

A new window on the universe: the non-detection of gravitational radiation

Detecting gravity waves

In 1969, Professor Joseph Weber, of the University of Maryland, claimed to have found evidence for the existence of large amounts of gravitational radiation coming from space. He used a new type of detector of his own design. The amount of radiation he saw was far greater than the theoretical predictions of astronomers and cosmologists. In the years that followed, scientists tried to test Weber's claims. No-one could confirm them. By 1975, few, if any, scientists believed that Weber's radiation existed in the quantities he said he had found. But, whatever it looks like now, theory and experiment alone did not settle the question of the existence of gravitational radiation.

Gravitational radiation can be thought of as the gravitational equivalent of electromagnetic radiation such as radio waves. Most scientists agree that Einstein's general theory of relativity predicts that moving massive bodies will produce gravity waves. The trouble is that they are so weak that it is very difficult to detect them. For example, no-one has so far suggested a way of generating detectable amounts of gravitational radiation on Earth. Nevertheless, it is now accepted that some sensible proportion of the vast amounts of energy generated in the violent events in the universe should be dissipated in the form of gravitational radiation, and it is this that may be detectable on Earth. Exploding supernovae, black holes and binary

stars should produce sizeable fluxes of gravity waves which would show themselves on Earth as a tiny oscillation in the value of 'G' – the constant that is related to the gravitational pull of one object on another. Of course, measuring 'G' is hard enough in itself.

It was a triumph of experimental science when, in 1798, Cavendish measured the gravitational attraction between two massive lead balls. The attractive force between them comprised only one 500 millionth of their weight. Looking for gravitational radiation is unimaginably more difficult than looking for this tiny force because the effect of a gravity wave pulse is no more than a minute fluctuation within the tiny force. To exemplify, one of the smaller gravitational antennae in operation in 1975 (the detectors are often referred to as antennae) was encased in a glass vacuum vessel. The core consisted of, perhaps, 100 kilograms of metal yet the impact of the *light* from a small flashgun on the mass of metal was enough to send the recording trace off scale.

The standard technique for detecting gravitational radiation was pioneered by Weber (pronounced 'Whebber') in the late 1960s. He looked for changes in the length (strains) of a massive aluminium alloy bar caused, effectively, by the changes in gravitational attraction between its parts. Such a bar, often weighing several tons, could not be expected to change its dimensions by more than a fraction of the radius of an atom as a pulse of gravitational radiation passed. Fortunately, the radiation is an oscillation and, if the dimensions of the bar are just right, it will vibrate, or 'ring' like a bell, at the same frequency as the radiation. This means that the energy in the pulse can be built up into something just measurable.

A Weber-bar antenna comprises the heavy bar with some means of measuring its vibrations. Most designs used strain-sensitive 'piezo-electric' crystals glued, or otherwise fixed, to the bar. When these crystals are distorted they produce an electrical potential. In a gravity wave detector the potential produced by the deformation of the crystals is so small as to be almost undetectable. This means that the impulse from the crystals must be amplified if it is to be measured. A critical part of the design is, then, the signal amplifier. Once amplified, the signals can be recorded on a chart recorder, or fed into a computer for immediate analysis.

Such devices don't really detect gravity waves, they detect vibrations in a bar of metal. They cannot distinguish between vibrations

