Elements in the strategy for the future of ground based gravitational wave research

R. Weiss

Dawn II Meeting

Georgia Institute of Technology

July 8. 2016

The elements of the program

- Strong observing program
  - Known and predicted sources
  - The dark sky and unexpected sources
  - The known sky in new ways
  - The correlations with E&M and particle astronomy
- Strong technical development program
  - Unique relation of the science to the sensitivity
  - Instruments operating at the limit of technology
- Strong analysis program

An example science evolution

- Black holes
  - Distribution of masses and spins vs z and astrophysical setting
  - Origins
    - collapse of ordinary stars
    - product of the first stars
    - dynamical formation
    - Primordial
  - Precision tests of GR
  - Cosmology with black holes
    - Cosmic metric and derivatives
      - H and w with different systematics
      - Large scale structure of the universe
      - Consistency of cosmological parameters

Possible future of ground based work

- Near term 5-10 years
  - Operating costs: LIGO lab $40M/yr , LSC $10M/yr
  - New detector components in 4km facilities ~$30M -$100M
- Longer term > 10 years
  - Refurbishment of 4km facilities ?? 25 year lifetime
  - New facilities allowing improved sensitivity
    - Longer 40km L and or buried triangle ~ $1B

Science politics

- What disciplines are interested in the science
  - Astronomy: populations, evolution, specific systems, supernova, cosmology
  - Physics: Strong field GR tests in understandable systems, gravitation on large scales, consistency of cosmological solutions, nuclear physics: equations of state , r process heavy element formation, supernova, wave kinematics
- Would gravitational wave research be a priority for either discipline? Enough for $1B?

Strategies

- Require a reputable scientific group (not only in GW research) to establish priorities for the science.
  - Example: the Sessler-McDaniel panel in 1986
- If astronomy: need to be part of Decadal Study in 2020 (begins in 2018)
  - CMB, pulsar timing and space based are in Decadal
  - Ground based would be competing with many large projects
  - Astronomy is already having trouble supporting the operations of its facilities
- If Physics: no longer Decadal Studies, does not fit into HEPAP
- Need a NRC panel to review the field in late 2017 or early 2018
  - Full spectrum: CMB, pulsar timing, space based, ground based
  - Both physics and astronomy representatives
  - Scientists (not all in our field) to evaluate the importance of our science, the technology and costs
- Why a NRC panel
  - Need the authority of the NAS to convince congress
  - Useful in the approach to private donors

What needs to be done

- Establish the charter for such a panel
- Prepare the science case for successive sensitivity improvements by factors of ~3 indicating where the breakpoints are demanding new facilities
- Indicate broadly the technical changes associated with increments in sensitivity
- Estimate approximate costs and schedules for the sensitivity improvements

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February 4, 1986

Mr. Marcel Bardon
National Science Foundation
1800 G Street, NW
Washington, DC 20550

Dear Marcel,

I spoke last month at the General Electric Research Lab, and I had a chance to talk with Roland Schmitt about gravity-wave work. He mentioned to me the interest in the community to do "gravity-wave interferometer" work, and I am reasonably familiar with the various approaches.

I am firmly of the view that we do not need right now to spend $40 M or $100 M on such an effort.

If there is such an interest, then I think it would be useful to have a two-week summer study, where people who are not involved in the experiments have a chance to contribute, to reduce the cost, and to provide a wider community which is informed in the matter. I think that this was done reasonably well with the DUMAND effort.

Enclosed are some Star Wars items which you may not have seen.

Sincerely yours,

Dick Garwin

Richard L. Garwin

Encl:
11/17/85 "Reagan's Plan Caught Many Administration Insiders by Surprise," by Frank Greve San Jose Mercury News. (111785..FG)
09/25/85 "The Case Against Star Wars," by Philip W. Anderson, in Princeton Alumni Weekly. (092585.PWA)
09/13/85 "The Strategic Defense Initiative: 'Star Wars'," by John Bardeen. (091385..JB)
09/00/85 "SDI: The Grand Experiment," pages 33-64 of the September issue IEEE Spectrum including: Introduction 'The challenge of all time,' by Donald Christiansen;

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

Report to the NSF

Panel on interferometric observatories for Gravitational waves January 1987

5. SUMMARY

A) A strong case has been made for the scientific value of the goals of the project.

B) Though there are large uncertainties associated with the strengths of the many different kinds of astrophysical sources and the ultimate capability of interferometric detectors, there is a high probability that this facility will ultimately provide for a giant leap in our understanding of the gravitational force, one of the most fundamental forces of nature, as well as our knowledge of astrophysical phenomena.

C) It is anticipated that this facility would uniquely provide the most sensitive and certain prospect for detecting astrophysical events and identifying their nature. Essential to this capability is the twin nature of the two interferometers. Though companion efforts in other countries are highly desirable, a common management of the two LIGO detectors is important both for the coordination of the observational program and for the analysis and identification of observed events. This facility would provide for a continued and thriving development of the field.

