KAFKA ON THE CONFERENCE CIRCUIT

[1976] I discussed it with Weber in '71 at the conference in Copenhagen, GR6. And Weber of course did not agree. But the discussions did not come to an end because Weber had to leave suddenly because his [first] wife had died. Then in the Spring of '72 I wrote an article for this Gravity Research Foundation called `Are Weber's Pulses Illegal' and this made Weber very angry. Then in July when I came to Varenna as an observer I just wanted to meet Weber there and I remember that when I came there was a table for lunch and Weber and Dicke and some other experimentalists, I think Fairbank, were sitting there, and Weber say 'hello, this is Peter Kafka who called me a fraud' [laughter]. Of course I had not called him a fraud -- if you need it you can see what I said.

WEBER ON DEDICATION

[1975] ... is having someone who is dedicated, who wants to work on the experiment until he's sure its working properly. I think that's a key issue. I can't recall ever having set up a complex experiment which worked well when it was first turned on ... With the sort of atmosphere and the sort of situation [which we have now] people aren't likely to put themselves out to confirm the earlier results ...

FAIRBANK ON THE START OF HIS PROGRAM.

[1972] In 1970 [the correct date is 1969 -- HMC] there was a conference on general relativity at University of Cincinnati. At that conference Weber gave some of his results, and I made some comments about cooling down a detector and looking for the Crab pulsar.

As time went on, of course, Weber got signals, and apparently he was getting coincidences, and the theorists began to talk of the signals one could get from collapsing supernova and eventually black holes and so we shifted our interest to looking at these pulse signals. ... We were interested to see whether we could build a detector that would see the gravitational radiation that was predicted to come from these events [supernovae in other galaxies]. Weber was obviously seeing something that was obviously greater in intensity than was predicted ... either there was some startlingly new aspect to gravitational radiation, or Weber was seeing something else. And we felt if we could build an antenna that was sensitive enough to see the predicted events, it would of course see with very high resolution what Weber was seeing.

Now the only way that one can improve the sensitivity of these detectors appreciably is by cooling them down to low temperatures. The reason for this is that one is looking at the present time for events that occur in the order of a thousandth of a second and the resonant frequency for these events is such that if you detect them with an antenna the ends of which are connected by communication through sound waves, so it has to be resonant for sound velocity. And the gravity waves are moving at the velocity of light ... and so one loses the ratio of sound velocity to the velocity of light squared in the sensitivity of the detector. But there isn't any other practical way to do it other than mechanical detectors. ... The advantage of a mechanical detector like Weber's is that one isolates it from the vibrations of the earth, and it seems like the best kind of detector. So both for physical size and also because of the resonant condition, one is limited to a bar of the order of magnitude of the size of Weber's if one is looking for that kind of signal. So the only way of increasing the sensitivity is decreasing the background noise, including the noise of the receiver, and have enough sensitivity to see a smaller signal.

Now the noise in the bar goes ... as [the temperature]. So in principle if one could cool the bar by a factor of 100,000, one could improve the sensitivity of the detector by 100,000. ...
... In addition to this one has two other advantages of going to low temperatures. One can put a superconduction shield around the bar, and this completely shields out electromagnetic radiation. So one can eliminate the possibility that you are getting electromagnetic pickup. The other advantage is that if one coats the bar with superconducting Niobium-Titanium one can float the bar on a superconducting magnet and this makes a much better isolation system from mechanical vibration. It can be -- after you do as well as you can with springs, you can have the last stage with a magnet. It also means the bar is supported along its entire length instead of hanging from a cable, which also eliminates the possibility that the bar is creeping. It also means that you have a technique for supporting a very long bar. If you wanted to do the experiment with a 75 or a 150 foot long bar you would have to go to something like superconducting magnets.

So it looks in our calculations, which were done jointly by people at Stanford and Bill Hamilton at LSU, that we could gain by at least 10^5 by going down to [a few degrees], so we set out to design a detector that would take advantage of this.

FAIRBANK ON EXPERIENCE

[1972]... one reason that we can do it is that we've been working in this direction for a very large number of years. We're doing the gyro relativity experiment in space which involves a very perfect gyroscope at liquid helium temperatures and a cooled telescope. And we're building a superconducting accelerator -- we're doing some other projects of this kind -- so we've built up quite a technological background -- and one of the motives from my standpoint for doing this was to build up this technology, because I felt that if you could reduce the noise like this there were very fundamental experiments in physics that could benefit from this. ... So the gravitational radiation, I think, was a logical companion to the use of low temperatures for these things.

PIZZELLA ON THE START OF HIS PROGRAM

[1995] In 1970 I decided to start a gravitational wave experiment after hearing from Weber. So I still had to decide which kind of experiment. I knew that I want to look for gravitational waves but I didn't know exactly how and then in 1971 I learned about the proposal by Fairbank over the cryogenic containers. So I knew right away that that was the kind of experiment I wanted to do. So in 1971 we started a collaboration with the University of Stanford -- Fairbank -- and with the University of Louisiana, with Professor Bill Hamilton -- and we agreed to construct three large cryogenic antennas. And then we started right away, and then we started constructing an antenna, which later we called the EXPLORER, which you can see in the picture on the wall there (indicating).

And after we started this experiment we found out that in order to properly construct the experiment we had to learn many things. We had to construct a prototype so what we did was to -- we did two things -- we constructed a cryogenic smaller antenna which you can see ALTAIR, is the name, and also other smaller antennas. ... and then we also constructed a room temperature antenna, which we call the GEOGRAV because we thought we could perhaps learn something about the noise coming from the Earth -- the vibrations of the Earth. So we built two types of detector. Also we continued constructing the EXPLORER detector. We finally installed the EXPLORER detector at CERN and it started to operate about ten years ago but not continuously because one has to make improvements. So we take data for say one month and then stop to make improvements then we take the data for another month and so on. Right now this detector is in operation. It is the most sensitive gravitational wave detector in the world; in fact it is easily the most sensitive because it is the only gravitational wave detector in operation right now! [Laughter -- this, remember, was 1995]

FORWARD ON EARLY DAYS IN INTERFEROMETRY

[1972] Now its very difficult to make a wideband antennae and four guys -- three or four guys -- tried and none of them were successful. [A], who still works for me, he is now working on an artificial intelligence project; and [B] who is getting his PHD now at Caltech and [C] who now runs one of the electronic engineering groups -- and err somebody else, but I've forgotten all the people. Um, I'd been working on it and this guy [Phillip Chapman] walks in off the street and tells me how to do it!

So, err, I put together a [inaudible] team. And more important was that Phil had a pot of money and err could help support this. All of this [gravitational wave detection] work has been supported by Hughes at about a one man level for nine or ten years now. And usually a project only lasts three or four years on internal money and by that time you should have been able to get some outside support.

So we started working on the design we ended up with our present design [plus or minus a few complications?] and I'll take you down and show you what it looks like and let you listen to it [the output of the interferometer was sent to a loudspeaker so you could hear its vibrations]. And the task that Gay [Gaylord Moss] was given was to come up with a laser that was quiet enough so that we could go down to here [indicating a noise level on a diagram on the blackboard]. And we tried normal lasers -- it didn't work -- and we found the Spectra Physics 119 would work -- it didn't get us quite there because there wasn't much power in it. And we understood then why it worked -- why the others didn't -- and we ended up with a Spectra Physics 125 which is a powerful laser and made it go single mode and got rid of the noise and Mr Moss essentially did all the work and got the laser so that it could measure displacements like this over a short distance.

And then next job was to make it measure vibrations and displacements like that over long distances.

And now we had an antenna that is equivalent to an eight-and-half meter long bar. [It] will measure at any frequency from 500 cycles to 20,000 cycles with this kind of sensitivity. And we got it really working well in October -- and we spend -- its still quite sensitive to vibrations -- people walking round the room and acoustic noise. We can only operate it late at night and so for the past two months Mr Moss and I have been uh -- uh taking shifts we've worked from one or two in the morning until seven in the morning, when people start arriving. And we'd get the antenna working and we'd tape-record the output -- on an Ampex tape recorder -- and we'd sit there and listen to it ... and, err, monitoring its output, and making sure that it's kept in working condition, and it's still a practical research tool

And we intend to collect about 100 hours of data. We're about 75 hours through now -- the antennae -- as we lived with the antenna, we learned various things were wrong with it and we've been constantly improving it in terms of getting rid of excess noise. And we now have a batch of tapes and I've been writing letters -- and [getting] in communication with the various people that I know telling them what we have and saying that, uh, what we'd like to do is two things: one is analyse our tapes see if we find anything unusual on [them]. And we found a few things that are unusual in the sense that they are chirps and tones and clicks that are coming through the laser interferometer [and] that are not on the laser, cos we're monitoring that. And they are not in the room because we have a microphone that's turned way up so it even hears our stomach rumble. And it is not on the floor because we have a seismometer. And ... its not on the line because we have servo system for each monitor. We think we've covered most of the possibilities.

