KIP S. THORNE

Autobiography for the Nobel Prize

March 2018

My Youth

I was born in 1940 in Logan, Utah, USA, a college town of 16,000, nestled in a verdant valley in the Rocky Mountains.

My father, David Wynne Thorne, was a professor of soil chemistry at the Utah Agricultural College (since renamed Utah State University). Over his lifetime he had a major impact, through research and consulting, on arid-land agriculture, not only in the USA but also in the Middle East, Pakistan, and India. He was an intellectual inspiration to me.

My mother, Alison Comish Thorne, with a PhD in economics, aspired to be an academic, too. However, her career was thwarted by Utah's nepotism law that forbad the wife of a University employee from also working for the University; so she devoted most of her life to community organizing and community activism, and to raising and mentoring five children. Her lifetime impact on the community led the University to award her an honorary doctorate in 2000, when she was 86; and in 2004 when she died, a giant headline in the local newspaper, the Herald Journal, read "Old Radical Dies".

Our parents encouraged my siblings and me to pursue our own interests, treasure our individuality, think for ourselves, and not automatically accept the dictates of the culture in which we lived. This and much more about my youth are described in my Mother's autobiography, *Leave the Dishes in the Sink: Adventures of an Activist in Conservative Utah*.

As a small boy, watching plows create snow banks as high as 3 meters in front of our home, I aspired to become a snow plow driver. Then, when I was eight, my mother took me to a lecture about the solar system at the local Mormon church (Logan's fifth ward), a lecture by a professor from the University. I was enthralled, so my Mother suggested we make a model of the solar system on the sidewalk alongside our home. We drew the sun as a circle four and a half feet in diameter (about a meter), and then she showed me how, mathematically, to take the solar system's actual dimensions and scale them down to this 4.5-foot sun. With our calculations completed we drew each planet as a circle at the appropriate distance from our sun. It was amazing to me: the Earth was a half-inch diameter circle a bit beyond the fourth home north of ours; and Pluto was a tiny circle about 3 miles away, in North Logan. I was hooked. I began to devour everything I could find about astronomy in local libraries and bookstores.

