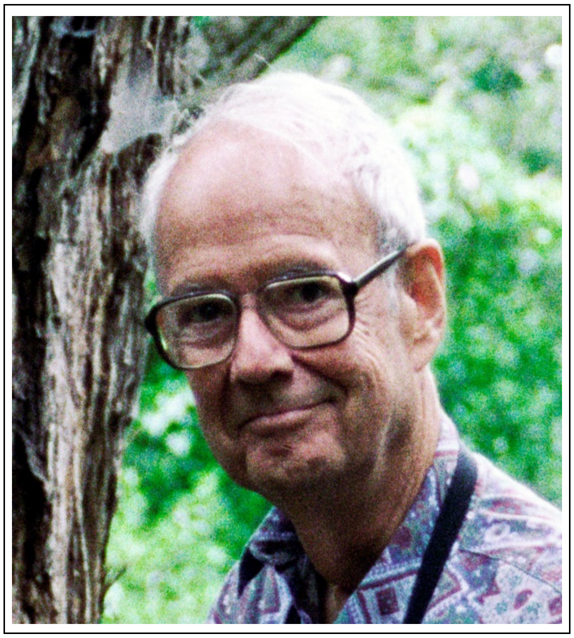

Frank Estabrook, c. 1995

FRANK B. ESTABROOK
(Born 1922)

INTERVIEWED BY
SHIRLEY K. COHEN

February 28, 2007

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Gravitational waves, JPL

Abstract
An interview on February 28, 2007, with Frank B. Estabrook, Distinguished Visiting Scientist at the Jet Propulsion Laboratory. Dr. Estabrook received his MS (1947) and his PhD (1950) in spectroscopy at Caltech. He joined JPL in 1960, becoming senior resarch scientist there from 1979 to 2006 and Distinguished Visiting Scientist in 2006. In this interview, he discusses his youthful fascination with general relativity and his later research, with H. D. Wahlquist, J. W. Armstrong, and B. Bertotti, on the development of proposals to detect gravity waves by means of the Doppler tracking of spacecraft. He discusses the involvement of Caltech theoretical physics professor K. S. Thorne in these efforts, and the inclusion of gravity-wave experiments in the Galileo, Mars Observer, Ulysses, and Cassini missions.

Administrative information

Access

The interview is unrestricted.

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ORAL HISTORY PROJECT

INTERVIEW WITH FRANK B. ESTABROOK

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Frank Estabrook in Hawaii, c. 1995. Photo by Marc Estabrook

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Frank B. Estabrook **by Shirley K. Cohen**
Pasadena, California **February 28, 2007**

Begin Tape 1, Side 1

COHEN: Welcome, Dr. Estabrook. Let me ask you first when your interest in gravitational waves started.

ESTABROOK: Well, it wasn't really gravitational waves; it was Einstein's theory of gravity. I just had a boyish interest in and love of it, I suppose. I think a lot of kids who start out in astronomy or physics hear about these things—quantum theory and gravity—and they want to find out what they are. Actually, the formal start was probably a course I took at Caltech from Richard [Chace] Tolman in 1946-47. It was the last time he taught the course. He had been very influential, high up in the war bureaucracy. He came back to the campus and taught for two years. He taught general relativity one year and statistical mechanics the next; and I was fortunate to have him in both. Not long afterward he died [September 5, 1948—ed.]. It was an elegant course, and he used, of course, what at that time was the very famous book he had written, *Relativity, Thermodynamics, and Cosmology* [Oxford, U.K.: Clarendon Press, 1934], and we studied out of that book. And then I worked on the book, and from then on more or less was devoted to general relativity after that course. I wasn't able to do a PhD in it; after Tolman died, there was nobody on campus to be a thesis advisor for a number of years. I think Bob [H. P.] Robertson was appointed and then immediately went off on leave of absence, to be Eisenhower's chief scientist in Europe. During those years—'47 up to '50—he wasn't here, and I got my PhD in 1950. I did my work in spectroscopy as an experimentalist, and due to the kindness of my thesis advisor, I got through. My advisor was Robert B. King, whose father [Arthur S. King] was the famous Mount Wilson spectroscopist in Pasadena.

COHEN: So then you had various other positions, but you came back to Caltech in 1960?

