU = 10^5 erg/cm^2/Hz).

The NSF program has, so far, been supporting efforts designed to detect gravitational wave bursts coming from astronomical sources with frequencies between 100 and 1000 Hz. These sources fall on the left side of Fig. 1. Stars heavier than our sun are thought to end their evolution by forming a supernova leading to a neutron star or by collapsing to a black hole. In our galaxy, such events are rare, but the precise event rate is rather uncertain. First generation room temperature resonant bar antennas had strain sensitivities of $h \sim 10^{-16}$ and were unable to detect any such events, however they should be within reach of the second generation cryogenic receivers expected to be operational shortly. To increase the event rate, receiver sensitivity must be improved. Then a larger volume of the universe may be explored for violent events such as neutron star births, and nearby weaker sources such as core quakes in neutron stars may be probed.

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*K.S. Thorne, in Theoretical Principles in Astrophysics and Relativity, W.H. Reid and P.O. Vandervoort, ed. (University of Chicago Press, 1978).

Another possibility is to work at a lower frequency and build a detector specially designed to seek periodic sources (e.g., the Vela pulsar). Several months of investigation time would be needed to reduce background noise.

Alternatively, there appears to be great potential in searching at very low frequency for sources, either burst or periodic. These are shown on the right side of Fig. 1. In order to detect supermassive black hole events in galactic nuclei or quasars, or the weaker but more conservative sources such as binary star systms, space experiments will be essential. This will require the development of enormous interferometers or precision Doppler tracking of deep space problems, and can only be carried out by NASA.

4. Current Experimental Projects

Outside of the U.S., there are the following major efforts on narrow-band bar antennas substantially all of which include or plan cryogenic cooling. In the SOviet Union (Moscow State University) Braginsky leads a large and extraordinarily well-financed effort. He has focused on extremely high Q dielectric crystal antennas and has reported the highest crystal Q value obtained anywhere. An Italian group under Amaldi began in Rome and has lately moved, in part, to CERN where they will enjoy the cryogenic facilities and site formerly occupied by the Gargamelle bubble chamber. They have concentrated on large aluminum bar receivers of the type found also at Stanford and Louisiana State University (LSU).

At the Max Planck Institute in Munich, Billing heads a large, well-supported group which currently emphasizes free-mass antennas with laser interferometer readout. This effort is evidently farthest advanced of any of the free-mass systems although Drever (at Glasgow) is not far behind. After long involvement with bars and many successful related efforts, Drever's group has switched their main effort to free-mass systems. A double L-shaped system in ultra high vacuum is in an advanced state of readiness even as Drever himself is establishing a large effort along similar lines at Cal Tech. At Tokyo University, Hirakawa's group has pioneered a tuned antenna for periodic sources, and put forward a new and higher Q aluminum antenna material (type 5056). The Tokyo group has good cooperative arrangements with Maryland and LSU in the U.S. Other advanced work on high Q (niobium) bars is carried out at Perth, Australia. Work with small crystals also proceeds in Saskatchewan, Canada. There are also two groups in the Peoples' Republic of China, one in Peking and one in Canton, but little is known of the technical details of this work except that it involves resonant bars and interferometers.

The main U.S. effort (supported by NSF) is at six universities. Four of these concentrate on bar antennas while two address free-mass systems. Metal bar systems at cryogenic temperatures are in advanced preparation at LSU (Hamilton) and Stanford (Fairbank) with a Maryland (Weber) effort not far to the rear. At Rochester (Douglass) and at Maryland (Weber) work goes forward on high Q-silicon and sapphire bars while each retains an activity in the area of metal bars. Both Rochester and Maryland maintain also room temperature bar antennas, one of which is being converted to optical readout (Rochester).

Free-mass work in the U.S. is concentrated at MIT (Weiss) and potentially at Cal Tech (Drever). The MIT work is farther advanced but is still at an early stage. On the basis of the previous Glasgow operation, it is a reasonable expectation that the Cal Tech effort will come rapidly into operation at least in a prototype phase.