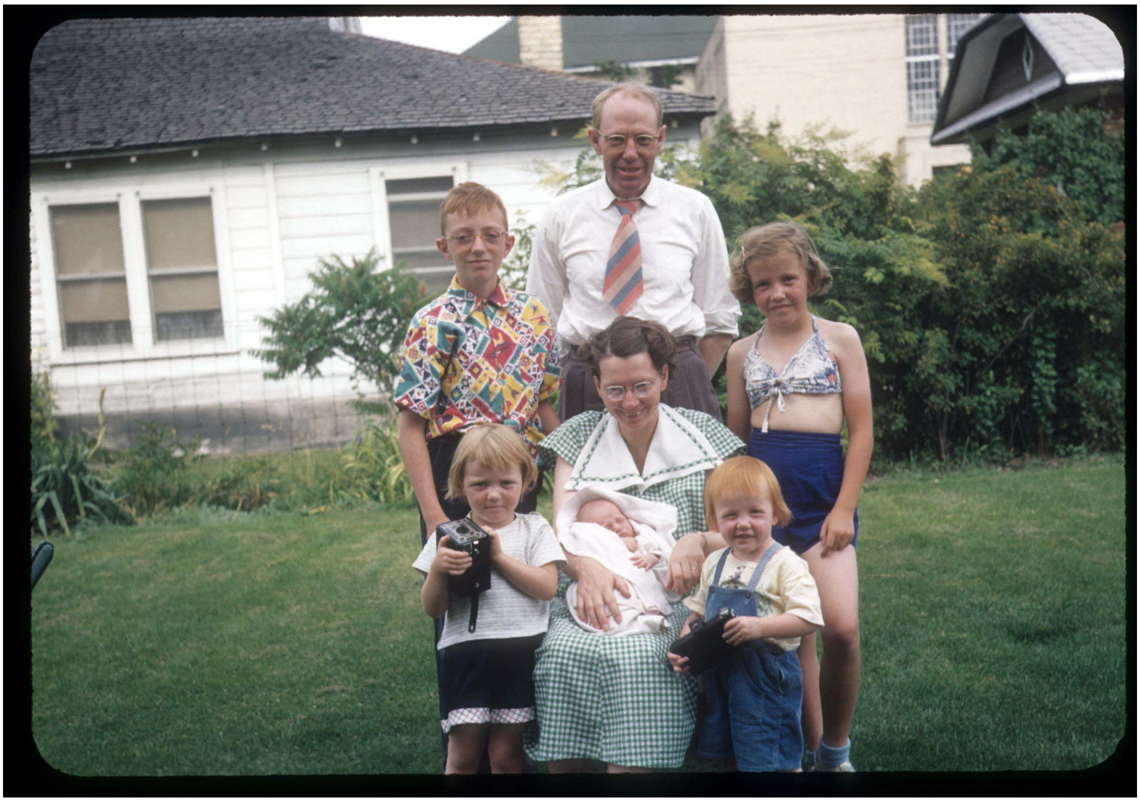

Figure 1. Back row: Me (age 11), my father Wynne, and my sister Barrie; front row: my mother Alison and siblings Sandra, Lance (the baby) and Avril; in Logan, Utah, summer 1951.

Five years later I discovered, in a bookstore in Salt Lake City, a paperback edition of *One, Two, Three, …, Infinity* by the physicist George Gamow. It dazzled me. It revealed the role of astronomy as a subfield of physics, the role of mathematics as the language of physics, the beauty of Einstein's relativity, and the power of physical laws to explain the universe. I read it three times and decided I wanted to become a physicist, pursuing a quest to understand the universe. Two decades later, when I had started publishing my own research, George Gamow sent me a letter inquiring about ideas in one of my publications. Thrilled, I wrote back, telling him I was a physicist because of having read his book three times. In response, he sent me a copy of *One, Two, Three, … Infinity* in Turkish, with an inscription "To Kip so that he would not be able to re-read it a 4th time". That book remains one of my most treasured possessions.

My mother encouraged each of her other children to pursue their own chosen dreams. My sister Barrie, two years younger than I, became a professor of sociology. My sister Sandra, eight years younger, became one of the first female forest rangers for the US Forest Service. My sister Avril, nine years younger, became a professor of psychology. And my brother, Lance, eleven years younger, became an artist in wood.

Our ancestors, on all genealogical lines, joined the Mormon church and migrated to Utah on foot, on horseback, or in covered wagons before the railroad arrived (1869). Throughout my youth, our parents, Alison and Wynne, taught an adult Sunday school class, focusing on comparisons between Mormon thought and culture, and other religions and the ideas of great philosophers.

For me as a youth, Logan and its Mormon culture and history provided an idyllic environment, and I still treasure my Mormon roots. However, in my teen age years, as I learned more and more about science and discovered its power for explaining Nature and the Universe in testable and tested ways, and for producing technology that can improve dramatically the lives of people, and as I contrasted this with the more magical and less verifiable character of religion, I gradually lost interest in religion and even in whether God exists. (Much later, when my mother was 75 years old, at her urging, she and all her children resigned our membership in the Mormon Church, because of the church's discrimination against women. My sister Barrie had already been excommunicated for her feminist activities.)

As a teen ager in the 1950s, I had an active social life. I played saxophone and clarinet in a dance band, participated in exhibition dancing, edited the high school year book, and was on the high school debate squad, partnering with the future All-American and All-Pro football player Merlin Olsen. But my deepest passion continued to be physics. While others were building telescopes, I – having been captivated by Mr. Thomas's high school course on axiomatic Euclidean geometry in two and three dimensions – formulated it in four dimensions. I recall my excitement upon discovering, through a sequence of lemmas and a theorem, that in four dimensions the intersection of two planes is generically a point, not a plane.

In the summer of my eighth birthday I was at loose ends, so my mother sent me to typing school at Logan High School. "This may be useful to you someday," she said; and indeed it became very useful decades later, in the era of computers. In my fifteenth summer my parents enrolled me in a geology course and an analytic geometry course at the University, opening my eyes to phenomena I had not dreamed existed. Thereafter, throughout high school, I continued taking an occasional university course.

My University Student Years

Despite my university experience as a teenager, when I arrived at Caltech as a freshman in September 1958, I found myself overwhelmed. I had had no calculus, I was a slow reader, and it quickly became evident that my thinking was slower than that of most other Caltech freshmen. I stumbled and struggled for a year and a half, but gradually developed my own ways of mastering the physics and mathematics that were coming at me like water from the proverbial fire hose. Most valuable of all was a series of notebooks that I developed for myself – one for each major class that I took. In each I wrote down the most important ideas and results I was learning, in my own words and equations, and formulated my own mathematical proof and/or physical explanation for each major result. I continued this through graduate school, then abandoned it for about 15 years, and then started up again in the late 1970s, when I was trying to master new topics and tools relevant to astrophysics and to gravitational-wave experiments. I still find myself consulting those old notebooks from time to time.

