search & discovery

No gravity-wave confirmation by the new experiments

Two new experiments have failed to detect gravity waves of the intensity originally reported by Joseph Weber in 1969–70. Richard L. Garwin and James L. Levine of IBM Research Center, operating a Weber-type antenna at 1695 Hz, report[1] no signals after 18 days of serious data collection. At Bell Labs, J. Anthony Tyson, who has a Weber-type antenna sensitive to 710 Hz, found[2] no signals after three months of operation in the present set-up.

The search for gravity waves continues to expand as more detectors come on the air.[3] Two other groups in preliminary reports had failed to detect signals. Vladimir Braginskii of Moscow State University, after running a two-detector experiment for 20 days, reported[4] no events. And Ronald Drever (University of Glasgow), using a pair of detectors, reported at a meeting in Paris in June that he saw no signals.

Meanwhile, the pioneer in the field, Weber (University of Maryland), continues to see coincidences between his detectors at Argonne and Maryland.

Garwin and Levine have an aluminum detector weighing 300 pounds, 60 inches long and 7½ inches in diameter, whose mass is one-tenth that of Weber's cylinders. Some critics have complained about the small mass of the detector. It was meant to be a prototype, and the IBM team is now operating a 1200-pound detector. A piezoelectric ceramic cube is sandwiched between the end and an effectively stationary mass. The bar is a few feet away from a print shop. To isolate the system, the cylinder is supported by a steel cable from a three-stage mechanical filter.

The IBM experimenters look at the amplitude of the bar's oscillation and sample it 40 times per second. Both the energy and the energy increments show a Boltzmann distribution as expected from theory. Garwin and Levine see no excess energies above the Boltzmann distribution.

Using an electrostatic force calibrator at one end of the bar, the experimenters apply a known amount of energy. They calculate that their sensitivity is such that they could see a single event whose energy is 1.5 kT if it happened once every 10 days or so. Their sensitivity is about the same as that of Weber's two-bar experiment during 1969–70, they say. The new IBM detector is expected

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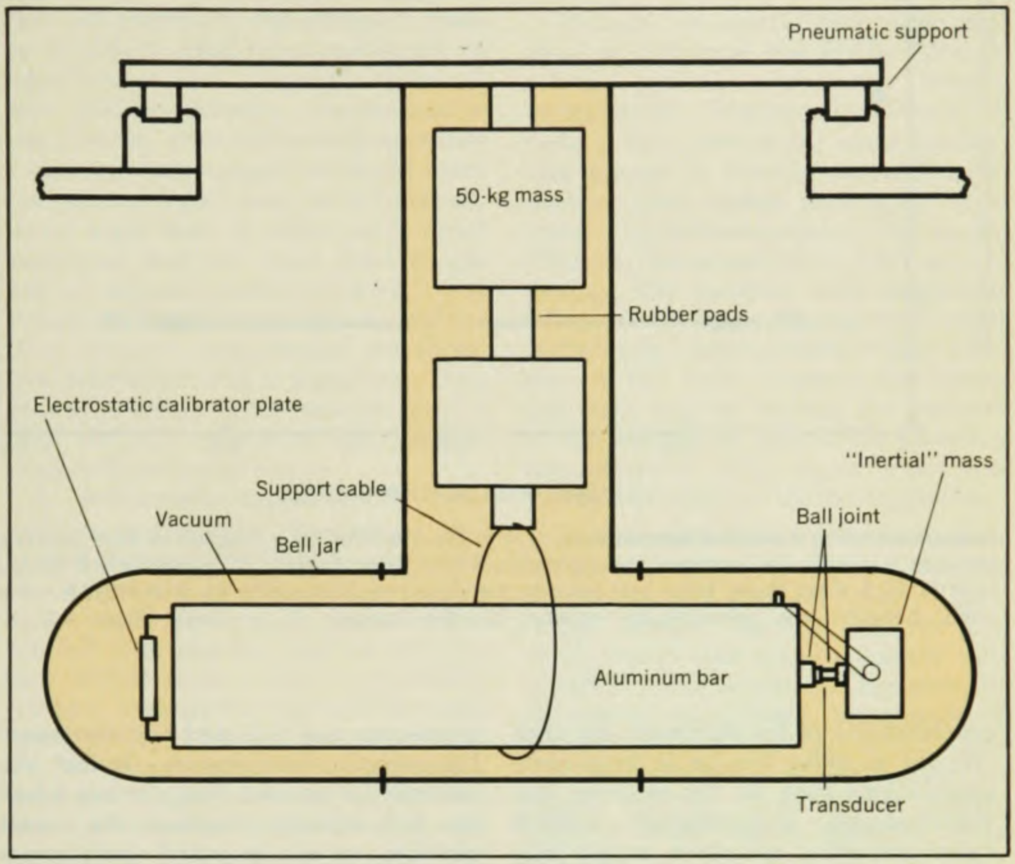