ESTABROOK: I had a checkered career. I went off to Ohio first [assistant professor, Miami University, 1950-1952] and then I was down at [North American Aviation] Downey, working in nuclear reactor physics. It wasn't particularly an academic career at all. You say I came back to Caltech—I actually came back to JPL [Jet Propulsion Laboratory]. My wife and I liked Pasadena; we had already been in Pasadena [for the preceding five years], when I worked for Army Ordnance, which had a research-contracting office in Pasadena. We gave out basic research contracts to academia on the West Coast. We were a branch office of the Office of Ordnance Research [OOR], which was in Washington, D.C.—or maybe North Carolina, I don't remember. But anyway, I was in and out of Caltech with research contracts for that five years, from 1955 to 1960. There were actually some very fine people here who got contracts from us. We tried to pick good people and tell them to propose something and we would fund them. We weren't putting any strings on it at all; it was the quality of the work. Frank Press was one of them, and other people in seismology. Pol Duwez, [John D.] Roberts in chemistry. And then UCLA, Berkeley, Seattle (Hans Dehmelt)—up and down the coast. I also then had the chance to interact with Bob Robertson, who helped me with some little research problems I had conceived.

COHEN: It seems that a big highlight of your career was meeting Hugo Wahlquist.

ESTABROOK: The branch was closed and I moved to JPL in 1960. I hatched a deal with them to work half-time on relativity and half-time on reactor physics, which they were interested in, briefly, at that time. One of the first people I met when I came to JPL was Hugo Wahlquist, who became my lifelong colleague. Hugo had completed a master's degree at Caltech in astronomy [1954], and he was a very independent-minded person. He discovered that he did *not* enjoy measuring plates and doing observational astronomy. He had been working part-time at JPL, and he simply converted to full-time; he was doing plasma physics research when I arrived. And somehow—I don't remember, looking back on it, when we first met. Hugo thinks he remembers. But we became friends, and I sort of introduced him to general relativity, and he is such a brilliant guy that he learned it—just like I had—from the bottom up. There was a renaissance going on in those days, with lots of new work in relativity coming from Germany and England. In particular, from a man named Professor [J. L.] Synge—actually Ireland; he's from Dublin. And we studied that work.

COHEN: And that work had been done about that time?

ESTABROOK: Yes, it was sort of a renaissance in relativity—about 1960. The people who worked with Einstein had just about run out of time, and new people came in. There were new ways, new mathematical methods, of treating relativity—which, as I say, started with Synge and the English school and the German school under Jordan. Hugo and I learned that, and we made a contribution. After about two years of working together, we formulated what we called the dyadic (tetrad) version of Einstein's field equations. It's the same physics, but it was a new mathematical formulation—new partial differential equations—and it made a big difference; we were able to solve a number of problems. And during the 1960s, we were free to [do this] work at JPL. The money was coming in, it was a new laboratory, the space agency [NASA] was brand-new. They weren't overstaffed with bureaucrats at the top. They just funded JPL and let [JPL director William H.] Pickering and the people at JPL decide what they would work on, under general guidance. As long as Hugo and I were able to publish papers—significant papers—we were free to do so, for about ten years.

COHEN: Ah, that must have been wonderful!

ESTABROOK: It was great, and we learned a great deal.

COHEN: Was there anybody here on campus who you collaborated with?

ESTABROOK: No. Well, Bob Robertson had come back in 1949 or '50 and I had maintained contact with him. But I'd never worked under him or with him. I'd take things to him once in a while and he would critique them, or tell me that somebody else had already done that. But what happened was he died.

COHEN: That was from an automobile accident [August 26, 1961—ed.]. A great tragedy.

ESTABROOK: Carl Anderson was head of the physics division around that time [1962-1970], and they worried some about who was going to work Robertson's notes up into a book. I declined that, because I'd sort of been doing it differently, in my own way. But Anderson needed someone to teach his course—an absolutely marvelous opportunity for me. So twice I taught Physics 236. It still has the same number today. It was *the* course in relativity. I came down from JPL and I was paid half-time by the campus those years—probably ’62-’63 and then again ’64-’65; it was taught every other year.

COHEN: Did you enjoy teaching?

ESTABROOK: It was the greatest experience of my life. Of course, then I *really* learned.

COHEN: Yes, there’s nothing like teaching, for that.

