THE EVIDENCE FOR DETECTION OF KILOHERTZ GRAVITATIONAL RADIATION

Richard L. Garwin

IBM Thomas J. Watson Research Center

Yorktown Heights, New York 10598

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Abstract The negative results of Levine and Garwin (Phys. Rev. Lett., 16 July, 1973) are updated with one month's data at 6 times better sensitivity. James L. Levine's analysis of the Frascati results is discussed, showing that that group threw away a factor 10 -- 20 in sensitivity by the unfortunate choice of a 0.3 second filter time constant. The experimental results of J. Weber and his group are then discussed from two points of view -- (1) Can anything other than gravitational radiation have produced his delayed-coincidence plots, and (2) could gravity waves have produced his delayed-coincidence plots?

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OUTLINE. Here I will present briefly our latest results and comment on two other experiments of which we are aware at 1.6 kilohertz.

First, the status of our experiment[1,2]. In September 1973 we extended our published experiment to a 500 kilogram bar and made improvements in the transducer sensitivity.

Slide one will remind you of the kind of antenna which we use. I want to call your attention in particular to the electrostatic calibrator plate (which we use to excite impulsively known amplitudes of oscillation in the bar and so to simulate gravity waves) and to the small piezoelectric transducer.

Slide two shows the signal processing train. Here please note that we have no filters following the lock-in -- we integrate the lock-in output for one sampling period and then reset the integrators for the next sampling period.

Slide three shows the vector amplitude in the phase plane, for a period which encompasses the application of a calibrating pulse. Here I want to emphasize that our signal-processing algorithm is such that the response to a calibrating pulse (and so to a gravity wave) is independent of the pre-existing state of oscillation of the bar and also of the arrival time of the gravity wave within the integrating period.

Slide four shows the response of the system (mechanics, electronics, plus computer processing algorithm -- in the normal running state of the system) to 100 calibrating pulses, each of which would have given the bar at rest 900K. Please keep this in mind for comparison with the methods used by Bramanti, et al.[3]

Slide five shows the results of this processing for 27 days in December 1973. The straight line is the Boltzmann distribution which we calculated we should have observed on the basis of amplifier noise and the bar at room temperature. Note that the effective temperature for this Boltzmann distribution is 17K.

Slide six Presents our results in the form of the upper limit on arrival rate of gravity waves which would give our antenna any energy specified along the x-axis. For comparison, we show our best estimates of the arrival rate versus assumed energy to reproduce Professor Weber's results in 1970 and 1973.

A more detailed discussion of his experiments comes later in this talk.

THE FRASCATI EXPERIMENTS

As you know, Bramanti, Maischberger, and Parkinson have reported an experiment in a form very similar to the Boltzmann distribution of impulses which I showed you on Slide 5.

Slide seven shows the results. Here Figure 3 should be the Boltzmann distribution of energies in the antenna, with an effective temperature of 300K, while Figure 4 is a histogram representing the distribution of the impulses, here obtained by subtracting a vector amplitude from the one 0.1 seconds later. This subtraction of the vector amplitude insures that the sensitivity to a gravity wave is independent of the pre-existing oscillation of the antenna, but the subtraction of successive amplitudes allows a 4-to-1 variation in impulse energies for the same gravity wave, depending upon the arrival time of the gravity wave within the 0.1 second sampling interval. Unfortunately, although these authors did have an electrostatic calibrator on their bar, they used it only as a means of detecting large-amplitude oscillations of the bar, and not as a means of inducing impulsive excitations of the bar, thus imitating gravity waves. Had the authors done the latter, they would have discovered that their seemingly innocuous filter time constant of 0.3 seconds, combined with the 0.1 second differencing interval, reduced the amplitude sensitivity to gravity waves by a factor of 3 (and the energy sensitivity by a factor 9), while not reducing the Brownian noise of the bar at all. The combination of the simple difference algorithm (which allowed a spectrum of response of output amplitudes to a given gravity wave) and the 0.3 second filter time constant impaired the sensitivity of the Frascati experiment by more than a factor of 20 in comparison with that which could have been yielded by the same bar, amplifier, and a slightly-modified processing algorithm.

THE EXPERIMENTS OF THE UNIVERSITY OF MARYLAND GROUP. What are the experimental facts published by the Maryland Group in support of their claims of dectection of gravitational radiation?