By the middle of my sophomore year at Caltech, I got my feet under myself and started enjoying my studies thoroughly, and moving through difficult material at a reasonable pace.

In the summer before my first, second, and third years of college (1958, 1959, and 1960), I worked as an engineer's assistant in the Great Salt Lake Desert, designing solid propellant rocket engines for the Thiokol Chemical Corporation's Minute Man Intercontinental Ballistic Missile – engines that would later power the space shuttle. This gave me my first taste of “big science”, it showed me how various components of an R&D program should come together on a predefined time schedule, and it showed me how Nature can confound a research program: hot, turbulent gas swirling near the entrance to the rocket nozzles kept eroding the slowest-burning solid propellant (the “inhibitor”) so rapidly that the turbulence ate into the rocket casing, blowing the nozzles off the engine. The explosions, in test after test of our evolving design, were spectacular and frustrating.

In the summer before my fourth college year (1961), I got a job doing theoretical astrophysics research under the inspiring mentorship of the astronomer Jesse Greenstein. The result was my first published paper, on “The Theory of Synchrotron Radiation from Stars with Dipole Magnetic Fields”.

Ever since reading *One, Two, Three, … Infinity*, I had been fascinated by relativity. During my fourth year at Caltech I decided that was the direction I wanted to go for my PhD, so I spent many hours in the Caltech physics library trying to read relativity articles in research journals such as *Reviews of Modern Physics*. It soon became evident that by far the most interesting research on general relativity was being done by John Archibald Wheeler at Princeton University and his students, so I applied there for graduate school – despite Jesse Greenstein’s warnings that the only significant application of relativity was the expansion of the universe. In Jesse’s view, and that of many other eminent astronomers and physicists of the era, relativity was a dead end.

At Princeton, John Wheeler was an even more inspiring mentor than I expected, and his young associate Charles Misner added to the inspiration. From Wheeler and Misner I learned about black holes, neutron stars, singularities, and geometrodynamics (the ill-understood nonlinear dynamics of curved spacetime). In parallel, I sat in on the weekly research group meetings of Robert Dicke, whose focus was experiments to test general relativity; and there I met and admired postdoc Rainer (“Rai”) Weiss.

In that era, when relativity theory was far ahead of experiment and was only weakly tested, I somehow understood that the interface of the theory with experiment could become a fruitful and exciting area of research, so I not only immersed myself in Dicke’s experimental-gravity milieux; I also spent much of my first year at Princeton getting hands-on experience with experiment. In the bowels of the Princeton physics building there was a cyclotron (particle accelerator) on which, under the mentoring of assistant professor Edwin Kashy, I explored the internal structure of the nuclei of Rhodium atoms. This was rather far from relativity, but that experience (like my earlier experience with big science at Thiokol) would turn out to be extremely useful later, when I embarked on gravitational wave research.

In the summer of 1963, I spent eight weeks in a relativity summer school at the *École d’Été de Physique Theorique* in the French Alps. There Wheeler and Dicke gave inspiring lectures, and I met gravitational waves in depth for the first time, in lectures by Rainer Sachs (University of Texas) on the elegant, mathematical theory of the waves, and by Joseph Weber (University of Maryland) on his pioneering experimental effort to discover gravitational waves from the distant universe. I hiked with Weber in the surrounding Alps, we talked at length about his experimental program, I became a convert to the importance and possibilities of gravitational wave experiments, and I became rather fond of Weber himself.

I completed my PhD in June 1965 and spent one more postdoctoral year at Princeton, honing my theory research skills. In 1966 Willy Fowler (who would win the 1983 Nobel Prize for explaining the origin of the elements in stars) invited me back to Caltech as a postdoc, and I jumped at the opportunity. In May, while driving from Princeton to Caltech to start my new job, I stopped in Chicago for discussions with Subrahmanyan Chandrasekhar (who would share the 1983 Nobel Prize with Fowler). Over the following decade both Fowler and Chandrasekhar made major contributions to my chosen areas of research and influenced me substantially (Fowler on relativistic stars; Chandrasekhar on black holes and gravitational waves), and both became dear friends of mine.

