Detection of gravity waves challenged

Richard L. Garwin, and Joseph Weber

Citation: Physics Today **27**, 12, 9 (1974); doi: 10.1063/1.3129024
View online: https://doi.org/10.1063/1.3129024
View Table of Contents: https://physicstoday.scitation.org/toc/pto/27/12
Published by the American Institute of Physics

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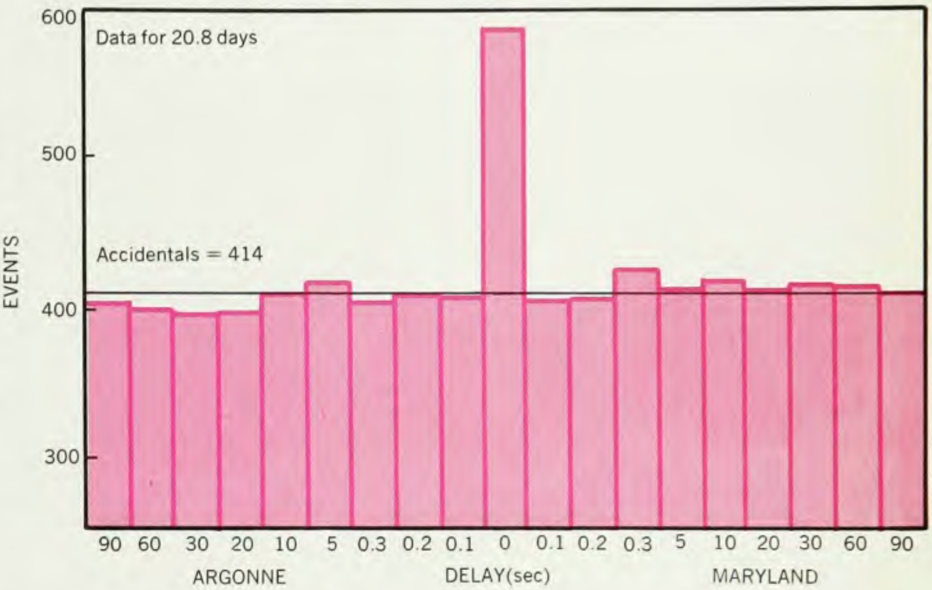
PHYSICS TODAY has published two long and informative articles on gravitational radiation.[1,2] Both articles fairly credit Joseph Weber for his pioneering analysis of the prospects for detection of gravitational radiation, and for the design and construction of detectors of gravity waves.

Your readers are probably unaware of the present situation, which I summarized in a paper presented at the "Fifth Cambridge Conference on Relativity" (CCR-5).[3] My paper concluded "that the Maryland group has published no credible evidence at all for their claim of detection of gravitational radiation."

I discussed Weber's experiment from two points of view:

- Can anything other than gravitational radiation have produced his delayed-coincidence plots, and
- Could gravity waves themselves have produced his delayed-coincidence plots?



Coincident events as function of time delay; real events are zero-delay value minus accidentals (copied from figure 1c in ref. 4). Figure 1

With respect to the first point, David Douglass (CCR-5) discussed the details of the computer-programming error he had discovered in the latest and most persuasive data published[4] by the Maryland Group. The details of this error were communicated in a letter by Douglass to Weber, 14 September 1973, and the Maryland Group found and corrected the error within a week. The nature of the error is such that any above-threshold event in antenna A that occurred in the last or the first 0.1-sec time bin of a 1000-bin record is erroneously taken by the computer program as in coincidence with the next above-threshold event in channel B, and is ascribed the time of the latter event. Douglass showed that in a four-day tape available to him and included in the data of reference 4, nearly all of the so-called "real" coincidences of 1–5 June (within the 22 April to 5 June 1973 data) 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 of figure 1.

With respect to the second point, figure 1 shows the "real" coincidences confined to a single 0.1-sec bin in the time-delay histogram. James L. Levine and I observed that the Maryland Group used a 1.6-Hz bandwidth "two-stage Butterworth filter."[4] We suspected that mechanical excitations of the antenna (whether caused by gravity waves or not) as a consequence of the 1.6-Hz bandwidth would not produce coincident events limited to a single 0.1-sec time bin. Levine has simulated the Maryland apparatus and computer algorithms to the best of the information available in reference 4 and has shown that the time-delay histogram for coincident pulses giving each antenna 0.3 kT is by no means confined to a single 0.1-sec bin, but has the shape shown in figure 2.

Therefore, I claim that it has been demonstrated that the coincidence data of reference 4 *did not* result from gravity waves, and furthermore *could not* have resulted from gravity waves.

