Rainer Weiss Biography

I was born in Berlin, Germany on September 29, 1932. My father was a neurologist who had rebelled against his family of well off intellectual German jews with connections to the Rathenau family. He had become an idealistic communist. My mother was a Christian who had rebelled against her family by becoming an actress. Berlin in 1932 was balkanized with no strong central government. Different parts of the city were Nazi, Communist and Weimar Republic controlled. After giving testimony about a botched operation by a Nazi doctor, my father was assaulted and held captive in one of the Nazi districts. My mother with help of her still politically connected family managed to get him released. He emigrated to Prague, Czechoslovakia and after I was born my mother joined him there. In 1937 they had another child, Sybille. In September of 1938 the family took a vacation to the Tatra mountains in Slovakia. In a hotel filled with expatriate German jews we all gathered around a gothic looking wooden radio and heard Neville Chamberlain give Czechoslovakia to Hitler to avoid a second world war. The hotel emptied in hours with people rushing to the consulates in Prague with the hope of leaving before the Nazis took over. Most did not get visas to leave. We were lucky, the fact that my father was a doctor coupled to the charity of the Stix family of St Louis, Missouri, who had given bond for a large number of professional jews, we did get a visa to come to the United States and arrived in New York in January 1939.

After several years in New York City public schools I received a scholarship to the Columbia Grammar and Preparatory School beginning with middle school and graduating from high school in 1950. I became interested in electricity and electronics. It was a great time for someone with those interests. By the end of 1946 Cortland St in lower Manhattan had become flooded with government war surplus electrical and electronic components. Motors, transformers, vacuum tubes, capacitors…. could be bought for pennies in quantities limited by your ability to carry them home on the subway. I started making money by fixing radios. By 1947 several critical developments had made high fidelity audio systems possible: phonograph records with large dynamic and spectral range, low distortion audio amplifiers , FM broadcasts of live concerts. By chance a movie theater in Brooklyn had a fire where the loudspeakers behind the screen were destroyed. They were available for the taking. I assembled a system with an FM tuner and power amplifier driving a repaired speaker and invited some of the émigré family friends to listen to the New York Philharmonic. They were blown away, they thought they were at Carnegie Hall. I got orders to build similar systems for them and soon I had more orders than I could handle. By the time I was a senior in high school, I was stuck on a problem too hard for me to figure out at the time. The problem was the surface noise on the records, especially, in piano pieces, When the music was fast and loud such as the first and last movements, the noise was tolerable but in a slow quiet movement it was all one heard. The concept I had was to make the bandwidth of the amplifier vary as a function of the amplitude of the music- make the bandwidth smaller in the quiet parts to reduce the record scratch which was primarily at high frequencies and increase the bandwidth in the louder sections. I simply did not know enough mathematics and real electrical engineering to make headway. So I applied to various technical colleges and managed to get into MIT.

At MIT decided on Electrical Engineering as a major. The initial curriculum was about mechanical structures, motors and generators along with a little circuit theory but nothing about filters and electronics. The things I wanted were all in advanced courses and in those one went in sequence. By the end of the sophomore year I decided to major in physics as the course structure was less rigid. That summer I met an absolutely stunning girl who introduced me to folk dancing and piano music I had never heard before. She was a music major at Northwestern University. The long distance love affair lasted for part of the fall and winter but seemed to be falling apart in the late winter and spring. I decided to go to Evanston Illinois to salvage it despite the fact that I was enrolled as a student at MIT and the gentle but firm messages that end meant end. I returned to MIT several months later but had flunked out.