Slide eight shows their data of 1970 in the form of a histogram of coincidence rate vs. time delay for the signals between an antenna at the Argonne laboratory and at the University of Maryland.[4]

Slide nine shows their data published September 17, 1973, in the form of a time-delay histogram between those same two antennae, but with better time resolution, better electronics, and better "algorithm".[5]

In addition, we have Professor Weber's comments at international conferences, his speech at The New York Academy of Sciences and elsewhere. Thus he has revealed (Warsaw meeting, Tucson meeting, etc.) that he has detected coincidences (amounting to 2.6 standard deviations) between the Maryland antenna and one operated by Professor David Douglass at the University of Rochester -- these coincidences were not at zero time delay but at 1.2 seconds, corresponding precisely (it has been dramatically emphasized) to a one second offset and a 150 millisecond error in the setting of the University of Rochester clock.

There are thus two questions about the totality of the data of the Maryland group:

(1) Can anything <u>other than</u> gravitational radiation have produced these results, and

(2) Can <u>gravitational radiation</u> have produced these results?

We shall attempt to answer these two questions. But first, we ask "of Weber's published data, which are 'better'?" Note that the 1970 data correspond to about two coincidences per day with a 10:1 real-to-background ratio. With a signal like that, it takes about 2-4 days to verify the existence of gravitational radiation to a considerable statistical significance. The 1973 data correspond to about seven per day "zero-delay excess", but with about a 1:2.5 real-to-background ratio. Under those circumstances, it takes about ten days to obtain as much significance statistically as the 1970 results yield in four days. The 1973 antenna are very similar to the 1970, and Professor Weber states that the system was much more sensitive in 1973 than in 1970. However, the large 1970 signals don't seem to have been observed in the 1973 data. The 1970 processing considered only those gravitational waves which <u>increased</u> the existing energy of each antenna; the processing in 1973 detected those events which either <u>increased</u> or <u>decreased</u> the pre-existing energy of the antenna. Therefore, for <u>equal sensitivity</u> the incidence rate of 1970 should have produced in 1973 four times as many events as in 1970, and four times as many have been detected, but in a much more sensitive apparatus! But this increased detection rate is at the cost of a background which has increased not by a factor 4 but by about a factor 25! Anyhow, Professor Weber says that the University of Maryland has not been able to reproduce the 1970 results -- these results therefore do not satisfy the simplest requirement to be regarded as a physical fact, and I propose that they now be dropped from all consideration as evidence for gravitational radiation.

Thus, we are left with the "1973 data". Many in this room have heard Professor Weber mention in his speeches a computer programming error in connection with these data. Indeed, as you have heard from Prof. Douglass at this meeting, such an error was discovered by Professor David Douglass and communicated by him in a letter of September 14, 1973, to Professor Weber. Here Professor Douglass demonstrated by a detailed comparison of individual coincidences that a zero-delay coincidence is <u>falsely</u> recorded whenever a particular one of the antenna channels shows an above-threshold event in the first or last time bin (0.1 seconds) out of a block of 1,000 bins. The numbers of such false coincidences is enough to explain the zero delay excess reported as the 1973 data.

Thus our question (1) is answered in this case, not only that something besides gravitational radiation could, but that something <u>did</u> account for essentially all of the so-called "real" coincidences of June 1-5 (within the April 22-June 5, 1973, data) published by Weber, et al. There is no reason to believe that the April 22-June 1 data were analyzed in a different manner; so we are left with <u>no</u> published data from the University of Maryland group in support of the detection of gravitational radiation.

Now for the coincidences between the University of Maryland antenna and the Rochester antenna -- at an offset of 1.2 seconds. You have heard at this meeting from Professor Douglas that the University of Maryland group misunderstood the time origin of one of these antennae, and the "significant coincidence excess" was really appropriate to a gravity wave that took four hours, zero minutes, and 1.2 seconds to travel between Maryland and Rochester! In this case, too, gravitational radiation could not have produced the claimed result (question 2). In our own laboratory we have analyzed the power of various "algorithms" to detect coincidences between two tapes containing pure independent computer generated-noise signals. We have used what we regard as an inadmissible procedure of adjusting the algorithm "to maximize the zero-delay coincidence rate." By segmenting the data, with the algorithm "optimized" for each segment, we can get any number of standard deviations of coincidence excess.