Two other facts were discussed at CCR-5 that should be more widely known:

First, Weber has revealed at international meetings (Warsaw, 1973, etc.) that he had detected a 2.6-standard-deviation excess in coincidence rate between a Maryland antenna and the antenna of David Douglass at the University of Rochester. Coincidence excess was located not at zero time delay but at "1.2 seconds," corresponding to a 1-sec intentional offset in the Rochester clock and a 150-millisecond clock error. At CCR-5 Douglass revealed, and Weber agreed, that the Maryland Group had mistakenly assumed that the two antennas used the same time reference, whereas one was on Eastern Daylight Time and the other on Greenwich Mean Time. Therefore, the "significant" 2.6 standard deviation excess referred to gravity waves that took four hours, zero minutes and 1.2 seconds to travel between Maryland and Rochester.

Second, in view of the fact that Weber at CCR-5 explained that when the Maryland Group failed to find a positive coincidence excess "we try harder," and since in any case there has clearly been selection by the Maryland Group (with the publication of data showing positive coincidence excesses but with no publication of data that does not show such excesses), James L. Levine has considered an extreme example of such selections. In figure 3 is shown the combined histogram of "coincidences" between two independent streams of random computer-generated data. This "delay histogram" was obtained by partitioning the data into 40 segments. For each segment, "single events" were defined in each "channel" by assuming one of three thresholds, a, b or c. That combination of thresholds was chosen for each segment which gave the maximum "zero-

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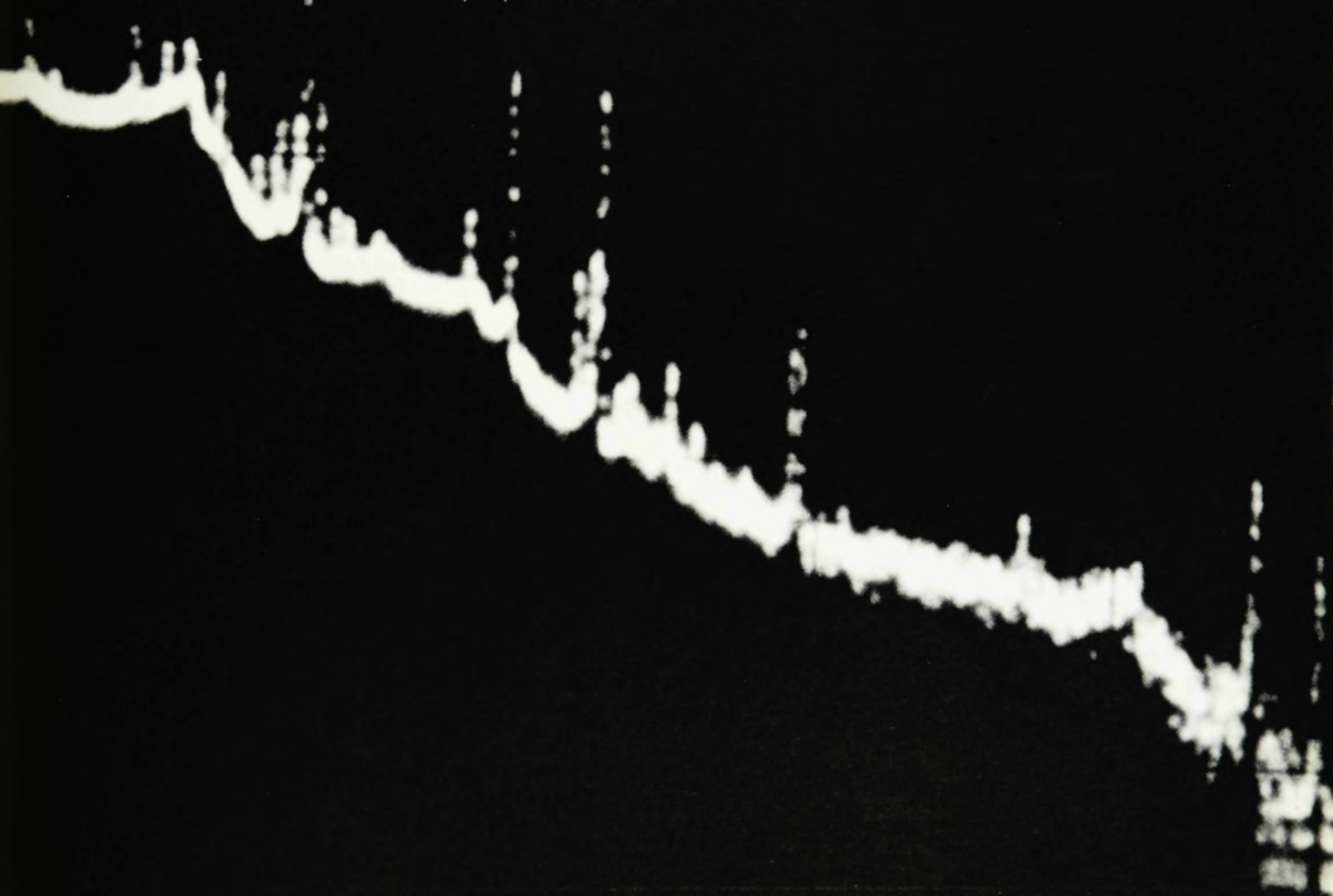