The summer of 1953 I looked for a job doing electronics in the Research Laboratory of Electronics (RLE) in building 20 at MIT. By chance I walked into the atomic beam laboratory of Jerrold Zacharias. They said they could use some one to do carpentry and I took the job. After building a dry ice chest, I began to work on the electronics of an atomic clock, a new idea using the hyperfine transition in atomic cesium as observed in the pristine conditions of an atomic beam. Zacharias and I worked together on an improved atomic clock that was intended to be precise enough to measure the Einstein gravitational redshift between a clock placed in a valley and another on the top of a neighboring mountain, an altitude difference of approximtely 3km. The basis of the clock was to gain a long observing time in a vertical atom fountain. We worked on the experiment for about 2 years but never measured any atoms following the parabolic trajectory. After Zacharias become involved in revamping secondary school science education, I continued with the experiment and found that there are no slow atoms in a beam. The more numerous fast ones scatter the slow ones out of the beam. The Zacharias fountain concept when applied to atoms slowed by laser Doppler damping is now the basis of the most precise atomic clocks. I continued in Zacharias's lab as a graduate student developing a universal atom detector based on ionization in a beam . The development of the detector allowed many different atomic and molecular species to be used as reference systems for a more precise atomic clock. But this effort got overtaken by the Mossbauer technique and later by the laser atomic clocks. I finally did my PhD thesis on the hyperfine structure and electric dipole moment of the rotation states of the uninteresting Hydrogen Flouride molecule .

The gravitation and General Relativity bug had bitten. As a Postdoc I worked with Robert Dicke at Princeton. Dicke almost single handedly had revitalized experimental relativity after the Chapel Hill meeting in 1957. He brought General Relativity back from mathematics into physics. Learned important techniques from him for precision experiments, in particular, negative feedback control of mechanical systems to damp and the use of the control signals to read out the mechanical variables; as well as the importance of suppressed carrier techniques to place the interesting variable above the 1/f noise of the electronics. The specific experiment I worked on was a search for scalar gravitational waves predicted by the Brans –Dicke Scalar Tensor theory. The experiment was designed to observe the excitation of the high Q earth normal mode at 20.4Hz which has spherical symmetry. The instrument was a fused silica gravimeter operated in vacuum maintained at stable temperature on a vibration isolation platform to reduce seismic excitation of higher frequency normal modes in the instrument. Shortly after the instrument was commissioned the great Alaska earthquake of 1964 caused the mode to ring and persisted for ½ year at amplitudes hard to model. I left to take a faculty position at MIT in the fall of 1964.

I started a new research group in RLE at MIT dedicated to research in cosmology and gravitation. The work was supported by the Joint Services Grant to RLE. The initial effort was to search for changes in G with epoch by using ultra stable gravimeters based on electric field supported plates to measure g at several locations on the earth. The field to support the plates was measured by using the molecular transitions of a beam of molecules passing between the plates referred to atomic clocks. It became clear that one also needed to make measurements of the Earth's dimensions so a program to absolutely stabilize a laser against an atomic or molecular reference was also begun. In the course of the laser stabilization program a laser interferometer was constructed that operated at the quantum limit of the phase noise.

In 1967 I was asked to teach a graduate course in General Relativity. I did not know the mathematics used in General Relativity even though I had listened to a course at Princeton. I could also not admit to the Department that I knew very little of the formal theory. So I struggled being at best a day ahead of the students. The students became interested in the Weber experiments that had begun to detect pulses in the aluminum bars that could be attributed to gravitational waves. I tried to understand the Weber experiments but got stuck in understanding the interaction of the gravitational wave with the bar. I felt the notion of a tidal interaction putting stresses in the bar was an incomplete way of describing the interaction. Rather, I developed a completely geometric means of explaining the interaction based on the ideas of F.A.E. Pirani. I asked the students to consider the gedanken experiment of two free masses travelling along neighboring geodesics. Each mass equipped with a synchronized clock to time the light travel from one mass to the other. Then to repeat the experiment when a gravitational wave came between the masses. The travel time would change and be a straight forward way to determine if a gravitational wave had interacted with the masses. The students did this as a homework problem with mathematics not more difficult than that used in Special Relativity with a time varying metric.