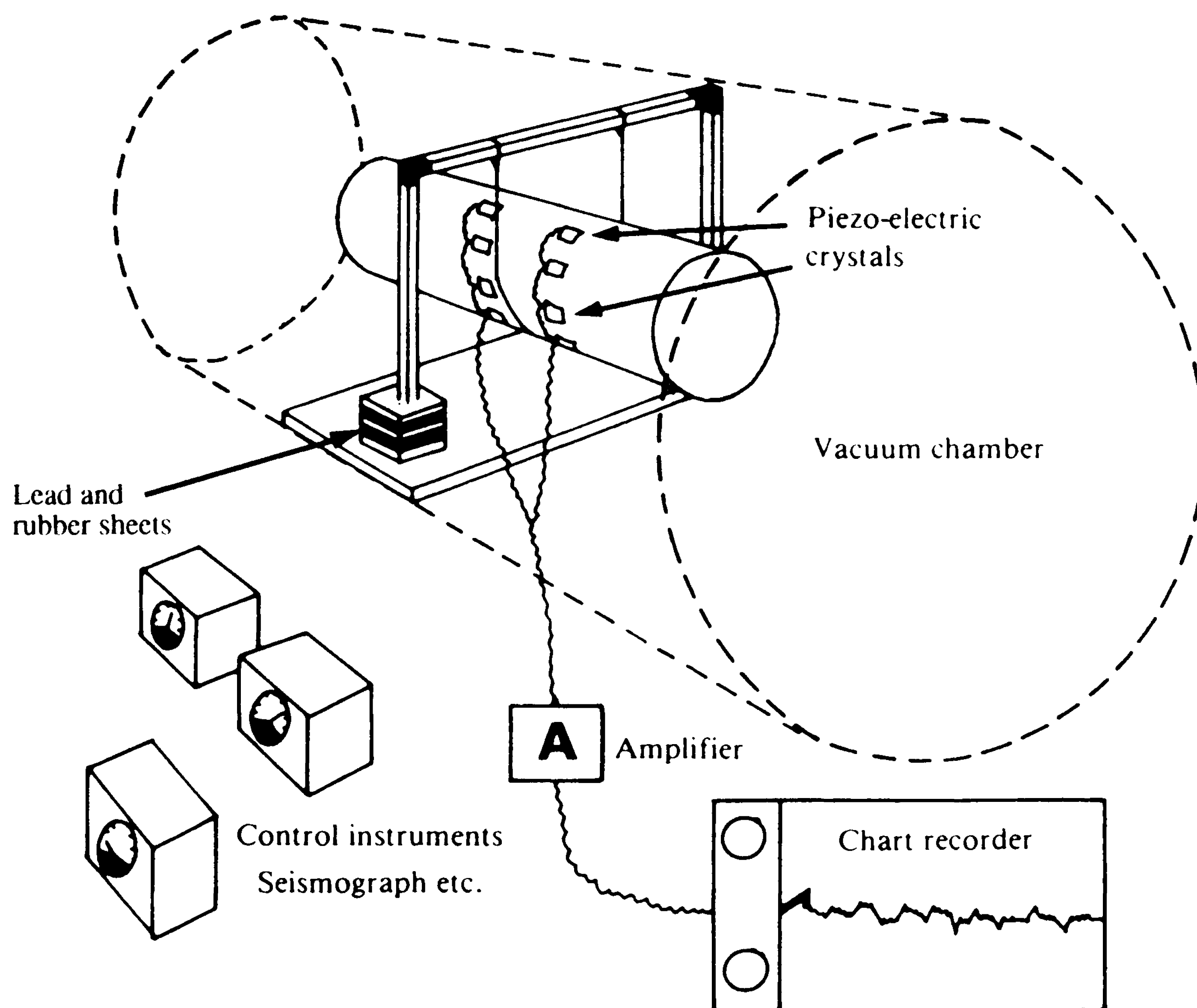


Figure 5.1. Weber-type gravity wave antenna. Compare Weber's method of seismic insulation with the heavy concrete foundations used in the Michelson–Morley experiments (see chapter 2). Heavy foundations actually link the apparatus firmly to the ground thus making certain that vibrations will be channelled through to the apparatus. Remember that Michelson (chapter 2) discovered this his apparatus would be disturbed by stamping on the ground 100 metres from the laboratory. Weber-type detectors are much less sensitive than this due to the ingenious insulation and the narrow waveband of the radiation.

due to gravitational radiation and those produced by other forces. Thus to make a reasonable attempt to detect gravity waves the bar must be insulated from all other known and potential disturbances such as electrical, magnetic, thermal, acoustic and seismic forces. Weber attempted to do this by suspending the bar in a metal vacuum chamber on a thin wire. The suspension was insulated from the ground in an original and effective way by using a stack of lead and rubber sheets.

In spite of these precautions the bar will not normally be completely quiescent. So long as it is at a temperature above absolute zero there will be vibrations caused by the random movements of its own atoms; the strain gauges will, then, register a continual output of 'thermal noise'. If this is recorded on graph paper by a pen recorder (as it was in many experiments), what will be seen is a spiky wavy line showing random peaks and troughs. A gravity wave would be represented as, perhaps, a particularly high peak, but a decision has to be made about the threshold above which a peak counts as a gravity wave rather than unwanted 'noise'. However high the threshold it must be expected that occasionally a peak due entirely to noise would rise above it. In order to be confident that some gravity waves are being detected it is necessary to estimate the number of 'accidental' peaks one should obtain as a result of noise alone, then make certain that the total number of above-threshold peaks is still greater. In 1969 Weber claimed to be detecting the equivalent of about seven peaks a day that could not be accounted for by noise.

