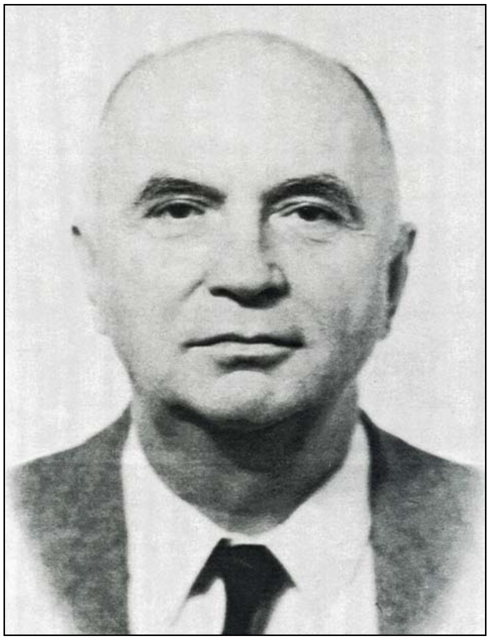

VLADIMIR B. BRAGINSKY
(b. 1931)

INTERVIEWED BY
SHIRLEY K. COHEN

January 15, 1997

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the LIGO Series Interviews

The interview of Vladimir B. Braginsky (1997) was originally done as part of a series of 15 oral histories conducted by the Caltech Archives between 1996 and 2000 on the beginnings of the Laser Interferometer Gravitational-Wave Observatory (LIGO). Many of those interviews have already been made available in print form with the designation "The LIGO Interviews: Series I." A second series of interviews was planned to begin after LIGO became operational (August 2002); however, current plans are to undertake Series II after the observatory's improved version, known as Advanced LIGO, begins operations, which is expected in 2014. Some of the LIGO Series I interviews (with the "Series I" designation dropped) have now been placed online within Caltech's digital repository, CODA. All Caltech interviews that cover LIGO, either exclusively or in part, will be indexed and keyworded for LIGO to enable online discovery.

The original LIGO partnership was formed between Caltech and MIT. It was from the start the largest and most costly scientific project ever undertaken by Caltech. Today it has expanded into an international endeavor with partners in Europe, Japan, India, and Australia. As of this writing, 760 scientists from 11 countries are participating in the LSC—the LIGO Scientific Collaboration.

Subject area

Physics, LIGO

Abstract

Interview, January 15, 1997, with Vladimir B. Braginsky, experimental physicist, Moscow State University.

Recalls family background and childhood in the USSR during World War II. Matriculates at Moscow State University 1955, PhD 1959, joins faculty 1969. Work with Y. B. Zel'dovich on search for quarks and detection of gravitational radiation; work with Vitaly Ginzburg on detecting time dependence of gravitational constant. Comments on Andrei Sakharov. Joins Communist Party in Khrushchev era. Science hierarchy in the USSR. Constraints on foreign travel. Meets John A. Wheeler in 1968 at international conference; gives a talk on quantum measurement; invited to visit Princeton, Harvard, University of Maryland, and Caltech, 1970. Discusses Joseph Weber's gravitational-wave experiment. Admiration for Kip S. Thorne. Early impressions of LIGO project on visits to Caltech in 1981 and 1984. His group at Moscow State University becomes LIGO collaborator. Comments on 1962 work of M. E. Gerzenstein and V. I. Pustovoit in gravitational-wave detection. Visit from Thorne in Moscow, 1977, with invitation to join LIGO. Comments on R. W. P. Drever and Rainer Weiss; on disagreements between Drever and Rochus (Robbie) Vogt, LIGO director 1987-1994. Fairchild Scholar at Caltech, 1990; LIGO's technical difficulties; project's disarray. Expresses optimism re LIGO directorship of Barry Barish and potential improvements in LIGO sensitivity. His laboratory's work on mirror suspension.

Administrative information

Access

The interview is unrestricted.

Copyright

Preferred citation

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH VLADIMIR B. BRAGINSKY

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Vladimir B. Braginsky **by Shirley K. Cohen**

Pasadena, California **January 15, 1997**

Begin Tape 1, Side 1

COHEN: Welcome, Dr. Braginsky. I'm glad you have agreed to have an interview with us. Welcome to the Caltech Archives.

BRAGINSKY: The pleasure is mine.

COHEN: Could you please start by telling us just a little bit about what your parents did and how you came into science the way you did.

BRAGINSKY: I belong to the generation of the Soviet people who may be considered the lucky ones, because the war [World War II] started when I was ten years old and was over for the Soviet when I was fourteen. I was not directly involved in the war, but the memories are still strong. And there is some kind of biological program in me about starvation, about hunger, because the wartime was very tough for me—as it was for all children.

COHEN: What city did you spend the war in?