Early Years as a Caltech Professor

When I arrived back at Caltech in 1966, there was a paucity of theoretical physics faculty working outside elementary particle theory. Particle theory was in the doldrums and I was bubbling over with research problems involving black holes, neutron stars, and gravitational waves, so a number of outstanding physics graduate students gravitated toward me, looking for interesting research problems. By late winter, although just a postdoc, I had built a research group of five graduate students and was having a wonderful time working with them. Then in the spring, to my great surprise, the University of Chicago – under Chandrasekhar's influence – offered me a tenured associate professorship. To my great joy, Caltech matched the offer, and almost overnight I was a tenured member of the Caltech faculty.

One of the great things about Caltech is the support that the administration and one's colleagues provide to young faculty members, to help them reach their potential. Maintaining a research group of five or six graduate students and several postdocs, as I was doing almost from the outset, is not cheap. Initially most of the expenses were covered by Fowler's research grants from the National Science Foundation (NSF) and from the Office of Naval Research. In 1968, when Fowler became a member of the National Science Board, which oversees NSF, he arranged for me to take over from him as the Principal Investigator on his large NSF grant. Under my leadership, that grant was renewed time and time again over the next forty years and remained my largest source of research funding until my formal retirement in 2009, whereupon my successor, Yanbei Chen, became the grant's Principal Investigator, and remains so today, after several renewals.

My group's initial research topics – black holes, neutron stars and gravitational waves – were all subtopics in a brand-new field called *relativistic astrophysics.* This new field grew out of the discoveries of quasars (1963; Maarten Schmidt at Caltech), pulsars (1967; Tony Hewish and Jocelyn Bell at the University of Cambridge), cosmic X-ray sources (Ricardo Giacconi and colleagues at American Science and Engineering), and the cosmic microwave background radiation (CMB 1964; Arno Penzias and Robert Wilson at Bell Labs, and then Robert Dicke and his group at Princeton). Thanks to these observational discoveries, relativity was suddenly relevant to a whole lot more in the universe than just its expansion. The merger of these discoveries with the theoretical ideas of Wheeler (Princeton), Yakov Borisovich

Zel'dovich (Moscow), Dennis Sciama (Cambridge), Fowler, Chandrasekhar, and others, gave rise to relativistic astrophysics.

Very early in the development of this new field (summer 1965; before moving to Caltech), I attended the Fifth International Conference on General Relativity and Gravitation, in London. There I met and initiated close friendships with a few physicists who would profoundly influence my life and career. Most important, perhaps, were Stephen Hawking (a student of Sciama) and Igor Novikov (a young colleague of Zel'dovich).

Hawking had contracted Amyotrophic Lateral Sclerosis only two years earlier. In London, walking with a cane and talking with modestly mutilated enunciation, he lectured about his recent insights into the big bang. I was mesmerized by his science and also his personality. We talked in the conference corridors and found ourselves kindred spirits. Although, in the subsequent half century, Hawking's research on black holes and the big bang has greatly impacted my gravitational wave work, we have never collaborated on research, and when together we have spent more time discussing life and death and love, than physics; so I shall describe the details of our friendship elsewhere, not here.

In London, Igor Novikov lectured about new insights in relativistic astrophysics that he and Zel'dovich had been developing. I had studied the Russian language as a Caltech undergraduate, and in London I found that my Russian was about as good (or bad!) as Novikov's English, so we stumbled along in a semi-coherent mixture of the two languages, exchanging astrophysics ideas and initiating a friendship that would soon grow strong and deep.

In 1968, with my new Caltech research group beginning to make an impact, I was well prepared to take advantage of the next international conference on general relativity, this time in Tbilisi (Soviet Georgia). There I met Zel'dovich in person for the first time, and Zel'dovich introduced me to Vladimir Braginsky, who was building a research program in gravitational-wave experiment at Moscow University in parallel with Joseph Weber's in America. This was the beginning of my career-long research collaboration with the groups of Braginsky (on gravitational waves and experimental tests of relativity) and of Zel'dovich and Novikov (on black holes and neutron stars, and later on wormholes and time travel). To facilitate our collaborations, Braginsky, Novikov and I began traveling back and forth between Moscow and Pasadena with typically one trip per year in one direction or the other – despite the raging cold war. For a few details, see my book *Black Holes and Time Warps: Einstein's Outrageous Legacy.*

During my first dozen years at Caltech, 1966–1978, gravitational waves were only a modest portion of my group's research portfolio. Our larger foci were black holes, and other astrophysical phenomena where gravity is so strong that it must be described by Einstein's relativity laws rather than Newton's laws – primarily neutron stars and dense, relativistic clusters of stars. My students and postdocs (sometimes with a little help from me) used general relativity to analyze the structures and astrophysical roles of these objects, and also how they would behave when disturbed – their pulsations and their emission of gravitational waves. This fed into the main thrust of our gravitational wave research: our evolving vision for the information that can be extracted from gravitational waves, when they are ultimately detected; and more broadly, our vision for the future of gravitational wave astronomy; see my Nobel Lecture.