Current status of Weber's claims and of gravitational radiation

Weber's claims are now nearly universally disbelieved. Nevertheless the search for gravitational radiation goes on. Weber's findings were sceptically received because he seemed to find far too much gravitational radiation to be compatible with contemporary cosmological theories. If Weber's results were extrapolated, assuming a uniform universe, and assuming that gravitational radiation was not concentrated into the frequency that Weber could best detect, then the amount of energy that was apparently being generated would mean that the cosmos would 'burn away' in a very short time – cosmologically speaking. These calculations suggested that Weber must be wrong by a very long way. The apparatuses now under development are designed to detect the much lower fluxes of radiation that cosmologists believe might be there. The new antennae are 1000 million times more sensitive; they should detect fluxes 1000 million times smaller than Weber said he had found.

Though Weber's first results were not believed because of the amount of radiation he claimed to see, he eventually managed to

persuade others to take him more seriously. In the early 1970s he developed his work in a number of ingenious ways, leading other laboratories to attempt to replicate his findings. One of the most important new pieces of evidence was that above-threshold peaks could be detected simultaneously on two or more detectors separated by a thousand miles. At first sight it seemed that only some extra-terrestrial disturbance, such as gravity waves, could be responsible for these simultaneous observations. Another piece of evidence was that Weber discovered peaks in the activity of his detector which happened about every 24 hours. This suggested that the source of the activity had something to do with the rotation of the earth. As the earth rotated, carrying the detector with it, the sensitivity would be expected to vary if the radiation came mostly from one direction in space. The 24 hour periodicity thus indicated that his detectors were being vibrated by an extra-terrestrial source rather than some irrelevant earth-bound disturbance.

What is more, the periodicity at first seemed to relate to the earth's disposition with regard to the galaxy, rather than with regard to the sun – the periodicity related to the astronomical day. This was important, because as the earth moves in orbit round the sun, one would expect the time of day when the detector was most sensitive to change with the seasons. (The geometry is just the same as in the Michelson–Morley experiment; see chapter 2.) This suggested that the source must be outside the solar system – again a strong indicator that it was cosmic events that were causing the gravity wave detector to vibrate rather than something local and uninteresting. This effect became known as the 'sidereal correlation', meaning that the peak periods of activity of the detector were related to the earth's relationship to the stars rather than to the sun.

Persuading others

It is worth noting at this point that with an unexpected claim like Weber's it is necessary to do much more than report experimental results in order to persuade others to take the work sufficiently seriously even to bother to check it! To have any chance of becoming an established result it must first 'escape' from the laboratory of its

originator. Persuading other scientists to try to *disprove* a claim is a useful first step. In Weber's case different scientists were convinced by different experimental developments. Some thought one feature was convincing whereas others thought the opposite. For instance, the first of Weber's elaborations was the demonstration of coincident signals from two or more detectors separated by large distances. Some scientists found this convincing. Thus, at the time (1972) one scientist said to Collins:

[] wrote to him specifically asking about quadruple and triple coincidences because this to me is the chief criterion. The chances of three detectors or four detectors going off together is very remote.

On the other hand some scientists believed that the coincidences could quite easily be produced by the electronics, chance, or some other artefact. Thus:

. . . from talking it turns out that the bar in [] and the bar in [] didn't have independent electronics at all. . . . There was some very important common contents to both signals. I said . . . no wonder you see coincidences. So all in all I wrote the whole thing off again.

Another elaboration adopted by Weber involved passing the signal from one of the detectors through a time delay before comparing it with the signal from a distant detector. Under these circumstances there should be no coincidences – that is to say, any coincidences would be purely a product of accident. Weber showed that the number of coincident signals did indeed go down when one signal was delayed with respect to the other, suggesting that they were not an artefact of the electronics or a matter of chance. Several scientists made comments such as '... the time delay experiment is very convincing', whereas others did not find it so.