I've just been down arranging to have the data digitalised and analysed and we'll be working that. Well end up with something like err -- oh my guess is on the average we've been getting off the tape so far, about a hundred things that we'll obviously see. So you'll probably weed it down to something like twenty after we do cross correlation between the signal channels and the monitor channels because there may be things there that we can't hear because our ear isn't as good as a computer -- but they are there nonetheless.

And those twenty, we'll see if there is any reasonable explanation for them. For instance it could be that the microphone just overloaded, and it was a sound of somebody moving you know, ourselves moving in the lab. And if it was the usual noises that we hear we won't count it. We may end up with one or two unknown events that are unexplainable and we'll publish the times um -- in GMT.

At the same time I've asked other people to tell us when they've seen anything unusual. And we'll go through our tapes and really take a look at that period there may be something there that we missed. And I have asked -- I've just written to Weber requesting that he send us, uh -- he's getting about 5 events a day now and we have a stretch of a whole day's worth of data -- not a day's worth but 24 hours worth of data -- we should have ten events in that. So I have requested that he send me the times of those ten, if its a good stretch of tape, and we'll go through all of it and see if we can see anything unusual

WEISS, FORWARD, AND COLLABORATION

[1972] Weiss: Forward, I know him well.

Collins: Have you ever visited him?

Weiss: No, but I know pretty much what he has -- he comes here a lot. He's actually always here on recruiting missions for that company [Hughes]. I know exactly what he has -- we've discussed it, and he has all my proposals and he has all the stuff I've written on this over the years. He started before us but its fundamentally my idea.

Collins: Will you want to look at his set up before --

Weiss: Well I know what he's got and I don't see much point in it. I mean, the kind of stuff he has, we've built here too, of a different variety. He's really built just an interferometer. And years ago we built an interferometer here that measured very, very small displacements but didn't use it for gravity waves -- we used it for something else just as crazy. So you can say, well the problem of working together has been brought up quite often between Forward and myself. And at this stage, before there's any large amounts of money, it's probably better to work independently. And then when it comes down to the point when you have to make a decision on a large installation, then you can't build it independently. You know, whatever techniques come out of what he's done, and what I've done, and eventually you put a thing that costs a million dollars together, or something like that, or a space experiment, you've got to use the best that exists. You see so it's not quite at the stage yet where you want to collaborate.

[1975] Forward: Rainer and Phil Chapman and I were trying to put something together to work together, but just couldn't. Part of it I'm sure was just Rai's and my personality. I'm in an industrial organisation, and what I wanted to see proposed was building two systems -- one here and one there [MIT] -- and doing cooperative work on the research. And the MIT administration refused to write a joint proposal with an industrial corporation where the industrial corporation actually did some research.

And finally Rai submitted a proposal himself. It was reviewed by Maischberger in Germany who said `gee that's a good idea, I'm going to build one myself,' and proceeded to build a duplicate.

There's a little bit of a problem there in terms of professional -- whether he should have done it or not. But [Maischberger]'s written a letter to me asking if I want to cooperate with him in building a laser interferometric antenna. I don't know whether he's written to Rai Weiss. But on the basis that the Germans were gonna go ahead and fund a copy of this proposal, NSF, which probably would not have funded it, gave Rai some money. So he's getting $100,000 I understand, over two years.

His proposal was to build the instrument that we already had built in '72, basically. So, because of the MIT administration Rai's got to start from scratch rather than start from where we were in '72.

KAFKA REVIEWS WEISS'S PROPOSAL

[1976] Kafka: ... you know that they [his Munich experimenter colleagues] are now considering the next generation experiment. And there, of course, this is again in a way an imitation, because this was originally Rainer Weiss who proposed it from MIT. And this is also interesting for a sociologist -- how information sometimes spreads. Because I didn't know about that and I got a report from Rainer Weiss, for the NSF, to referee and usually such a thing is of course confidential and you say your opinion and don't mention it to anybody. But since I don't really -- somehow I feel as an outsider in that field, and I thought it would be reasonable to talk about it and treat it as a public, more or less, public matter and also I didn't understand much about the experimental possibilities so I had to talk to the experimentalists anyhow. And since they were following similar ideas anyhow, then it was unavoidable that we discussed all these things in detail. And I simply gave them copies and I told Weiss that I had talked about it.

Obviously he was extremely angry about it. Although I don't really see why because I mean we would always have given him credit. But I remember when I wrote him letters to apologise for my procedure he didn't even answer and he was probably very angry. I don't know -- perhaps he didn't have time to answer, or wasn't interested. But I think he was very angry because he didn't get money from the NSF. Certainly not because of my report -- I had certainly written that it would be very interesting and responding and so on. But somehow they were just cutting down on their gravitational experiments so he obviously didn't get the money so he must have been angry that Munich used his report.

Then afterwards I found out that similar things had been written up somewhere in a published report, or a distributed report [THIS IS THE RLE REPORT] so it was not such a, such a bad thing that I used his report for the NSF.

Still, it is an interesting question how I should have behaved in that case -- I'm not quite sure. I think I haven't done anything bad because I thought this: `science isn't secret and one should be able to use the information.' Other people probably also use the information and don't say it. That's probably the way it's done. And then I thought since I discussed it with them, then of course, then I felt I should mention it to him and tell him that we knew everything about this report. But somehow it makes people angry obviously. Yeah -- so that's the history from my point of view.

WEISS'S 1976 LETTER TO ISAACSON

November 18, 1976

Dr. Richard Isaacson
National Science Foundation
Washington, D.C. 20550

Dear Dr. Isaacson

I am writing in regard to the N.S.F. Grant MPS75-04033 entitled "Interferometric Broad Band Gravitational Antenna". As you know this grant has been given a no-cost extension from June 1976 to the end of November 1976. At present there are about $25K remaining in the grant which are not committed. How this has happened I will describe below. I am asking to have the grant extended for another 6 months and in addition during these 6 months will submit a grant renewal request.

In order that you might understand all of this, I have to tell you some of the history of this project. The idea that one could use free masses sensed interferometrically as a large baseline gravitational wave antenna has been evolving in my mind since 1971. At that time I had several undergraduates working on aspects of such a system, in particular senior theses were carried out on laser noise measurements and suspension design and testing. By 1972 a very good graduate student, Kingston Owens now at the Princeton Plasma Laboratory, began construction of a small interferometric antenna which was to become his Ph.D. thesis. The work at that time was supported by the military under the Joint Services Electronics Program. By the end of 1972, the military had lost interest in gravitational wave research and I applied to the N.S.F. for research support. The proposal to the N.S.F. was unfavorably reviewed at the time most likely because it was too big a step from acoustic gravitational wave detectors which still looked promising especially as the Weber experiments had not been convincingly confirmed or refuted. I made a request from the Sloan fund at M.I.T. which was granted. This money, about $20K, was used to buy equipment but could not be applied to salaries. Kingston and I continued to work on the antenna through 1973 and evolved a second proposal to the N.S.F. which was approved in June 1975.

Before this time however Kingston's support had run out and I put him onto another project. In September of 1975 a new graduate student, supported by other funds, began working on the project; frankly he did not work out and left in January 1976. In the course of this year I have made numerous attempts at interesting other students in the experiment with little success. Gravitation research, although viewed as fascinating, is considered too hard and unfortunately profitless not only by the average student but also by much of the physics faculty. In short, the atmosphere if not outright hostile to such research is certainly sceptical. Even I was becoming more timid and in fact was seriously considering returning the unspent funds in the N.S.F. grant and getting out of the gravitational radiation research business entirely.

Several factors have increased both my interest and optimism. First, in the course of running a N.A.S.A. sponsored study on the uses of space in experimental relativity, it dawned on both Peter Bender and me that interferometric systems using the large baselines in space afforded a real chance to look for gravitational radiation from slow objects such as stellar binaries. Although these measurements appear impossible from the ground; in space the dominant noise may well be the cosmic proton flux and a 1 kilometre baseline system, using laser interferometers loosely coupled to a frame, could measure the radiation from several binary systems with integration times of the order of a few months. Bender and I will study this further, hopefully with a small grant from N.A.S.A. The important thing is that aside from just setting better upper limits on the gravitational radiation flux, there is now an actual, albeit remote, goal to continue development of interferometric antennas. The second point is that I have found a good physicist, Peter Kramer now a post doc at Arizona, interested in working with me at M.I.T. on the prototype antenna.[i] He would like to start working on the antenna as soon as possible. Finally, I hear from various colleagues that the transducer problem in the cryogenic high Q bar antennas is formidable and this is driving several groups toward interferometric sensing systems. In particular Ron Drever has come to the same conclusion as I that that interferometric large baseline systems are the most promising direction for the future.