ESTABROOK: And there were brilliant students. Bill Burke was a student the first year; he was a junior taking a graduate course; he got an A in the course. The second time I taught it, Bill was my TA. And Jim Gunn, who’s a famous astronomer these days. Bill Kinnersley. So I learned a great deal. Hugo and I were doing our research sort of in parallel at the same time, so that set me up.

COHEN: Now, when did you get involved with rockets?

ESTABROOK: Well, of course, JPL was flying spacecraft, and I had friends in various divisions around JPL. But to tell you the truth, most of my friends were probably in the basic research area at JPL in those days. But about 1970, the research climate changed at JPL because NASA became mature. JPL’s programs had become mature; we were firing interplanetary spacecraft off to the planets, and the precision tracking at JPL was one of their technological triumphs. They learned how to do things called phase-locked loops, which enabled them to navigate spacecraft with great precision, and navigating around the solar system with great precision of course reminded everybody of the classical Einstein tests of the motions of planets, which showed up in small extra effects beyond Newton’s predictions—the tests that were the diagnostic experiments to validate Einstein’s theory. So by the 1970s, JPL was doing what they called precision tests of general relativity. These were light deviation, anomalous time delays and advance of perihelion, and I was part of that. In fact at one point I put a couple of terms into their tracking equations to account for general relativistic effects, and I became friends with the guys who were doing that radio science—primarily John Anderson; later on, Ronald Hellings.

About 1970, Hugo and I started having trouble getting funded.

COHEN: So the climate was changing.

ESTABROOK: The climate had changed. At JPL, there's no such thing as tenure, and you have to hustle to get interesting tasks where there's money to pay your salary. And a little bit cynically, but also because of the interest already at JPL in general relativity, Hugo and I talked about what we could think of to do that would be interesting and would help pay our salaries.

COHEN: And would be of interest to the funders.

ESTABROOK: And the next step beyond spacecraft tracking for solar-system tests, the next clear step, was obviously to, at some point, look for gravitational waves—which are entirely different. They wouldn't be due to local perturbations of gravity; they would be coming from the far reaches of the universe, generated by extreme cataclysmic events, which now we know a lot more about. They really *are* out there—black holes, super massive sources.

COHEN: But they're still being sought.

ESTABROOK: Well, we're still looking for gravity waves.

But Richard Davies, who was a friend of ours at JPL—he's still alive and well in Pasadena—Richard had made a preliminary calculation of certain kinds of transverse waves and what they would do to what's called Doppler tracking, where from Earth we monitor the position of a spacecraft by recording the frequency and phase of the signal that's transponded back and forth to it. There had been a man who was a visiting associate of John Anderson—a fellow whose name was Alan Joel Anderson, from Sweden—who had actually taken a very short stretch of data and looked at it and made the rash pronouncement that he saw gravity-wave effects in it. This was not correct. But it was in the air, and so we—

COHEN: People considered it?

ESTABROOK: They considered it but made no analysis of the sensitivity of the experiment and the background noise and what the signal-to-noise ratio would be—and essentially the feasibilty of any such tracking. So Hugo and I actually did that, in a rudimentary way. We first published the derivation of the correct effect that one would see in the Doppler tracking of a distant spacecraft, due to the time of transmission to the spacecraft and back, if a gravitational wave came through the solar system. It interferes with the signal you observe from the spacecraft in essentially three different moments, due to the echoes that are going on. So it's a very characteristic signature, which one might observe. [Estabrook, F. B., and H. D. Wahlquist, "Response of Doppler Spacecraft Tracking to Gravitational Radiation," *Gen. Relativity and Gravitation* 6:5, 439-47 (1975)]. We pointed that out, and I then started making some friends in the communications division at JPL, where people were actually doing the tracking and getting the data back.

COHEN: These were the experimentalists?

ESTABROOK: We didn't call them experimentalists. They were sort of observers. They were engineers who were doing the tracking of spacecraft using high-precision Doppler. They had installed very-high-precision frequency standards, and they used the huge antennas at the different DSN [Deep Space Network] antenna complexes to monitor the spacecraft. They were getting long stretches of data. I talked to a man, first, whose name was Richard Goldstein, a very well-known radio physicist in that division. Richard pointed me to two other men to talk to—Charles Stelzried and his partner, Boris Seidel; and they educated me on how the DSN, the Deep Space Network, worked. How they did their tracking, how they sent up the signals, where they came from in the frequency and timing system, what the precisions and the uncertainties were, and also in the reception—the signal-to-noise ratio, how it influenced the precision of the measurements you can make. So I became friends with them, and Hugo and I were able to put actual limits on the sensitivity we could achieve if we had a sustained spacecraft-tracking experiment devoted to gravity waves. And it *was* feasible—at a level four, five, six orders of magnitude poorer than LIGO [the Laser Interferometer Gravitational Wave Observatory].