Slide ten shows these results. Now how about question number (2) as regards the 17 September time-delay histogram? You have seen how Bramanti, et al. reduced their signal detectability by a factor 10--20 relative to the residual antenna Brownian noise, by the unfortunate choice of a 0.3-second filter time constant. The Maryland group in its 1973 publication says that they used a 1.6 Hertz bandwidth obtained by a "two-stage Butterworth filter." Without quite knowing how the Maryland group defines bandwith in terms of the filter parameters, we have simulated such a coincidence experiment in considerable detail, using the same algorithm embodied in the computer programs which were used to obtained the published histograms.

<u>Slide eleven</u> is a plot of the time delay histogram for coincidences obtained from 162 pulses applied simultaneously to two simulated bars. Here we have grouped the results in bins of one second width. You will see that these pulses, of a magnitude to give each bar at rest 300K of energy, in no sense would provide a time-delay histogram with a single bin "zero-delay excess" of 0.1 second width! In fact, the detection width is more like 5 or 10 seconds than 0.1 seconds. We then considered the possibility of much smaller pulses applied to the two antennas. For such pulses, the coincidence detection efficiency at a time resolution of 0.1 seconds becomes small, and we have used instead of direct simulation a two-stage procedure which is equally valid.

<u>Slide twelve</u> shows the "single events" vs. time after pulse application produced by 1100 pulses of 0.33 kT. You will see that there is still no single bin which contains the majority of the single events. When two distributions like that of slide eleven are folded together, we obtain

<u>Slide thirteen</u>, which shows the time-delay histogram of coincidence detections produced by the small pulses applied simultaneously to two bars. I emphasize that we have used every scrap of information which we have been able to deduce from Professor Weber's published data, and we have not been able

to obtain a time-delay histogram in which the response to gravity wave excitations would be confined to a single bin. Thus, we must answer question (2) negatively -- that gravity waves could not have produced the results published in September 1973.

Recapitulating: We believe the evidence is good that the supposedly significant excess events at zero delay published by the Maryland group in 1973 were due to a computer programming error, which was discovered by Professor David Douglass and which the University of Maryland group has now located and corrected. Furthermore, we believe that those experimental results are of a form which could not have been caused by gravitational radiation, had there been no computer programming error. We thus conclude that the Maryland group has published no credible evidence at all for their claim of detection of gravitational radiation.

The single most convincing way to exhibit the sensitivity of a pair of gravitational wave antennas is to use the electrostatic calibrators attached to those antennas at least since July 1973 to provide those antennas with impulsive excitations of the type which would be produced by the kind of gravitational radiation being sought. Whatever computer algorithm is being used to search for gravity waves can then be used (without change!) to produce the time-delay histogram for the records including the coincidence calibration pulses, and in this way the efficiency of the coincidence apparatus can be determined as a function of simulated gravity wave "energy." We published such data for our single bar in July 1973 -- it is easy to obtain, and it requires the least detailed analysis of the mechanical, electrical, and algorithmic aspects of the experiments. I urge the University of Maryland group to provide such hard data in their publications instead of vague comments as to the existance of coincidences "at the level of 1/100 kT". I urge them also to republish the time-delay histograms of April 22 - June 5, computed with the algorithm used at that time, but with the programming error corrected.

REFERENCES

1: J.L. Levine and R.L. Garwin, Phys. Rev. Lett. 31, 173(1973).

2: R.L. Garwin and J. L. Levine, Phys. Rev. Lett. 31, 176 (1973).

3: D.Bramanti et al, Lett. Nuouvo Cimento 4, 665 (1973).

4: J. Weber, Nature 240, 28 (1972).

5: J. Weber et al, Phys. Rev. Lett. 31, 779 (1973).

FIGURE CAPTIONS

1: Gravity wave detector.

2: Signal processing chain.

3: Phase plane display of large calibration pulse.

4: Detection of 900K calibration pulses using normal computer program. Solid curve is theoretical.

5: Histogram of observed impulsive energy changes referred to an antenna at rest, for 27 days in December 1973.

6: Upper limit for the arrival rate (per day) of pulses delivering energy E_g to the antenna at rest. The curves labeled 'Weber--' are our estimates.

7: Histograms of energy and impulsive energy changes for the antenna of Bramanti et al. We have assigned full-scale values to the (originally unlabeled) horizontal axes.

8: Time-delay histogram of coincidences from Ref. 4.