In short, this letter is intended to tell you that damn little actual progress on the prototype antenna has been made in the past year aside form some minor work on fast electronics for the fringe detection system. I know that on the basis of the experimental progress alone a renewal request at this time would not survive a review procedure. On the other hand some real progress in the theoretical developments has been made and that now with the active interest of another capable physicist, progress in the experimental work on the prototype antenna can be expected.

I would like the N.S.F. to continue the no-cost extension for another 6 months. Kramer can begin in February of 1977. About $11K of the remaining funds would be used to support him; the rest, amounting to $14K, used for materials and services, especially technician time and shop costs. During this period Kramer and I would formulate a renewal proposal to the N.S.F.

Sincerely yours

Rainer Weiss
Professor
Department of Physics

WEISS WANTS TO LOOK FOR KNOWN SOURCES

[1975]The view of many theorists is that gravitational radiation has got to be there and they're not interested in that. They're already interested in using gravitational radiation as a way of looking into astrophysics. As a way of looking at supernovae, looking into whether there are black hole collisions ... they believe there is gravitational radiation and they want to use it now if they could get a detector to do astrophysics with.

That's actually the view of many of the theorists that I know. They're not so charmed by the idea -- `well you can discover gravitational radiation' -- they know in their bones it has to be; they don't know the propagation velocity but everyone assumes it's `c' [the velocity of light].

My view is that somebody ought to detect the damn stuff just to see if it's there. So if you can dream up a system that will look for something so that you know damn well when you look out into space that it has to radiate, that's what you ought to look for -- that's my instinct -- it's a cautious instinct.

THE BLUE BOOK DISCUSSION

The review given above has addressed quite a few individual source models, with some attention given to technical issues which limit our confidence in our understanding of the sources. Several points need to be stressed, independently of any particular model.

1. Although the predictions have large uncertainties, reflecting our ignorance of important aspects of astrophysical processes, there are many reasonable arguments which do predict detectable amounts of gravitational radiation. Indeed, one might consider the uncertainty as a virtue, for it shows how much there is to learn from the study of gravitational radiation. Actually making the gravitational wave measurements is the best way to learn about stellar collapse, for example.

2. The models considered by most theorists span only a narrow range of the space of possible sources. Note that the three best studied sources (supernovae, pulsars, and compact binaries) all involve, either directly or as an immediate antecedent, the gravitational collapse of a 1 M_O [M_O = the mass of our sun] to a neutron star. It is not that is this is the only chain of events that can lead to production of gravitational waves. It is just the only well-known process.

[THE $_O$ IN M_O SHOULD HAVE A LITTLE DOT IN THE MIDDLE]

3. When gravitational waves are finally detected, it will open a whole new window on the universe. To use a phrase of Thorne's (1982)[ii], the information carried by gravitational radiation is nearly "orthogonal" to that carried by electromagnetic radiation. This is true in the sense that electromagnetic radiation is usually generated as the superposition of a multitude of atomic processes, while gravitational radiation can only be produced in large amounts by coherent motions of large masses. Furthermore, the collapsing core of a supernova is obscured by the envelope of the star which has great optical depth. The collision of two black holes may take place in the absence of sufficient gas to generate substantial amounts of light. Thus, we can expect not only to see more clearly into processes we already know about (such as stellar collapse), but also to be made aware of phenomena heretofore completely hidden.

4. The surest way to make new discoveries is to employ radically new means of looking at the universe. The early days of radio astronomy and X-ray astronomy were so exciting at least in part because the application of new technology revealed phenomena whose existence had barely been dreamt of. (This has been discussed recently by Harwit 1981).[iii] The next generation of gravitational wave detectors will achieve more than five orders of magnitude improvement of sensitivity (in energy) over the best presently operating detectors. In addition, the search will be extended from a few extremely narrow frequency channels to the entire band from 30Hz to several kilohertz. This is not merely incremental progress, but a brand new opportunity for discovery.

5. The detection of gravitational waves remains the single most important task of experimental relativity. The existence of and nature of gravitational radiation is a prediction of Einstein's which has remained unconfirmed for over half a century. As Eardley (1982)[iv] has pointed out, the current state of this field of research is analogous to the state of particle physics between the demonstration of the necessity that the neutrino exist (the analog of Taylor's important work) and the actual physical detection of the neutrino itself. We cannot claim to understand gravity until we have detected gravitational waves.

WEBER DISCUSSES CALIBRATION UNDER THE NEW CROSS-SECTION

The first calibration, the one that the Australians have said disproves the [new] cross-section, was done by Joel Sinsky and I think that's referenced here, and I think that's a harmonic ... The pulsed calibrations at the end of the bar -- a bar and a plate at the end -- the results of those are [microphone drops to floor -- we scramble for it] -- again the forces there are consistent with all the cross-section cardinals? since one isn't interacting with the whole bar one is just interacting with the one quadrupole on the very end and -- er- {Collins: so you say this sort of calibration would or would not see the 1984 cross-section?} Ah -- well, this is certainly consistent with it because that just says that the cross-section is a result of interacting with individual planes and here your interacting with an individual plane and so ... one isn't interacting with the entire bar. [080] ...

The big cross-sections are valid for a gravitational wave that's incident and one says that there are 1010 planes and that quantum theory says that you can exchange gravitons with any one of those planes without identifying the plane ...

[277] {Collins: What's the ratio of the cross-section for pulses as opposed to the harmonic input? Is it possible to say that?} Well it depends on the numbers involved. For the aluminium bar, [lots of sounds of Weber doing calculations on the board for about five minutes] ... so putting all this together we get a number like 106 extra -- the extra factors here -- so for the continuous case, so for the aluminium bar its about the same. ... I don't think there's anything wrong with the calibrations the most important calibration hasn't been done. The most important calibration would be to get a pulsed source that would interact with the whole bar the way a gravity wave does. And it won't do to have a rotating wheel because that's not like a pulsed excitation, and if you put a pulsed source near one end of the bar, that's not like a gravitational wave either and so one would like to have a pulsed

source that's 30 -- 40 feet and puts out enough energy to be seen and I think you could do that the more sensitive ones, but I'd be surprised if you could do it with ...

WEBER ON SN1987A SEQUENCE OF EVENTS

[1995] ... and what to me is most impressive is that the theory that I published in 1984 and 1986 predicted the pulse heights [of gravitational radiation from the supernova] quite accurately. And it seems to me that if you see something like a supernova and you see pulses, and then after you've seen them you measure them, you write and publish your theory and say your observations confirm your theory, that that's not as impressive as if you publish your theory in advance of the supernova and then you look at the pulse heights and distances and you find that they check the theory very well.

SCHUTZ ON WHY HE ATTACKED THE SN1987A CLAIMS

[1996] <u>Collins:</u> Can you remember when you decided to write that paper [criticising the claims of Weber and Pizzella about the supernova].
<u>Schutz:</u> Many years before it was published. Initially, I mean, I guess I felt soon after I saw the claims were being made, and the publications, and the talks at conference and so on, and I would talk to my colleagues and feel that they -- the people particularly in the laser-interferometer business -- not only universally had misgivings about the way the statistics were being handled and the claims that were being made, but also a great fear that, that here was another Weber incident in the making where this field would get a bad reputation among other physicists, and I guess I formed the conclusion pretty soon that we had to police ourselves in a way, that there had to be a critique of this coming from within the gravitational wave community. And I spoke to a few other people in the field and they all agreed that that had to be done but none of them wanted to do it. And I had very little to lose in the sense that I had no particular professional relationship with these people -- I had not worked on bars -- although I had friendly relations with them, so I thought the right thing to do was put a student onto it ...
<u>Collins:</u> You said there was a `fear'. What did the fear consist of.

Schutz: Well I must say that what people were afraid of did not happen, but it's not because we published this paper. What people were afraid of was that as the Rome group and other people assessing things get more and more, let's say, entrenched, in their claims they would be tempted to move from a sort of reserved position they were originally taking -- we have these events but we don't understand them -- to a stronger position which is the only things these things could be is gravitational waves. And people were very afraid of that claim, because Weber had made his claim -- you know he'd said `I can't imagine them being anything but gravitational waves,' and it turned out that they weren't and so in many ways that gave the field a bad reputation. If you said to a fellow astrophysicist you were working on gravitational waves, they would sort of snigger about it.

And the field was just becoming reputable again as we were pushing the interferometers as respectable, as having a respectable chance of getting to the right sensitivity and so there was a lot more money at stake now. Funding agencies were beginning to say we could imagine spending a lot of money on this.