Weber made the announcement of the measurement of gravitational waves in 1969 when he described the results of coincident excitation of the bars in Chicago a lab at College Park Maryland and in an experiment shack 8 miles from his lab. The announcement began a global response to confirm his measurements by others. By 1972 it was becoming clear that no other group could confirm his results. I went back to the gedanken experiment in the General Relativity course to see if it could be converted into a real experiment. There were no clocks with sufficient precision to detect gravitational wave strains of 10^{-21} but if one used Michelson interferometers with multiple passes in km length arms, suspended free masses for the mirrors, high power coherent light sources to illuminate and modulation tricks to reduce the 1/f noise; it looked possible to do a measurement of the gravitational wave strain at the level of 10^{-21} at frequencies above 10 Hz in a terrestrial instrument. An analysis of the projected noise in such an instrument was made and put into a Progress Report of the Research Laboratory of Electronics in 1972. I asked the management of RLE for funds to build a 1.5 meter prototype of such an instrument – about $50K of joint services funds became available.

Initially I worked with undergraduates to get the prototype started as there was no hope of making a gravitational wave detection with such a small instrument. The major part of the work would be technical development rather than gravitational wave science. At that time a PhD thesis in physics had to be a publishable scientific result. Graduate students started on the prototype and then would get their PhD on other projects in the laboratory.

Shortly after teaching the Relativity course, in part because an excellent student and I had become interested in cosmology and the cosmic microwave background (CMB) radiation had just been discovered, the group began a program to measure the spectrum of the radiation to determine its thermal nature. This could only be done by measuring above the atmosphere from high altitude balloons or from a space platform. The measurements were initially supported by the Joint Services Program and later by NASA. The CMB measurements, first of the spectrum and later of the angular distribution of the radiation, were the mainstay for graduate students in the group. The measurements established: the thermal nature of the radiation, discovered the significant perturbations of the angular distribution of the CMB by dust in the galaxy and led directly to the design, planning and execution of the COBE mission.

The corrosive effect of the Vietnam war led to the end of military support for research in cosmology and gravitation in RLE requiring me to write proposals to the NSF to continue the prototype. The idea took a while to be accepted, reviewers were skeptical ranging from too hard to do to impossible since the properties of the light were said to be also affected by the gravitational wave in such a manner as to cancel the measurement. Reviewers at the Max Planck Institute for Astrophysics in Garching, who had been working with Weber bars, saw optical interferometers as an interesting way to detect gravitational waves and called me to ask if I would mind if they begin research on them and if I had any students who might be interested to come to Germany to work with them. The group did extremely well demonstrating a 3 meter prototype and later a 30 meter system showing the anticipated scaling. At about the same time a group in Scotland led by Ronald Drever, who also had been working on Weber type bars, began work on interferometric detectors. Eventually, we did get funded and the MIT prototype was completed as well.

In 1975 I was asked to chair a committee for NASA to look at the role the space program could play in cosmology and gravitation. Peter Bender, Charles Misner, Robert Pound were members of the committee. I asked Kip Thorne to present his ideas to the committee. He came to Washington in the middle of the summer without a hotel reservation. Before his report to the committee, we spent the night together talking about ideas a new experimental group in gravitation at Caltech might work on. I described interferometric detection of gravitational waves to him. Kip had not realized their potential. After consulting with others Kip suggested this to the Caltech administration and engaged Ron Drever and Stan Whitcomb to start a group at Caltech with a significant internal investment. The NASA committee among other projects also suggested interferometric detection of gravitational waves in space leading to the LISA project .

Based on the success of the prototypes, the credibility the Caltech decision gave the field, and my difficulties to have graduate student PhD theses accepted, I proposed to the NSF to carry out a feasibility study with industry to design and construct a long baseline interferometric detector system with sufficient sensitivity to intersect estimates for the gravitational waves incident on the earth from plausible astronomical sources. The study took 3 years. It established scaling relations for costs looked at the readiness of the technologies in lasers, optics, electronics, vacuum systems, isolation systems and came to the conclusion that building a two site detector on multi-km long scales within the United States was feasible and timely. However, at a significantly increased investment in the field by the NSF.