COHEN: Ah, I was going to ask you about that.

ESTABROOK: But we succeeded in selling the idea to a lot of people as sort of a pre-experiment, because we were just using the onboard equipment and DSN instrumentation that was already in place; we weren't asking for lots of money. So it was an experiment that was possible, that was unique. It had a very low chance of success, but we sold it as a worthwhile thing to search for and to get experience in, until the future brought better technology.

COHEN: At that time, LIGO was already in the air? Or, you know, being talked about?

ESTABROOK: In 1963 I had another formative experience, which was that I went to a summer school in the French Alps for eight weeks. It was very influential, because a lot of young people from around the world were there and we all got to know one another; it was run by the DeWitts from Texas. Kip Thorne was there as John Wheeler's graduate student, so we got to know Kip in 1963 at that summer school. And at that summer school, Joe [Joseph] Weber gave a series of talks, and that was the first I had heard about Earth-based gravity-wave experiments. Joe Weber's work was well underway at that point. I don't remember if anyone talked about—well, they must have—interferometry. There later was a fellow [Robert L. Forward] at Hughes Research [Laboratories] who had been a student of Joe Weber's, who built the first interferometer to look for gravitational waves. But I don't know whether LIGO per se had been— I think Kip had to be here. He came to Caltech in 1966, and he took over [my] course. I think Kip must have been here several years before he and [Ronald W. P.] Drever and [Rainer] Weiss started LIGO.

COHEN: Well, Kip was not an experimentalist, so—

ESTABROOK: Well, he educated himself in all of that, of course, too. And so, starting in the 1970s, Kip was our backer at JPL.

COHEN: In doing this work?

ESTABROOK: In saying, "This is not nonsense. What these guys are proposing is reasonable, and it supplements LIGO."

COHEN: So this was the first experiment, in some sense.

ESTABROOK: Well, Joe Weber's was the first.

COHEN: No, no, I'm talking about Kip's involvement—

ESTABROOK: Well, Kip was a backer of Weber in those days, of course. So anyway, it grew then. After a few years, we were able to make some proposals to upcoming deep-space missions, not just Earth-based. Gravity-wave searches in space have to look for very long wavelengths. (LIGO searches for gravitational waves a million times shorter in wavelength, or a million times higher in frequency than those we looked for.) This is dictated by the stability curve of the frequency-timing standards (e.g. hydrogen masers) that the DSN uses for its tracking. So we had to have spacecraft that went at least as far as Mars in order to have comparable time delays for reasonable experiments. So we started proposing for missions like Voyager and ultimately Galileo. [Tape turned off; then back on]

COHEN: We were talking about the experiments.

ESTABROOK: Hugo and I, through our engineering friends in Division 33—Division 33 is the telecommunications division at JPL, which is where all the spacecraft tracking and DSN operating is done—met a gentleman there named John Armstrong, who became a lifelong colleague. Armstrong and Wahlquist and I did all the subsequent gravity-wave tracking at JPL. John Armstrong is a radio astronomer, so I think he knows your husband [Caltech astronomer Marshall Cohen]. He's spent time at Green Bank [the National Radio Astronomy Observatory], and so forth.

COHEN: I'm sure they do know each other.

ESTABROOK: The reason we first went to him is that he was a specialist in the fluctuations of the interplanetary plasma. When we do microwave tracking, it goes out to the spacecraft and back through a very tenuous but real plasma of charged particles. And then, right at the Earth, the microwave signal also has to go through the Earth's troposphere. John had measured and done a lot of observational work on the effects of the media on radio astronomy; and he was able to help us put limits on our precision in that tracking. He became, as I say, a colleague, and he learned gravity waves from us and became a co-experimenter of mine on Galileo, the spacecraft that went to Jupiter, and then he and Hugo became the experimenters on the Cassini mission to Saturn, which is still going on. On the way out to Jupiter, and on the way out to Saturn, there were long periods of time when we were able to get tracking data. We wanted the spacecraft to be very quiet and not have other experimenters moving it around. This, again, is why these experiments were essentially free experiments: They didn't impact the planetary community much, or hardly at all, because the main purpose of these missions was planetary science—we were just riding along. We had a little trouble getting onboard sometimes. These planetary scientists didn't necessarily think that gravity waves were a proper use of their spacecraft, and NASA headquarters had to be educated. Ed Weiler helped a lot in this and supported us. I might say that the Voyager mission never did allow us to do gravity waves, because [Voyager project scientist] Ed [Edward] Stone was not very sympathetic. He thought the primary purpose of the mission was to do planetary science, and at one point Kip and I went to talk to Ed, and we were unable to sway him.