Gravity-wave detector used at IBM has a mass of 120 kg and is sensitive to 1695 Hz.

Record superconductor at 22.3 K

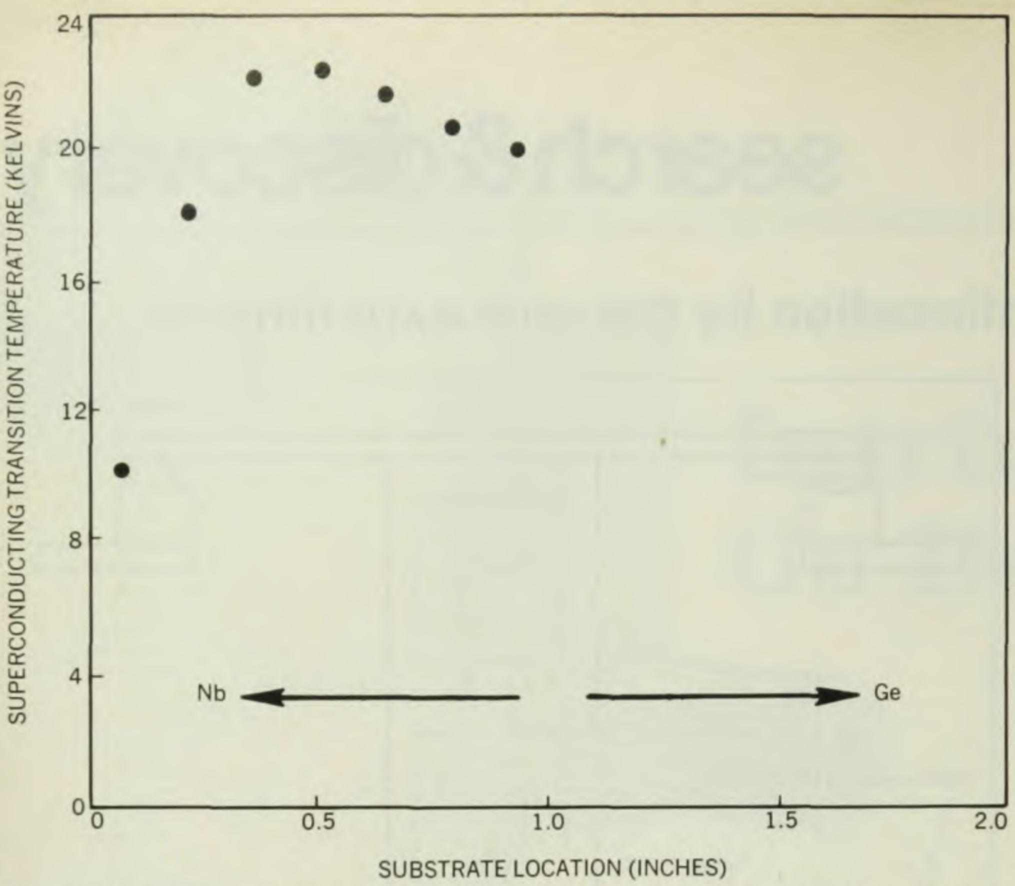
A new record-high superconducting transition temperature, T_c = 22.3 K for Nb_3Ge, was announced recently. This temperature is, most significantly, more than two degrees above the boiling point of hydrogen; a significant step may therefore have been taken to move superconductivity applications from a technology based on liquid helium to one based on liquid hydrogen. The previous high transition temperature reported was 20.98 K for a Nb–Al–Ge pseudobinary alloy.