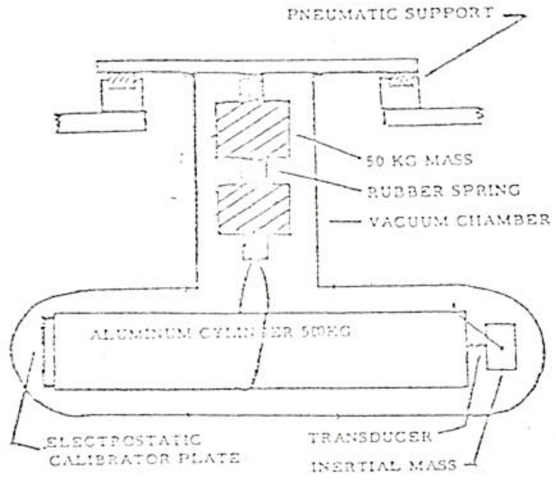
9: Time-delay histogram of coincidences from Ref. 5.

10: Computer experiment demonstrating the creation of apparent correlations between two sets of uncorrelated random numbers. The data were partitioned and the thresholds adjusted by trial to achieve best zero-delay excess for each partition.

11: Computer simulation of the system of Ref. 5, for large (1 kT) pulses. Each point represents an average of ten 0.1 second bins.

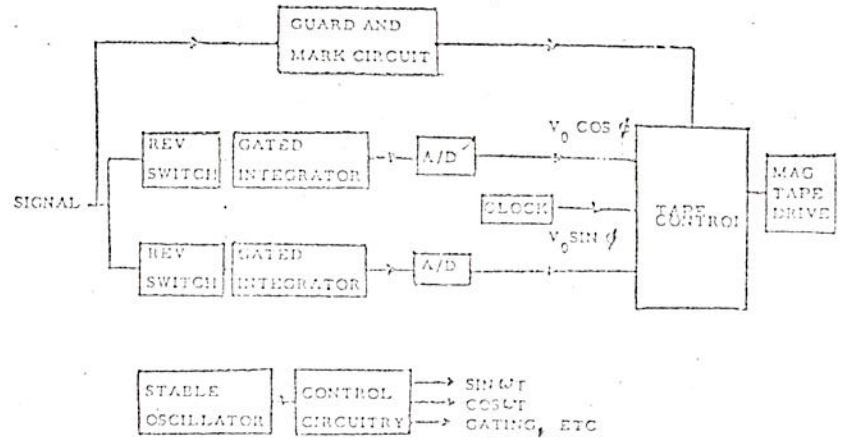
12: Single-antenna Detection probability as a function of time after excitation for small (.33kT) pulses, using the above simulation.

13: Expected form for the time-delay histogram for small pulses, as deduced from Fig. 12 above. We have not included random fluctuations in the bin values.