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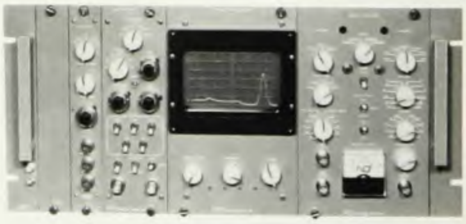

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delay coincidence" rate for that segment. The result was 40 segments, each selected from one of nine "experiments." The 40 segments are summarized in figure 3, which shows a "six-standard-deviation" zero-delay excess. This "experiment" demonstrates in a simple manner the extreme importance of publishing the details of the selection of data in the processing algorithm that might be used by the Maryland Group in any future publications.

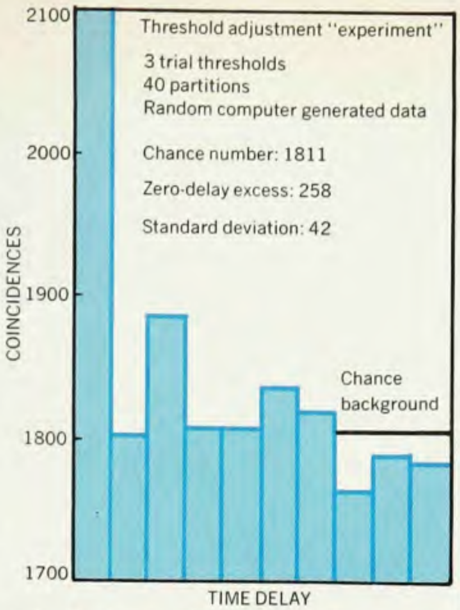
RICHARD L. GARWIN
Thomas Watson Research Center
Yorktown Heights, New York

References

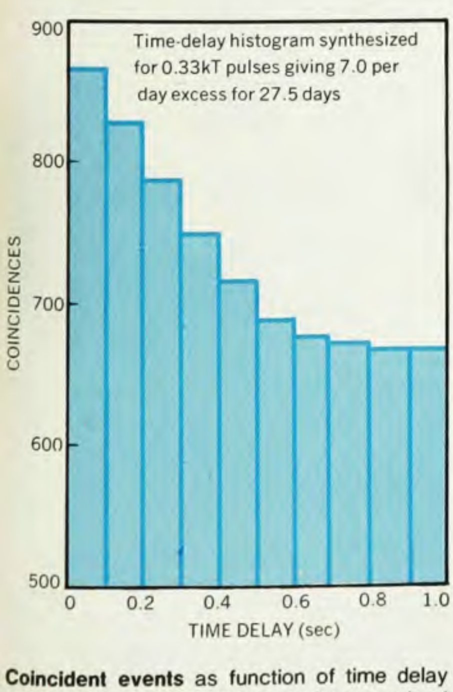
1. J. L. Logan, "Gravitational Waves—A Progress Report," PHYSICS TODAY, March 1973 (page 44).
2. T. J. Sejnowski, "Sources of Gravity Waves," PHYSICS TODAY, January 1974 (page 40).
3. R. L. Garwin, "The Evidence for Detection of Kilohertz Gravitational Radiation," presented at the Fifth Cambridge Conference on Relativity (MIT) 10 June 1974. (Available from the author.)
4. J. Weber *et al*, Phys. Rev. Lett. **31,** 779 (1973).

WEBER REPLIES: The reader who takes time to study Richard Garwin's letter and my reply will want to know what is really going on. Therefore, I will first give a brief account of Garwin's and my activities and then proceed to answer the criticism.

We at Maryland have developed an-



Coincident "events" as function of "time delay" between two sequences of random numbers. To show the possible effect of selection bias, one of three thresholds was chosen for each antenna for each of 40 data subsegments that maximized the zero-delay excess for the particular segment. Figure 3



Coincident events as function of time delay expected for gravity waves exciting a pair of antennas incorporating 1.6-Hz bandwidth Butterworth filters. Figure 2

tennas for gravitational radiation. Coincident changes in output of two widely separated antennas were reported in 1969. Others have been carrying out observations since 1972. Our antennas have a mass of one and a half tons.

Richard Garwin is a respected scientist associated with the respected IBM Research Laboratory. He attempted to check our two-detector coincidence experiment by a search for sudden changes in output of one detector and obtained negative results. The real issue here is whether physicists can have a higher degree of confidence in the Garwin negative result than the results published by us.