In the next to last section of this biography, I describe the style in which we carried out this research. That style included extensive interactions with colleagues from other institutions, including Zel'dovich, Novikov, Braginsky, and also Leonid Grishchuk in Russia; Hawking and Brandon Carter in the UK; Wheeler, Chandrasekhar, Fowler, James Bardeen and James Hartle in the US; and many more.

Caltech's Early Research in Gravitational-Wave Experiment

In his Part I of our joint Nobel Lecture, Rai Weiss describes the early history of experimental research on gravitational waves, including (very briefly) at Caltech. Here I shall add some details about the genesis and early years of the Caltech experimental effort.

My early ideas about gravitational-wave experiment were influenced profoundly by Vladimir Braginsky. After Weber's 1969 announcement that he might be seeing gravitational waves, Braginsky (1969–1972) was the first other experimenter to build and operate gravitational wave detectors using the "bar" technology that Weber had initiated), and was the first to fail to find the waves that Weber appeared to be detecting (1972), and among the first to move on toward second generation detectors (1974). In 1972, after Rai Weiss wrote his seminal paper proposing the gravitational-wave detectors – "gravitational interferometers" – that would ultimately be used in LIGO (see my Nobel Lecture), I turned to Braginsky for insights and advice about future gravitational wave experiments.

It was my many discussions with Braginsky in 1972–1976, as well as those with Weiss, that convinced me gravitational-wave detection was truly feasible and led me in 1976 to propose to Caltech that we create a research group working on gravitational-wave experiment. My first choice to lead our Caltech group was Braginsky. After many months of struggling with the idea of moving from Moscow to Caltech, he told me *No*. Even if he managed to get himself and his family through the iron curtain to California, the consequences for his professional colleagues and friends left back in Moscow could be dire, he thought.

When I asked Braginsky whom we should go after to lead the Caltech effort, at the top of his list was the same person as Weiss suggested to me: Ronald Drever of the University of Glasgow. Why? Because of Drever's high creativity and his experimental insights. (For example, Drever had already proposed operating the arms of gravitational interferometers as Fabry-Perot cavities, which has turned out to be a major improvement on Weiss's original design.) So, I suggested Drever to the Caltech physics and astronomy faculty, and after many months of learning about him and other candidates, they chose him to initiate our new experimental effort. The Caltech administration made him an offer which after many many more months, in 1979, he ultimately accepted. The next year we recruited Stan Whitcomb from the University of Chicago to assist Drever in leading our experimental effort. (Today Whitcomb is the LIGO Laboratory's Chief Scientist.)

As a precursor to Drever's acceptance, the Caltech administration pledged roughly two million dollars of Caltech's own private funds for the construction of laboratories and equipment for the new experimental group, including, most importantly, funds toward a prototype gravitational interferometer with 40-meter arms.

This was the first substantial investment in gravitational interferometer research by any institution in the US: Neither MIT (Weiss's home institution) nor the National Science Foundation had yet been willing to commit significant funds for such research. With Caltech on board, Weiss, Drever, and I, working with NSF's Richard Isaacson, were able to trigger significant NSF funding from 1979 onward.

[I take great pride in Caltech's early and enthusiastic commitment to this field and unwavering support from the 1970s through today. Caltech's atmosphere of collegiality, intellectual ferment, and easy communication across fields of science, and our administration's enthusiastic efforts to help us find the funding needed for realizing our dreams, have anchored me to Caltech throughout my career, as they also anchored Richard Feynman and many others of my colleagues.]