BRAGINSKY: I spent half in Moscow and half in evacuation in Samara—that's along the Volga River—together with my mother, my aunt, and my cousin. Most of those who lived in Moscow and some other cities nearby were evacuated. I was born in '31. The war started in '41, when I was ten years old. Before the war, I had six uncles—after, only two. And my step-grandmother—the mother of my stepfather—was executed in the Babi Yar [massacre]. You know where Babi Yar was, near Kiev.

COHEN: Yes.

BRAGINSKY: Babi Yar was a small canyon where [many many thousands of] Jews—also non-Jews—were exterminated. What else? I was lucky because my parents were bureaucrats who worked for the government. But before the war I was lucky because my mother was working in the Soviet Embassy in Brussels. Before the war started, I had been with her in Brussels for four years.

COHEN: So you lived in Belgium?

BRAGINSKY: Yes, for almost four years. We left Belgium after the beginning of the Second World War. It was September, and the Soviet Union was not involved in the war yet. We crossed Germany and had to wait something like three weeks in Berlin until the Soviet and German armies completely occupied Poland.

COHEN: Now, your father was not with you?

BRAGINSKY: No. Well, my biological father and my mother had divorced when I was very young. My mother met my stepfather in Brussels among the employees of the Soviet technicians. When they got married, I was adopted by my stepfather, whose last name I am still using. In September 1941, my stepfather went as a volunteer for the war. He was forty-one years old. It was not mandatory for him to go to the front, but he volunteered and went to the front lines for about six months, then he was drafted back. He got the task of supervising the ammunition shipment from the U.S. to the Soviet Union via Iran. He survived the war, another instance of good luck for me. Then I finished high school in ’49.

COHEN: And that was in Moscow that you finished high school?

BRAGINSKY: Yes. I was a third-generation Muscovite. My grandfather, the father of my mother, was one of the younger sons of a poor peasant in Belorussia—the village name was Bolshie Prussy. According to the family tradition, the junior son remained on the land and inherited the land. The senior sons had to leave.

COHEN: Oh, is that right? That's turned around from the way it is here.

BRAGINSKY: Yes. The older brother of my grandfather went to the United States and lived over here more than twenty years, until he died here. My grandfather went to Moscow and became a worker—just an ordinary worker at the railway station. My mother got some kind of education—high school, some kind of pre-college-type courses. She learned French. She learned—How do you call it?—fast writing.

COHEN: Stenography?

BRAGINSKY: Stenography, yes. And she got a job at the Ministry of Foreign Affairs at a junior or low managerial level. She worked in Paris first, where she met my father. According to the Chinese system, I was born not in Moscow but in Paris—in the Chinese system, they add nine months to your age. But I was born in Moscow in '31. I finished high school, then went to Moscow State University, studying physics. I started to work in January of 1955 in the Department of Physics there. I am still working at Moscow State University. Since 1969 I have been a member of faculty.

COHEN: So, you've been in the same place all this time.

BRAGINSKY: Yes.

COHEN: But you must have done some advanced degree.

BRAGINSKY: Yes, I got some degrees.

COHEN: They're all from Moscow State University?

BRAGINSKY: Yes. I am devoted to this university. It's not the best one in the world, but in the Soviet Union—Russia—I guess it was one of the best. Probably the best one—the oldest, at least.

COHEN: And you worked with a group there?

BRAGINSKY: Sure. I was lucky, once more, at the university. I had good friends nearby with whom I was able to talk, to chat, to discuss science. I finished a course of study—we call it the chair—which was oriented mainly to engineering science in 1954. But after I got the master's degree [diploma] and finally the PhD in 1959, I decided to quit applied physics and go deeper into pure physics, toward the fundamental problems. And during my life I met many good mentors. The list is long—fifteen persons—but there are several who are outstanding. One of them was an outstanding scientist, Zel'dovich [Yacov Borisovich Zel'dovich]. I wrote eight papers on the search for quarks and on the detection of gravitational radiation with him, not anything which may be called revolutionary physics, but some good work. Another paper I wrote with Vitaly Ginzburg about the possibility of detecting the time dependence of a gravitation constant. I have met Sakharov [Andrey Sakharov] many times. He visited my lab many times before he got married to this lady.

COHEN: Elena Bonner?

BRAGINSKY: Oh…[Pauses]

COHEN: [A slight chuckle] A disaster?

BRAGINSKY: Oh, for a husband, yes, such a wife is a disaster. I am sorry. That's my private point of view.

COHEN: But you're not alone. I've heard this before.

BRAGINSKY: To some extent, he was a victim of her ambition. In many ways she was an excellent wife. But she was a director. And if you have a director behind you, pushing you to break a wall by butting your naked head against it, you may suffer, and that's exactly what happened. He was, by the way, a deep fellow in science before he got involved…

COHEN: In politics?

BRAGINSKY: Yes. By the way, he was not so influential inside the Soviet Union; he was almost ignored in the Soviet Union by the majority of the population. Only a very tiny circle of people knew what he was doing in politics, what he was trying to do, what he claimed to do, although it was more or less known in the West. But the political changes were not due to Sakharov; they were despite him. Without [Soviet president Mikhail] Gorbachev, nothing would have happened—nothing.