John Gavaler of the Westinghouse Research Laboratories made the announcement at the Symposium on Superconductivity and Lattice Instabilities held last month in Gatlinburg, Tennessee. The discovery[1] was the result of his work on metallurgical methods of producing a purely stoichiometric form of Nb_3Ge. After producing his samples and determining their transition temperatures in the Westinghouse Laboratories in Pittsburgh, Gavaler took them to the Francis Bitter National Magnet Laboratory in Cambridge; there, he found that the critical magnetic field at the boiling point of hydrogen was over 30 kG and its extrapolated value at absolute zero would be 370 kG.

When we asked Gavaler about the

Critical temperature raised to 23.2 K

Note added in proof: Louis Testardi of Bell Labs announced in a postdeadline paper at Gatlinburg that he, J. Wernick and W. Royer had obtained T_c = 23.2 K for Nb_3Ge. They made the discovery only a few days before the conference using a technique similar to the one used by John Gavaler of Westinghouse Research Labs.



Superconducting transition temperature (T_c) of Nb–Ge films as a function of their location beneath a composite niobium and germanium sputtering target. T_c reaches the record high of 22.3 K on those films that form in the purely stoichiometric form of Nb_3Ge. For films beneath the germanium portion of the target, T_c is less than 4.2 K.

practicability of his discovery, he said: "As far as using Nb_3Ge in large-scale applications such as, for example, the Westinghouse experimental 5-MVA superconducting generator, we are only halfway there." The material, he explained, has been demonstrated to have a transition temperature of 22.3 K, good magnetic-field properties, and stability at room temperature. The trouble is, however, that the samples of this material are now in the form of films ⅛ in. × ½ in., which are only a few thousand angstroms thick. The problem now is to make it in a form suitable for high power applications, and this is a substantial engineering task. He also said, "As far as low power applications, such as microwave and magnetic field detectors, the material is already in a proper configuration and therefore devices of this type should be soon forthcoming."

Matthias. Nb_3Ge grows in the β-W structure (also called A15 structure), as does $Nb_3(Al\text{-}Ge)$, Nb_3Sn and other superconducting compounds. The superconducting properties of Nb_3Ge were originally studied by Bernd Matthias (University of California, La Jolla and Bell Laboratories). He first made the substance by arc melting and found the transition temperature to be $T_c = 6$ K, much lower than he expected. He reasoned that the material he produced was off stoichiometry, that is, the ratio of the niobium atoms to the germanium atoms was not 3:1, and this depressed the transition temperature. In fact, the material he had was $Nb_{3.3}Ge$ but it had the A15 structure because the excess niobium atoms assumed germanium sites in the crystal lattice.

The purely stoichiometric form of Nb_3Ge is unstable at high temperatures, so that Matthias and his collaborators next tried to produce it by a fast quenching method that would freeze in a metastable structure. They used the method called "splat cooling," where the molten niobium–germanium mixture is blown out of the furnace onto a cool collecting plate. The material they produced in this way had transition temperatures ranging up to 18 K. Some independent x-ray studies indicated that they had almost achieved the purely stoichiometric form and progress stopped there until Gavaler brought a new metallurgical process into the picture.

Gavaler makes his samples of Nb_3Ge by using some novel variations on the standard dc sputtering technique. A niobium sheet is connected to a slice of germanium to form a composite cathode. The substrates upon which the Nb–Ge compound is formed are positioned about one inch from this cathode. The sputtering argon gas pressure is kept high (0.3 Torr) and the temperature of the substrate is kept between 700°C to 950°C. Gavaler told us that the sputtering becomes like a diffusion process under these conditions. The high pressure of the gas reduces the mean free path to less than a millimeter, so that a thorough mixing of the sputtered niobium and germanium particles occurs, on an atomic scale, by the time they reach the substrate. The substrate temperature is high enough to permit some mobility but not high enough to break up the metastable structure of the purely stoichiometric form of Nb_3Ge.

