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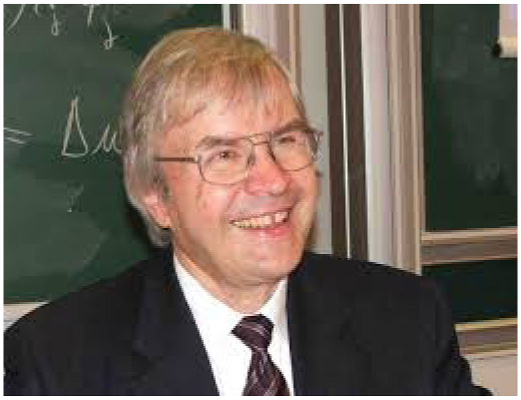

I was born in Heidelberg, Germany, on October 30, 1941. My parents had moved there from their native Breslau a few years earlier. As far as I can tell, I am the only academic in our family. My father Karl Hänsch was a businessman engaged in the export of farming machinery, while my mother Marta raised her three children as a house wife. My younger brother Julius entered the book printing business, and my sister Lucia married a fellow physics graduate student and now helps run a small electronics engineering company.

Growing up during and after the second world war left some vivid memories. I can still see our family huddled together in the basement bomb shelter of our home in Heidelberg listening to the piercing sound of air raid sirens. After the war, our family had lost its estate in Breslau, and we had to share our small ground floor apartment in Heidelberg with some war refugees as subtenants. Without childhood diversions such as television, my brother and I gained a strong sense of independence and adventure by playing in the bombed ruins at the nearby railway station or exploring the many hiking trails on the slopes of the Gaisberg and Königstuhl. My father had long been disillusioned with the Nazi political leadership and raised us as rebels in spirit, distrusting any official authority.

My father also kindled my early interest in science. During the first world war, volunteering at a pharmacy, he became interested in medicine and chemistry. In Heidelberg we lived at Bunsenstrasse 10, in the house that had once belonged to the chemist Robert Bunsen. When I was about six years old, I asked my father what Bunsen had done to have a street named after him. On the next day he brought home a Bunsen burner which we connected to the gas stove in the kitchen. With a sprinkle of table salt, the blue flame turned to a bright yellow. My father explained that this is the characteristic color emitted by sodium atoms that are excited in the flame. It was obvious to me that I had to find out more about light and atoms. A little later, my father took me to visit the metallurgical laboratory of the Heinrich Lanz AG in Mannheim, where I was impressed by researchers in white lab coats who allowed me to look into their fancy microscopes. At a time when other boys dreamt about steering steam locomotives, I started to see myself as a future scientist.

In 1952, I entered the Helmholtz Gymnasium in Heidelberg, then located at the Kettengasse in the old town below the castle. Although the school emphasized modern languages and science, my father enrolled me in a rather small class with Latin as the first language to maintain my option of studying medicine. During the later years we enjoyed some remarkable teachers. Dr. Mampel, our physics and chemistry teacher, gave me free reign of the school's collection of demonstration apparatus, and Dr. Biser, a Kaplan at the nearby Jesuitenkirche, who later became an eminent religious philosopher, turned the obligatory religious studies into a fascinating course on Western philosophy.

Early on, my interest in science dominated my activities outside school. I eagerly read popular science and science fiction books from the public library until I learned how to check out textbooks from the University library. I also liked doing experiments with my own hands. Intrigued by the world of chemistry, I started to spend my weekly allowance in pharmacies willing to sell substances like fuming nitric acid or white phosphorous to a young boy who stored his growing collection of chemicals in the bedroom of his parents. After an intimidating accident with bomb-making materials, my interests moved from chemistry to physics and electronics. Around 1957, I acquired an old cold cathode X-ray tube which I operated at home after winding a large Ruhmkorff-style induction coil. I also built a transistorized Geiger counter to perform experiments with a radioactive sample of 0.1 millicurie of Mesothorium which I had bought at a factory for radioactive luminous paint. To calibrate the Geiger counter, I went to the nuclear physics laboratory of Professor Otto Haxel at the University of Heidelberg, where an assistant was very kind and willing to introduce me to the real world of physics research. At that time, I set my sights on becoming a nuclear physicist and university professor.