When Perestroika started, the system was rooted. I have to confess I was a member of the Communist Party. I had joined the Party during the cold war, during the Khrushchev era. I was full of illusions that it was possible to create socialism with a human face, and very soon I realized that it was going to be a long wait, that nothing serious would happen, and that the only pleasure I got from the Party were three reprimands for wrongdoing, or incorrect behavior.

COHEN: Did you have to go to meetings?

BRAGINSKY: Sure. Absolutely!

COHEN: And all this time you're in your physics group in Moscow University, working, doing your science?

BRAGINSKY: I have to explain how the science was organized in the Soviet Union, at Moscow State University. It was the same hierarchical system that still exists in the Russian Academy of Science, to which I have to confess I do belong. There's a Department of Physics in the Moscow State University, which consists of thirty-two chairs [divisons].

COHEN: Thirty-two chairs?

BRAGINSKY: Yes. A typical one has thirty to fifty employees, including three to five full professors, assistant professors, researchers, managers, and technicians. The head of this chair is one of the professors—the chairman or professor with chair. The output of the chair is ten to twenty students with master's degree or PhD per year. I have to confess, I am a professor who has a chair.

COHEN: Well, it's OK.

BRAGINSKY: The scientific groups in the chair sometimes appear and sometimes disappear.

COHEN: But the professor stays?

BRAGINSKY: Yes, absolutely. In the Soviet era, the scientific groups depended on financial support, which came mainly from militaristic sources and was not small. There was a commission called the Commission of Military Industrial Problems—a supremely high-ranking commission in the Soviet era, fantastically powerful and fantastically wealthy. Any kind of academic research, which might or might not result in any kind of military profit, was funded. Plus, our standard of living was, let me say, very preferable.

COHEN: Very nice.

BRAGINSKY: Yes. And, in fact, we were able to do anything we wanted, any kind of research, especially if we made the claim that we might produce something which would detect the enemy better than other devices or aim the missiles, or something else, better. In that case we were guaranteed to get more money. I have to confess that I was using the military money, as were ninety-nine percent of my colleagues in the Soviet Union. But, to justify my behavior, I was always trying to avoid any kind of classified work, and I succeeded in this. I never overtly worked on explosives. I tried to formulate tasks in such a way that they appeared more academic. And doing non-classified work meant that I would be able to have contact with foreigners and go abroad. I was, in fact, more or less lucky with the permission to go abroad. I never got permission during the Soviet era to go to France or England, but I was turned down to go to the U.S. only twice.

COHEN: Do you understand why?

BRAGINSKY: No. It was an illogical system. Five times to Italy and never to France. I visited France only after the Perestroika, after the collapse of the Soviet Union. Probably the people at the KGB were not completely certain about my moral stability.

COHEN: Maybe it was because you spoke French.

BRAGINSKY: *Probablement*.

COHEN: So, when did you become interested in gravitational waves? When did this work begin?

BRAGINSKY: This may not be interesting from the point of view of the history of science, but for myself it was a turning point. I got my master's degree in 1954 and PhD degree in 1959. The PhD degree was partly in applied science and partly in theoretical science—it was about the transitional radiation which was invented by Ilya Frank and Vitaly Ginzburg in 1945.

COHEN: Ilya Frank…?

BRAGINSKY: Ilya Frank—he is a Nobelist. And Ginzburg is not yet awarded. But he will be! I will do my best!

Transitional radiation appears when a charge which flies with a constant velocity is passing from one medium to another one. It was before the start of the great laser era, and at that time there was some hope that this type of radiation would be of some use in electronics. It turns out it was not of great use. But I got a PhD. After this, I decided to try to find a completely new field and at that time, I have to confess, I didn't have a good mentor, because the Department of Physics was oriented to applied physics rather than to fundamental problems.

COHEN: I see. It was more engineering?

BRAGINSKY: More engineering, yes. Meanwhile, I had started to read some books and look around, and I found many mistakes in the textbooks. And in one of the good textbooks—an excellent textbook called *Statistical Physics* by a good physicist—I found a crude mistake. The mistake was located at the initial point of a research program. The question was very simple: What is the smallest force of mechanical momentum—which is applied to a mechanical oscillator—that it is possible to measure? There was a mistake in the book. The good physicist who wrote the book made a mistake and used the full variance instead of using a fraction of it, where the signal is. It turns out that the correct classical answer was zero for the smallest force possible. But a quantum effect may appear. His classical answer was not the correct one. I published an article on quantum limits, and I gave a talk about this in 1968 at an international conference, where I met Johnny Wheeler [John Archibald Wheeler]—just an absolutely marvelous person. I owe him a lot, because six months after we met at that conference I got an invitation to visit five universities: Princeton, Harvard, Caltech, and some others.

COHEN: I think maybe you're being too modest. It must have been not such a little thing that you did.