Of course not all the material deposited on the substrates is purely stoichiometric. The material deposited nearer the germanium portion of the cathode is germanium-rich, and the material deposited nearer the niobium is niobium-rich; in between, there are gradations. After the material is deposited, Gavaler measures the properties of the films as a function of their locations. The niobium-rich films have superconducting transition temperatures starting at 9 K. The T_c's increase as the material becomes more truly stoichiometric to the record high of 22.3 K and then decrease rapidly as the material becomes germanium-rich (see figure). In fact the very germanium-rich films show no superconducting behavior.

When he performed the sputtering under conditions of lower gas pressure or at substrate temperatures that were too high or too low, Gavaler found that the maximum transition temperature he could achieve was substantially reduced. —RJC

Reference

1. J. R. Gavaler, Appl. Phys. Lett. 15 October 1973.

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to be four to six times more sensitive than the old one.

The IBM team says that either Weber is observing very large pulses and seeing essentially all of them (in which case IBM should have seen them) or he's seeing smaller pulses that come through at a very high rate so he does not see all of them (in which case IBM might not have seen them). Levine told us that Weber's detector sensitivity depends on the thermal energy that happens to be in the bar, a value that fluctuates. Weber records the square of the amplitude, on a 0.1–1-sec resolution basis, throwing away the phase information, Levine said. IBM, on the other hand, measures both the phase and the amplitude.

Levine remarked that a single detector was all that was needed for a null comparison with Weber's techniques. If the IBM experimenters had detected any signal, they would need a second antenna to eliminate effects of earthquakes, building vibration, and so on. "But if you see nothing, then there's nothing there." A second detector in that case would only have the effect of doubling the sensitivity.

Tyson has an aluminum bar weighing 8000 pounds, 12 feet long, 2.5 feet in diameter, and its mass is about three times that of Weber's cylinder. Almost all of the experimenting has been done with the main longitudinal mode, which is at 710 Hz. It is also sensitive at 2080 Hz. Once a new amplifier is added, Tyson expects the sensitivity at 2080 Hz will be about the same as Weber has now at 1660 Hz.

By putting fake gravity waves into the antenna, Tyson gets blips on a calibrator plate. For example, he applies a small glitch at ($\frac{1}{4}$) kT and sees if it shows up on his chart recorder. By this technique he finds that the detector can tell if the energy has increased by as little as ($\frac{1}{4}$) kT in 1 sec. Tyson says that his present sensitivity is 40 to 100 times Weber's sensitivity in 1969-70.

Tyson told us he found no events above ($\frac{1}{2}$) kT in three months of observation, and from random statistics, he would have expected none. He feels that one of two conclusions can be drawn. Either the source Weber saw in 1969-70 has decreased its intensity or else he was seeing something other than gravitational radiation. Tyson noted that Weber is still seeing his effect and has now increased his sensitivity by a factor of ten or more. He remarked that the effect has always remained right at the noise level, so that it corresponds to a flux that is much smaller now than it was in 1969-70.

A coincidence experiment is now operating between the original antenna and another run by David Douglass at the University of Rochester. They hope to have a sensitivity ten times better at 710 Hz than Weber's present sensitivity (at 1660 Hz). Some critics of the experiment have noted that the spectrum of the source may not be wide enough to be picked up by the antenna. In earlier experiments, though, Weber had detected signals with a disc antenna sensitive to 1030 Hz, implying a fairly broad-band spectrum. The antenna, Tyson says, now has an effective temperature of 40 K at 0.1 sec resolution and 13 K at 1 sec resolution; the Rochester antenna is less sensitive by a factor of two.