To have a bar detector group come up and say that they had seen gravitational waves then to have that shot down in flames would probably have been very damaging to the prospects of extracting money from funding agencies for the interferometers.

It could have gone the other way. It could have gone `the interferometers will save the day and settle this question once and for all' but it just made people feel that we'd get another reputation of being flaky scientists.

Collins: The aspect of the story that makes sense to me is that if gravity waves could be detected by bars, what's the point of building LIGO?

Schutz: That wasn't in my mind at all because I never believed that this was real. In fact I think it might have been the other way around. If they really had seen gravitational waves and the ironic thing is -- these were room temperature detectors -- if they'd had their cryogenic detectors turned on at the time of the supernova I think they might well have seen gravitational waves and I think that would have done the field an enormous amount of good. It wouldn't have stopped the development of interferometers because you'd suddenly have had a calibration point. You'd have know what excitation was produced by an event that was relatively near by -- the Large Magellanic Cloud -- astronomers would have said `that event was rare -- once every thirty years' but we suddenly would have had a calibration, and we would have said with complete confidence with the interferometers, `Now that we know what these things do we need an increase in sensitivity so that we can improve the event rate from once every thirty years to once every week,' or something like that and that was always the argument for the interferometers.

Collins: That makes perfectly good sense if its cryogenic bars, but not if it's room temperature bars, because if its room temperature bars its new physics.

Schutz: Yes, that's right, that's right. But that didn't seem to me a serious possibility that this was likely to be the case. The energetic grounds and everything else were so outrageous.

Collins: But suppose I say to you it's not you who's important, it's the people who are doing the funding who are important and they're less sophisticated.

Schutz: That's right

Collins: Would you say that it's important to make sure that it's known among a wider circle that room temperature bars aren't detecting this. In other words, for the funding agencies, room temperature bars seeing the supernova are a threat to LIGO.

Schutz: I see what you're saying. I don't recall that that was ever part of my motivation. I never -- the funding agencies -- the people in the funding agencies that I talked to. For instance the person who is in charge of NSF funding for LIGO was Richard Isaacson. Now he's a very sophisticated relativist. He would have formed the same opinion as I did right from the outset. So I don't think it would ever have been a serious consideration for him. He might have had to defend that position within NSF.

Collins: But he himself is not in a position to make that $300M allocation.

Schutz: That's right. ... what I didn't want to see was that that would become an issue and give the field a bad name. Because it seemed to me it would effect the funding but not because people felt in a sense that interferometers were doing something that could be done more easily by somebody else, but in the opposite way, that interferometers were participating in a field that was full of crazy people.

Collins: I find that more difficult to make sense of. Even a poor old sociologist can see that there's a huge difference between Weber and trying to detect one hundred million times less.

Schutz: Well yes -- I think this bears on the sociology of the approval process for the interferometers, so there's a much bigger question here of competition between different fields for funds and the need to convince people who are not in the gravitational radiation field that it's worth spending money on this project. And this was handled in different ways in different countries. So in America it seems that it's always been the case that astronomers were always the most sceptical about the value of building these things and so in the States the NSF arranged it that the funding always came from physics, and that astronomy was not a major motivation for LIGO and that astronomers were kept on the side and they took a lot of sniping shots at it, but they were outside the mainstream of approval. ... And I met many of my astronomy colleagues who were by nature opposed to the idea anyway, and who would have been very happy to have been able to point to my bar colleagues as examples of bad scientists who -- you know -- if -- if there are these guys in gravitational waves doing that how do we know your other colleagues in the interferometry field are not going to be just as crazy. And we put all this money into something and maybe you see something maybe you don't but how do we know when you come and claim to have seen something that you're not just as crazy as everyone else.

That was an argument I was afraid of having to deal with, so that was the reason, the main motivation, in my case for writing this paper. ...

Collins: Was Rich Isaacson, [the Program Director for Gravitational Physics at the NSF] concerned about the funding implications?

Schutz: Yes. Rich was very happy when [he learned the critical paper was to be written]. ... So were [some of the principal scientists at the American and British interferometer groups.] [A principal scientist at an American group] helped get the paper published. ... Interestingly Phys Rev didn't want to publish the paper in the first place, not because there was anything wrong with it, but because it was a criticism of work that had not been published in Phys Rev to start with. ... And Phys Rev just wanted to wash their hands of it -- they didn't want to be involved in this. And [the principal scientist] wrote a letter to the editor supporting [the] assertion that this was a global issue, not one to do with bad selection of articles for journals, but one that clearly affected the United States because of LIGO, and that it was appropriate for Phys Rev. So Phys Rev did publish it.

...

Collins: ... let me give you my theory so you can see where I'm going -- I believe that within the inner networks of scientists, the people who really know what's going on, often a very small number, people know what's right and what's wrong and there often isn't a need to write a paper to rebut something.

Schutz: Yeah, that's right, that's the point of view that the editor of Phys Rev took, I think.

Collins: So, in this particular case, the fact that [all those people wanted it published] I want to explain that that bears a relationship to what was going on with the funding of LIGO at the time. That's the hypothesis I'm trying to pin down.

Schutz: Oh yeah! We all saw it as related, and we all saw the publication of this as helpful in the case for LIGO.

THORNE ON HIS REACTION TO WEBER'S NEW CROSS-SECTION

[1995] He started announcing this [the new cross section] at conferences. And we were in the early stages of trying to get LIGO developed and approved and so our funding officer in Washington

[Richard Isaacson] was rather concerned about this and the impact on LIGO. If Joe [Weber] is right then there is no need to develop such a big expensive instrument.

... I read what Joe wrote. I think the first thing I read was probably what he published in the proceedings of an Indian conference which he sent to me in preprint form [probably Weber, 1986]. He probably sent it to me with a cover letter at the time ... So I read it, I spent some significant time looking at it, thinking about it. I invited Joe up to give a seminar on it -- he was at Irvine -- because I felt it ought to be exposed, the community ought to think about it. But I already by that time had pretty clear views on it. ...

It fairly quickly became obvious to me that Joe was simply wrong -- that the analysis was severely flawed in a number of ways. But I did not want to make a big deal of this in public. I felt that Joe had pioneered this field. He had pioneered it in a wise direction. ... And I just did not want to be attacking him in public. ... and I did not want to do to him what [another scientist] had done [a forthright public attack] ...

And so I adopted a policy for myself on this that I was not going to write anything about it at all, but, whenever Joe gave a talk on this at a conference, if I was present at the end of the conference I would stand up and say, unequivocally, that in my view this analysis was wrong, that Joe and I had discussed it, we had agreed to disagree, and that I would be happy to discuss details with anybody who was interested privately. And so this became a sort of minuet [between Joe and me at conferences]

That occurred conference after conference between Joe and me. Joe would get up and talk about this and I would reply in that way -- a brief reply. I felt my credibility in this was such that people would listen and that would be enough. Which Richard Isaacson felt was not enough because it was not just the community that was involved. And he felt that something had to be written.

<u>Collins:</u> What do you mean, not just the community?

<u>Thorne:</u> We were in the early stages then of the process of trying to get LIGO approved at NSF and then ultimately through Congress. And that process involved a much wider group of people than people who were experts in gravitational waves and relativity. ... Because LIGO is such a big project, and the most expensive project that NSF has ever done, it was inadequate to just have me as a member of the community stand up and say that Joe was wrong and people within the community being pretty clear on it. They [the outsiders] could see that as well. He felt it had to be in writing so it could be shown to people in writing.

Uhm, I was very reluctant to put something in writing in Physical Review, which would be the natural place. Again, I have a lot of respect for Joe as the pioneer of our work and he is in the late years of his career and has suffered a lot of personal pain in this field. I just didn't want to do that. And so I wrote a piece for a conference proceedings which ... basically discusses the flaws in his analysis, and I let it lie at that.[v] ... the situation changed to some degree when Preparata got into it. I thought seriously of writing a critique of both Preparata and Weber for Physical Review, but I never got around to it. And I think, probably, analysing my own psychology, I didn't get around to it -- I put it a low priority -- again because I just did not want to be doing that to Joe in that public a venue, and I also, it was my sense of the political situation that we were not in that much danger from Joe. In terms of LIGO being funded, and so in the end I let it rest.

I discussed it at great length with various colleagues, and Leonid Grischuk who is a very close friend of mine, did write a piece that I think he published in Phys Rev that was basically a follow up of my piece in this conference proceedings that laid it out for a broader audience, and also to some extent discussed the Preparata business, but I declined to co-author that. But Leonid, I thought it was sufficient for him to do it. I did not want to be seen -- I did not want to see myself in that kind of a role, any more public than having done it in a conference proceedings.