Study at the University of Heidelberg

After the Abitur in 1961, I enrolled at the University of Heidelberg as a physics student. During the first two years most of my energy went to the study of mathematics. The lectures on physics and chemistry seemed like entertaining diversions by comparison. I was awed by the power and elegance of pure mathematical reasoning. But after a while I realized how much the complexity of an abstract formalism can sometimes distract from true physical insights. Since then I have acquired a compulsion to always try and construct the simplest possible intuitive model to "understand" a physics phenomenon. Such models have often helped me to perform quick order of magnitude estimates and to rapidly weed out half-baked ideas. Playing around with intuitive concepts I frequently arrive at interesting ideas only to find out that the results have been worked out long ago and are well known. But every once in a while I have experienced the immense joy that comes with some entirely new insight or invention.

After the Vordiplom in 1963, I enrolled in the Betatron laboratory of Professor Hans Kopfermann for the Grosspraktikum, an initial laboratory project of about six months. Unfortunately, Professor Kopfermann had died just before I could begin work on my assignment, the construction of a transistorized fast linear gate for a semiconductor detector of alpha particles which I quickly completed. In the spring of 1964, I attended my first meeting of the German Physical Society. Listening to different talks in a nuclear and particle physics session describing the work of large teams working at big machines, I lost some of my enthusiasm for this kind of research.

Instead, I became intrigued by the growing excitement about lasers which had been invented a few years earlier. In the neighboring Institute of Applied Physics at Albert-Überle-Strasse, Professor Christoph Schmelzer had started to design a linear accelerator for heavy ions that was later realized at the GSI in Darmstadt. Since he felt that lasers might help to synchronize the phases of the individual resonators, he had hired Dr. Peter Toschek, a former student of Professor Wolfgang Paul in Bonn, as an assistant to set up a laser group in Heidelberg. Visiting this laboratory I was awed by the sight of a helium neon laser with its glowing discharge tube emitting an intense collimated beam of red laser light that produced an otherworldly speckle pattern. I sensed a large unexplored new world, and I instantly decided to switch fields. Fortunately Peter Toschek accepted me into his group so that I could pursue my two years of diploma research on gas lasers. Since commercial lasers were not yet available, we had to build everything ourselves, including the glass discharge tubes with their electrodes and Brewster windows, the gas filling stations, the high voltage power supplies, and even the dielectric mirrors and their adjustable mounts. In hindsight, this was excellent training for a budding experimentalist. My adviser was a scholar of high intellectual standards who made sure that we kept track of every single publication in the emerging field of lasers and quantum electronics. In my diploma research, I studied saturation effects in the gas laser medium by observing the light emitted spontaneously to the side. In the end I was able to determine a number of previously unknown radiative transition rates in the neon atom.

After receiving my physics diploma degree in 1966, I continued to study laser saturation phenomena in my thesis research. I had become intrigued by the sharp central Lamb dip, a drop in laser power, that Ali Javan had first observed when scanning the frequency of a single mode gas laser across the Doppler-broadened gain profile. The Lamb dip allowed a new kind of nonlinear Doppler-free high resolution spectroscopy, albeit limited to the study of laser transitions or to the “inverted Lamb dips” produced by molecular absorption lines in accidental coincidence.