IBM DETECTOR - F = 1637HZ

FIGURE 1



SIGNAL PROCESSOR

FIGURE 2

PHASE PLANE DISPLAY OF CALIBRATION PULSE

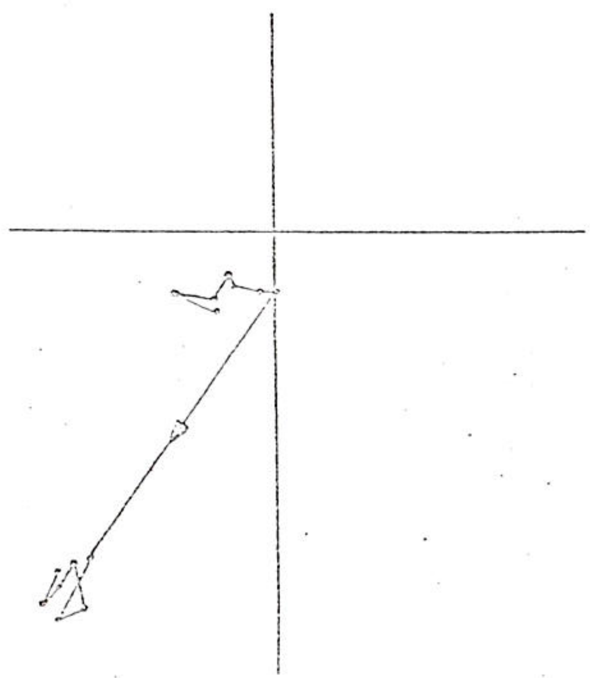


FIGURE 3