Weber's discovery of the correlation of peaks in gravity wave activity with star time was the outstanding fact requiring explanation for some scientists, thus:

. . . I couldn't care less about the delay line experiment. You could invent other mechanisms which would cause the coincidences to go away . . . The sidereal correlation to me is the only thing of that whole bunch of stuff that makes me stand up and worry about it . . . If that sidereal correlation disappears you can take that whole . . . experiment and stuff it someplace.

Against this, two scientists remarked:

The thing that finally convinced a lot of us . . . was when he reported that a computer had analysed his data and found the same thing.

The most convincing thing is that he has put it in a computer . . .

But, another said:

You know he's claimed to have people write computer programmes for him 'hands off'. I don't know what that means. . . . One thing that me and a lot of people are unhappy about, is the way he's analysed the data, and the fact that he's done it in a computer doesn't make that much difference . . .

Picking the right experimental elaboration to convince others requires rhetorical as well as scientific skills.

The experimenter's regress

By 1972 several other laboratories had built or were building antennae to search for gravitational radiation. Three others had been operating long enough by then to be ready to make tentative negative reports. Now we must imagine the problems of a scientist attempting to replicate Weber's experiment. Such a scientist has built a delicate apparatus and watched over it for several months while it generated its yards and yards of chart recorder squiggles. The question is: are there peaks among the squiggles which represent real gravity wave pulses rather than noise? If the answer seems to be 'no' then the next question is whether to publish the results, implying that Weber was wrong and that there are no high fluxes of gravity waves to be found. At this point the experimenter has an agonising decision to make; it could be that there really are gravity waves but the negative experiment is flawed in some way. For example, the decision about the threshold for what counts as real peaks might be wrong, or the amplifier might not be as sensitive as Weber's, or the bar might not be appropriately supported, or the crystals might not be well enough glued to allow the signals to come through. If such is the case, *and* if it turns out that there are high fluxes of gravity waves, then in reporting their non-existence, the scientist will have revealed his own experimental incompetence.

Here the situation is quite unlike that of the school or university student's practical class. The student can have a good idea whether or not he or she has done an experiment competently by referring to the outcome. If the outcome is in the right range, then the experiment has been done about right, but if the outcome is in the wrong range, then something has gone wrong. In real time, the question for difficult science, such as the gravity wave case and the others described in this book, is, '*What is the correct outcome?*'. Clearly, knowledge of the correct outcome cannot provide the answer. Is the correct outcome the detection of gravity waves or the non-detection of gravity waves? Since the existence of gravity waves is the very point at issue, it is impossible to know this at the outset.

Thus, what the correct outcome is depends upon whether there are, or are not, gravity waves hitting the earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we won't know if we have built a good detector until we have tried it and obtained the correct outcome. But we don't know what the correct outcome is until ... and so on *ad infinitum*.

This circle can be called the 'experimenter's regress'. Experimental work can only be used as a *test* if some way is found of breaking into the circle of the experimenter's regress. In most science the circle is broken because the appropriate range of outcomes is known at the outset. This provides a universally agreed criterion of experimental quality. Where such a clear criterion is not available, the experimenter's regress can only be avoided by finding some other means of defining the quality of an experiment; and the criterion must be independent of the output of the experiment itself.

Scientists at their work

What should the consequences of the experimenter's regress be? Because no-one knows what counts as the correct result, it is not easy to see who has done a good experiment. We might, then, expect gravity wave scientists to disagree about who had done their experiment well. We might think they would disagree about whether a particular result was the outcome of incompetence on the part of the

experimenter and/or flaws in the apparatus. Some scientists would think that Weber saw gravity waves because his methods, or his apparatus, were faulty. Others would think that *failure* to see the radiation must be a consequence of lack of skill, insufficient perseverance, or bad luck. One of the authors of this book, Collins, interviewed most of the scientists involved in the gravity wave work in Britain and America. Such disagreement was precisely what he found. The following set of comments, taken from interviews conducted in 1972, show how scientists' views about others' work varied. In each case, three scientists who come from three different laboratories are commenting on the experiment of a fourth.

Comments on the experiment conducted at W

Scientist (a): . . . that's why the W thing, though it's very complicated, has certain attributes so that if they see something, it's a little more believable . . . They've really put some thought into it . . .

Scientist (b): They hope to get very high sensitivity but I don't believe them frankly. There are more subtle ways round it than brute force . . .