In my own experiments, I studied the cross saturation of two coupled laser transitions in neon that share the same lower level. Soon, I observed strange line asymmetries that could not be understood within a hole burning model. I tentatively ascribed the observed phenomena to Raman-like two photon transitions and the dynamic Stark effect. After laboring for considerable time as a theorist I was able to explain the observations quantitatively with a semiclassical model that relied on the density matrix formalism to account for quantum interference effects in coupled three-level systems. This work, published in 1970 with Peter Toschek, is still cited frequently today, because it laid the groundwork for the understanding of phenomena such as lasers without inversion, electromagnetically induced transparency, and slow light. In January of 1969, I received my doctor degree from the University of Heidelberg (Dr. rer. nat., “summa cum laude”), and I continued to work in Heidelberg for another year as an assistant of Professor Schmelzer. Aspects of coherence and quantum interference have remained a recurring theme in my later research, with intuitive insights from classical wave optics often guiding my thoughts and ideas.

At Stanford University

In March 1970, I left Germany to join the laboratory of Professor Arthur L. Schawlow at Stanford University as a NATO postdoctoral fellow. I had first met Art Schawlow, co-inventor of the laser, at a summer school at Carberry Tower in Scotland in 1969, and I was immediately captivated by his warmth, his keen mind, and his contagious sense of humor. Fortunately, Art agreed to take me on as a postdoc.

Arthur L. Schawlow (right) and me at Stanford University.

On my way to California, I stopped at the east coast to visit Ali Javan at MIT and Bill Bennett at Yale University. I also arranged a visit to the famous Bell laboratories at Holmdel. There, Charles Shank and Herwig Kogelnik showed me a small pulsed dye laser, pumped by a nitrogen laser made by AVCO. At a repetition frequency of 100 Hz, the beam looked almost continuous to the eye, and the color could be changed by simply tilting the angle of a diffraction grating. This was a far cry from the few pulses per minute that I had obtained playing with a simple home-built flashlamp-pumped dye laser at Heidelberg, following the discovery of lasing in organic dye solutions by Peter Sorokin and Fritz Schäfer in 1966.

When I arrived at Stanford, I told Art Schawlow about the interesting experiments at Bell Laboratories, and I proposed that I would like to try and make a nitrogen laser pumped dye laser so highly monochromatic that it could be used for Doppler-free saturation spectroscopy of gaseous absorption lines. When Art asked me how I would go about it, I explained that I would try holographic diffraction gratings, Lyot filters, etalons, or whatever else was necessary to restrict laser action to a single longitudinal mode. Art and I had already discovered that we shared a strong passion for clever gadgets. Art was sufficiently intrigued by my proposal to let me purchase an AVCO nitrogen laser, using funds from a post-Sputnik era Army contract. The nitrogen laser arrived in July of 1970 and immediately proved to be an irresistible toy. During the next six months we enjoyed some very entertaining experiments, ranging from edible lasers to dye laser image amplifiers.

Soon, I found the intellectual atmosphere at Stanford quite exhilarating. I was surrounded by legendary scientists such as Felix Bloch or Robert Hofstadter, and I could discuss laser science with some heroes of my graduate student years, including Tony Siegman, Steve Harris, and Robert Byer. At the heart of budding Silicon Valley , one could sense a “can do” atmosphere that seemed immensely liberating. Art kept pointing out that one did not have to know everything about a field in order to discover something new. If our German approach to research had resembled well-planned agriculture, the work at Stanford could be compared to game hunting.

With my instinctive aversion against organized planning, I enjoyed this atmosphere tremendously. At least, we did not have to be afraid of research results that made all planning obsolete. We soon found ourselves at the heart of a revolution in laser spectroscopy that brought plenty of such results.

Towards the end of 1970, I began to focus my efforts increasingly on the goal of making a widely tunable dye laser highly monochromatic. Like other experimenters before me, I was working with a rather small beam diameter inside the dye laser cavity. Suddenly, I realized that the spectral resolving power must be limited if only a small number of grating lines is illuminated. I happened to carry a small Zeiss monocular telescope in my pocket, which I often used to read the small print of slides or transparencies from the back of a lecture room. Quickly I mounted this telescope as a beam expander inside the cavity to fill the grating area more efficiently, and instantly I observed a dramatic improvement in the laser line width. With a larger beam expanding telescope and an additional etalon inside the cavity, the spectral width of the pulsed dye laser could be reduced to an unprecedented 0.0004 nm, and an additional external filter etalon soon permitted the first experiments on Doppler-free saturation spectroscopy of atomic resonance lines. To this end, I devised a scheme for saturation spectroscopy outside the laser cavity that was highly immune to the intensity fluctuations of our still primitive dye lasers. The technique became later known as Hänsch-Bordé method, since Christian Bordé in Paris had independently pursued similar ideas.