BRAGINSKY: No, it was not little, because I claimed that quantum mechanics appears under specific conditions, not only with atoms but also with a chair or a table. It occurs with any kind of device, if you have first a high-precision measuring device, and second, if you just manage to isolate this table or this chair from the environment. This concept is essentially a very simple one and was not immediately accepted, but Johnny Wheeler appropriated it.

COHEN: He liked it.

BRAGINSKY: He liked it. And this is why I was invited. But, I have to confess, when I wrote the formula I did not completely understand what it meant myself. Now it's called the decoherence in the thermostat. This term, "decoherence," appeared many years later. And the so-called "standard quantum limit" associated with decoherence was coined by Kip Thorne in the late seventies. In 1964, Zel'dovich asked me to repeat the [R. A.] Millikan experiment—the measurement of the electrical charge of an electron on a test mass—with a small difference. Zel'dovich wanted me to do the same experiment but, instead of using small drops with a weight of 10^{-10} grams, with a test mass a million or ten million times heavier, which was not very easy to do at the time. Murray Gell-Mann, a professor from Caltech, had published his idea about the systematics of baryons, and he got a Nobel Prize for this work. The question was: Do quarks exist as a separate stable particle, or do they exist only together within the proton, neutron, and so on? There was the possibility that they might be stable alone, and, if so, we might be able to disprove the famous Robert Millikan result and find a fractional charge. We didn't.

COHEN: OK. So Millikan is safe.

BRAGINSKY: Yes, yes. Moreover, the gluon approach to this problem appeared due to the experiment which we were using, to some extent. There is no one-third or two-thirds of an electronic charge. It took me five years to get the numbers to my satisfaction. Now we know, because we got a zero result, that the charge of the electron is equal to the charge of the proton at a level of 10^{-21}. Not so bad. I was curious, at the same time, to explore the idea of a weak or very small force—it is possible to measure a force acting on the mechanical system. We tested the principle of equivalence and found it correct. In a parallel to this research—trying to formulate the limits of sensitivity in quantum measurements—I spent two years checking the Joe Weber result: the discovery of the gravitational waves, which had attracted my attention as a challenge. But we got negative results.

COHEN: Had you ever met Joe Weber?

BRAGINSKY: Yes, in 1970. [The University of] Maryland was on the list of places Johnny Wheeler arranged for me to visit.

COHEN: Oh, so you did come to the United States.

BRAGINSKY: Yes, in 1970. It was my first visit here. It made quite an impression.

COHEN: Now, when did you learn your English? I mean, when were you using your English?

BRAGINSKY: If a student is not too lazy, he may learn good English in a university. If he is lazy, he'll be able to read the technical text and translate.

COHEN: So you learned English on your own in Moscow?

BRAGINSKY: Yes. Certainly my English isn't bad now. It was much worse at that time, but I was able to communicate. And the vocabulary of the physicist is small—500 words, I guess. As for my life, it's a typical biography of a very lucky Russian scientist. Nobody from my family suffered from the purges. It's true that I lost roughly one-half of my close relatives in the war. But it was typical—inevitable. The war was cruel—fantastically cruel. Twenty-seven million were killed. It was a holocaust for the Jews, yes, but it was also a holocaust for the Russians, on the same scale.

When I was at a point when I was starting to wonder about what was interesting and what was challenging about physics, I was spending many hours in laboratories and speaking with people and attending many seminars to formulate some kind of problem for myself which would occupy me for a certain time—not forever, perhaps, but something not trivial which would be ignored two years after publication. And I found that there were several problems in physics which deserved attention, time, and money, and one of the problems was raised by Joe Weber, a good experimentalist and a person for whom I feel affection. The problem was the detection of gravitational waves resulting from astrophysical catastrophes. Part of the problem was the very high level of illiteracy among the theoreticians, to which I was an honest witness. This started with the first not-correct publication by Joe Weber in 1969. The title of the article was "Evidence for Discovery of Gravitational Radiation." Starting from this moment, and then for something like six or seven years, there were a lot of publications which drew incorrect conclusions.

COHEN: By other physicists?

BRAGINSKY: And theoreticians, astrophysicists, whose work was just wrong, or very biased. And when I met Kip Thorne for the first time at an international conference in Georgia in 1968, I was very impressed by his presentation. His talk was about the modes of the oscillations of neutron stars. It was mathematically rigorous, absolutely superb. And I started to talk with him about this business. He was very cautious and very careful in his formulation of his attitude to the possible sources of gravitational radiation. It turned out to be really one of the most accurate formulations, and he was one of the deepest physicist I ever met. In fact, he never predicted anything wrong. I know many theoreticians who have made wrong predictions, pretty famous ones, including [Paul] Dirac and many others. Not to mention Einstein, who didn't accept the quantum theory. Kip was very cautious. And I liked this, despite the fact that it took two years—from ’69 to ’71—to prepare and finish the first test of the Weber bar [detector], and we got a zero result.