For me, the late 1970s and early 1980s were a particularly exciting period:

Drever, commuting back and forth between Caltech and Glasgow, made several inventions that would significantly improve gravitational interferometers:

- *power recycling* (recycling unused light back into the interferometer – which was also invented independently by Roland Schilling in Garching, Germany).
- *resonant recycling* (tuning the response of the interferometer to waves of different frequencies by recycling some of the signal back into the interferometer before extracting it. (A few years later, Brian Meers improved on Drever's version of this and it got renamed *signal recycling*.)
- the *PDH technique for stabilizing the frequency of lasers* (adapted by Drever from an earlier microwave idea by Robert Pound, and then first demonstrated by John Hall and Drever in Hall's lab in Colorado). This is now widely used in other areas of science and technology

While Drever was inventing and commuting, Whitcomb and the students and postdocs that he and Drever hired were focused on building and perfecting the 40-meter prototype interferometer on the Caltech campus, and with it exploring technical issues that had to be surmounted in any ultimately successful gravitational interferometer.

In parallel, Carlton Caves and my other theory students and I – with very helpful input from Drever and Whitcomb – embarked on *Quantum Nondemolition* research: an effort to devise ways to circumvent the Heisenberg uncertainty principle in gravitational interferometers and other gravitational wave detectors. This effort was triggered by insights from Braginsky, much of it was in collaboration with Braginsky and his group, and it continues to this day; see my Nobel Lecture for details.

LIGO

In 1984 – building on successes with the interferometer prototypes at MIT, Caltech, Glasgow and Garching, and building on a feasibility study for kilometer-sized interferometers that Weiss and his MIT group and Whitcomb had carried out – Drever, Weiss and I founded LIGO as a Caltech/MIT collaboration. MIT was unwilling to make any substantial institutional commitment to LIGO until a few years later, so Caltech became our collaboration's lead institution. Weiss and Barish sketch the subsequent history of LIGO in their parts of our joint Nobel Lecture.

From 1984 to 1987, I served as the "glue" that held our Caltech/MIT collaboration together, mediating between Weiss (who understood clearly that collaboration was essential for success) and Drever (who needed to be in complete control of all he did in order to remain creative and productive, and so had difficulty truly collaborating). It was with great relief that I relinquished my mediation role in 1987, when the three of us turned over the leadership of LIGO to our first director, Robbie Vogt, who quickly molded us into a truly functional, joint Caltech/MIT team.

In the meantime, Braginsky – despite having endorsed Weiss's gravitational-interferometer ideas in the 1970s – focused the energy of his research group unwaveringly on a variant of Weber's "bar" gravitational-wave technology. Braginsky was concerned that, to succeed, gravitational interferometers would have to become extremely complex (which they indeed are today, with 100,000 data channels that monitor their subsystems and the environment); and he worried that this complexity might ultimately doom the interferometers to failure.

Throughout the late 1970s and the 1980s, Braginsky and I both commuted back and forth between Moscow and California, maintaining a tight collaboration (particularly on quantum nondemolition techniques and technology; see my Nobel Lecture). And throughout this period, Braginsky advised Drever, Weiss, and their colleagues about interferometer R&D and planning. In the late 1980s, when Braginsky saw the progress that was being made with the prototype interferometers and saw the Caltech/MIT plans for a proposal to the NSF to construct LIGO, he became convinced that the probability of success was reasonably high; so he went home to Moscow, shut down his bar-detector research, and initiated in its place a whole new research program in support of LIGO. This had a profound effect on me, bolstering my confidence at just the moment my Caltech/MIT colleagues and I were developing our proposal and plans for LIGO construction.