When Art Schawlow saw the first Doppler-free spectra of the sodium D lines which I had left on his desk after an exhilarating night, he suggested that we should do the same with the red Balmer-alpha line of atomic hydrogen. This line had been at the center of attention of atomic spectroscopists in the 1930s, because of suspected discrepancies from the predictions of the relativistic Dirac theory. With Issah S. Shahin, a graduate student from Jordan, we quickly set up an old-fashioned Wood-style hydrogen gas discharge tube, and soon we were able to resolve single fine structure components of the red Balmer line for the first time so that we could observe the 2S Lamb shift directly in the optical spectrum. A few years later, the first laser measurement of the Rydberg constant improved the accuracy of this important fundamental constant by an order of magnitude. This was the beginning of a long quest for ever higher resolution and measurement precision in optical spectroscopy of the simple hydrogen atom which permits unique confrontations between experiment and fundamental theory. This pursuit has culminated in the invention of the femtosecond laser frequency comb, a tool that is revolutionizing precision measurements of time and frequency, as recounted in my Nobel Lecture.

When word about the new tunable laser and its powers spread, an unending series of visitors began to file through our unpretentious little laboratory, and an article describing the dye laser soon became a “citation classic.” This experience taught me that a simple and imperfect proof-of-principle experiment can sometimes find a much wider resonance than a complex experiment of intimidating perfection. In 1973, Art Schawlow and I were named “California Scientists of the Year” by the California Museum of Science and Industry in Los Angeles for this work. At the same ceremony, William Hewlett and David Packard were honored as “California Industrialists of the Year”. With such recognition, it became easy for me to clinch tenure as Associate Professor at Stanford. Soon, I received offers of full professorships from the University of Heidelberg, Yale University, and Harvard University. In the end, I decided to remain at Stanford, accepting a promotion to full professor in 1975, and I continued to work close to Art Schawlow for another 11 years before returning to my native Germany in 1986. Our early work with hydrogen was prominently cited when Art Schawlow received the Nobel Prize for laser spectroscopy in 1981.

Many of my other highly cited papers from the Stanford years describe relatively simple experiments such as ultrasensitive fluorescence spectroscopy with the power to detect the light from single atoms, sensitive intracavity absorption spectroscopy with a multimode dye laser, the first demonstration of continuous wave Doppler-fee two-photon spectroscopy, and the experiments with my student Carl Wieman on Doppler-free polarization spectroscopy. Even the roots of the laser frequency comb can be traced to the exhilarating seventies at Stanford. With my student Jim Eckstein and visiting Lindeman Fellow Allister Fergusson we used the comb of regularly spaced longitudinal modes of a mode-locked sub-picosecond dye laser to measure some fine structure intervals of atomic sodium.

Discussing possible ways to increase the interaction time of hydrogen atoms with a laser beam, Art Schawlow and I came up with the idea of laser cooling of atomic gases in early 1974. Vladilen Letokhov in Troisk was one of the first to start experiments with one-dimensional radiation pressure cooling of a sodium atomic beam. It took ten more years until Steve Chu and his team at Bell Laboratories realized “optical molasses”, demonstrating 3-dimen-sional Doppler cooling as envisioned in our original proposal. Considering the dramatic subsequent developments, Art and I have often regretted that we did not immediately follow up our proposal with our own experiments. However, we did not know how to laser cool hydrogen atoms, and there were many other interesting things to do that seemed much easier. Art still remembered some unhappy experiences with atomic beam machines during his thesis research at the University of Toronto and advised against any experiments involving serious vacuum. In 1978, my former thesis adviser Peter Toschek and his group were the first to demonstrate the related method of laser cooling of trapped ions that had been proposed in 1975 by Hans Dehmelt and David Wineland.