COHEN: This was with the bar?

BRAGINSKY: With the bar, yes. At that time, I decided to go ahead, to improve the sensitivity, according to some kind of theoretical understanding about what is possible and what is not possible, determining whether the limitations were inherent in the original work or were just the result of stupidity by our predecessor.

COHEN: Was this your first visit here, in ’81?

BRAGINSKY: No, no. I first visited here in ’70, then in ’74, ’78, ’81, and ’84. In brief, when I visited Caltech in ’81, I saw the beginning of the LIGO prototype. A few years later, in ’84, I saw the progress and decided for myself that there was no hope for me to compete with LIGO.

COHEN: Is that right?

BRAGINSKY: Absolutely. But not only for me, for many more people.

COHEN: So, the way to go is the interferometer, not the bar?

BRAGINSKY: Yeah. The reason is very simple. The gravitational wave is a very simple effect—it’s a tidal wave. To measure a tidal wave, you have to have something extended in two test masses, as far as possible—at least at the distance of the wavelength. The sensitivity goes up as one over the length and with the masses as one over the square root—the temperature is also under the square root. Thus, with four kilometers, the bar cannot compete, because you have here ten kilo mass and even about ten tons, but ten tons and ten kilo are under the square root, and three meters, four meters—it’s not four kilometers, it’s 1,000. It’s an enormous value. There’s another reserve which is crucial. It’s much easier to isolate from the hostile environment—from the heat-bath thermostat, to isolate a gently suspended mirror—than to isolate a mode of mechanical oscillation inside of the bar. Thus, it was a critical moment in my lab.

To return to the prehistory of LIGO, there was a small group in my lab in Moscow which was devoted to doing the bar experiment with better sensitivity than Joe Weber and others. There is always some kind of competitive mood. You have to be first; you have to get better numbers—at least, better sensitivity. Eventually we had to quit, we had to abandon this stuff. We said, "Let's try to find something better." Now, finally, after all these events—Perestroika, and so on—I'm associated with LIGO, as a collaborator, and my colleagues in Moscow are also working for LIGO.

COHEN: So now you are a collaborator here?

BRAGINSKY: Yes, our group is working for LIGO.

COHEN: So your group in Moscow is working as collaborators here?

BRAGINSKY: Yes, and we got money from Caltech and a small amount from the Russian government—not a lot, but something.

COHEN: Enough?

BRAGINSKY: Not enough. Never enough! None of the experimenters will tell you it's "enough." Sufficient to survive, under present conditions. From Caltech we are receiving much more than the money than we are receiving from the government. [Moscow State University is federal—ed.]

Now more about the history of LIGO. Theodore H. Maiman invented the laser in 1960.[1] Immediately after this, my two good friends Misha [M. E.] Gerzenstein and Slava [V. I.] Pustovoit—two good fellows—published an article saying the simplest way to detect a gravitational wave was to suspend two mirrors and to use a laser which would permit us to detect small vibrations. That's all. That was in 1962, just two years after the Maiman publication.

COHEN: You mean the idea followed immediately upon the discovery of lasers?

BRAGINSKY: Sure. These guys, Gerzenstein and Pustovoit, understood that lasers were a powerful source of coherent light which would produce an enormous revolution in optical interferometry.

COHEN: So, was this paper known?

BRAGINSKY: Yes, it was known. But it was known after some people here, honest people, had made the same conclusion. I don't know who was the first here. The article about LIGO in *Science* in 1992, I guess, is a good article, an honest article.[2] It contains a good historical introduction.

COHEN: At about this time, had you ever visited in Glasgow to see what was going on?

BRAGINSKY: Never.

COHEN: So you had not met Ron [Ronald W. P.] Drever before you came here?

BRAGINSKY: He visited me when I was in Moscow in '74 or '75—something like this. And we met at conferences. I'm not sure about the year we became friends—'76, '77, '78—something like that. Kip Thorne visited me in Moscow in 1976 or '77 and said, "Do you want to join LIGO?" That was before LIGO was initially established. At that time, the term "LIGO" was not used, but Kip tried to organize a group of people to do some serious work. At that time there was Ronny Drever in Glasgow and Rainer Weiss, a good fellow and excellent physicist, at MIT. They were doing, on the tabletop level, a preliminary experiment—the first approximation—let me use this word. They did a test of different versions which might or might not disprove or prove the feasibility of this concept. And they accumulated experience during many years—at least five more years of work. It is difficult enough to adjust two mirrors being suspended gently in such a way to be able to detect small vibrations. At this time, a crucial role for all of this business, and the organization of LIGO, belongs to Kip Thorne. He has a good intuition and a good understanding of the situation. He had a deep and broad education. He understood that it was time to start to consider this type of endeavor seriously—to make something which will lead to the positive final result, to overcome serious doubts in the very beginning. And Thorne came to Moscow and asked me to join this work, and I said, "If I go, I will be considered a defector. I will be separated from my family automatically. There will be some kind of punishment for me to serve as a good example to others not to follow my way. I may be separated from my family for ten or fifteen years."