Grischuk has replied; in the end it's so obvious that his analysis does not make physical sense and the community understands that and we are no longer in a situation where it is necessary for us to explain that to a larger community.

Collins: So if there were still questions over the funding of LIGO --

Thorne: -- and if this were a significant issue in these questions then I might think differently but I don't see any need to fight those battles in public when the community has reached a consensus on it ...

WEBER ON HIS WORK AS AN EMPIRICAL TEST OF RESONANT TRANSDUCERS

[1995] as a person whose done both theory and experiment, I would say that if one kind of instrumentation produces data, and its not just one pulse ... and two copies made by an independent groups see pulses and also the two copies made by the independent group see the sidereal anisotropy, to me that's evidence that the centre crystal instrumentation has something going for it. And the other view is that since end-crystal instrumentation is supposed to be more sensitive if the end-crystal instrumented bars haven't seen anything it must be that the center-crystal instrumented ones are doing something terribly wrong. I'm afraid I just don't follow that argument, but the reason that there isn't room to stretch your arms in my office is that this other view has prevailed and the laws under which federal officials distribute money require review of proposals by a certain segment of the scientific community and if a certain segment of the scientific community says that it has built more sensitive detectors and found nothing, that gives the sponsoring agency complete legal right to deny funds to me otherwise they'd be criticised for it.

ANOTHER VIEW OF THE 1987A RESULTS

[1995] [Isaacson is] very much afraid -- I understand his point. [A] paper on coincidences might damage the LIGO project. ... If you talk with Isaacson, as soon as you mention Supernova 1987, he will start with -- I don't know what he is going to say. It is something which one should not even mention. Well, he will talk with you. He will tell you many things, I think, but in the United States it has caused a lot of problems, this supernova business. ... You can understand Isaacson's point of view. He sits in a committee where there are astrophysicists from other fields, and he wants to get the money for LIGO, and they ask him `Why do you want the money for LIGO, they discovered already this and that?' So, ... he's fighting for this money with other astrophysicists in other fields and he thinks, rightly, that if one -- Well, he's right, I agree with him in the sense that we must be absolutely sure that we've got something, because if we publish something that is not so sure, then we'll only cause problems. So I understand his point.

SCHUTZ ON PIA ASTONE'S VISIT TO CARDIFF

[1996] I can tell you exactly how that started. It started partly out of politics, in that, erm, I had been critical of results that the Rome Group announced. ... And as a way of kind of keeping relations between the Rome group and my group cordial, Pizzella sent Astone -- his main data analysis person -- to Cardiff for three months and we asked ourselves when she got there, what are we going to do together? And we started talking about things and we realised that nobody had really ever thought about this and I didn't understand why it was, and I learned a lot about bars from talking to her. And it just sort of grew out of that. And we realised well -- it's possible to think about these things, particularly in the near term, ... So it started out as a question of what can you do between a bar and an interferometer because we had a bar person in an interferometer group.

CERDONIO ATTACKS THE PERTH-ROME COINCIDENCES

[1996] Cerdonio: I think we should delegate as the next frontier in bars to understand better the sources of noise. David's been just doing beautiful work with electrical shocks and seismic disturbances ... so then, it must be something else. I think we should look internally. I believe that we don't have enough studies of mechanical creep of the structure. All the structure of these bars -- you see the rate is almost the same in all the three experiments -- roughly speaking what you see [indicating Pizzella] and what Blair sees is just what we see. In ... we did one study of internal mechanical creep. Because all the structure is in very high tension. In many places you have tensions which are very close to breaking. That's all I want to say.

WHAT IF BARS SAW GRAVITATIONAL WAVES?

[1995] Saulson: There would be no easier way to make the case for LIGO than to say, `Even at sensitivity levels that are way poorer than LIGO will achieve you can see signals and now if you build something as good as LIGO you will have a new kind of astronomical telescope.' I mean this is what we want to do anyway, but our biggest problem is convincing people that we will see anything at all. If there were good reasons to be certain we would see stuff except we would see it at much better signal-to-noise and with better bandwidth and everything like that, then all of us would relax, not just for political reasons but because we would know we were doing good science instead of marginal science. So no -- this is not it [the source of the competition]. ... the self-interest works the other way.

[1995] Thorne: It would be wonderful for us if bars saw the waves before interferometers were up and running, because we could tweak the design of our instruments on the basis on the benchmark provided by the bars.

[1996] Hough: I think most people wouldn't expect [them] to see anything, but equally well we'd be delighted if they do, and are very keen that these experiments continue, and hope that they do see something.
Collins: Why?

Hough: Because it would be a fantastic boost to the field if gravity waves are seen. So much can then be done with the broader band interferometers. In many ways the best thing that can happen is for a discovery from the bar detector people.

[1995] Collins: Was there ever a phase during the funding of LIGO when it was important to shoot down the claims of low sensitivity bars?

Thorne: That was view pushed by Joe and it's a natural thing for one to wonder about. And if I came from outside I would wonder about that. But I would have just loved for Joe to be right. I would love to see the field shaken up. It makes it much more exciting for a researcher if there is something fundamentally wrong with the way we have been thinking about things before and there was some possibility of real revolution in our understanding of how bars operate ...

HAMILTON DEFENDS RESONANT TECHNOLOGY AT THE JERUSALEM MARCEL GROSSMAN MEETING IN 1997

[1997]This is not an anti-LIGO talk -- let me start out by saying that. But this is what we thought they should do. And I think people ought to be aware of what's going on in the US at this point, because I think that some of us in other countries are going to face this same problem -- I'm afraid that they might if they aren't aware of it and take some action immediately.

We view the TIGA detector, or spherical detector, as being an important adjunct to a network of interferometers. As David [Blair] says, our experience comes from trying to look for events and our experience is that the more the detectors we have the better you are going to be able to do. What we are finding in the US right now is a very great tendency to believe that because the interferometers are potentially so much more sensitive than the bars, and because we have been working on bars for so long and haven't gotten any further than we've gotten -- you know $3x10^{-24}$ -- whatever -- because we haven't gotten any further than that the bars should be shut off -- should be turned off in order to --. Now that's not gonna happen. They are not turning us off at LSU or anything like that, but we're seeing increasing pressures to do so in order to save money for the interferometers.

The interferometers are expensive; the interferometers have potentially better sensitivity and so why not? Why not turn them [the resonant bars] off, save some money, and proceed ahead with the situation once the interferometers get working. -- Then finally we won't need the bars -- that's the argument.

I think that that's short sighted -- I said that this was a political statement -- and I suspect that we need to make is a case that I have not had the wit to be able to make yet. ... Maybe some of you with more experience in other fields might be able to provide a real service to gravitational wave detection by working up the argument -- Our tendency has always been to emphasise sensitivity -- OK? Now suppose you have two detectors, three detectors, whatever you will, of relatively large sensitivity, relatively good sensitivity -- let's say 10^{-22}, something like that. Suppose you have another detector, of a different type, with sensitivity of 10^{-21}, or something like that. If both detectors see events -- if the detectors are all operating at the same time, what does it do to increase your confidence in a detection -- leave out of it the total problems that the characteristic of the spherical detectors is much better directionality than you get with the array of interferometers. Keep into the account the kind of information you were getting from David about how no matter what you do theoretically -- and Guido has done this in years passed -- no matter what your theoretical estimates are on how big you signal to noise ratio you need, in actual fact when you come to doing it in the real world you find it is more noisy than you would expect theoretically.

And so if someone has the wit to be able to couch this argument, I think it might do a great deal for the all of us. But that's all I have to say.

A questioner then remarked that GEO600 as a small detector has the same problem. Hamilton replied:

[1997]I agree with you, I think we all have -- the GEO600, the TAMA group, I think they all have the same argument to make and I expect the political problem is going to come. Well let me pose it this way -- we put the TIGA proposal in to NSF a year ago -- a group of people in the US. And the proposal was very well reviewed -- maybe some of you were reviewers in which case let me thank you because you did a very nice job on it --. However, the decision was made that because LIGO is so expensive, and because so much money is being put into interferometers, let's wait, that this may not be, say ten years down the road, may not be an effective detector. And if that's happening to us, why I expect it is something that will begin to happen other places.

From the floor a woman spoke

[1997]Can I just squeak (sic), I think the main argument you should stress is that the bars are working and the interferometers may need a long time -- much more than they think.
<u>Pizzella, who was chairing the session, spoke sotto voce:</u> But they don't think so.

Hamilton: We make that argument -- That's the first one. But no -- a good argument -- the topic is called diversity in the submarine signal detection business, and I just haven't been able to put all the pieces together to make it work right yet. But maybe someone here -- well I know that most people here are smarter than I am, so maybe someone else could do it.