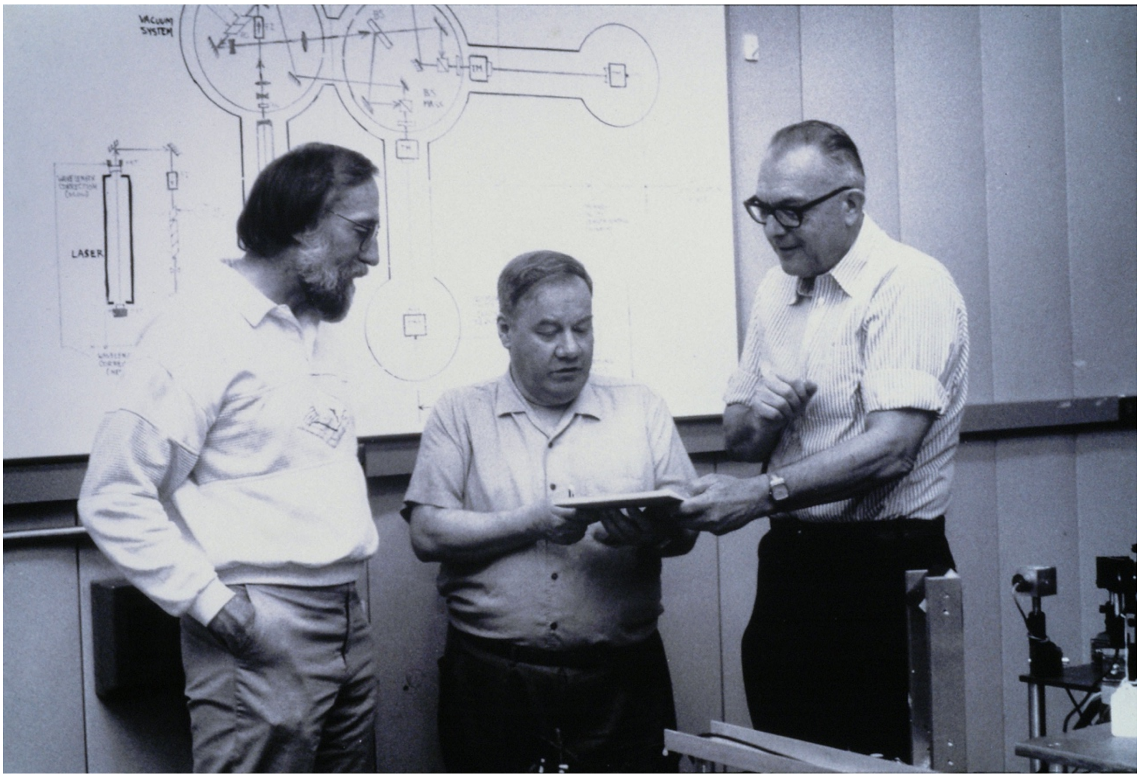

Figure 2. Me, Ron Drever and Robbie Vogt in 1978 [Credit: Caltech.]

In the early 1990s, under Vogt's leadership, we secured approval from NSF for LIGO's construction and we took major steps toward construction. Then in 1994–2001, our second director, Barry Barish, transformed LIGO from a small Caltech/MIT project into a large international collaboration, and led us through the construction of LIGO's facilities, the installation of LIGO's first interferometers, and the writing of a proposal for the advanced interferometers that have now succeeded in discovering gravitational waves; see Barish's part II of our joint Nobel Lecture.

In 1992, with LIGO starting to move forward, I wound down other efforts (including the theory of time travel) that my theory research group was doing and I refocused our research almost completely onto theoretical support for LIGO. This included analyzing sources of noise in LIGO's interferometers and ways of controlling the noise (see my Nobel Lecture), a beefed-up effort on quantum nondemolition, and a renewed effort to understand sources of gravitational waves, and the shapes of their waves – their *waveforms*.

It was only then that I began to realize how difficult would be the analysis of LIGO's data – finding weak gravitational-wave signals amidst LIGO's noise, and extracting the information carried by the signals. Fortunately, my former student Bernard Schutz, at the University of Cardiff, UK, had recognized this as early as 1986 and had begun then to lay foundations for the data analysis (see my Nobel Lecture). To bring Caltech up to speed on the data analysis, we imported Bruce Allen from the University of Wisconsin; and he, together with a number of my students and postdocs, dove into the problem while I cheered them on. Soon thereafter, Barish, as LIGO's second director, created the LIGO Scientific Collaboration (LSC), which facilitated expanding the data analysis effort to scientists at many other institutions; see Barish's Part II of our joint Nobel Lecture.

To help educate the many hundreds of scientists who joined the LIGO effort in the late 1990s and the 2000s, I created in 2002 an online course in gravitational-wave physics that included videos of lectures about all aspects of the field, by the best experts.

By 2002, it seemed to me that I was no longer much needed within the LIGO Project. The students and postdocs I had trained, and other LSC theorists, could play the roles that I had been playing, and could do it at least as well I, if not better. So, with a sigh of relief (because by personality I did not really like working in a large project), I left day to day involvement with LIGO, and focused my attention largely on building at Caltech a research effort on computer simulations of colliding black holes and other sources of gravitational waves; see my Nobel Lecture for details.

One consequence of my departure from day-to-day LIGO work was my non-involvement in advanced LIGO and its triumphant discovery of gravitational waves.

The credit for that ultimate success, and for all the rich insights about the universe that have begun to flow from it, belongs largely to the younger generation of LIGO/Virgo scientists and engineers, and also to my Nobel Prize co-laureates Rai Weiss and Barry Barish, who have continued to make major contributions in the advanced-LIGO era.