5. Technological Dividends

Although no astronomically significant data is yet at hand (or likely in the near future), the continuing efforts to improve instruments have already led to a number of high-technology spin-offs. More may reasonably be expected in the future. The high Q mechanical resonators have application in stable oscillator technology. New designs for sensitive accelerometer have led to extraordinary advances in gravity gradiometers. These appear to have interest for geology and for tests of the inverse square law of gravitation. Clearly new frontiers have been reached in the area of low noise amplifiers and more progress is needed. Stable, anti-seismic platforms are needed but not yet at hand. Displacement transducers, stable microwave cavities and microwave frequency sources have all been improved by the gravity wave projects.

Finally, the need for exploring measurements at the quantum level has sparked lively debate and serious proposals to determine energy differences less than one antenna quantum. Aside from clarifying this aspect of macroscopic quantum mechanics, there can be little doubt that herein may lie the key to more general evasion of back-reaction in measurement.

6. Facilities

A. Bar Antennas

Present facilities in the U.S. include a variety of Bar Antennas ranging up to 4800 Kg mass. Although the only presently operating bar is a room temperature device of modest size (Maryland), the majority of effort is being directed toward cryogenic systems. Facilities at LSU and Stanford have identical bars (4800 Kg) and similar cryostats but differ in approaches to the problem of suspension and sensors. At Stanford a mechanical suspension system consisting of an overhead cradle and tension rods has been operated at 1.7 K, with the bar displaying a Q of 2.6 x 10^6 under a technique which is expected eventually to provide superior noise isolation, but only partial success has been achieved thus far due to incomplete superconductivity of the levitation system. Both the Maryland and Rochester groups have developed the facility for low temperature measurement of high Q materials and have studied materials such as aluminum 5056 alloy, sapphire, and silicon crystals.

Instrumentation of the bar detectors is fairly diverse. The oscillations of the bar must be sensed by a transducer which, in order to achieve optimum signal-to-noise ratio, must transfer a large fraction of the bar's vibrational energy to the following amplifier. Both resonant and non-resonant transducers are in use. There is general agreement on the principles required to match the bar, the transducer, and the amplifier but the several groups are following somewhat different lines. The piezoelectric transducers used in early work have given way to various designs based on low temperature, and in particular, superconducting technology. It is more sensitive, and it is convenient now that the new generation of bars are to be operated at low temperature. One type of transducer consists of a persistent current loop in which the motion of the bar perturbs the magnetic field. The resultant modulation of the current in the transformer is coupled to a DC SQUID which parametrically transforms the signal up to microwave frequency. Other designs use the motion of the bar or the 'proof mass' to modulate the difference in frequency between two superconducting microwave cavities.

In some laboratories the development of the transducer-amplifier chain is far advanced and working systems are already available although not perfected. All the groups working on the conventional bar detectors have at least detailed designs. The development of these extremely sensitive transducers and amplifiers, and in particular the DC SQUID, will be useful in other fields of technology and physics. The absence of small, rapid turn-around cryogenic "test beds" may introduce delays in some cases for ultra low noise studies of transducers and amplifiers. All of the facilities may require considerable additional effort in seismic/acoustic isolation and suspension techniques to reach the desired sensitivity. This will likely require construction of acoustic houses, isolation platforms, or conceivably the relocation to more favorable environments.

The performance of a bar detector can usually be improved by having a higher Q bar, a determined effort should be made to obtain large aluminum bars of 5056 alloy if the small sample Q of nearly 10^8 can be preserved. This effort could be appropriately coordinated by the NSF. Since there are four U.S. groups which are quite far advanced in the development of bar antennas, it seems unfruitful, in the absence of new ideas, to encourage other laboratories to attempt to duplicate existing facilities. However, research undertaken in collaboration with the present groups would be highly desirable.

The present facilities, when operational at the desired sensitivities, appear quite adequate for the immediate goal of searching for gravitational radiation down to strain levels of 10^{-18}. The convincing demonstration of the detection of gravitational radiation would require a time coincidence between two or more systems operating with similar sensitivity and frequency.

In summary, the highest short term priority should be given to the most expedient completion of and extended operation of present facilities with emphasis on simultaneous operation whenever possible. Attendant to this priority will be a continuing program, at all sites, to improve understanding of noise sources and suppression techniques, and the further development of transducer technology.

B. Free-Mass Systems

The present state of free-mass systems in the U.S. is less advanced than the cooled bar system and they are less widely distributed. In particular, at the present time work is underway at MIT (Weiss) and a program has been proposed for Cal Tech (Drever). The Cal Tech effort will follow closely some of the developments already made in this area by Drever in his group at Glasgow.