COHEN: I would guess that when he makes up his mind, he makes up his mind.

ESTABROOK: But I did get on Galileo as an experimenter, and once we got on Galileo, that broke the ice, and we've been on several successive missions. Armstrong was the PI [principal investigator] on the gravity-wave experiment on the Mars Observer [MO], which disappeared when it got to Mars. It isn't usually said out loud, but the only science that was ever done on that mission was the gravity-wave search on the way to Mars, which John Armstrong did. John had the data, and we took, in a simultaneous experiment, data from both Galileo and MO and the Ulysses spacecraft; the PI there was Bruno Bertotti, from Italy, who is a good friend of ours.

COHEN: Now, you've published papers with all these people.

ESTABROOK: Oh, yes. We got data, and we did a lot of analyzing of the data. There's not as much published on the data, because, as truly anticipated, we did not find any gravitational waves. The Cassini data John Armstrong is still working with, so the verdict is not in yet, but at some point he and I gave up with the Galileo data.

We had a bad experience with Galileo. There are a lot of ironies connected with it. The original Galileo mission was proposed, and I, as I said, put in a proposal to use it during cruise, to search for gravity waves. I said in my proposal that it would be nice to put some extra equipment on board. Now, having said a moment ago that these were free experiments, then we got greedy, of course, and so we wanted to improve them. The standard tracking frequency that the DSN was using in those days was what's called S-band, which is about twelve-centimeter-wavelength microwaves. Essentially the next technological jump after S-band is X-band, which is about three-centimeter microwaves. And there are various reasons for this, mainly in the increased amount of data that they can send back to Earth by modulating the beam, and so on. Everybody at JPL thought eventually that would be the thing to do, and we wanted to hurry that process up, because by increasing the frequency by a factor of three-and-a-half, we would make the solar system's plasma much more transparent. So it would increase our sensitivity, to a lower level, which is a good thing. The noise from interplanetary plasma fluctuations would go down by a factor of ten in going to X-band. So the minute I got on Galileo, I began agitating to get X-band onboard. It was already onboard also as a downlink frequency, and we wanted to get it onboard as an uplink frequency. This would have meant quite a bit more transmitter capability at the DSN, and it would have meant an extra receiver onboard Galileo.

COHEN: Were you able to talk them into this equipment?

ESTABROOK: They said flatly, "No." They didn't want to do that. At this point, Kip—well, maybe I started it, but Kip certainly reinforced it, and between the two of us we started a letter-writing campaign. I wrote a letter and Kip more or less— At JPL, I was a little unclear as to whether I was supposed to really get out there as a government contractor, or employee—whether I should fight that way—and Kip had more clout by that time. So he sent this letter to various places in the community over his name, and they wrote their own letters back—and these were a lot of very influential people, starting with Stephen Hawking and a lot of others.

COHEN: [Laughter] All of Kip's entourage.

ESTABROOK: Yes, Kip pulled out all the stops. He wrote a letter, too. It's all in my archive, all these people who wrote, and they wrote these letters usually to the head of JPL or to the head of NASA. In the latter case, it was passed down to the head of JPL, where Pickering, or more often [planetary program director Robert J.] Parks, would handle it. And the irony was that Parks would usually hand it down to me to draft an answer for him.

COHEN: And this was your letter!

ESTABROOK: Yes. So I closed the loop.

COHEN: [Laughter] So, were you successful?