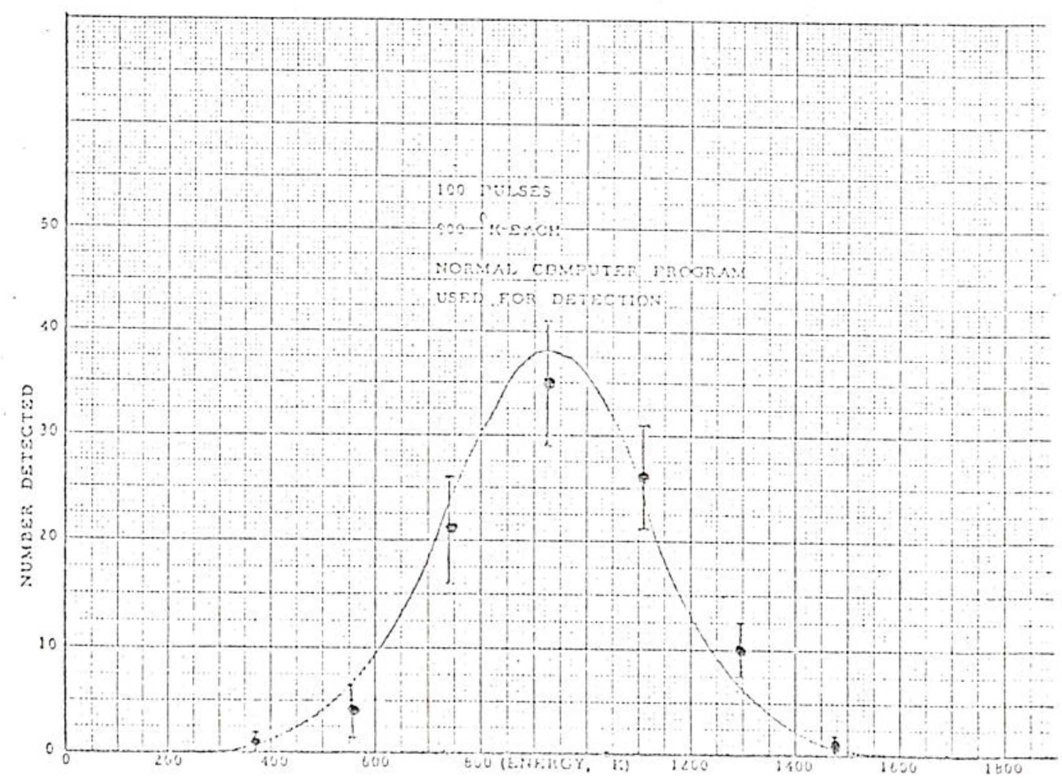


FIGURE 4

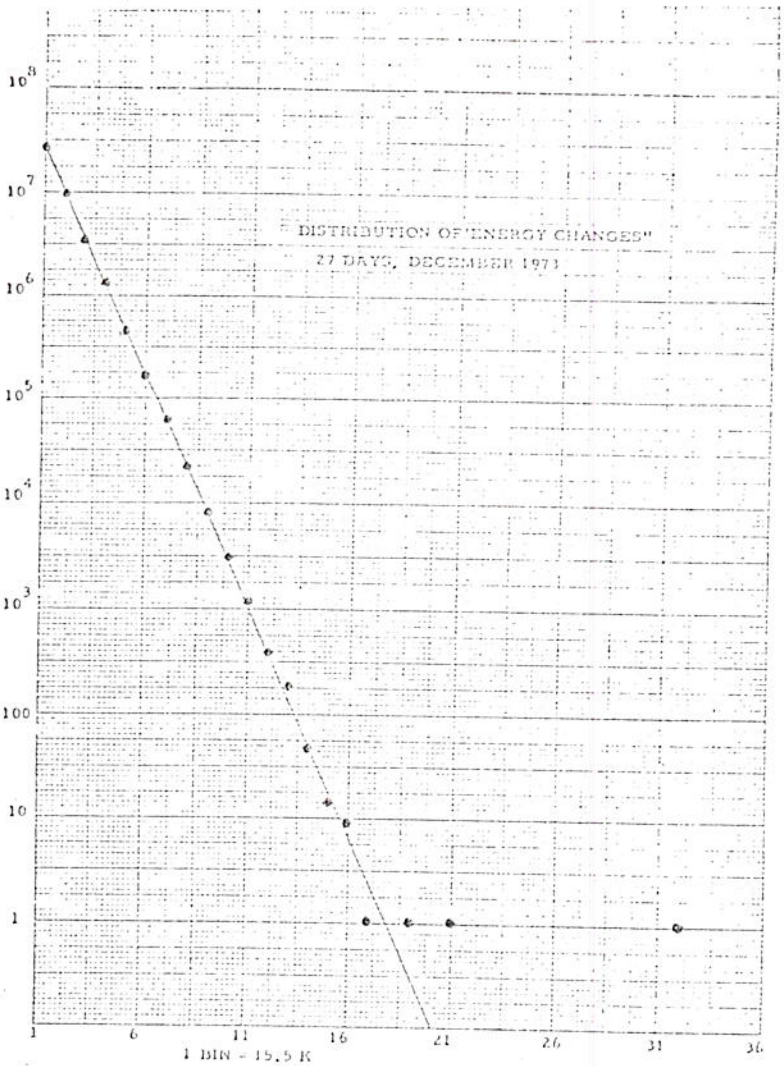


FIGURE 5

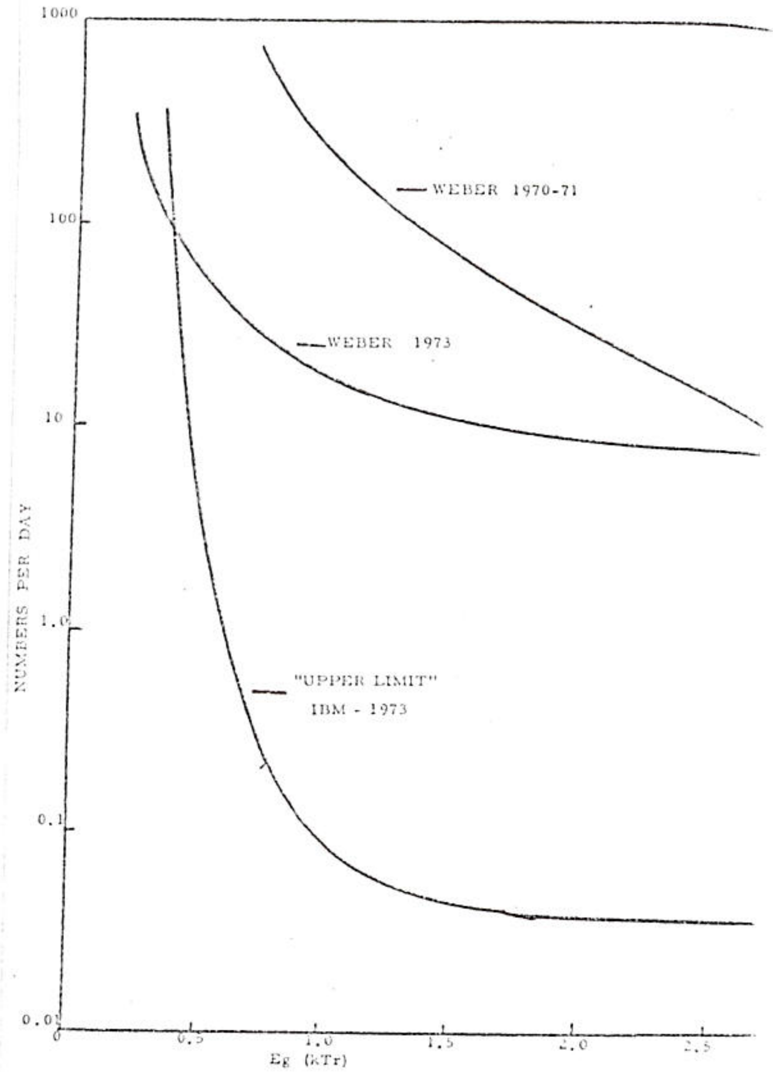
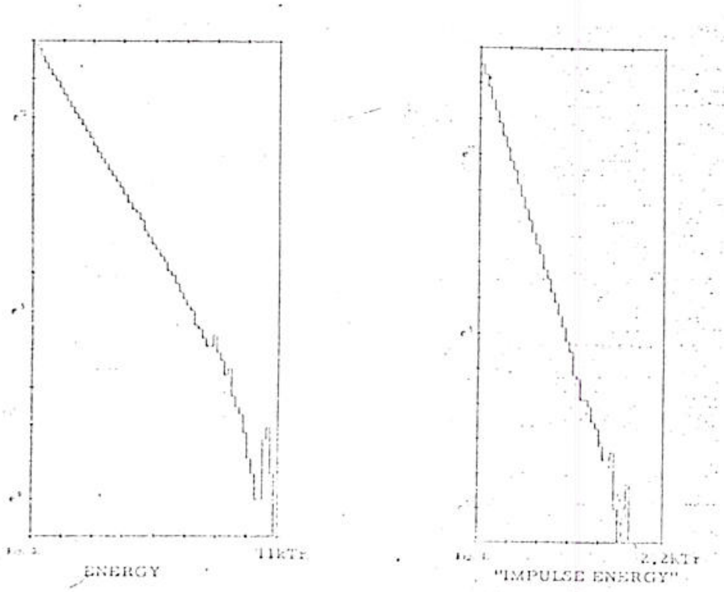