As contrasted with bar antennas, free-mass systems read out by laser interferometers offer both advantages and disadvantages. The likely displacement sensitivities with interferometry are poorer ($\sim 10^{-16} - 10^{-17}$ meters) than are found in superconducting transducers. On the other hand, free-mass systems of the order of 50 meters can obtain comparable strain sensitivity to that expected in current cooled bar systems while retaining the convenience of room temperature operation, albeit in ultra high vacuum.

As further contrasted with bar systems, those using free masses tend to have broad-band sensitivities. This needs careful qualification since seismic background and sensitivity to local mass motion lead to severe problems. THese are such as to impose a low frequency cut-off for earth based systems which may not be substantially below 100 Hz without significant new developments. The high frequency performance of such systems is, in principle, shot noise limited which, for current cw laser powers, seems to lead to kilohertz upper frequency limits.

A table-top realization of a prototype free-mass system is under assembly at MIT. Its successful operation at or near fundamentally limited interferometer performance will be a major milestone in this process. Operation at a sensitivity near the level required for even optimistically estimated astronomical sensitivity will require significant scaling up and seismic isolation. An already scaled up version of such a system is proposed for Cal Tech while a somewhat advanced few meter systems should reach operation in Glasgow within a year or so.

Presuming successful operation of these prototype systems, further scaling of detector size will follow along with multi-detector correlation measurements. In a first step toward this type of potential signal recovery Drever is proposing operation of L-shaped antennas in pairs both at Glasgow and at Cal Tech. The next jump will likely be toward kilometer baselines with attention to seismically favorable sites. Even larger systems can be considered still for use on earth but at some point satellites in space become the natural extension. Ultimately, it has been proposed that three drag-free satellites at Lagrange points of the earth-sun system may prove a best approach to long period weak sources.

7. <u>People</u>

The difficulty of experimental gravity wave research is well matched by the high quality of the people currently working in the field. The problem has attracted well-known, senior experimentalists, who have in turn attracted a number of very good younger colleagues to their laboratories. The Committee was impressed by the level and range of inventiveness being applied to these very difficult experimental problems.

The quality of people in this field is also important to long-term support planning. Suppose, in the worst case, that when technological limits are reached (10 to 15 years) no gravity waves have been detected. What then? The answer we received was that there are plenty of other interesting problems in related areas of physics to work on. We don't believe that the field, as currently populated, will stagnate; if it becomes uninteresting, it will simply disappear and people will move on to more interesting problems.

Although some laboratories seem to be adequately staffed at this time, it did appear that the work could be moving faster at most places with one or two more people. A particularly important position seems to be the "second in command"--the relatively mature research worker who keeps the research moving ahead day-to-day.

Several of the principal investigators brought up the problem of young scientists working in a field whose ultimate scientific result may be 10 years away. How do they stand out and establish themselves? There was also concern about whether, under these conditions, the field can continue to attract good young people over the long haul. Others, experienced in the field, see no such problem. They say that new sensitivity limits from intermediate detectors are interesting (and publishable) and that the high level of technology provides excellent training in instrumentation and experimental technique. Also, most laboratories have related work underway (gravitation, low temperature, solid state) which provides opportunities for part time research with shorter time scales. The Committee has no specific advice, but notes that a similar problem occurs in certain other areas of experimental physics.

8. Institutional Settings

The current level of institutional support and commitment is surprisingly uneven at the laboratories we visited. One institution has singled out this field for a major new initiative; the principal investigator has been given generous allocations of faculty positions and institutional funds. Meanwhile, another investigator (fully as capable, in our opinion) is having difficulty obtaining local support or, in some respects, even a benign tolerance. With this one exception, however, we sense that gravity wave detection has caught the imagination of colleagues, and that most investigators have good suport within their institutions.

The Committee asked itself whether this work is best carried out at universities. We feel that the answer is yes, at the present time. The instruments and problems are still of a size that can be handled in a university setting. This may, of course, change if the trend continues toward larger, more complex, instruments. The level of financial and technical support needed and the probability of success may eventually reach a level where a national facility will be more appropriate, but we do not see this happening within the next five years.