ESTABROOK: The first campaign? No. Then another opportunity came along, just a year or two later, for another space flight, called the International Solar Polar Mission. There were to be two spacecraft, one to go around the sun from north to south and the other from south to north. One was going to be a NASA spacecraft and the other a European spacecraft. And we persuaded Bruno Bertotti to apply to be the experimenter aboard the European spacecraft, and Hugo applied as the experimenter on the U.S. spacecraft. They both got onboard with a joint gravity-wave experiment. This was still in S-band, and then Kip fired up another letter campaign to put an X-band on those spacecraft. The Europeans—no way. But we put up a campaign to get it onboard the American spacecraft. The project people were unwilling to do it, but meanwhile we had an ally in the Deep Space Network, because although NASA headquarters was very suspicious of gravity-wave experiments, our DSN managers at JPL sort of liked it, because it put exciting new technological challenges in front of them. They *wanted* to do X-band. They wanted to improve their system and their sensitivity and their time-keeping. They were installing H-maser timekeeping standards at this time, which we very much needed. So they succeeded in getting another branch of headquarters to put up what they called technology-demonstration money. So, in order to demonstrate the technology, they got the X-band downlink funded for the American Solar-Polar mission.

COHEN: So it did go.

ESTABROOK: The mission was then cancelled. [Laughter]

COHEN: After all that!

ESTABROOK: After all that. Bruno Bertotti and the Europeans went ahead and did a solo flyby of the sun from north to south—that was the Ulysses mission. But it was S-band. Meanwhile, Galileo had been having ups and downs and been cancelled and then reinstated, and the astronaut disaster [*Challenger*, 1986] meant that they couldn't fly what's called a Centaur stage in the space shuttle, which had been scheduled to launch Galileo. So Galileo's launch had to be an unmanned launch, and they then had to stretch out the Galileo mission by at least three or four years, to go into the inner solar system and do another gravity assist before they could get to the outer solar system and Jupiter, where they wanted to go. So there was time, and after the Solar-Polar mission was cancelled, we succeeded in getting the X-band technology demonstration transferred at the last moment to Galileo after all.

COHEN: Well, that all sounds very exciting!

ESTABROOK: So Galileo went with X-band. And this had helped us a great deal financially, because Galileo had been stretched out for year after year after year, and all those years I got half-time funding from Galileo.

COHEN: OK, so it was of some use. [Laughter]

ESTABROOK: It helped me; and Hugo and I were working about half-time on all this and about half-time on the basic theory we were devoted to.

COHEN: So then, these were all preliminary experiments. Even though they were not successful, they were very important.

ESTABROOK: It's the way the game had to be played. NASA puts a lot of [emphasis on] what they call feasibility. They don't want to fly things for the first time. If it's been flown before or demonstrated on a technology demonstration, or if a fair amount of money has been spent on it and the reliability becomes more secure, it becomes easier to get it onboard.

COHEN: So then you continued with this work until you retired, I gather.

ESTABROOK: Yes. In the final years Massimo Tinto, Armstrong and I did a lot of studies on how LISA [Laser Interferometer Space Antenna] data will have to be analyzed. Our experience with the delayed Doppler responses of spacecraft, and the resultant sensitivity to gravitational waves, resulted in our invention of Time Delay Interferometry [TDI] for multiple spacecraft. TDI was then adopted by LISA as their baseline technique for isolating gravitational wave signals otherwise buried in the noisy Doppler data to be taken at its three spacecraft.

COHEN: And you've gotten all these papers together, and you say you have a few more boxes. Is there anything you'd like to add to this?

ESTABROOK: Well, yes. The denouement with Galileo was that its big antenna didn't open. When they were finally on the way to Jupiter, after all those many years, they opened up their main communication antenna—which we were going to use and to which our X-band transmitter was tied—and the big umbrella-like antenna hung up and would not open. The entire mission was run with small auxiliary antennas, which only had S-band. So it was all for naught. The first time an uplink-downlink high precision X-band gravitational wave experiment has been accomplished has been with Cassini.

COHEN: And you don't know yet—

ESTABROOK: John Armstrong has gotten data at three successive oppositions, when Cassini was least affected by plasma coming from the sun. He has X-band data from Cassini. [Tape ends]

Begin Tape 1, Side 2:

ESTABROOK: And Bruno Bertotti had persuaded the Italians to supply for Cassini K-band transponder, which is three times, four times, better than X-band. So the Cassini experiment, which has been run in the last ten years, has really been a K-band transponder experiment that resulted from international cooperation—and they've got data, too.

COHEN: Well, who knows, maybe they'll find gravity waves someday. Well, let me say thank you, and this has been very interesting.

[Tape is turned off]