Scientist (c): I think that the group at . . . W . . . are just out of their minds.

Comments on the experiment conducted at X

Scientist (i): . . . he is at a very small place . . . [but] . . . I have looked at his data, and he certainly has some interesting data.

Scientist (ii): I am not really impressed with his experimental capabilities so I would question anything he has done more than I would question other people's.

Scientist (iii): That experiment is a bunch of shit!

Comments on the experiment conducted at Y

Scientist (1): Y's results do seem quite impressive. They are sort of very business-like and look quite authoritative . . .

Scientist (2): My best estimate of his sensitivity, and he and I are good

friends ... is ... [low] ... and he has just got no chance [of detecting gravity waves].

Scientist (3): If you do as Y has done and you just give your figures to some ... [operator] and ask them to work that out, well, you don't know anything. You don't know whether those [operators] were talking to their [friends] at the time.

Comments on the experiment conducted at Z

Scientist (I): Z's experiment is quite interesting, and shouldn't be ruled out just because the ... group can't repeat it.

Scientist (II): I am very unimpressed with the Z affair.

Scientist (III): Then there's Z. Now the Z thing is an out and out fraud!

Not only do scientists' opinions about the same experiment differ, but every experiment differs from every other in countless ways. Indeed, it is hard to know what it means to do an experiment that is *identical* to another. As one scientist put it:

Inevitably in an experiment like this there are going to be a lot of negative results when people first go on the air because the effect is that small, any small difference in the apparatus can make a big difference in the observations. ... I mean when you build an experiment there are lots of things about experiments that are not communicated in articles and so on. There are so called standard techniques, but those techniques, it may be necessary to do them in a certain way.

It is easy, then, to find a difference that will explain and justify a scientist's views about the work of another. Variations in signal processing techniques, in design of the amplifier, in the material of the bar (did it suffer from 'creep?'), in the method of attachment of the piezo-electric crystals, and in many other factors were cited in defence and criticism of the various experiments. Technical arguments, however, were not the only sources of judgement of others' experiments. Other grounds for doubt extended beyond what are usually thought of as science. In 1972, experimenters were casting around for non-technical reasons for believing or disbelieving the results of the various experiments. The list of reasons they provided at the time included the following:

1. Faith in a scientist's experimental capabilities and honesty, based on a previous working partnership.
2. The personality and intelligence of experimenters.
3. A scientist's reputation gained in running a huge lab.
4. Whether or not the scientist worked in industry or academia.
5. A scientist's previous history of failures.
6. 'Inside information'.
7. Scientists' style and presentation of results.
8. Scientists' 'psychological approach' to experiment.
9. The size and prestige of the scientist's university of origin.
10. The scientist's degree of integration into various scientific networks.
11. The scientist's nationality.

As one scientist put it, in explaining why he disbelieved Weber's results:

You see, all this has very little to do with science. In the end we're going to get down to his experiment and you'll find that I can't pick it apart as carefully as I'd like.

The competence of experimenters and the existence of gravity waves

These arguments over whose work is well done are part and parcel of the debate about whether or not gravity waves exist. When it is decided which are the good experiments, it becomes clear whether those that have detected gravity waves, or those that have not been able to see them, are the good ones. Thus whether gravity waves are there to be detected becomes known. On the other hand, when we know whether gravity waves are there to be detected we know which detectors are good ones. If there are gravity waves a good apparatus is one that detects them; if there are no gravity waves the good experiments are those which do not see them. Thus, defining what counts as a good gravity wave detector, and determining whether gravity waves exist, are the same process. The scientific and the social aspects of this process are inextricable. This is how the experimenter's regress is resolved.

Gravitational radiation: 1975

After 1972, events favoured Weber's claims less and less. In July 1973 negative results were published by two separate groups (two weeks apart), in the scientific journal, *Physical Review Letters*. In December 1973, a third group published negative results in the journal, *Nature*. Further articles claiming that there was nothing to be seen even as the sensitivity of the apparatus was increased were published by these groups and also by three other groups. No-one has since concluded that they found anything that would corroborate Weber's findings.