9. NSF Support Pattern

The equestion of the adequacy of funding of existing projects may be considered from both a relative and an absolute viewpoint. By relative we mean funding levels with respect to our perceptions and actual experience with NSF funding in other areas of low energy physics. We take absolute to mean current funding with respect to what might be needed for a maximum rate of progress on any given experiment. Division into relative and absolute funding levels is important since, unless funds are unlimited, the relative treatment of different areas of physics which the NSF has decided to support should at least be comparable.

(a) Relative Funding

A crude assessment of relative funding in gravitational physics may be made by noting that the annual funding level for this area in FY 78 was $777,000. This sum was spread over four groups consisting of five professors (tenured) and four assistant professors (non-tenured). The average funding level is thus $87,000 per faculty member. This level is certainly commensurate with or higher than funding in low energy areas which is typically of the order of $50,000 per faculty member and per research project. We note at this point however that the total number of people, including post-doctoral associates, graduate students, and technicians, associated with these four projects is approximately 30 so that the project sizes are more roughly compared with to high energy physics groups than to typical low energy (condensed matter or atomic and molecular) research groups, where an average of four to five personnel per project might be more typical. Given this consideration it is apparent that in a very rough sense the relative funding for groups building gravitational radiation detectors is quite reasonable when compared to other low energy physics research programs.

(b) Absolute Funding

The Committee Feels that of those groups which have received funds on a continuing basis over the past seven years (Louisiana State University, Maryland, Rochester and Stanford), the NSF portion of the funding has been sufficiently restrictive so that without substantial non-NSF gravity related support the groups at LSU, Maryland and Stanford (Rochester) would be far behind the point they have currently reached. Each of these groups has been extremely resourceful in finding large sources of non-NSF and other non-University support to supplement their NSF funding. This is particularly true for construction but it is also true for salaries and materials and supplies. Such practice is not unusual in low energy research but the degree of resourcefulness shown by some of the groups has been truly exceptional.

The remarks just made regard past funding. As of the last two years, funding appears to be tight but not unduly restrictive. This is true except for the case of Maryland where a large overall effort using common technology and facilities (3 faculty, 9 in the group) is clearly being funded from other than gravity related support (NSF funding was $60,000 in FY 78). In any case, we feel that all interesting research directions (bars, laser-interferometers, and resonant detectors) are being adequately supported although a moderate increase (adjustment for inflation at a minimum) in the average funding level would certainly be justified. This statement implies that with the advent of two strong new groups on the scene since 1978 (Cal Tech and MIT) and with increased support for the Maryland group, a substantial (possibly $400,000 - $500,000) increase in annual funds for gravitational radiation detector research seems reasonable. We say this in view of our opinion that even with a total of six groups in the field there will be no unnecessary duplication of effort and since the groups are all strong and viable there should be no increase in funds for one group at the expense of another.

10. Milestones

For the several groups working on bars, there are several developments likely already in 1980. Among these we note:

°Sensitive optical monitoring of a room temperature bar,

°Completion of new transducers,

°Testing of new amplifier schemes, and

°Occasional operation of the cryogenic bars.

In the next year, 1981, we would hope to see:

°Progress toward understanding noise,

°Improved transducer systems,

°More frequently, possibly routine cold operations, and

°Demonstration of sensitivity below $h = 10^{-18}$.

The following year, 1982, is the last one for which it seems possible to make detailed projections. This third year may be expected to yield:

°Noise temperatures below 1 K,

°Extended operation of individual detectors with overlap, and

°Generation of data at a level at which unusually close, unusually violent, or unexpected events might be detected.

Somewhere in the 1983-85 period, important questions about the future need to be raised. How these will appear at the time is naturally very hard to say. A lot will depend on the happenstance of possible (unexpected but not forbidden) real event detection and on the results of presently underway studies on the quantum mechanics of large objects and the associated limitations of physical measurement. According to the way in which these questions are resolved, the level of effort which should be assigned to bar devices beyond 1985 may be in need of revision upward or downward.

For the case of free-mass detectors, it is anticipated that already in 1980, we shall see:

°Attempted operation of a "small" system with 10 watt laser and passive suspension,

°Beginning efforts toward seismic and optical isolation, and

°Progress toward understanding of quantum mechanical limits.