FIGURE 6



BRAMANTI ET AL, LETTERE AL NUOVO CIMENTO 1973

FIGURE 7

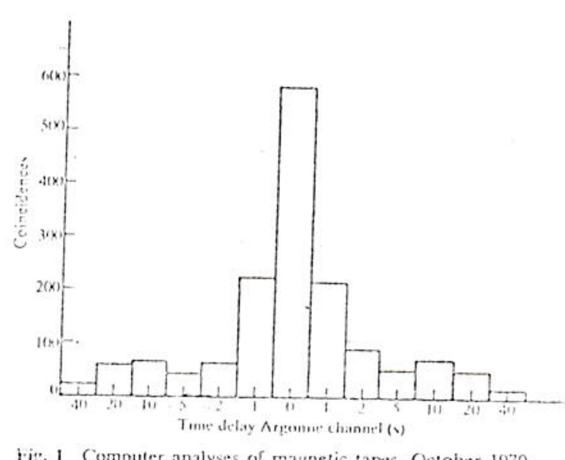


Fig. 1 Computer analyses of magnetic tapes, October 1970–February 1971.

FIGURE 8

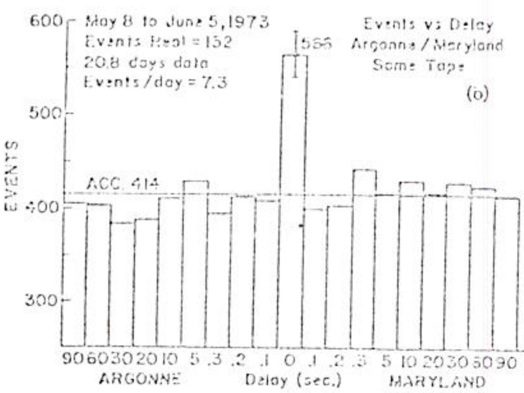