At Stanford in the mid-seventies, one could smell the beginning microcomputer revolution, ignited by the early microprocessors introduced by the nearby Intel Corporation. After soldering together my own IMSAI computer from a kit and advancing from binary programming by flipping switches to assembly language, I became an avid visitor of the weekly meetings of the Stanford Hombrew Computer Club, where Bill Gates sold rolls of punched paper tape with 4k and 8k versions of ALTAIR BASIC. Steve Jobs, the later founder of Apple Computer came to my undergraduate classes on electricity and magnetism. In 1976, Art Schawlow and I bought one of the first Apple I Computers, a bare printed circuit board, at the Mountain View Byte Shop. Both Art and I succumbed to the microcomputer fever, spending a forbidding amount of personal money on a growing collection of computers and peripherals, so that our offices started to look like space mission control centers. Art sometimes joked about our role as early adopters: “The pioneers are the ones with the arrows in their backs.” Around 1980, Art and I even started a small mail-order software business, selling a little graphics program Autoplot written in BASIC for the Radio Shack TRS80 computer. Sales were brisk until the hardware became obsolete.

My long exposure to the challenging intellectual climate at the Stanford Physics Department played an important role in shaping my own academic values. I witnessed an Assistant Professor who ruined his chance for tenure by writing a book reviewing science rather than pursuing original research. I could easily live with such priorities because doing research is what I love most. For the same reason, I have always resisted trading academic freedom for the power that comes with administrative and management responsibilities, even though I am grateful to colleagues who shoulder such burdens.

At Munich and Garching

In 1978, Professor Herbert Walther at the University of Munich invited me to spend a sabbatical in Germany with an Alexander von Humboldt Senior US Scientist Award. Herbert had played a key role in promoting laser science in Germany. Since 1976, he was directing a Project Group for Laser Science in Garching that became the Max-Planck Institute of Quantum Optics in 1981. A few years later he helped to organize a very tempting lure to make me return to my native Germany. In March 1986, after agonizing over the decision for almost two years, I accepted an offer to join the Ludwig-Maximilians University of Munich as a Professor of Experimental Physics and to build a new Division of Laser Spectroscopy at the Max-Planck-Institute that was just about to move into an attractive new building at the southern end of the Garching research campus. The University laboratories which I inherited from my predecessor, Professor Josef Brandmüller, where located downtown, at Schellingstrasse 4, in the Max-Vorstadt, surrounded by bookstores and small restaurants. Since I felt that the downtown location would make it easier to attract graduate students, I decided to set up laboratories at both locations.

With considerably more space, positions for assistants and graduate students, and ample start-up funds at my disposal, it was obvious that I could pursue many more projects than before, but I could no longer maintain my Stanford style of closely working with a small group of graduate students, getting intimately involved in every detail of one or two hot experiments. At first, this adjustment felt rather painful. I had to rely much more on the initiative and judgement of my students and postdocs, resigning to the expectation that they would make expensive mistakes. Fortunately, I have been able to attract some exceptionally gifted young coworkers, sometimes with an energy, patience, and discipline that far exceeds my own. The best young people blossom if they feel free to follow their own ideas. Therefore, I try to guide my students in rather subtle ways, letting them arrive at their own insights, goals, and research plans during our discussions, while I show enthusiasm and excitement when they move in a promising direction. Once the fire is lit it tends to become self-sustaining, and I am rather proud that more than thirty of my former students and postdocs are now Professors at universities around the world, running their own laboratories. Since I did not want to give up my own hands-on experimental work, I established my own small "toy" laboratory at the University, equipped to set up improvised laser and optics experiments quickly, to explore crazy new ideas and to stay abreast of ever advancing technologies.