COHEN: This was before the Perestroika.

BRAGINSKY: Yeah. It was ten years before Perestroika. When I said no, Kip showed me the list of the candidates, which included Ron Drever.

COHEN: So you said Ron Drever was certainly the best person to come.

BRAGINSKY: Yes. Ron Drever is an excellent experimentalist. And, at the same time, he's a chamber musician, he's not a director of a big symphonic orchestra. On the level of the small scale, the small research on the tabletop, he's able to do it. He has done a lot of preliminary works. Rai Weiss has done very similar work and was to some extent a much deeper fellow—a broader physicist. Ron and Rai have created the initial basis for the beginning of the prototype. And in the first few years, from '81, I was a witness. In '81, there was nothing, just zero. Only the building with a rectangular corridor.

COHEN: Right.

BRAGINSKY: But within three years, they took off. They reached the level of sensitivity of the bar people, who had spent—after the disapproval of the Joe Weber experiment—more than ten years working.

COHEN: So they were on the right track.

BRAGINSKY: Yes. They were moving very fast. They had started from zero. But after three years, their prototype was substantially larger than any kind of device you may have had on your tabletop. It was forty meters—not three meters, not two meters, but forty.

COHEN: Big.

BRAGINSKY: In a vacuum, with special suspension, some good results were produced. And for three years, this type of collaboration was a good one, a profitable one, and an efficient one. The following three years we were not very successful.

COHEN: Now you're talking about from '84 to '87?

BRAGINSKY: Something like that. No substantial success. But in '87 I got an invitation once more and applied for a visa and was told no. It was not the first time.

COHEN: You don't look for a reason.

BRAGINSKY: No, never, never. If I asked "Why?" some bureaucrat would say, "Let's not discuss this business. You have another invitation?" "Yes." "What kind of country?" "Finland." "You have to go there."

COHEN: To Finland?

BRAGINSKY: Yes. "But it's not interesting," I would say. "But you have to go." So I went to Finland. It was a COSPAR [Committee on Space Research] conference. I went and met a lot of friends.

COHEN: A vacation?

BRAGINSKY: A scientific vacation. I delivered a talk, and so on and so forth. Thus it was not a pure vacation. The state never will send you on a scientific vacation. You have to produce something. You have to get information. You have to work hard. After I went to Finland, there was some kind of misunderstanding which, to the best of my knowledge, finally made it—I am trying to find a mild word—completely impossible for Robbie [Rochus E.] Vogt and Ronny Drever to work together.

COHEN: Robbie came to be the director of the project?

BRAGINSKY: That was—I don't know, exactly—in '86 or '87. [Vogt became LIGO director in 1987—ed.] Then in '90 I received the Fairchild award from Caltech.

COHEN: And you came here.

BRAGINSKY: Yeah, I came here, very happily, with my wife. She was very impressed—let me say—by California. The beauty, et cetera.

COHEN: It's nice here.

BRAGINSKY: Yeah, sure it's nice.

COHEN: Were you here the whole year?

BRAGINSKY: No, for three months only. I had teaching obligations in Moscow, students, you know. That stay coincided with the beginning of—let me be straightforward—a scandal, which was not very pleasant. The roots of the scandal lay in the idea of team spirit, and—this is absolutely essential in American culture—the principle of individualism. The single combat warrior is an American concept, if you will. Initially, LIGO was created as a team of coworkers. There was no division in responsibilities or different types of physical and technical problems which had to be solved in the first stage, or the second, and so on. It was evident from the start that LIGO was a very unusual project. It was much more sophisticated than any kind of high-energy physics project, like an accelerator, a big bubble chamber, or similar detectors, because in those cases you know at what level of energy the effect is observable; the only hope for results is to increase the luminosity of the beam, for example; the number of times you get to observe something increases as the beam gets brighter. In the case of LIGO—in fact, LIGO is a very simple machine—you have two test masses, and you have to solve only two problems: to isolate these test masses from any kind of influences from a hostile, noisy, dirty environment, which includes Brownian motion, which is inevitable. The mirror has to be isolated from micro-kicks, random kicks from the environment, so that the tests can be run in a vacuum. Second, the fiber on which the mirror is suspended has to be very thin. A third factor is the loss of mechanical energy.

Begin Tape 1, Side 2

BRAGINSKY: […] I guess, in my case, it's a true confession. I hope.

COHEN: Why don't you proceed with the groups, not the science so much, but rather the interaction of the groups that you found when you came in '90.

BRAGINSKY: First, I have to explain what was done—I don't want to say "wrong," but not in an optimal way. In this case, I have to describe some science to you. It's very simple.

COHEN: OK, go ahead.