During the course of the study Kip and I came to the idea that Caltech and MIT should do the LIGO project together. In 1983 Kip, Ron and I jointly presented the result of the study to a NSF committee reviewing large physics projects. We received a remarkable endorsement stating that gravitational wave detection was a high risk but also high payoff field. The technology development and eventual scientific results were worth a significant investment by the NSF. After this meeting I tried to interest the MIT administration in engaging a project manager to begin to organize the effort but there was not much interest. Kip convinced Caltech to bring a project manager from JPL, the result was that the management of the LIGO project became centered at Caltech. For about 3 years Kip, Ron and I tried to jointly direct the project while the NSF management tried to convince the National Science Board to actually begin LIGO.

In 1986 Richard Garwin suggested to the Physics division of the NSF that if they really wanted to begin a large new project to detect gravitational waves, a study of the field by unimpeachable and independent scientists was needed to assess the technology and the importance of the science. A committee was assembled to study: the state of the prototype development, the industrial capabilities in optics and lasers, the engineering of the vacuum and vibration isolation systems. The science of the gravitational wave sources and data analysis techniques were also presented in a week of fact finding. The committee recommended the LIGO project go forward without compromise and with the potential capability to make a detection. It also recommended that Caltech and MIT seek a single director.

In 1987 Rochus Vogt became the first Director of the LIGO project. He reorganized the research programs of the Caltech and MIT groups to avoid redundancy and insisted the groups make difficult decisions in the technology. He also led the combined groups to write a strong proposal to design and construct LIGO. The proposal contained most of the then current knowledge about interferometric gravitational wave detectors and the gravitational wave science as well as the engineering for the facilities and a more authoritative cost estimate. The proposal envisioned two sites and an initial detector with then current technology followed by an advanced detector with improved sensitivity. He also instituted a structure of coupling engineers and scientists in the design of the vacuum system, beamtubes and facilities to assure that the difficult and unique instrument requirements were met by the facilities. The LIGO project received approval in 1990. The Hanford and Livingston sites were chosen in 1992.

In 1994 Barry Barish became the second director of LIGO. He oversaw the construction of the facilities, the installation and commissioning of the initial interferometers and the first searches for gravitational waves. In 1997 he instituted the LIGO Scientific Collaboration which now includes about 1000 scientists from approximately 75 institutions and 15 nations. I was the first spokesperson for the collaboration followed by elected spokespersons Peter Saulson, David Reitze and Gabriela Gonzalez.

My other roles after the formation of the project have been to be the scientist associated with the engineer responsible for the design. construction and functioning of the beam tubes. Later, once the detector was installed, to guide commissioning of the detector at Livingston and then to help understand the detector performance, especially, to diagnose unanticipated noise in the system.

I continue to play similar roles in Advanced LIGO but now there are many more skilled people more knowledgeable than I in the project. The group at MIT has trained 49 PhD and MS physicists and over 55 undergraduates in the technology and science associated with interferometric gravitational wave detection some of whom are now part of the LIGO project.

Family: Rebecca and I got married in 1959. At the time she was a plant physiologist. In 1962 Sarah our daughter was born and in 1967 our son Benjamin. Rebecca became a children's librarian retiring only a few years ago. Sarah has become an ethnomusicologist with a specialty in Indonesian Gamelan music. She is currently a faculty member and rector at Yale-NUS in Singapore. Sam, her son, is about to be an undergraduate at Yale. Benjamin is an art historian. He is the Leonard A. Lauder Curator of Visual Culture and Head of the Collection at the Museum of Fine Arts in Boston.

Hobbies: Physics is both my profession and hobby. I love classical music and play the piano for my own pleasure. When younger I hiked and swam.