In 1972, a few scientists believed in the existence of high fluxes of gravity waves, and very few would *openly commit* themselves to their non-existence. By 1975, a number of scientists had spent time and effort actively prosecuting the case against Weber. Most of the others accepted that he was wrong and only one scientist other than Weber thought the search for high fluxes still worth pursuing. One might say that the problem posed by the experimenter's regress had been effectively solved by 1975 – it was now 'known' (by nearly everyone) that an antenna that detected high fluxes of gravity waves was a dud, and one that did not had every chance of being a well-designed experiment. How did this come to pass?

Weber, it seems, was not very surprised at the flood of negative results. A respondent reports that Weber felt that, since a negative result is the easiest thing to achieve, then negative results were to be expected:

... about that time [1972] Weber had visited us and he made the comment, and I think the comment was apt, that 'it's going to be a very hard time in the gravity wave business', because, he felt that he had worked for ten or twelve years to get signals, and it's so much easier to turn on an experiment and if you don't see them, you don't look to find out why you don't see them, you just publish a paper. It's important, and it just says, 'I don't see them'. So he felt that things were going to fall to a low ebb ...

But it is hard to have complete confidence in an experiment that found nothing. It is hard to see what made scientists so confident that their negative results were correct as long as Weber was still claiming

to see gravity waves. Why were they not more cautious? As one scientist remarked:

... [a major difference between Weber and the others is that Weber] spends hours and hours of time per day per week per month, living with the apparatus. When you are working with, and trying to get the most out of things you will find that, [for instance] a tube that you've selected, say one out of a hundred, only stays as a good noise tube for a month if you are lucky, but a week's more like it. Something happens, some little grain falls off the cathode and now you have a spot that's noisy, and the procedures for finding this are long and tedious. Meanwhile, your system, to the outside, looks just the same.

So lots of times you can have a system running, and you think it's working fine, and it's not. One of the things that Weber gives his system, that none of the others do, is dedication – personal dedication – as an electrical engineer which most of the other guys are not ...

Weber's an electrical engineer, and a physicist, and if it turns out that he's seeing gravity waves, and the others just missed it, that's the answer, that they weren't really dedicated experimenters ... Living with the apparatus is something that I found is really important. It's sort of like getting to know a person – you can, after a while, tell when your wife is feeling out of sorts even though she doesn't know it.

This feature of experimental work must make scientists wary about drawing clear conclusions from a set of negative results. It is another way of expressing the experimenter's regress. How did they gain enough confidence to damn Weber's findings?

How the debate closed

By 1975 nearly all scientists agreed that Weber's experiment was not adequate but their reasons differed markedly. Some had become convinced because at one point Weber had made a rather glaring error in his computer program; others thought that the error had been satisfactorily corrected before too much damage was done. Some thought that the statistical analyses of the level of background noise and the number of residual peaks was inadequate; others did not think this a decisive point.

Weber had also made an unfortunate mistake when he claimed to have found coincident signals between his own detector and that of an entirely independent laboratory. These coincidences were extracted from the data by comparing sections of tape from the two detectors. Unfortunately for Weber it turned out that because of a confusion over time zones, the two sections of tape he compared had been recorded more than four hours apart so that he was effectively conjuring a signal out of what should have been pure noise. Once more though, it was not hard to find scientists who thought that the damage had not been too great since the level of signal reported was scarcely statistically significant.

Another factor considered important by some was that Weber did not manage to increase the signal-to-noise ratio of his results over the years. It was expected that as he improved his apparatus the signal would get stronger. In fact, the net signal seemed to be going down. This, according to many scientists, was not how new scientific work ought to go. What is more, the correlation with star time that Weber first reported faded away. Again, however, these criticisms were only thought to be decisive by one or two scientists; after all there is no guarantee that a cosmic source of gravity waves should remain stable.

It goes almost without saying that the nearly uniform negative results of other laboratories were an important point. Nevertheless, all of the, roughly, six negative experiments were trenchantly criticised by Weber and, more important, five of them were criticised by one or more of Weber's critics! This should come as no surprise given the analysis in earlier sections of this paper. The one experiment that remained immune to criticism by Weber's critics was designed to be as near as possible a carbon-copy of the original Weber design. No-one thought it was crucial.

What seems to have been most important in the debate was the trenchant criticism, careful analysis, and confrontational style of one powerful member of the physics community Richard Garwin. As one scientist put it:

... as far as the scientific community in general is concerned, it's probably Garwin's publication that generally clinched the attitude. But in fact the experiment they did was trivial – it was a tiny thing ... But the thing was, the way they wrote it up ... Everybody else was

awfully tentative about it . . . It was all a bit hesitant . . . And then Garwin comes along with this toy. But it's the way he writes it up you see.