HAMILTON DEFENDS RESONANT TECHNOLOGY AT THE CERN AMALDI MEETING IN 1997

When I started preparing my slides 3 weeks ago I was very disturbed about gravity wave detection in general.

So I want to start my remarks by saying that if you see [me being] pessimistic, especially in the later part of this talk, I want to express at the beginning that after this meeting I am much more optimistic about our future, and about where we are going with gravity wave detection and especially, I'm much more optimistic about the future of resonant bars. I think you'll see the reason for my pessimism as we go along through the talk.

Also for the sake of everybody here who's supposed to be in the laser [interferometer] business, let me remind you all that though its hard to keep it in mind as you work on all the problems of building the interferometers, it's hard to keep in mind that we really are in the business of trying to detect gravity waves. So I hope that you will see from this talk today that there are some detectors working that are already pretty damn good. and that are trying to detect gravity waves now.

And to that point, let me tell everybody that we are having a meeting, those of us with resonant detectors, to try to work out the basic protocols for a data center, if you will, whereby events can be -- if events are seen on detectors -- events can be automatically disseminated to others with capabilities. ... [Disseminated] over the web, and correlations can be made and so forth. In other words we're beginning to expand the international cooperation that's been going on for a number of years -- the detectors in Italy and our detector for instance -- and the Australian detector now operating and the new AURIGA detector. So we're really on the verge of being able to have a really true international network -- the beginning of an international network to look for gravitational waves. So the data event centre is beginning to take place.

I would hope ... that the capability will be there for integration of this with the plans of the interferometers, for instance, when they get to the point of taking data. So I am much more optimistic now than I was.

Really my optimism comes from seeing the plans in Europe. In Europe we're seeing the resonant detectors -- the spherical detectors being actively planned and built in Italy [and] in the Netherlands. We're seeing the very strong possibility that we will be able to have a spherical detector in Brazil. So the future of resonant detectors is, I think, now considerably better than you're going to see in my slides. So let me just make that point.

I think that people tend to write off, in all the excitement about building new [interferometric] detectors -- and all of the fancy things that the new detectors are going to do -- people tend to write off what the existing detectors are already doing. Not everybody [does this], but people tend to discount them a little bit. So part of this talk is intended to show you, as I stated originally, that these are pretty damn good detectors. I think they are and so in part of this talk I hope to convince you that they are.

Now, the work that I'm reporting on I'm not -- I was not asked to give a review talk for everybody so mostly I'm going to be reporting on the work that we've been doing at LSU; I'll show you the individuals involved. ...

Here Hamilton went into a technical discussion about the different units of measurements used by interferometers and bars and how they are to be compared. He then explained that they had been attempting to improve the ALLEGRO [LSU] detector by installing a much improved SQUID [Superconducting Quantum Interference Device], the heart of the amplifier in a cryogenic bar, but that this turned out to have unexpectedly increased their noise levels by a factor of four rather than reduced it. This has given them a lot of trouble. He would explain a little later that he believes a lot of the trouble with the cryogenic devices is that they had not spent enough time concentrating on the SQUIDS but that new and much better devices had been developed and that he anticipated very great improvements in the future. He explains that when all this is working properly they expect a strain sensitivity 10^{-21} over a bandwidth of 20-30Hz.

We've been looking for continuous signals from 47 Tucanae [the half-fingernail-sized area he had already explored] and the galactic center. And I hope that you will be impressed with the curves that I'm about to show you. And we've spent a lot of time trying to propose the TIGA project in collaboration with what we call the `gravity wave coop' in the United States. And it is the aspects of this that had me really very discouraged earlier on. But as I say, seeing what's going here has got me considerably more fired up.

Now, when I say that these detectors are pretty good I showed you an old experimental curve that many of you have seen in the past. The black curve that is shown on here is the measured strain sensitivity of the ALLEGRO detector. ... Here, over a rather narrow frequency admittedly, we are at 7 or 8×10^{-22}. ... It's [the bandwidth is] not 500 Hz, it's not a kilohertz, but on the other hand the numbers that we're talking about as we get more realistic with interferometers, are not the numbers that we started out with ten years ago.

... And then let me just point out that measurements were done -- because we'll come back and talk about the spheres -- on explosively bonded aluminium -- and people have been scared to death of trying to make large aluminium spheres or something like that but Bill Duffy and Mark Bocko have shown that this probably is not a problem -- the losses of making explosively bonded spheres can probably be handled and the Q's would be high enough to make it worthwhile. ... Now one thing that we have been seeing in the United States is a very large feeling among a substantial number of people that resonant detectors have had their day, and they're not really very sensitive and we can't push them very much further.

But one reason I want to talk about this here is that it is one of these things: [...] If we're seeing this kind of thing in the United States, I would not be at all surprised that it begins to strike some of the rest of you. In fact I've already heard that the politicians are beginning to make noises about `We're putting too much work into gravity wave detection and we should --' You've heard the arguments -- `There's only a finite amount of money. If you're going to do this then you have to cut off that.' And this is the kind of thing that we're beginning to see, I believe, in our [resonant mass] end of things.

I said I wanted to convince you that resonant detectors are sensitive, so let me show you some even graphs. Evan Mauceli and his thesis work -- working with Warren Johnson -- decided to search our data. We have five-years of data. ... We have continuous data from 1991 through 1995. I've been trying for 5 years to get someone who wasn't a gravity wave experimentalist, namely a theorist, to take some of this data and take a look at it, and see if there's anything in it that can be done, or at least to work with us and tell us what stupid things we're doing, or if we're doing anything right. I have had zero success. Not quite zero -- let me give credit where credit is due -- Sam Finn took a long look at our data and others have promised to look at our data -- but in Palottino's talk he commented that we need more and more effort as we go along and we try to detect things; we need more and more effort on data analysis, and we just have not gotten it from the community. We're all in the business of measuring gravity waves. But it's been a very uphill fight. If I can attract anybody's interest in this talk I hope you'll talk to me or talk to Pizzella or Coccia or David Blair, Cerdonio, any of the people running detectors. If you're interested in doing this kind of thing please try to give us a hand. Most of us building detectors are simple plumbers. We know how to screw things together. It's not necessarily the case that we know how to analyse data.

Now I hope that this impresses someone. Here's a result from Evan Mauceli of a strain detector looking for a CW sine wave coming from 47 Tuc. So this the strain that we detected coming from 47 Tuc. Notice where we are -- coming down and touching -- you can call it three you can call it four or three-and-a-half, whatever you will, times 10^{-24} -- OK? -- for strain.

Our bandwidth is rather small ... Now, what would we expect to find? I should point out for those aficionados of signal processing -- there are a few here -- there are implemented across this -- there are 10^5 optimal filters looking for the appropriately frequency modulated [signal, modulated] by the Doppler shift [and which is also] amplitude modulated by the rotation of the bar going around.

... What we would expect to see if there was a CW source we might expect to see something like that [indicates spikes on the curve]. Is that a gravity wave? It is not!

Here's the same thing we get from the minus mode ... If we normalise those -- if we normalise the amplitude -- plot a histogram -- you can see that they are less than 2 standard deviations away -- that's not significant. Those guys that were sticking up in the plus mode -- there we have it again. Again, 1.8 sigma, nothing significant.

[Now here is] New data -- never before disclosed to anybody -- in fact disclosed to me only yesterday [laughter]. The [bar has been `pointed' at the] center of the galaxy! Are there any gravity waves in the center of the galaxy. Well -- here's the plus mode -- notice incidentally, for reasons unknown to us, our noise [?] is a little bit over. Are any of those events significant? No! But this is how well these detectors can do.

I hope that these numbers -- I didn't see anyone faint out there from seeing numbers this small -- but these, if you've been looking at this kind of thing, these are damned impressive. These detectors are pretty good.

Here is the histogram of those events for the plus mode. Again, nothing significant. But we've got a number of years of data. We go back and repeat this -- in fact we can repeat this for any place in the galaxy. Those of you that have been looking at this problem for LIGO and VIRGO, realise that there's a tremendous computation on this. So that's the reason we're looking at only small regions. Here's the center of the galaxy minus mode. And again, nothing significant.

... I want to say a word or two about spherical detectors. This is a lead on to what you heard from others and, as I said, I prepared these viewgraphs before I left and I was really very discouraged. And let me repeat now for those who came in late, because of the attitude I've seen, and the interest that I've seen in Europe and Brazil, I am much less discouraged than I was. I am talking about the particular configuration that Steve Merkowitz and Warren Johnson came up with: the truncated icosohedral antenna -- what we have called the TIGA. ...

Now puts up a projected sensitivity curve for TIGA and shows that it is better than LIGO I, [in its narrow frequency band] but stresses that he is not criticising LIGO I.