Karl Ludwig Kompa, Herbert Walther, Siegbert Witkowsky and me at the new Max-Planck-Institut of Quantum Optics in Garching in 1986.

Shortly before moving to Munich in April 1986, I had met Gerd Binnig, the co-inventor of the scanning tunneling microscope, at the laboratory of Cal Quate at Stanford. Gerd told me that he was planning to move to Munich to set up an IBM physics group as an advance guard for an envisioned new IBM research laboratory. Since I was intrigued by the possibility of combining tunneling microscopy and laser spectroscopy, I offered Gerd to set up shop for a few years at our downtown university laboratory. In October of 1986, we could help celebrate the Physics Nobel Prize to Gerd Binnig and Heinrich Rohrer. Even though IBM soon abandoned the plans for a Munich laboratory, Gerd and his group stayed with us for the next ten years. During this time we pursued some serious research on scanning microscopy ourselves, including studies of biomolecules on graphite surfaces with Wolfgang Heckl, and a proof-of-principle demonstration of an aperturte-less scanning optical near field microscope with Johannes Pedarnig. Unfortunately, the combination of tunneling microscopy and laser spectroscopy did not advance as easily as hoped. When Gerd Binnig returned to the IBM Rüschlikon laboratory in 1996, scanning microscopy had evolved into a large worldwide enterprise and we would have had to concentrate considerable resources to compete effectively. Instead, we decided to abandon our microscopy research.

At that time the quantum physics of ultracold atoms had become an important focus of our research. With Andreas Hemmerich we were the first to demonstrate and explore two- and three-dimensional optical lattices bound by light in the early 1990s. With Tilman Esslinger, we explored new tricks for laser cooling below the recoil limit. After Immanuel Bloch joined us as a graduate student in 1997, we realized Bose-Einstein condensation (BEC) of rubidium atoms in a novel magnetic QUIC trap in February of 1998, as only the second group outside the USA. We later exploited the high magnetic stability of our small trap in the first continuous wave atom laser that dominated German science news in 1999.

In the same year, Markus Greiner joined our group as a diploma student. He constructed a new BEC apparatus transporting cold atoms magnetically from a magneto-optical trap into a high vacuum glass cell where BEC can be achieved without the need for any further laser cooling.

Continuing his work as a Ph.D. student, Markus Greiner used the unobscured optical access to load the Bose-Einstein condensate into a three-dimensional optical dipole force lattice potential formed at the intersection of three orthogonal far-detuned standing wave laser fields. In 2001, he and Immanuel Bloch were the first to demonstrate a reversible quantum phase transition from a wave-like superfluid atomic state to a particle-like Mott insulator crystal, by simply adjusting the height of the lattice potential wells, as theoretically predicted in 1998 by Dieter Jaksch and Peter Zoller. This realization of a strongly correlated quantum gas has triggered an avalanche of work at the interface between atomic physics and condensed matter physics, with the prospect of quantum simulators for elusive phenomena ranging from antiferromagnetism to high Tc superconductivity.

Starting around 1994, we have also been exploring microscopic magnetic traps for the manipulation of cold paramagnetic atoms. Such traps can produce large field gradients and field curvatures without the need for strong currents. From the beginning I was intrigued by the prospect of tayloring complex magnetic potentials with lithographically fabricated circuit patterns, combining traps, wave guides, and other atom optical elements to create a quantum laboratory on a chip. Discussing such ideas with some of my coworkers in a Schwabing restaurant, I sketched a proposal for a conveyor belt for cold atoms onto a napkin. Graduate student Wolfgang Hänsel soon modeled the magnetic fields on his computer and concluded that such a device might work. In 1996, Dr. Jakob Reichel had joined our laboratory after receiving his doctorate with Christoph Salomon and Claude Cohen-Tannoudji at the ENS in Paris. Together with Wolfgang Hänsel, and later joined by Peter Hommelhoff, he set out to realize such an atom motor chip. With the mirror-MOT we found an effective method for loading cold atoms into microscopic traps, and soon we could demonstrate a working conveyer belt for atoms. In June 2001, we were the first to achieve Bose-Einstein condensation entirely on a microfabricated atom chip. More recently, Philip Treutlein has demonstrated long atomic coherence times close to the chip surface, raising the prospects for accurate atomic clocks and for quantum information processing on a chip.