BRAGINSKY: There are only two tasks to solve the problem. To isolate the mirror from any kind of kicks from outside, and make a sensitive device which will permit you to measure a small vibration between two mirrors—one mirror relative to another. That's all. It was evident that in this case the research group had to be divided into two parts. One would make the meter—the read-out system. The other group would make the mirror and suspend it gently. The initial mistake made by Ron Drever, or by Rai Weiss, was the decision: "Let's do this stuff all together."

COHEN: Oh. Instead of separating tasks.

BRAGINSKY: Yes. But, in fact, there are many subtasks that appear from time to time, and disappear from time to time. And there were some good PhD dissertations done by some good students along the way. But it would be much better, and it would be much more profitable psychologically, if initially people would be divided into subgroups. Probably—I don't know, I cannot prove this—but one of the reasons for the scandal was the fact that Ronny wanted to dominate, to control all the events, and always, somehow, managed to interfere. When it turned out that three years after the first successful rise of the sensitivity, there were three or four more years without great success, the mess was inevitable. Ronny Drever—let me repeat this once more—is a chamber musician, he's not a conductor. The team at that time was eleven or twelve scientists. Initially there were three PIs [Principal Investigators], three wise people, including Ron. The decision by the president to replace the PIs with one PI [one director] was a wise decision. The scandal resulted, which is unfortunate. I feel sympathy for Robbie, and I have affection for Ronny. They are good guys. They both are good scientists, but unfortunately Caltech offered them only one task, not two. It would have been possible to divide the tasks, but, unfortunately, that didn't happen. And why Robbie was demoted [as director, 1994], I don't know. Now things are going on reasonably fast, and I am happy.

COHEN: Do you find things are proceeding again? I mean, is there progress now?

BRAGINSKY: Yes, of course, but there are some kinds of events which are not so pleasant. But they might have been predicted. Two scientists quit: Alexander Abramovici and Andy [Andreas] Kuhnert, a German. They quit because, to a great extent, all the members of the team initially were condemned to be considered martyrs.

COHEN: Now, these were people in your group or they were here at Caltech?

BRAGINSKY: No, no, no. They were the LIGO group. The Caltech group.

COHEN: So they quit?

BRAGINSKY: They quit, yes. They are both working at JPL [Jet Propulsion Laboratory] now. Initially it was possible to predict that the task would occupy, let's say, ten or probably fifteen years. It started in '81 with a rich soil from which LIGO started to grow. It was started then from zero and now it's '97—it's been sixteen years. And initially it was done in such a way that there was a group of researchers who were, according to the initial spirit, working as a united team. Now, any person is, more or less, an individual. He wants to be proud of what he has done in the past. And if he was looking back and seeing many coauthors with him on an array of publications, it's not so pleasant. And the team was working in such a style as to say: "OK, we have a problem. Let's gather together and let's discuss it. It's one way, it's another way. Let's try this, let's try that. OK, we have more or less solved it. Now we have another problem. Let's go together and try to solve this one."

COHEN: No individuality of purpose?

BRAGINSKY: Yes, yes, yes. It's not so good—and it wasn't so good. Some fraction of this style still remains. [LIGO director] Barry [Barish], as I understand it, has reorganized the style. He brought with him a big team of managers and engineers to make this stuff less artistic and more solid, more reliable in the future. I very much hope that he will succeed.

COHEN: Do you think that was a good thing?

BRAGINSKY: What happened with Barry Barish?

COHEN: Yes.

BRAGINSKY: You know, it's easy to be a good judge in retrospect. But the historical perspective has to be large. Probably it's too premature for me to say. It was an event—Robbie was demoted and Barry was promoted. I cannot say definitely whether it was good or bad. I have no point of view. They both are personalities. They both have iron fists when it is necessary. Sometimes it is necessary. They both have a concern for how to do things. As a PI, they both are absolutely responsible and honest researchers. And some order has to be established, certainly. Some division of tasks has to be made. As far as I can understand, what is going on now is that there is some kind of distribution of the tasks, which permits the group to produce publications and oral presentations at symposia, and so on.

COHEN: So that's better?

BRAGINSKY: It's much better. But still, two guys have quit. But certainly, if somebody is a PI, he wants to reach a final goal. Without question, the PI has to be merciless sometimes. Any communication with group members can be merciful, but the PI has to be mainly merciless. And if he sees the shortest way to solve a problem, this step, another step, and so on, he sometimes has to forget—I don't want to say "to ignore"—the feelings of others.

COHEN: So tell me, what do you think will be the future? Do you see LIGO actually succeeding in some amount of years that you can estimate?

BRAGINSKY: Yeah. What I do know definitely at present, from the point of view of the fundamental laws of physics, is that there is no limit to the sensitivity of this machine. No limit.

COHEN: So you think it's really fantastically good?

BRAGINSKY: I mean, in principle.

COHEN: In principle, yes.

BRAGINSKY: As soon as you don't see any limits, the only problem is to find a way to push the envelope—it's an American expression.

COHEN: Yes.