D) It is important to proceed directly to the construction of a long baseline interferometer in a timely manner since many aspects of the detector development program cannot otherwise be tested.

E) The rate of detectable extragalactic events increases as the cube of the interferometer sensitivity, thus putting a high premium on the long baseline. Though a multistage, or phased authorization to the final configuration was carefully considered, the panel does *not* recommend this approach. We recommend full authorization with phased construction and appropriate milestones.

F) The plans as described in the presentations and in the various documents provided appear to be well conceived. The procedure which has been employed in drawing up the existing designs and in making the cost estimates appears reasonable and adequate for proceeding to the final design for submission. Effort should continue to examine design alternatives which may decrease costs, particularly in the area of the vacuum system and enclosure. We do not recommend that the project be delayed by this process of re-examination. It is important to make the choice between Fabry-Perot and Michelson interferometer type detectors before submission of the final design. However, it remains important to develop advanced detectors and therefore research should continue to this end.

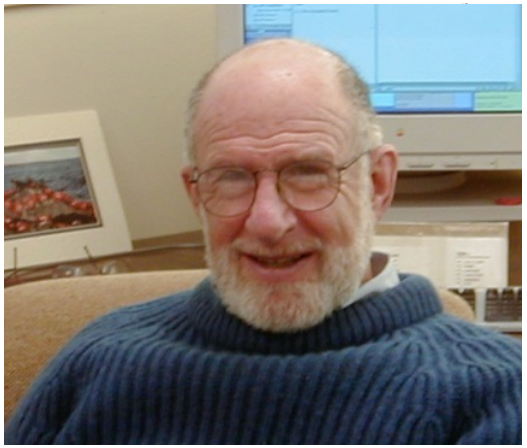
G) Because of the magnitude and dual nature of the facility, with laboratory sites widely separated, it is especially important that the construction and operation be well managed. The panel feels that the project requires a single scientific project leader of high stature to direct the activities. Efforts should immediately be directed to providing such leadership.

H) In looking forward to the utilization of the facilities it should be recognized that in addition to a budget for its operation, adequate funds will be required to support both the needs of experimental groups and further detector development.

I) In conclusion, the panel enthusiastically supports this development effort and urges that the plans for the project be refined along the lines indicated and that the design be completed. We recommend, then, that the construction project be brought to the National Science Foundation Board for consideration and (hopefully) for funding.

Panel Members:

Daniel B. DeBra
Val L. Fitch
Richard L. Garwin
John L. Hall
Boyce D. McDaniel
Andrew M. Sessler
Saul A. Teukolsky
Alvin A. Tollestrup



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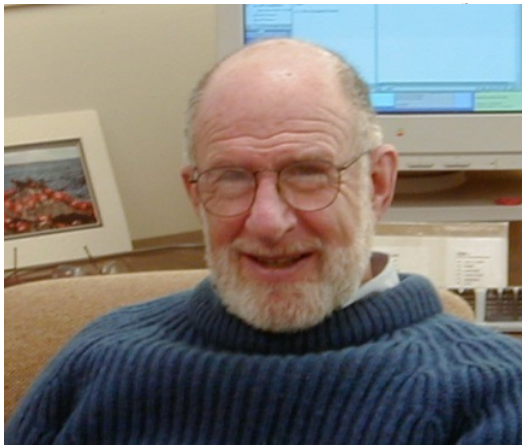

A. Sessler

B. McDaniel

R. Garwin

WORKSHOP COMMITTEE

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R.Spero	Caltech	A.Brillet	Orsay
K.Thorne	Caltech	P.Tourrenc	Paris
S.Smith	Caltech		
M.Zucker	Caltech	P.Bender	Colorado
G.Leuchs	Max Planck	N.Christiansen	MIT
A.Rudiger	Max Planck	J.Livas	MIT
R.Schilling	Max Planck	M.Burka	MIT
		A.Jeffries	MIT
J.Hough	Glasgow	P.Linsay	MIT
B.Meers	Glasgow	P.Saulson	MIT
B.Schutz	Cardiff	R.Weiss	MIT
H.Ward	Glasgow		

LARGE BASELINE PLANNING AND ENGINEERING

I.Corbett	Rutherford Appleton
R.Elder	JPL
J.Kien	NAL
V.Lobb	JPL
F.Schutz	Caltech/MIT

EXPERTS IN LASERS AND OPTICAL TECHNOLOGY

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R.Byer	Stanford	H.Kogelnik	Bell Labs
S.Ezekiel	MIT	P.Silverglate	Perkin Elmer
C.Volk	Litton	J.Hannon	Kodak
Kotik Lee	Perkin Elmer	*A.Szoke	Livermore
L.Hackel	Livermore	A.Siomba	Perkin Elmer

OBSERVERS

A.Komar	NSF	*F.Allario	NASA Langley
R.Isaacson	Illinois/NSF	M.K.Wilson	NSF
H.Willard	NSF		

*did not attend