Well, as you saw, one of the things that we were involved in recently was putting in a proposal to build one of these in the US. And as all the insiders know -- as most of you do I suppose -- the coop proposal was declined and we were told there was no money for it. We were told it would cost us $20M to build. Now, since my business is building things I, of course, think that it would cost less. But this [that is, the NSF figure of $20M] may be realistic. And within the US an advisory committee recommended against additional funding. And, as a matter of fact, they went further than that. They implied that we really ought to be trying to cut resonant detectors out altogether. And I think -- my own opinion is that that advisory committee's advice was not very good. But, incidentally, I should also point out that in fact many of you -- the TIGA proposal -- I don't know who the referees were for the TIGA proposal -- but the TIGA proposal was quite well reviewed scientifically, but there just isn't any money for it. And so we have the famous problem of the zero-sum game. It's what we're beginning to see in gravity wave detection and what I'm afraid [of is what] a lot of us that are trying to build gravity wave detectors are going to see down the road: `Well, we've put all this money into gravitational wave detection and we haven't seen anything yet -- let's cut it off.' And we are all in the same boat. ... So let's stick together if we can.

Because it [TIGA] was rejected should we give up? And the answer that I have given to other people is that the advantages are compelling: I don't believe the cost is as high as stated. And the work that we did in the gravity wave coop is certainly available to any of the other collaborations. We are certainly happy -- we are eager to work with other groups to get these spheres developed.

I think that we will find that as we get into the business of actually trying to detect gravity waves. The sort of stories that you've heard about various [filters?] and things like that -- we are always going to be looking right at the threshold. Is this a gravity wave or is it not? Then, the more detectors that we have: LIGO, two-and-a-half detectors -- VIRGO, another detector -- GEO, a detector different orientation -- the Australian. How well does it improve our understanding, or our confidence in whether we have gravity waves on having additional detectors. Our experience is the more detectors you have the better off you're gonna be. And so I just pass that on. ...

And I don't think that all these things are cast in stone. I think strong arguments can change minds and I hope we will develop those arguments in the future. Such as, what I just said, improvement in detectability with additional detectors. Can you have confidence that you've seen a gravity wave? I think the complementary technology is of really striking benefit, and the work done that most of you know about by Michelson and [?] showing that source location capability -- also the work done by Merkowitz and Johnson -- the source location that a spherical detector ought to be pretty damn good at directional capability -- at pointing out the direction from which a gravity wave comes. And they're not terribly expensive.

Hamilton put up an overhead setting out `The advantages of TIGA' including: **Omnidirectional and direction sensitive; Highest spectral sensitivity; Complementary technology to interferometers; Independent confirmation; Relatively low cost.**

> So -- with that I will repeat again that I am much more encouraged by the possibility of resonant ... detectors. I think they do have a future. I think there are people with the foresight to realise the importance of them and so I think the thing that we can all concentrate on is pressing ahead, again keeping in mind that our goal is not to build a super-detector, our goal is not to build a bar-detector, our goal is not to build a sphere, our goal is not to do resonant sideband extraction or power-recycling or this and that -- our goal is to detect gravity waves. And we've got to look at the best way to do that.
>
> Thanks very much.

An overhead labelled `**Should we give up?'** proclaimed:

No! -- The advantages are compelling and the cost is not as high as stated.

A true collaboration will be taken seriously -- The design work developed by LSU can save time and money

Strong and persuasive arguments can change minds

RAY WEISS'S LETTER PROPOSING INTEGRATION AMONG INTERFEROMETER GROUPS

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

...

April 30, 1998

TO: Leaders of the Gravitational Wave Projects

I would like you to consider a strategy for the opening days of the large baseline interferometer gravitational wave detectors that will provide the best for the science itself and can be held up as a model for how a new field is opened responsibly.

The proposed strategy is extremely simple.

A detection of gravitational waves is to be announced only after a statistically meaningful analysis has been performed of the data of ALL instruments that were observing throughout the world.

The instruments include the large and medium baseline interferometers, the acoustic [resonant bar] detectors and the prototype detectors.

The data analysis for the individual instruments is carried out by the scientists associated with these instruments and their collaborators. The data and statistical results are brought to a council composed of representatives from each observing group.

The initial publication is submitted in two parts. A paper from the group(s) making the observation and their analysis and a second paper from the council discussing the statistical significance in regards to the worldwide effort, in particular, the probability and confidence of detection in some of the instruments as well as the reasons for non-detection in others.

Suggest that the council be formed within the next year and that one of the functions of the council be to maintain an inventory of the schedule of operations of the various gravitational wave detectors throughout the world.

Sincerely yours,

Rainer Weiss
Professor of Physics
...

BARISH AND COLLINS DEBATE NOISE

<u>Collins:</u> The most important thing, which I wish I could convince you of, is that it's not my job to read the [scientific] papers - [I mean that] I should read the papers, but it's not my job to reach a scientific conclusion from reading the scientific papers.

Barish: Oh, I understand that, but I think you misinterpreted what I said. I think that if other people have the same confusion, or misunderstanding, that I have, that some of your work is dangerously close to journalism instead of sociology, what that traces back to is that some of the conclusions are based on techniques that are more journalistic than academic.

Collins: The discussions [that I record] and so on, and the quotations [that I use in the papers].

Barish: Yeh - and that it's mostly anecdotal - free floating anecdotal. If you get confirmation of one person from another you maybe make your conclusion: That's a journalistic [conclusion]. Or you go to three primary sources or whatever. If you get differences you pick that as something to discuss that's interesting. But I think you lose some depth by not -- I'm not saying you should make a scientific conclusion - but I'm saying [that] by not doing a more academic analysis of some of the same stuff and seeing what you can glean from it - and I invited you, since you had picked the 40 meter - and I think you'll find it's [the documentation] very lacking - to look at the documentation of the conclusions on the 40 meter.

If this is a science and [Bob] Spero is really making scientific statements, show me the documents so I can refute them, instead of anecdotal discussions and comments that come down the hall and this or that. If somebody wants to tell me that this is not subject to analysis, I want to see the argument. And if you can tell me that all he does is make a claim, then bullshit - I don't care. I want to see why. I challenge him through you right now, because I don't see the documents. I've read everything - OK - that he's every written. What is it [the unknown noise source]? Because maybe it isn't subject [to analysis], maybe this has some tricks in it, something I don't see, but to tell me that he just knows in his heart somewhere that this is not subject to analysis like the rest of science -- Show me something! - What is it that isn't [subject to that kind of analysis].

Collins: Well there's lots of interferometer science that hasn't been subject to analysis.

Barish: What?

Collins: Well, the very, very early days. I mean you couldn't say in the early days of high sensitivity [interferometry] [[Barish: not in the early days, no]] No - the difference is this, in a sense what Spero is claiming is that the bit of interferometry that he wanted to continue doing, still had the character of the early days of interferometry.

Barish: Why does he say that? [That interferometry is still in a state that requires inspiration or intuition.]

Collins: It's an empirical claim in the last resort. But that's why I've set it up to say its a difference in viewpoint. Three years will show us.

Barish: But look - now you claim that it's noise he's concerned about. Then he should show me a noise curve that couldn't be explained. We have a very good model - I'll show you - that explains [it] from his days - I havn't redone it - that is used to predict the sensitivities [ie the noise levels].

You can ask: What confidence do we have that we should get a certain sensitivity [ie noise level] on LIGO? Part of that confidence comes from the fact that the same models are used to fit the [noise] data on the 40 meter.

Collins: But let me remind you - his [Spero's] claim - his and Vogt's - and this is a quote in the paper - `every time we reassembled the interferometer in a new configuration we found twice as much noise as we expected and we couldn't get rid of it.'

Barish: That's bad experimentation.

Collins: You say it's bad experimentation - he says its `magic' - I'll use your phrase. I'm standing in the middle - you see.

Barish: But did he discover anything other than the noise sources we talk about? When you trace it down its all experimental noise. It's all electronic noise and so forth - it's just how well you did your experiment. We all do bad experiments ...

Collins: I presume he thinks there are still unknown sources of noise.

Barish: He never found an unknown source - all he found were technical issues.

...

So that's why I say `go read the documents.' What sources of noise other than lousy experimentation - which we all do - did he discover?

Collins: But supposing I read the documents and I went back to him and said `look Bob' [[Barish: but his arguments about the future have to come from some real experience. I dare you to go find something they discovered in the lab that wasn't already -- other than technical crap.]] Yeh, but look - I don't want to disgree with you, I'm just trying to explain my position. I can't go to Bob Spero, who's been working on interferometers all his life, and say look Bob, I'm a sociologist, and you don't understand interferometry.

Barish: I'm not asking you to.