BRAGINSKY: …to push the limits deeper and deeper, closer to zero, to higher and higher sensitivity. And this is why the lifetime of the facilities is thirty years, or something like this. And maybe not one but many generations of experimentalists will explore this machine. If they are able to substitute this mirror with another mirror, this laser with another—I don't want to use this word, another source of radiation—this suspension with another, and so on. I may give you an example of just one simple parameter of the quality of the mirror. Jeff Kimble, a good fellow and good friend of mine, has just demonstrated what is going on in experimental physics. In the very beginning of the laser era, the quality of the laser depended on the mirrors. In the very beginning, the reflectivity was at a level of ninety percent. In 1972, the reflectivity reached a level of ninety-nine percent.

COHEN: Reflectivity?

BRAGINSKY: Yes, reflectivity. Reflectivity means the fraction of energy which is deflected to the fraction which is absorbed. Ten percent, initially, was absorbed. In 1972—ten years after the invention of the laser—it was around one percent or one-tenth of a percent. Now it's a millionth part, and nobody knows what the limit is.

COHEN: So it just goes on and on.

BRAGINSKY: Oh yeah, on and on. This is only one example. With LIGO it is just like this.

COHEN: All the components, they really don't know where or how it goes.

BRAGINSKY: Yes. It's because there is no principal physical limitation. We know the obstacles, but we don't know how to bypass them. We don't know the loopholes, we have to invent them. It means new tricks and new technology.

COHEN: So you are completely optimistic about how well this is going to work.

BRAGINSKY: Oh, certainly not absolutely optimistic, because *people* are doing this. And if some not-very-well-educated bureaucrats with very high position say that a gravitational wave is not essential for the preservation of the race and that something else is much more essential, the loss of funding may destroy the testing. What I do think is that the trustees of Caltech are wise enough. But even the worst may happen. Say they decide it's over, that they will not give more money for improvement if the first run does not give a positive result, which may happen. It's not likely, but it may happen. The first stage, from my point of view, has to give something positive. I do think it will happen, if not at the first stage, two or three years later. Now, the next question: How frequently the bursts may happen? If we improve the sensitivity in the final version, the machine—I mean the LIGO—has to work, has to act as a machine gun—*boom, boom, boom, boom*—many bursts with many features. It's a completely new channel of information, which is priceless, priceless.

COHEN: Yes. So, you feel it will go on and there will be success, if not this year then next year?

BRAGINSKY: Yes. Again, the trustees may say, "No more money." Or the trustees may find that the cost is not so much to maintain, that to go ahead, to find new mirrors, new suspension, new pumping will not cost too much. It will cost $10 million per year—for Caltech, it's…

COHEN: It's nothing. So, you feel this has been a really good experience for you—to come here and to do this?

BRAGINSKY: I'm happy because I'm making some minor, but not zero, contribution to the advancement of LIGO. We are working on suspension of the mirror, and the mirror suspended in my lab has a relaxation time of three years.

COHEN: The work goes on in Moscow for the improvements here?

BRAGINSKY: Yes, and the know-how is transferred over here. Let me finish the sentence. The relaxation time of a pendulum—a regular pendulum—is now three years at room temperature. And now, at present, these guys—I mean Kip [Thorne] and Stan [Stanley E.] Whitcomb—they want it to be 100 years. And there is no principal obstacle to this, but we don't know the technology. We don't know what kind of thing we have to invent, but it is doable. There is other stuff, more sophisticated to explain in simple terms, but this is one of the examples.

COHEN: You're mentioning Stan Whitcomb for the first time.

BRAGINSKY: Oh, a good fellow and an excellent experimentalist—a fantastically delicate person, gentle, wise, and very well-educated and adept at public relations. A first-class experimentalist, no doubt. He is one of the gifted employees who is under the burden of all this stuff. The machine is a bit sophisticated, but I'm explaining to you just some key elements in the design. The key elements are, in fact, much more sophisticated. For instance, there are many feedback loops which keep these mirrors parallel one to another. It is not a simple system of two mirrors. There are many mirrors, in fact. It's not so simple.

COHEN: Well, I guess it's going to happen.

BRAGINSKY: You mean the final positive results?

COHEN: I hope so.

BRAGINSKY: It is the beginning of a long chain of discoveries. After the first burst of discoveries, the next step will hopefully follow. What is the mean frequency of the gravitational radiation? What is the shape? The shape has to depend on the internal structure of the stars. We don't know a lot about this. This is uncharted territory.

COHEN: Yes, well, it's exciting.

BRAGINSKY: It's promising.

COHEN: It's promising. OK, well, thank you.

BRAGINSKY: You are very welcome. Are you satisfied?

COHEN: Yes, I think so. **[Tape Ends]**

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[1] T. H. Maiman, "Stimulated optical radiation in ruby," *Nature* 187, 493–4 (1960).

[2] Alex Abramovici, et al., "LIGO: The Laser Interferometer Gravitational-Wave Observatory," *Science* 256:5055, 325-33 (1992).