Another scientist said:

Garwin . . . talked louder than anyone and he did a very nice job of analysing his data.

And a third:

[Garwin's paper] . . . was done in a very clear manner and they sort of convinced everybody.

When the first negative results were reported in 1972, they were accompanied with a careful exploration of all the logical possibilities of error. Understandably, the first scientists to criticise Weber hedged their bets. Following closely came the outspoken experimental report by Garwin with careful data analysis and the uncompromising claim that the results were 'in substantial conflict with those reported by Weber'. Then, as one respondent put it, 'that started the avalanche and after that nobody saw anything'.

As far as *experimental results* are concerned, the picture that emerges is that the series of negative experiments made strong and confident disagreement with Weber's results openly publishable but that this confidence came only after, what one might call, a 'critical mass' of experimental reports had built up. This mass was 'triggered' by Garwin.

Garwin believed from the beginning that Weber was mistaken. He acted on that belief as he thought proper. Thus he made certain that some of Weber's errors were given wide publicity at a conference and he wrote a 'letter' to a popular physics journal which included the paragraph:

[it was shown] that in a . . . [certain tape] . . . nearly all the so-called 'real' coincidences . . . were created individually by this single programming error. Thus not only some phenomenon besides gravity waves *could*, but in fact *did*, cause the zero-delay excess coincidence rate [in this data]. [Garwin's stress]

and the statement:

... the Weber group has published no credible evidence at all for their claim of detection of gravitational radiation.

Concerning some of their later work, a member of Garwin's group remarked to me:

At that point it was not doing physics any longer. It's not clear that it was ever physics, but it certainly wasn't by then.

and

We just wanted to see if it was possible to stop it immediately without having it drag on for twenty years.

Thus, without the actions of Garwin and his group it is hard to see how the gravity wave controversy would have been brought to a close. That such a contribution was needed is, once more, a consequence of the experimenter's regress.

Conclusion

We have indicated how the experimenter's regress was resolved in the case of gravity waves. The growing weight of negative reports, all of which were indecisive in themselves, were crystallised, as it were, by Garwin. After he had spoken, only experiments yielding negative results were counted and there just were no more high fluxes of gravity waves. All subsequent experiments that produced positive results must, by that very fact, be counted as flawed.

Reporting an experimental result is itself not enough to give credibility to an unusual claim. If such a claim is to be taken sufficiently seriously for other scientists even to try to refute it then it must be presented very clearly and with great ingenuity. Weber had to make a long series of modifications before his claims were given significant notice. Then, once the controversy was under way, a combination of theory and experiment alone was not enough to settle matters; the experimenter's regress stands in the way. We have seen some of the ways in which such issues actually are resolved. These resolving, or 'closure', mechanisms are not normally thought of as 'scientific' activities yet, without them, controversial science cannot work.

It is important to notice that the science of gravity waves after the resolution of the controversy does not look at all like the science of gravity waves before the resolution. *Before* the resolution there was real and substantial uncertainty, and it was very reasonable uncertainty. In spite of the large amount of scientific work that had been done and the large number of experimental and theoretical results that were available, things were not clear. At that point no-one could be blamed for thinking that there were two possibilities open and for being reluctant to plump for one side or the other. *After* the resolution everything is clarified; high fluxes of gravity waves do not exist and it is said that only incompetent scientists think they can see them.

Of course, the model also shows that a controversy once closed may, in principle, be re-opened. Professor Joseph Weber has never ceased to believe that his results were correct and, especially since 1982, after our story ends, has been publishing papers which provide new arguments and evidence in support of his view. The question is, will they gain the attention of the scientific community?

The, pre-resolution, gravity wave science of 1972 is the sort of science that is rarely seen or understood by the science student. Nevertheless, to stress a point that will be made again in the conclusion to the whole book, it is the sort of science that the research scientist may one day face and it is the sort of science that the public are asked to consider when, say, they listen to forensic evidence as members of a jury, or when they attend to public inquiries on technical matters, or when they vote for policies, such as defence or energy policies, which turn on technical matters. For many reasons then, it is as important to understand this unfamiliar face of science as it is to understand its more regular counterpart.