Collins: But that's what it would amount to.

Barish: Weeell!

Collins: And that's what I mustn't do. There's lots of historians and sociologists who do do that and its very important not to. It's actually much harder not to.

Barish: There's a lot of good debates. This isn't a very good one in my mind because its basically - it should be based on something. I should be able to grab onto something - you should be able to grab onto something. Like `he says there's this X source of noise, we fought it for years, there's some trick and we beat it in some ways. You guys havn't figured out what it is, now you go and you're gonna get hit by it,' and he can show you some things that they saw that were just unexplainable - OK? There are no unexplainable things that I can see other than that the instrument wasn't - the noise was bad because there was bad RF [Radio Frequency] or this and that. There's no evidence that there was this hidden noise source that was important that's not yet understood and that's gonna raise its head now and beat us because they didn't get around it.

There is none!

Collins: But he must must think there is.

Barish: Well where is it?

Collins: I don't know

Barish: [increasingly heated] Then as a scientist he should have written the damn thing down and documented why there was this [[Collins: well not everybody can. For instance we recently discovered a new noise source in sapphire which nobody had anticipated. You know it's got to be the case that unexpected things come up every now and again.]] Yeh but that wasn't an experiment, that was when you do an analysis of what you think something's gonna do: Did you think of all the possibilities?

Collins: But it could have been the other way round - we could have done the experiment first.

Barish: But that doesn't mean we won't hit new noise sources because we're moving into new territory, and it's not a scientific issue, it's a kind of emotional issue whether you think there's something that's gonna come - and that's where he is, and Mr Vogt is, as far as I can tell - that somehow you can't possibly get down there without hitting something brand new, but there's no evidence for it at all.

It doesn't mean that we won't hit something we've overlooked - I'm not saying that - but there's no evidence that comes from anywhere, so at this point its not a scientific argument, it's an argument that you couldn't possibly -- You didn't have to do any 40 meter stuff to find that. All this interferometry he talks about. His credentials to say this are zero. He never found an unknown source [the last said with great stress].

Collins: He said he did [Barish: what?] He and Vogt both say that they found unknown sources whenever they put the interferometer back in a new configuration.

Barish: No no, I call that technical sources.

Collins: But he doesn't agree with you

Barish: Then let me see one [source of noise] that I can read about. What is it?

Collins: He says it was a noise source we didn't expect. We didn't expect this noise source - that's his claim.

Barish: There are none. ... [Barish says that if Spero or Vogt could give him hard evidence or information about unknown noise sources he would `gobble it up.']

Barish: [pointing to a diagram on his computer which shows the noise sources that have been theoretically modelled compared with those which have been experimentally measured and showing that there is a pretty good match between the two] The claim is that these are the noise sources that matter, anything else is what I call technical noise. Technical noise is a way of saying that you havn't done your experiment very well - you know the way you have your [electrical] grounds done. The noise that comes from science - from physics - are listed here [ie, on the theoretical plot]. None of these - I claim that none of these [ie no new ones] have been observed by Mr Spero - we may have missed some down here. But the 40 meter - I'll try to find you a curve - this track here has been traced out and fit. And he didn't find anything fundamental - he didn't find anything fundamental up here. ... It's just a fact. Unless there's something that I have never been told. Which I can't deny. There may be something that I have not been told, but I have gone and looked at every document ever written on the 40 meter ... This LIGO curve ... Every noise that we know about has been analysed in a model. That same model fits the 40 meter.

Collins: OK - I'll go back to him. I'll say that what I heard is that there is no noise source on the 40 meter that hasn't been analysed.

Barish: I'll show you the closest to one that isn't well understood. Look, I worry about these things - it isn't that I'm burying my head. ... This is the 40 meter [indicating figure], and this is the displacement sensitivity; this jagged thing is the measured noise curve, and over in here, this line is the fit to shot noise and thermal noise, this line here - when you add it up, fits all this in here - is the seismic noise. And the only discrepancy we have is about a factor of two in this region here. It's down by about a factor of two. So our calculation of the shot noise seems perfect, our calculation of the seismic noise ... also seems perfect, and our calculation of the thermal noise is within a factor of two. The thermal noise is very hard to do a perfect calculation [for] because the problem is that you have a device that has finite dimensions and it has surfaces and there's different modes that get set up in here, and you sum over these modes and the calculation that was done is the sum of a bunch of things and you're not sure that you've done each term right because they're geometric and ... we know we can't calculate that to five or ten percent. So there's somewhat of a difference.

Now you can argue that maybe we're not really measuring the thermal noise or its something else that's bothering us in that region, and I can't deny it. However, our belief is that we're probably not quite as good at calculating in that particular region as we think we are.

Now, there's nothing else here. Even the little spikes in here are all explainable. [They come from resonances in the wires.] You calculate those and you get the line that goes through here which fits the data precisely. So this is all pretty well analysed, I mean as well as most things get analysed.

Now, let me tell you one other thing. This [sensitivity] is [about a 10-1^{9}] meters displacement. To get to LIGO sensitivity you don't have to do [two] orders of magnitude better than this [in noise reduction]. [If] You do this well [ie, as well as the 40 meter] over a longer baseline you gain because of the baseline. This is already at the level you have to do in LIGO, you just have to do it over a longer baseline - which is what I call engineering. This displacement sensitivity is what we asked for in LIGO - [10-19] meters. [[Collins: 40 compared to 4000 ...]] Yeh we gain a factor of a [hundred] giving 10-21 giving this curve for displacement sensitivity and then we ... OK?

So this is already at the level [required in terms of noise elimination] and there is no unknown source - there's maybe a factor of two, but nothing that is a factor of 20.

Collins: What configuration was this interferometer running in? Was it power-recycled at the time?

Barish: No.

***Collins: So if you say to get to 10-21 you only need the extra length, why do you need to use power-recycling?

Barish: In order to get the shot noise limit, which is this for us [indicating figure], we need more light in the big interferometer. It's photons per frequency bin, or per second, so to fill up the 4 kilometer you need more light.

Collins: So could there be some source of noise that's configuration dependent?

Barish: Of course.

Collins: Could it be that when you power-recycle [the interferometer] there could be some sorts of noise that aren't represented on this curve?

Barish: But [that's not] fundamental noise. Its noise that has to do with how well we bounce the light around or make a resonance, it's what I call technical noise. I agree there can be technical noise. ... Technical noise is boring, it's hard work, and they weren't very good at it on the 40 meter. Fundamental noise is a different matter.

...

Collins: I'm just bullshitting now, but [could there be] any unexpected non-linear effect that could be the result of putting in the power-recycling mirror or something?

Barish: But that again is what I call technical noise, unless it's quantum mechanical or something.

Collins: Well let it be quantum mechanical then.

Barish: We're not in the region where quantum mechanical -- [... .] We're not yet in those regions.

Is there something? I won't guarantee to you that we havn't overlooked something but there should be --

Collins: It's the quotes you see. Vogt's quotes in that paper: that when they reassembled the interferometer into more complicated configurations they found noises that they didn't expect -- And they're what you call technical noises?

Barish: They're technical noises, not fundamental noises. ... Beating technical noise can be just as hard but it's not fundamental. It just means you have to do work. If we build the instrument and we run into a noise source that we havn't thought about - let's say like the one in sapphire that you talked about. Well, hard work isn't going to solve the problem - you've picked a material that has a certain fundamental noise limitation due to thermo-elastic effects that you aren't going to get below, so that this graph that we drew isn't right - so it's fundamental noise. If it's not fundamental noise it's subject to just improving the things that limit you.

Collins: You see, I'm being devil's advocate because I'm trying to put their viewpoint.

Barish: But it's science not viewpoints. Somebody needs to say `Look we see ...'

Collins: No but the sociologist deals with viewpoints unfortunately [laughter]

Barish: But if you're going to do sociology of science you have to deal with science.

Collins: But that's where we came in at the beginning. It's very important in a sense not to. It's important for me not to, because I'd make myself look ridiculous. In other words you can deal with science so long as it's settled science.

Barish: You have to deal with it enough to know whether the debate has any sense.

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[i] Kramer would disappear from the picture.

[ii] The reference is to "Thorne, K. T., in Gravitational Radiation, ed. N. Deruelle and T. Piran, North Holland (1982)" [EDITOR: THIS REFERENCE, LIKE THOSE IN THE NEXT TWO FOOTNOTES, IS A QUOTE AND SHOULD NOT BE RE-WRITTEN]

[iii] The reference is "Harwit, M. Cosmic Discovery, Basic Books (1981)."

[iv] The reference is to "Eardley, D. M. in Gravitational Radiation, ed. N. Deruelle and T. Piran, North Holland (1982)"

[v] The reference is to Thorne 1992.