I continue to help the LIGO Project whenever called on for help, but that is less and less often as time passes (and almost entirely on political issues and not technical issues).

My Students and Postdocs

Over the near-half-century of my career, my graduate students and postdocs have done much more important and impactful research, while in my group, than I myself. I take great pride in their accomplishments, some of which I describe in my Nobel Lecture.

In many cases they took research problems that I suggested, and with very little help from me, brought the problems into soluble form, solved them, and made major discoveries; an example is the work by Alessandra Buonanno and Yanbei Chen on quantum noise in advanced LIGO interferometers (see my Nobel Lecture). In other cases, they identified important research problems themselves and, with little concrete input from me, brought the problems to fruition, with impactful results; examples are Carlton Caves' work on the origin of quantum noise, and Yuri Levin's work on thermal noise in interferometers (see my Nobel Lecture).

I patterned my style of working with students and postdocs after the styles of Wheeler, Dicke, and Zel'dovich (which I had observed up close) and of Robert Oppenheimer in Oppenheimer's Berkeley/Caltech years (the 1930s). I gave the students a lot of room and time and freedom to explore things on their own, flounder, and ultimately find themselves, with an occasional nudge from me. But I also gave them an intellectual environment in which to learn from each other, and from students and colleagues elsewhere – an environment that included weekly group meetings typically two hours long and sometimes far longer than that, with frequent participation by experimenters from the Drever/Whitcomb group and later the LIGO Laboratory, and by outside experts. It also included frequent trips to Santa Barbara to interact with the superb relativity group that James Hartle had created there, and frequent visits to Caltech by research leaders from around the world – for example, members of Zel'dovich's and Braginsky's groups, and Stephen Hawking and members of his Cambridge research group. We had an *Interaction Room*, with a huge blackboard, a refrigerator filled with drinks, and comfortable couches and chairs, in which we would gather for spontaneous discussions as well as organized discussions.

Over my 43 years of mentoring students and postdocs, roughly 2/3 of our time and effort went into gravitational-wave-related research, largely connected to LIGO or what would become LIGO, but also connected to LISA, Weber-type bar detectors, and sources of gravitational waves in all frequency bands. I describe some of this research in my Nobel Lecture.

The other 1/3 of our time has gone into a wide range of other issues in relativistic astrophysics, or relativity, including a highly enjoyable period of several years in which we asked ourselves whether the laws of physics permit an infinitely advanced civilization to build wormholes for rapid interstellar travel and machines for traveling backward in time. (Although such questions may seem weird or flaky, they are useful tools for probing the laws of physics in domains where experiment is not yet possible. For example, our research, and that of Hawking and his students, have convinced both Hawking and me that the poorly understood laws of quantum gravity control whether or not backward time travel is possible.)

A New Career at the Interface of Science and the Arts

Since 2009 I have turned much of my effort in a very different direction: collaborations about science with artists, musicians and film makers. Christopher Nolan's movie *Interstellar* was one fruit of this, and with Stephen Hawking and my long-time Hollywood partner, Lynda Obst, I have a second science-inspired movie in the works. With the painter Lia Halloran, I am working on a book about the Warped Side of the Universe (objects and phenomena made largely or wholly from warped spacetime, most of them sources of gravitational waves). And I have been doing an occasional multimedia concert about the Warped Side of the Universe with composer Hans Zimmer and visual effects guru Paul Franklin, using beautiful videos generated by numerical relativity physicists. I take great pleasure in these collaborations with brilliant and creative artists, who bring to our joint work talents and insights quite different from my own. These collaborations are my attempt to inspire nonscientists and especially young people about the beauty and power of science, in the same way as George Gamow's book *One, Two Three, … Infinity* inspired me, 65 years ago.

My Family

This is a scientific biography, so I have chosen not to discuss my two marriages (to Linda Thorne, 1960–1975; and then to Carolee Winstein, 1984–…), nor Linda's and my children Kares Anne Thorne and Bret Carter Thorne (and his wife Regine Thorne), and granddaughter Larisa Anne Thorne. Suffice it to say that they all have been tremendously important in my life and have provided a balance to my scientific work that has helped make me more productive. They all went to Stockholm with me to share in the Nobel Week festivities.

Figure 3. My family in Stockholm, December 2017: Bret, Regine, Carolee, me, Linda, Larisa, and Kares. [Credit: The Nobel Foundation.]