FIGURE 9

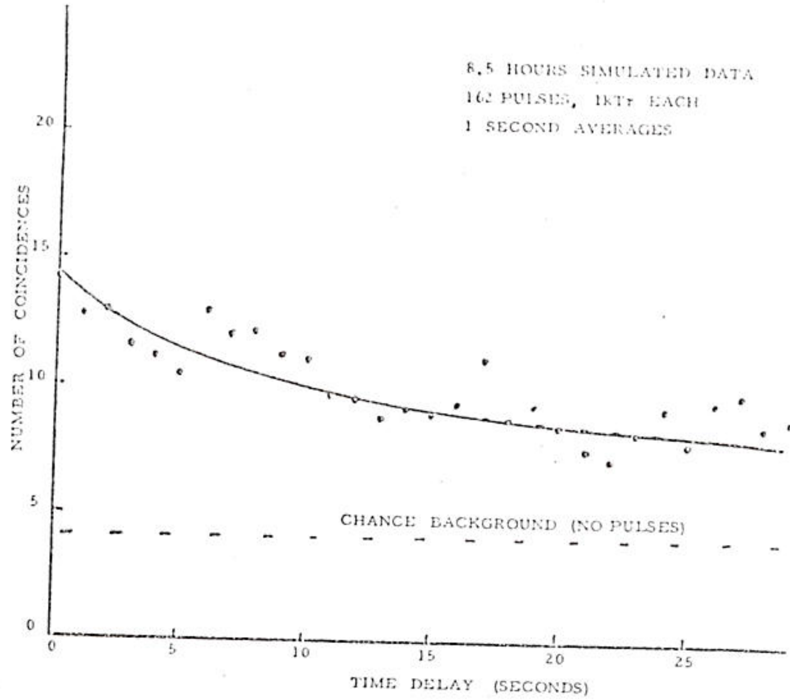


FIGURE 11

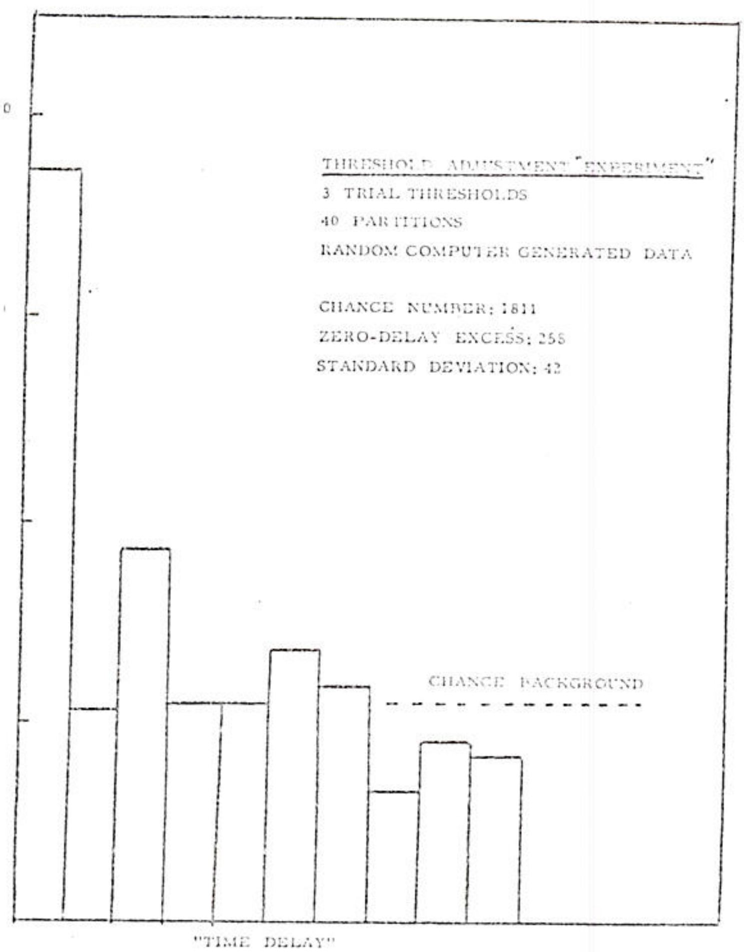


FIGURE 10

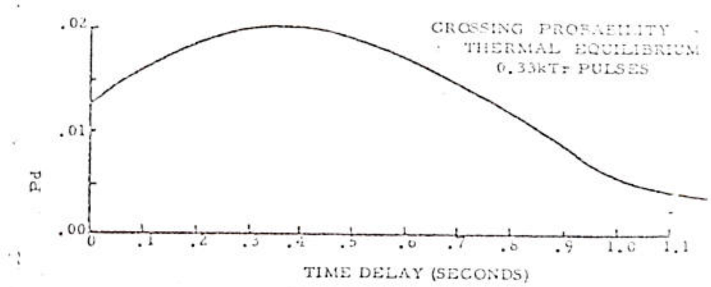


FIGURE 12

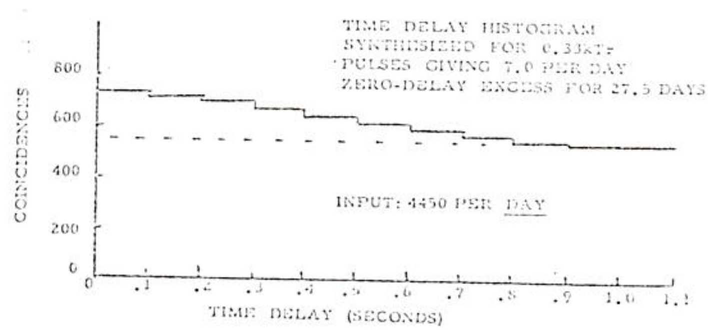


FIGURE 13