In the following year, 1981, it appears possible that:

°One or more systems operating under shot noise limited conditions may appear,

°Questions of optical decoupling and alternative laser modes and designs should be addressed, and

°Demonstration of sensitivity in a room temperature free-mass system comparable to that expected from second generation bar systems.

Because of the less advanced development of the free-mass systems, it is even more difficult in this case thatn in the case of bars to go into 1983 and beyond. Nonetheless, we would hope to see in this area by that time:

°An understanding of the seismic isolation (including active systems) needs and some progress toward their realization,

°First use of ring laser and of master oscillator/power amplifier configurations, and

°Operation of two or more detectors in overlapping periods.

As one looks past 1983 toward 1985 and beyond, it becomes evident that scale changes will be required for the free-mass systems. A scale length of 1 to a few kilometers begins to suggest itself--if adequate inertial platforms and laser frequency stability can be maintained.

Altogether, both channels appear to aim at a near-term sensitivity goal of 10^{-18}. This characterization is incomplete, and the review panel takes explicit notice of efforts to extend measurements to the "quantum limit" and beyond for resonant bar systems. It is also noted that for low frequency sources the free-mass systems will ultimately require scaling toward still larger baselines where one can, in principle, be assured of detectable signals. These will necessarily be found in drag-free satellites disposed either with respect to the moon-earth or with respect to the earth-sun system.

11. Conclusions and Recommendations

In our study of the U.S. program for gravitational radiation detection, we were strongly impressed by the present state of the field. The scientific goals of this effort, although technically quite difficult, appear to us to be amongst the most exciting research opportunities which currently challenge physics and astronomy. The small group of scientists involved in the experimental program are of impressively high caliber, and display the ingenuity, resourcefulness, high morale, and strong commitment appropriate to this long-term endeavor. The rate of progress in the recent past has been excellent, both in terms of increased instrumental sensitivity and generally useful high-technology spin-offs. In the very near future, second generation resonant bar receivers will be in operation and setting astronomically interesting limits on possible sources within our galaxy. The ultimate detection of gravitational waves, verification of the properties predicted by theory, and exploitation for observational astronomy, are believable consequences of present research directions. Reaching these goals, however, could take a decade of hard work and will require the development of new interferometric wide-band receivers as well as substantial improvement of current narrow band detectors.

Based upon our reading, discussions, and first hand observations, we have a number of recommendations for the participating university groups and funding agencies:

High priority should be given to rapidly integrating bar receiver components and achieving operating second generation receiver systems with greatly improved sensitivity. These should be capable of reliable operation before is devoted to the next level of new and difficult technical problems. Operating receivers should have sufficiently good time resolution to allow intercomparison and coincidence analysis for maximal scientific utility.

Instruments capable of detecting signals from better understood systems such as periodic astronomical radiators or laboratory post-Newtonian sources would be interesting to consider. Such ideas should be looked into, and explored if they become possible with demonstrated technology.

In view of the small size of the U.S. effort, NSF should work to maximize its effectiveness by encouraging cooperation between groups, coordination of programs, and exchanges of personnel. One important way to expedite this would be through sponsorship of small workshops where representatives of all groups could discuss and explore approaches to problems of mutual interest. The problem of system isolation is an important one, and will require increased attention in the future. NSF should be alert to opportunities in this area and should cooperate with mission agencies interested in working on this topic. The availability of high Q aluminum alloys for bar antennas would be an important development. Whatever NSF can do to encourage the casting of such bars in sizes up to five tons would be extremely worthwhile.

Our analysis of NSF funding in this field shows that past investment has not been anomalous in view of existing manpower and available opportunities. However, this year a substantial step in support is required to allow for scientifically desirable expansion in new directions, resulting, in part, from the entrance of two major research institutions into participation in this field. We hope that NSF will find it possible to provide the additional resources currently needed, and to continue its commitment to this small but exciting field over the long term.

In the coming decade, we note that NASA will be presented with opportunities to conduct space experiments to test general relativity and search for long period gravitational waves. Doppler tracking of deep space probes, or building enormous interferometric systems in space, will require complex logistical efforts, and will be far more expensive than laboratory research. We recommend that NASA coordinate its efforts with NSF, and participate in the construction of ground based prototypes and experiments to provide experience for the more difficult space efforts of the future.