One fascinating line of research at the MPQ until today has been pursuit of precision laser spectroscopy of the simple hydrogen atom. Numerous graduate students have advanced the state of the art during their thesis research in Garching, starting in the late 1980s with Reinald Kallenbach, Claus Zimmermann, Ferdinand Schmidt-Kaler, and Martin Weitz. With a beam of cold hydrogen atoms and a highly stabilized continuous laser source of 243 nm, we could greatly improve the resolution of the sharp 1S-2S two-photon resonance. Thanks to the efforts of Thomas Udem we later learned to build optical frequency interval dividers which made it possible to measure the optical frequency of the ultraviolet 1S-2S resonance against the infrared frequency of a transportable methane-stabilized He-Ne laser. This intermediate reference was shuttled to the PTB in Braunschweig many times for calibration with an elaborate harmonic laser frequency chain. These experiments led to a new Rydberg constant, tests of QED, such as a precise measurement of the Lamb shift of the 1S ground state, and accurate determinations of the rms charge radius of the proton and the structure radius of the deuteron. Krzysztof Pachucky, Savely Karshenboim and Ulrich Jentschura have provided essential theoretical support.

Starting in 1997, our efforts to measure the frequency of laser light led to the vastly simplified approach of the femtosecond laser frequency comb, as recounted in my Nobel Lecture. In a proof-of-principle experiment in the fall of 1998, we used a commercial mode-locked femtosecond laser with a comb spanning 70 THz to compare the frequency of a blue dye laser directly with the microwave frequency of a commercial cesium atomic clock in our own laboratory. In June of 1999, we reached a precision of 14 decimal digits in a comparison of the 1S-2S frequency with the transportable cesium fountain clock built at BNM SYRTE in Paris. A second such measurement in February 2003, took advantage of an even simpler octave spanning laser frequency comb synthesizer as first demonstrated in late 1999 in Boulder, CO and at Garching. Frequency comparisons of this kind allow sensitive searches for possible slow variations of fundamental constants.

Our laboratory has long been a partner of the ATRAP collaboration, one of two international teams working at CERN with the goal of applying precision laser spectroscopy to anti-hydrogen, searching for conceivable differences between matter and antimatter.

Over the years, I have enjoyed the hospitality of many universities around the world hosting me as a visiting professor or lecturer. I am particularly grateful to Professor Salvatore Califano and Massimo Inguscio at the University of Florence, Italy, who gave me the opportunity to participate in the creation of the European Laboratory for Nonlinear Spectroscopy (LENS) and to teach a course on laser spectroscopy to Ph.D. students at the University of Florence in the enchanting hills of Arcetri. A serendipitous experiment with Dr. Marco Bellini at LENS in February of 1997, on the coherence of white light continuum pulses produced with an amplified femtosecond laser was actually the crucial step that convinced me that an octave-spanning optical frequency comb synthesizer could be realized.

During my Stanford years, I had enjoyed the liberating climate of entrepreneurship that was omnipresent in the heart of Silicon Valley. Fortunately the old aversion between academia and industry is now waning in Germany, and in 2001, my former students Ronald Holzwarth and Michael Mei have taken the risk of starting a spin-off company, Menlo Systems GmbH, to develop commercial frequency comb synthesizers. The name has been inspired by Menlo Park in New Jersey where Thomas Alva Edison invented the light bulb.

Sometimes people comment on my growing collection of prizes and awards, wondering if I am pursuing research in order to win still more prizes. The honest answer is that I like prizes to reassure our sponsors that their money is being spent well. Prizes are also important as recognition and source of pride and motivation for our team. However, the most important reward for me has always been the joy that comes from new insights, discoveries, and inventions.