The IBM detector (*Phys. Rev. Lett.* **31,** 173, 1973) had a mass one tenth as great as our smallest operational detectors. Garwin apparently overlooked the great importance of temperature control or automatic tracking of his cylinder with a reference oscillator.

It is most unfortunate that for these reasons this IBM detector and a second one with mass 480 kg appear to be the least sensitive of nine operating installations.

Other physicists have joined me in wondering why there wasn't a very small fractional increase in cost to make the IBM installation the one with largest mass and highest sensitivity in the world. If this had been done, the IBM effort might have terminated the present controversy.

It is interesting that the one other group that has observed a statistically significant number of coincidences (Munich–Frascati), employed two de-

tectors having the same mass as the Maryland antennas and similar instrumentation.

Garwin has not analyzed any of our tapes and has not studied our computer programs. I deeply regret having to say that his analyses and statements about our experiments are not correct. For example, his equation 2 (*Phys. Rev. Letters* **31**, 178, 1973) cannot be correct because the detector relaxation time is not contained in it. A detector with infinite relaxation time will never cross threshold and never give a false alarm. Garwin has not correctly reported my remarks at CCR-5.

I do not write any computer programs and do not process any tapes. My first-hand knowledge of our experiments is based entirely on other data including real-time counting and pen-and-ink records. We have never reported results in publications or at meetings based entirely on computer analysis of one series of tapes. Programming errors cannot therefore change the character of our conclusions.

Our computing staff has done an enormous amount of excellent work with surprisingly few errors. I take pride in the statement of Garwin that "they [University of Maryland] acknowledged an error within a week." If significant errors are made by our computing staff, the responsibility is mine. Arrangements were made to have our programs checked long before questions were raised.

We sent several copies of one tape, our number 217, to other laboratories for analysis, and there was an error in one of our programs. However, two of the other laboratories processed the tape incorrectly making larger errors than ours and these incorrect results were widely disseminated by Garwin. Subsequently all groups reached reasonable agreement. This required 14 months. As a result we have documentation supplied by physicists at other laboratories, checking our programs. Such documents confirm that the event rate for tape 217, the one discussed by Garwin, is 8 per day, in agreement with our published data, after we corrected the error and all other known corrections are applied. Copies of these documents have been sent to editors and other workers in this field.

Our error was acknowledged and corrected data published in the *Proceedings* of the Paris Relativity Conference CNRS 220, June 1973, and CCR-5 Cambridge 1974, GR7 Tel Aviv, June, 1974, Proceedings of the Tel Aviv Conference, and other publications in press. The Tel Aviv Conference Proceedings preprint also gives data on periods when no coincidences are observed and answers the questions raised by Garwin concerning selection of data, histogram widths, and calibration.

Garwin's remarks refer to experiments involving two antennas at 1661 Hz and also to unpublished data on experiments involving antennas at 1661 Hz and 710 Hz. For the latter experiments we have reported a zero-delay excess of coincidences over accidentals, but have not claimed a positive result in open-literature publications or at meetings. Review of these data, applying all known corrections, leads us to conclude that there is a zero-delay excess, which is in fact larger than reported at Warsaw in 1973.

Computing errors have been an important factor in the politics but not in the physics of our experiment.

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Journal pecking order

Herbert Inhaber's article, "Is there a pecking order in physics journals?" (May, page 39) was mildly interesting and comforting, I suppose, to AIP, though it contained no surprises. No surprises? Inhaber thought he had one surprise—the first-place ranking of *Review of Modern Physics* in "immediacy index." This comes as no surprise to most physicists. There is one and only one reason for RMP's first-place ranking on immediacy. That is the appearance (annually until 1970 and now in odd years) of the "Review of Particle Properties" by the Particle Data Group. This Landolt-Bornstein of the high-energy physics world is the single most cited paper in RMP. The choice of 1969 as the comparison year also helped RMP's score. There were only 21 papers published in RMP that year, down from 70 in 1967 and 55 in 1968 (the average for the last 4 years has been 15.5).

Incidentally, if Inhaber had chosen 1972 for his comparison year, *Physics Letters* would have shown a high *"immediacy index."* It had the "Review of Particle Properties" that year. *Physics Letters* would then have been judged to obey the correct (that is, expected) pattern for immediacy, but for the wrong (?) reasons!

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University of California
Berkeley, California

THE AUTHOR COMMENTS: Jackson is correct in his first paragraph. The high "immediacy index" of RMP in 1969 was primarily due to the article by N. Barash-Schmidt and others of the Particle Data Group, which accounted for about 60% of the same-year citations in 1969. The same situation probably prevailed in 1970, when A. Barbaro-Galtieri *et al* received even more citations in the