Weber, when asked to comment, said, "The single-detector gravitational radiation experiments suggest that there are not *at the present time* enough pulses of energy exceeding $kT/4$ to account for observations of the period *1969-70.*

"That may be true. However, the 1969-70 experiments, like the present ones, were capable of recording coincidences in response to pulses of lower energy, albeit with a detection efficiency much less than one. Information on detection efficiency of present experiments is included in a paper in press in *Phys. Rev. Letters.* Earlier instrumentation did not permit measurement of that part of the energy not due to internal noise. However, we can examine two things which are known, to obtain some evidence for the energy input. Firstly, for a typical two-detector coincidence, the amplitudes reached by the detectors were often very different, suggesting that the amplitude was generally made up of a fairly small bit of real signal on top of a lot of noise. Secondly, the small ratio of three-detector coincidences to two-detector coincidences indicates that a detector only crossed threshold when the external signal and the intrinsic detector noise were in phase, once again suggesting that the input pulse energy was not large compared with noise. The statistics of both the amplitudes and the three-detector coincidences are therefore consistent with a significant fraction of observed pulse energies smaller than the $kT/4$ limits of the Garwin-Levine-Tyson observations."

A well-known experimenter, commenting on the three experiments, said he was impressed with the instrumentation employed by IBM and Bell. On the other hand, he said, he is also impressed with the big peak at zero lag that Weber finds. That is, recording the data from the two bars and then correlating them, Weber finds that when the two signal peaks are in coincidence with each other (zero-lag peak), the peak is "very convincing," sticking way out above the peaks at longer lags. —GBL

References

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2. J. A. Tyson, Phys. Rev. Lett. **31**, 326 (1973).
3. J. L. Logan, PHYSICS TODAY **26**, no. 3, 44 (1973)
4. V. B. Braginskii, A. B. Manukin, E. I. Popov, V. N. Rudenko, A. A. Khorev, JETP Lett. **16**, 108 (1972).

Anomalous water: an end to the anomaly

Several years ago the possible discovery of a form of water with anomalous properties—high density, high refractive index, low vapor pressure, low melting point—generated intense experimentation and heated debate (PHYSICS TODAY, September 1969, page 61; October 1970, page 17). In a new book[1] (*Recent Advances in Adhesion,* edited by Lieng-Huang Lee of Xerox Corp), Boris V. Deryaguin, whose group of workers at the Institute of Physical Chemistry of the Soviet Academy of Sciences in Moscow first reported this anomalous water[2] (later called "polywater" by others to suggest a polymeric structure), has concurred with his former opponents that the observed properties are caused by the presence of impurities rather than by a new structure of the hydrogen and oxygen atoms. Deryaguin states that he and his coworkers, using an electron-probe technique, detected silicon and/or some other impurities in even the cleanest samples of anomalous material.

Although the central issue of the existence of polywater has finally been resolved, several phenomena remain unexplained. One mystery, Deryaguin feels, is that fresh water vapor condensate appears to dissolve quartz and impurities more readily than aged liquid water. In their experiments Deryaguin, Churaev, Zorin and others were able to produce the material with anomalous behavior only by condensing water vapor periodically under pulsing vapor pressure on the walls of quartz capillaries, and then only by varying the pressure of the vapor; no anomalous behavior was observed when liquid water was introduced directly into the capillaries.

An explanation for this phenomenon has been proposed by Barry Brummer (Environmental Impact Center, Cambridge, Mass.). He feels that the material present will dissolve equally well in either liquid or vapor. However, in the case of water vapor, the amount of water is so small that the initial material that dissolves produces a more alkaline solution than liquid water; hence it more readily dissolves other material.

A second unresolved question is the exact nature of the material that was once taken to be anomalous water. Many impurities such as sodium, boron, silicon and carbon, have been found to varying degrees in the samples prepared by different experimental groups. Yet no one knows which of these elements are essential constituents of the anomalous material. Brummer and his former colleagues from Tyco Laboratory in Waltham, Massachusetts believe the material is primarily organic in nature, with the organic materials being drawn from the atmosphere.[3] They concur with Denis Rousseau of Bell Telephone Laboratories that the material behaves like a carboxylic acid, a substance found in human perspiration. Others have similar experimental evidence for the presence of both organic and inorganic material but do not take a stand as to which constituents are most important. Deryaguin suggests the formation of a gel or sol of silicic acid, with sodium atoms possibly penetrating into the condensate simultaneously. Others have felt that the silicon impurities present are in the form of "chunks" of glass and are not uniformly distributed